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Working Paper **6220**

NBER WORKING PAPER SERIES

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Working Paper 6220
http://www.nber.org/papers/w6220

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
October 1997

Glaeser thanks the National Science Foundation for financial support through a grant to the National Bureau of Economic Research; Luttmer thanks the Olin Foundation. Helpful comments were provided by Denise DiPasquale, Lawrence Katz, Ellen Meara and Sendhil Mullainathan. Justin Funches provided superb research assistance. This paper is part of NBER's research program in Public Economics. Any opinions expressed are those of the authors and not those of the National Bureau of Economic Research.

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NBER Working Paper No. 6220
October 1997
JEL Nos. R20, D45, H80
Public Economics

ABSTRACT

When there are binding price controls, there are shortages and the allocation of goods across consumers may not be efficient. In general, the misallocation costs of price controls are first order, while the classic welfare losses due to undersupply are second order. This paper presents an empirical methodology for estimating the degree of misallocation of housing units due to rent control in New York City. This methodology involves comparing the relative consumption of different demographic groups within the rent controlled area with the relative levels of consumption in a free market area. Our best estimate of the costs of rent control in New York due to the misallocation of rental apartments is 200 dollars per apartment annually.

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I. Introduction

Most research on rent control has focused on the extent to which rent control causes undersupply in the housing market (e.g. Olson, 1972, Fraser Institute, 1975, Downs, 1988, Gyourko and Linneman, 1989). Scholarly work on the minimum wage and other price controls has similarly focused on the effects of these controls on the overall quantity of jobs or goods (e.g. Card and Krueger, 1995, Murphy and Welch, 1996). The basic welfare analysis of price control, shown in Figure 1, is at this point a canonical ingredient of most introductory microeconomics classes. Most discussions of rent control limit themselves to the social costs of undersupply.[1]

Of course, for many years economists have known that the social costs of price controls are not limited to the undersupply of rental units (Hayek, 1931, Friedman and Stigler, 1946). Frankena (1975) focuses on the distortions to the supply of housing quality by landlords. Barzel (1974) and Cheung (1974) examine the social costs of queues or other rent-seeking behavior that can arise when goods in short supply are rationed. These costs are surely considerable and have received empirical attention (Deacon and Sonstelie, 1989, Olson, 1988).

This paper focuses on a possibly pernicious cost of rent control that has received much less theoretical analysis and almost no empirical analysis: the misallocation of housing across consumers.[2] The price mechanism is a relatively effective means of distributing goods of all types, including rental apartments, across individuals. When there are heterogeneous consumers, using prices to allocate goods efficiently is particularly important (Weitzman, 1974). In the free market, people who are willing to pay more than the cost of an apartment are allocated apartments, whereas those who are not, do not live in apartments. However, when rent controls are set so that demand exceeds supply, some mechanism for rationing apartments, such as queues, must be found: this rationing device may not allocate the apartments

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

[2] Luttmer (1997) provides an empirical analysis of the efficiency of the job rationing induced by the minimum wage. There do not appear to be major welfare losses from job misallocation due to the minimum wage in the U.S. labor market

efficiently. The analysis illustrated by Figure 1 implicitly assumes that rationing under rent control will ensure that those consumers who want apartments the most, actually receive apartments.[3] We do not believe that perfect allocation can be assumed without empirical support.

If goods are not efficiently allocated across consumers, then the classic welfare analysis is wrong and empirical work that limits itself to thinking about undersupply will underestimate the true welfare cost of price controls. In a simple model, we show that the welfare losses due to undersupply are second order while the welfare costs due to misallocation are first order. Therefore, for small enough impositions of price controls, misallocation must be more important than undersupply.

This paper begins by presenting a basic welfare analysis of rent control when there is a shortage of rental apartments and rental apartments are not necessarily perfectly allocated. This analysis follows Stiglitz (1979), Deacon and Sonstelie (1989), Hubert (1991), and especially Suen (1989), all of whom have presented various theoretical analyses of misallocation losses due to rent control. We present an alternative graphical analysis to Figure 1 that illustrates the first order losses from rent control. Following this analysis, we discuss the misallocation of different types of apartments across individuals (e.g. big apartments going to single or childless renters). Rent control leads to misallocation of this form for three reasons: (1) undersupply leads to a lottery which may misallocate apartments even if the relative prices of different types of apartments in the rent controlled sector are set correctly, (2) the relative prices of apartments in the rent controlled sector may be set incorrectly even if there is no undersupply and (3) the incentives that rent control creates, which limit mobility, may lead to a mismatch between individuals and apartments if individuals' tastes change over time.

The misallocation costs of rent control are not just of theoretical interest. A large anecdotal literature discusses how the wrong people end up in large but

[3] 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.

cheap New York City apartments. For example, Auletta (1975) describes the "Tobacconist to the World" Nat Sherman who rented a six room Central Park West apartment for 335 dollars per month. Sherman said of the apartment "it happens to be used so little that I think [the rent is] fair." This large apartment was allocated to someone who used it so little that the marginal cost of the apartment seemed close to the renter's marginal value of the apartment. Since this rent was far below market rates, this certainly implies inefficient allocation.

However, anecdotes give us little insight into how many apartments are misallocated or the magnitude of the misallocation costs in dollars. There exists no empirical literature attempting to measure these losses. This paper presents a methodology for assessing the degree of misallocation in price controlled areas. As our theory focuses on the heterogeneity of consumers, our empirical analysis starts by considering the distribution of tastes for apartment attributes in each demographic subgroup. Our primary assumption is that the overlap in the distribution of tastes across subgroups is constant over space.

This assumption implies that the overlap in the observed distribution of housing consumption for any two subgroups should be the same in any two competitive markets.[4] For example, if in one location the median member of subgroup A demands the same number of bedrooms as the 75th percentile member of subgroup B, then in other locations the median member of subgroup A and the 75th percentile member of subgroup B will have the same demand for bedrooms as well, subject to sampling error. In a market where price controls cause misallocation, the overlaps in the distributions of housing consumption of demographic subgroups will differ from the overlaps in free markets. This difference in overlaps allows us to infer a lower bound for the amount of misallocation in the rent controlled market.

The idea of our methodology is illustrated in Table 1. This table illustrates the overlap in the consumption of bedrooms between households from

[4]When employing our methodology on placebo groups (e.g Chicago or groups of high density cities), we find no evidence of misallocation in this groups, which supports the validity of this assumption.

various groups. We summarize the extent of overlap by the probability that a random member of group B is consuming strictly more bedrooms than a random member of group A. Conditional on our primary assumption, these probabilities should not be different for New York City than for cities without rent control, as long as the allocation in New York is efficient. However, as Table 1 shows, when we calculate these probabilities for the rent controlled sector of New York and compare them with equivalent probabilities for non-rent controlled renters outside of New York, the probabilities often differ.[5] For example, the first row shows that when we compare one person households with three or more person households, only 5 percent of the time does a random one person household inhabit a strictly bigger apartment than a random three or more person household in the non-rent controlled apartment buildings outside of New York City. In the rent controlled units of New York City, the comparable figure is 7.6 percent.

The second row shows that the probability that an apartment without children is bigger than an apartment with children is 10.3 percent in the non-rent controlled sector outside New York. In the rent-controlled sector of New York, the comparable number is 15.6 percent. If we do the same comparison for high school dropouts and college graduates, we find that outside New York the chance that a high school dropout is living in a bigger apartment than a college graduate is 28.8 percent. In the New York rent-controlled sector, this probability is 39.4 percent. For the other two comparisons, the differences are also significant.

These overlaps in housing consumption between different groups provide the basis for our estimation strategy, but to actually estimate dollar losses, we will need more specific assumptions.[6] For example, we assume a simple quadratic utility function to calculate the dollar losses from misallocation. As some of the differences in the overlap between groups in consumption may be caused by sampling error, we use a simulation technique to estimate the expected amount of differences in consumption overlap across cities given

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[5] To insure some comparability of the housing stock and for other reasons, we have restricted the sample to include only individuals living in buildings for six or more apartments (see section III for further explanation).

[6] Furthermore, in Table 1 the subgroups are quite coarse. Our full methodology is based on much finer subgroups which allows for a more precise characterization of overlaps.

that the underlying overlap in tastes is constant. Standard errors are found using the Efron (1979) bootstrap. In all cases, we find substantial and significant misallocation of rental housing and of the total number of bedrooms in New York City and we estimate that the lower bound of deadweight loss from misallocation of number of bedrooms is 32 dollars annually per rent controlled apartment in New York or more than 40 dollars annually per rent controlled or owned apartment in New York if we include the social losses due to misallocation of rental apartments (relative to owned apartments). These losses are over and above any welfare costs stemming from supply distortions or rent-seeking behavior.

These calculations only include misallocation in observed housing attributes and assume that the allocation across unobservable individual characteristics is efficient. Adjusting for these other factors could increase the deadweight loss from misallocation per apartment with six or more units in New York to more than 200 dollars annually, which would mean that misallocation is costing the city more than $200 million per year.

II. Theoretical Discussion of Misallocation under Rent Control

In this section, we explore the social costs due to misallocation under rent control. The first subsection deals only with the misallocation between owning and renting; the latter subsections deal with the misallocation of rental units among consumers.

1. An Analysis of Misallocation under Price Controls

This discussion is quite general and relates to the welfare costs of any price control. The ideas here are relatively old (e.g., Friedman and Stigler, 1946) and even the formalization has clear antecedents (Stiglitz, 1979). The supply-side of this market is characterized by a rising aggregate cost function, $\Pi(S)$, where S is the total number of rental apartments supplied. The marginal cost of an additional rental apartment is denoted $\pi(S)$, which is also rising with S.

There is a continuum of potential consumers who rent at most one unit of a stock of homogenous rental apartments. Each consumer is indexed with a real positive number i starting at zero; the density of consumers at each index number is one. Each consumer places a value, denoted $\theta(i)$, on renting an apartment where $\theta'(i)<0$, so that consumers are ordered by decreasing desire for the unit. These consumers have utility functions that are linear in a composite commodity, which includes non-rental housing, and which has a price of 1. Consumer utility is therefore $U(C,Y)=C+\theta(i)H$, where C is the composite commodity, H is a dummy variable that equals one if the consumer rents an apartment and zero otherwise. If consumer i* is indifferent towards consuming the apartment, then all consumers with $i \leq i^*$ will desire to consume the apartment at the given price, and the demand for the apartment will be i*.

In the free market equilibrium, consumers will continue to rent apartments until the point that $\theta(i^*)=\pi(i^*)$, where i* denotes the index number of the marginal consumer and also the total number of consumers that have rented an apartment. The value placed by the marginal consumer on a rental apartment must equal the marginal cost of supplying that consumer with a housing unit. In a free housing market, the total social welfare from the housing is $\int_{i=0}^{S^*} \theta(i)di - \Pi(S^*)$, where we use S* to denote the free market supply of the good that satisfies $\theta(S^*)=\pi(S^*)$ and $S^*=i^*$.

We now consider a price control that specifies a maximum rent of $\overline{R}$ which is below the free market rent. The quantity supplied, denoted S, will therefore equal $\pi^{-1}(\overline{R})$. Since quantity demanded at this price, denoted $D=\theta^{-1}(\overline{R})$, exceeds supply for any binding price control, we must assume an allocation mechanism that assigns apartments to individuals who want them. Two reasonable benchmarks are (1) that apartments will be allocated efficiently (which might happen if there was a pseudo-price mechanism such as waiting on line) and (2) that apartments are allocated completely randomly.[7]

[7] Indeed, random allocation is not an extreme assumption. It is entirely possible that individuals with less demand for these apartments could have an edge in receiving them, so that apartments are allocated away from individuals who want them most. An example of this phenomenon could be if transient consumers desire rental apartments most, but long term residents find it much easier to get those apartments.

Each of these benchmarks is a possible outcome of the allocation process if we assume that a fraction of the apartments (λ) are randomly assigned only to those individuals who would be living in the apartments given an efficient allocation (i.e. individuals for whom $i \leq S$) and a fraction of the apartments ($1-\lambda$) are randomly allocated among all consumers who want an apartment at its rent controlled price (i.e. individuals for whom $i \leq D$). Thus, the fraction of individuals receiving an apartment among those for whom $i \leq S$ equals $\lambda+(1-\lambda)S/D$, and the fraction of individuals receiving an apartment among those for whom $i>S$ equals $(1-\lambda)S/D$. When $\lambda=1$ the allocation is perfect and when $\lambda=0$ apartments are assigned randomly.[8]

The total social welfare created by the housing market under rent control is:

$$\text{(1)} \qquad \int_{i=0}^{S} \left(\lambda + (1-\lambda)\frac{S}{D} \right) \theta(i)di + \int_{i=S}^{D} (1-\lambda)\frac{S}{D}\theta(i)di - \Pi(S)$$

The marginal change in social welfare caused by a reduction in $\overline{R}$ is:

$$\text{(2)} \qquad \lambda \frac{\partial S}{\partial \overline{R}}(\theta(S) - \overline{R}) + (1-\lambda)\left(\frac{1}{D}\frac{\partial S}{\partial \overline{R}} - \frac{S}{D^2}\frac{\partial D}{\partial \overline{R}} \right) \int_{i=0}^{D} \left(\theta(i) - \overline{R} \right) di$$

The first term of the expression represents the marginal social losses from undersupply, which go to zero as the controlled rent goes to the free market rent. As such, these losses are second order. The second term represents the marginal social losses from misallocation and does not go to zero as the level of rent control becomes insignificant. As such, these losses are first order, which means that the total social costs of rent control will be first order whenever $\lambda<1$. In addition, for impositions of rent control sufficiently close to the free market price, the social losses due to misallocation must be larger than the social losses from undersupply.

Viewed another way, the deadweight losses from misallocation are:

[8] We assume throughout this section that it is impossible for individuals to trade apartments after they have been assigned. While this assumption appears to capture the reality of most rent controlled markets, there are certainly exceptions, and in these cases the misallocation costs are lower.

(3) $$\int_{i=S}^{S^*}(\theta(i)-\pi(i))di+(1-\lambda)S\left[E\left(\theta(i)\middle|i\leq S\right)-E\left(\theta(i)\middle|i\leq D\right)\right]$$

The first term represents the standard deadweight loss from underproduction. The second term represents the losses that accrue because the consumers who end up buying the apartments are not the consumers who would be living in the apartments under the most efficient allocation process. The welfare losses are shown in Figures 1 and 2. Figure 1 shows the classic triangle loss from the imposition of a rent control (i.e. assuming $\lambda=1$). Figure 2 shows the losses when there is random allocation across consumers (i.e. $\lambda=0$). In Figure 2, there is a classic welfare loss triangle, but there is also a welfare loss area that is formed between the demand curve and the expected valuation of consumers who want the apartment at the rent controlled price.

The connection between social losses and demand elasticity changes when we consider the social costs of misallocation (see Glaeser, 1996, for more details). Under efficient allocation, more inelastic demand yields higher social losses, since the welfare loss triangle under the demand curve grows larger. Under random allocation, more inelastic demand also means that fewer individuals will compete for housing, and this will decrease the extent of misallocation and the size of welfare losses. In the case of linear demand and completely random allocation, the two effects balance each other perfectly, and there is no connection between the elasticity of demand and the welfare losses from rent control.

The previous section considered apartments as a homogeneous good and considered only the losses that come from the "wrong" people renting and owning. The next sections focus on the misallocation of apartments among renters due to rent controls disturbing relative costs or creating barriers to mobility.

2. The Wrong Apartments

The empirical focus of this paper is on the misallocation of apartments across renters rather than on who gets these apartments. There are three separate reasons why rent control could disturb the allocation of apartments. First, the relative prices of different types of apartments could be warped by the presence of rent control (e.g. rent control reduces the cost of luxury apartments more than the cost of low quality apartments). Second, even if the relative price differences between different apartments are set efficiently, the rationing of apartments in short supply could still lead to misallocation of apartments. Third, rent control typically inserts significant moving costs, that come about either because landlords can raise rents when there are new tenants or because the presence of shortages may lead to a thin market that creates higher search costs which stymie mobility. Moving costs increase misallocation if individual demand changes over the life-cycle.

We illustrate these different mechanisms in three separate subsections. Throughout, we assume that there are two types of rent controlled housing, A and B, that are supplied in fixed quantities S_A and S_B after the imposition of rent control. The uncontrolled market has a perfectly elastic supply of both types of housing at higher free market rents. There are two types of consumers, H and L; the consumers of types H and L number Q_H and Q_L respectively. Type H individuals are willing to pay a premium of θ_H to live in type A housing rather than type B housing; the willingness of type L individuals to pay for type A housing relative to type B housing is θ_L. Type H individuals prefer type A housing more than type L individuals so $\theta_H>\theta_L$. The cost of providing type A housing relative to type B housing is such that H types would be living in type A housing and L types would be living in type B housing in a Pareto optimum. Misallocation losses occur whenever type H individuals inhabit type B housing or type L individuals inhabit type A housing.

We will let ΔR denote the price difference between A and B housing in the rent controlled sector; this price will be efficient as long as $\theta_H>\Delta R>\theta_L$, so H types will want to consume type A housing and L types will want to consume type B housing at the going price.

a. Efficient Prices ($\theta_H>\Delta R>\theta_L$) and Undersupply ($Q_H+Q_L>S_A + S_B$)

We assume that the relative prices of the two types of apartments are efficiently set, but also that the rent control binds so that there is a shortage of these apartments at the rent controlled prices. Individuals receive rent controlled apartments through a lottery. Just as in the first section, we assume a lottery structure which can range from being perfectly efficient (so that only H types are allocated A apartments and only L types are allocated B apartments) to being completely random (so any individual has the same chance of getting either type of apartment). An efficient lottery allocates type A apartments only to type H individuals and type B apartments only to type L individuals, which could result if individuals signed up to receive only one apartment. A random lottery allocates all apartments to all individuals.

We assume that a fraction λ of the both population types enters the efficient lottery and a fraction $1-\lambda$ enters the random lottery. Therefore, type H individuals receive a rent controlled type A apartment with probability $\lambda \frac{S_A}{Q_H} + (1-\lambda)\frac{S_A}{Q_H + Q_L}$, and a rent controlled type B apartment with probability $(1-\lambda)\frac{S_B}{Q_H + Q_L}$. Symmetrically, type L individuals receive a rent controlled type B apartment with probability $\lambda \frac{S_B}{Q_L} + (1-\lambda)\frac{S_B}{Q_H + Q_L}$ and a rent controlled type A apartment with probability $(1-\lambda)\frac{S_A}{Q_H + Q_L}$. Individuals who do not receive a rent controlled apartment will rent the appropriate apartment for them in the uncontrolled sector. The social losses from the lottery scheme come from H types in B apartments and L types in A apartments and total $(1-\lambda)(\theta_H - \theta_L)\frac{S_B Q_H + S_A Q_L}{Q_H + Q_L}$.

b. The Right Quantities ($Q_H=S_A$ and $Q_L=S_B$), but Distorted Prices ($\Delta R<\theta_L$)

As long as $\theta_H > \Delta R > \theta_L$, then only H types will want type A housing at the going price. There is no misallocation because only high demand consumers will end up receiving that type of housing.[9] However, when $\theta_L > \Delta R$, then all consumers will want type A housing. If a pure lottery is used to ration off

[9]The absence of misallocation is an artifact of the discreteness of H and L types and there would be some misallocation with all price distortions if there was a continuum of types.

this housing, then the same fraction $S_A/(Q_H+Q_L)=S_A/(S_A+S_B)$ of each type will receive type A housing. The total social losses from misallocation will equal the social loss incurred for each misallocated individual times the total number of individuals who are misallocated, or $(\theta_H-\theta_L)Q_HQ_L/(Q_H+Q_L)$. Thus, when rent control distorts the relative prices of apartments, then there will also be misallocation.

c. Right Prices, Right Quantities and Barriers to Mobility

In this case, we assume that $\theta_H>\Delta R>\theta_L$, $Q_H=S_A$ and $Q_L=S_B$, and the misallocation comes only from barriers to mobility. We now consider a dynamic model where the probability that individuals of each type leave the city is $(1-\delta)$ in each period. A new inflow of potential tenants occurs at each time period so that the size of the city and the city's composition between type H and type L consumers is constant over time. Since the rental differential is priced correctly, the inflow of new tenants will distribute itself efficiently; new type H consumers will live in type A apartments and type L consumers will live in type B apartments.[10]

Following the institutions of rent control, we assume large moving costs for individuals who want to change apartments. In New York, the ability of landlords to raise rents when tenants move is much higher than when tenants stay. As a result, the gap between the charged rent and the true market rent is much higher for long term tenants than for new tenants, which creates a strong incentive to stay in the same residence. Rather than explicitly model these costs, we will assume that individuals are unable to move apartments after they have entered their apartment. This immobility is important because there is a probability (denoted P_{HL}) that type H consumers will become type L consumers and also a probability (denoted P_{LH}) that type L consumers will become type H consumers. Changing types may represent individuals having children or having children leave the home, or any other demand change, which can be either stochastic or deterministic.

[10]Consumers are farsighted and realize that at some point their tastes may change. However, we assume that it still makes sense to choose one's currently preferred housing type. In practice, individuals often choose intermediate housing types to accommodate their expected housing demand (see Sinai, 1997).

We assume that the probabilities are such that the type H consumers who become type L consumers are exactly equal in number to the type L consumers who become type H consumers, which requires $P_{HL}Q_H=P_{LH}Q_L$.

Solving for the number of type L consumers living in type A housing in the stationary equilibrium yields $\frac{\delta Q_H Q_L P_{LH}}{(1-\delta)Q_H + \delta(Q_H + Q_L)P_{LH}}$. The total welfare loss due to misallocation is θ_H-θ_L times this amount. This social loss will be rising with δ, since new consumers are allocated efficiently and old consumers are not, and the social loss will be rising with P_{LH}, since it is the transitions between groups that causes the misallocation. The essence of this model is that the lock-in effect of rent control, combined with the fact that consumers' tastes change over the life cycle, implies that longer-term residents will particularly tend to live in inappropriate apartments in rent-controlled cities.

This section has outlined three mechanisms by which rent control can lead to misallocation of apartments across consumers. The next section describes our empirical approach to estimating the social losses due to misallocation induced by rent-control in New York City.

III. The Basic Empirical Framework and Preliminary Results

For our empirical methodology, we assume that individual i's utility function is linear in non-housing consumption and quadratic in housing consumption:

(4)
$$U_i = C_i - \sum_j \frac{\alpha_j}{2}\left(\theta_{ij} - H_{ij}\right)^2$$

where U_i is individual i's utility, C_i is the individual's non-housing consumption, H_{ij} is individual i's consumption of housing characteristic j and θ_{ij} reflects the individual's taste for characteristic j. As we will later emphasize, quadratic utility is not necessary for some of our results. In a free market equilibrium, individual i can choose any housing characteristics that satisfy the budget constraint $Y_i \geq C_i + \sum_j P_j H_{ij}$, which implies that $H_{ij} = \theta_{ij} - \frac{P_j}{\alpha_j}$, or that housing characteristic j and the taste for that characteristic are the same, modulo a constant that depends on price. We assume that prices are constant within a city. For simplicity, we consider only a single housing characteristic at the time and hereafter drop the j indicator.

Tastes are a function of the full range of an individual's characteristics, but in practice, we will only observe a subset of the individual's attributes, which we denote X_i. Therefore, we will define an error term μ_i so that $\theta_i = \theta(X_i) + \mu_i$. We assume that this error term is normally distributed with a variance, $\sigma_\mu^2(X_i)$, that depends on the individual's characteristics. Observed housing consumption is measured with an error term, v_i, which is also normally distributed with mean zero and variance $\sigma_{v_j}^2$. We define a total error term $\eta_i \equiv \mu_i + v_i$, which is normally distributed with variance $\sigma_\eta^2(X_i) = \sigma_\mu^2(X_i) + \sigma_v^2$. Thus, observed housing levels satisfy $H_i = \theta(X_i) - \frac{P}{\alpha} + \eta_i$.

Average housing consumed by an individual with characteristics X_i in a free market equals $\theta(X_i) - \frac{P_j}{\alpha}$. The differences across groups in the average level of housing consumed will be independent of price. The first step in our methodology will be to estimate the relationship between housing attributes and individual characteristics in the non-rent controlled markets outside of New York and the same relationship within the rent-controlled sector of New York City. In other words, we will run a regression of the form:

$$(5) \qquad H_i = Intercept_{MSA} + X_i'\beta_{US} + X_i'(\beta_{NY} - \beta_{US}) \bullet I_i^{NY} + X_i'(\beta_{CHI} - \beta_{US}) \bullet I_i^{CHI} + \eta_i,$$

where $Intercept_{MSA}$ reflects a metropolitan statistical area (MSA) specific intercept which will capture the effect of different prices, I_i^{NY} is an indicator function that takes on a value of one if the individual lives in New York, and I_i^{CHI} is an indicator function that takes on a value of one if the individual lives in Chicago and a value of zero otherwise. The coefficients β_{NY}, β_{CHI}, and β_{US} reflect the connection between individual attributes and housing consumption in the rent controlled sector of New York, in Chicago and in the remaining cities of the U.S. respectively. The model suggests that these coefficients will be the same if housing in New York is efficiently allocated and different otherwise. We have split Chicago apart from the rest of the U.S. because Chicago is the closest city to New York in size and structure, and we will treat it as a placebo.

Data Discussion and Preliminary Results

The housing consumption function derived from equation (5) is estimated using a sample consisting of renters in metropolitan areas living in buildings with 6 or more apartments. The use of the building size cutoff is chosen because rent control and stabilization are quite common in New York for apartment buildings with more than 6 units (65.1 percent of this sample reports having rent control in the New York City Housing and Vacancy Survey), but much rarer in smaller buildings (less than 3 percent of smaller rental building units report rent control). This distinction follows from a series of rent control and stabilization laws that essentially exclude apartment buildings with fewer than 6 units from rent control. For comparability, we also limit our non-New York sample to these larger apartment buildings.[11]

The construction of the data is described in the Data Appendix. We have combined American Housing Survey (AHS) data for 1993 with data from the 1993 New York City Housing and Vacancy Survey. Our decision to use two distinct, but quite similar, data sets was based on a desire to have a larger data sample for New York than the AHS provides.

The means and standard deviations of the variables that are used in the regressions are included in Table II; further means and standard deviations are shown in Appendix Table I. New York has a very similar number of total bedrooms per unit as the other two samples, but a much higher level of total maintenance problems. The means of the explanatory variables are reasonably similar across the samples.

[11]Restricting our attention to large apartment buildings makes sense because it ensures that we are looking at relatively high density areas similar to New York.

The second page of Table II shows the means for variables of interest which are not actually part of the regressions that we run. The mean and median rents for New York City are reasonably close to the rest of the nation, although New York is somewhat more expensive. The difference in rental costs becomes far more striking if we look at free market rents in New York.

Table III shows the results of the regression when the number of bedrooms is the dependent variable. A variety of coefficients are different between rent-controlled New York and the rest of the U.S. The effect of age on housing is positive after age 35 for New York and zero for the U.S. sample. The effect of income is quite different and much weaker in New York City. The effect of education on housing size is also much lower in New York.[12] Overall, the coefficients for the New York sample are quite different economically and statistically (as shown by the F-test on the bottom of the table), suggesting at least the possibility that rent control has distorted who consumes bigger housing units in New York.

The results for Chicago show no difference between that city and the U.S. sample. No individual coefficient is significantly different, and jointly the coefficients are quite statistically similar. However, the smaller sample size in Chicago may be responsible for our finding no significant differences.

The second regression repeats this procedure where total number of maintenance problems is the dependent variable.[13] Again, the New York coefficients are quite different from the national coefficients, while the Chicago coefficients are essentially the same as the coefficients for the rest of the U.S. The third regression shows results of a probit model of the renter/owner decision. While most of the coefficients are not significantly different when comparing the New York and the U.S. sample, the joint

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[12]One explanation for this fact is that better educated individuals in New York are living in neighborhoods where the price per room is higher. Future research will hopefully deal with this possibility which would require us to drop the assumption of a common price per room within each city.

[13] This variable is the sum of six zero-one variables indicating the presence of particular maintenance problems such as holes in walls and leaks. It is described further in the Data Appendix.

significance test shows that overall New York is quite different. Chicago is only marginally significantly different from the U.S. sample.

While these results are suggestive, they do not actually estimate the social losses from misallocation due to rent control. They also are dependent on the functional form assumptions that are inherent in linear regressions and this technique has no means of correcting for differences in the supply of housing attributes in different markets.[14] The next section expands the procedure so that we can deal with these issues and present actual dollar estimates of misallocation losses due to rent control.

IV. Methodology for Estimating Misallocation Costs

While the previous section provided suggestive evidence for the existence of misallocation, this section attempts to measure the size of that misallocation. The methodology hinges on finding individuals who appear to be consuming the wrong level of housing in New York and using existing estimates of the curvature of the utility function to estimate the gains that would accrue if misallocated individuals traded housing units. We take the supply of housing in New York City as given and will not examine any social losses due to undersupply. The purpose of the analysis is to measure whether the existing stock of housing is misallocated.

While we still assume a quadratic utility function for estimating the size of any social losses, this methodology is able to test for the existence of any social losses without that assumption. Our primary identifying assumption is that the amount of overlap of consumption across groups is constant over space. Figure 3 shows two distributions for two hypothetical subgroups of the population and the extent to which they overlap. We assume that this overlap does not change across cities except when there is misallocation. Changes in the overlap of consumption between groups due to changes in the overlap of tastes may be incorrectly identified as social losses due to misallocation.

[14] The assumption of quadratic utility makes supply differences irrelevant.

Estimating Misallocation

Estimating equation (5) provides us with coefficients, β_{US} and β_{NY}, that relate observable characteristics to mean housing consumption for particular types of individuals. We estimate how observable characteristics relate to consumption variances separately for the U.S. and New York by estimating regressions $\ln(\hat{\eta}_i^2) = X_i'\delta + \xi$, where $\hat{\eta}_i$ is the residual from equation (5).[15] We denote the predicted variance of housing demand for an individual with attributes X_i as $e^{X_i'\delta_{US}}$ if the U.S. coefficients are used and $e^{X_i'\delta_{NY}}$ if coefficients for New York City are used. The U.S. coefficients should reflect the distribution of housing demand in a free market. Because individuals' housing demand and housing valuation differs only by a constant (which drops out in the subsequent analysis), we can interpret the mean and variance of housing demand based on the U.S. regressions as the mean and variance of true housing valuations. The New York coefficients may not reflect actual valuations and can only tell us about the observed distributions of housing consumption.

At this point we follow the data and treat the attributes as available only in discrete units. In an efficient allocation, for every value of k, everyone consuming an apartment with k+1 or more units of the housing attribute must value the attribute at least as much as every individual consuming an apartment with k or fewer units of the attribute. Furthermore, the fraction of individuals allocated to an apartment of size k must equal the share of apartments of size k. These conditions are reflected in the following equation:

$$S(k) = \sum_i \omega_i F(\theta^*(k) | X_i'\beta_{US}, e^{X_i'\delta_{US}}) \text{ for all k,} \quad (6)$$

where $S(k)$ indicates the share of apartments in New York with k or fewer units of the housing attribute, ω_i is the share of the population with characteristics X_i and $F(.|a,b)$ is the cumulative distribution function of a normal random variable with mean a and variance b. This equation yields a

[15]We use ordinary least squares in the first stage and not generalized least squares because the efficiency gain from generalized least squares may be limited (or even negative) for noisy estimates of the covariance structure.

solution for $\theta^*(k)$, which can be interpreted as the highest level of valuation at which individuals should be consuming k or fewer units of the housing attribute given the existing supply and an efficient allocation of housing units. Equivalently $F(\theta^*(k)|X_i'\beta_{US}, e^{X_i'\delta_{US}})$ is the fraction of the New York population with characteristics X_i who should be consuming apartments with k or fewer units of the attribute if the allocation of apartments was efficient.

For example, suppose that there are just two equally sized subgroups in the population (X_1 and X_2) and that housing units are split equally among 3 size groups (k=1, 2, and 3). Figure 4 shows where the cutoff valuations $\theta^*(1)$ and $\theta^*(2)$ lie, and what fraction of each subgroup live in units of each size in the efficient allocation.

We can also use a similar equation:

$$S(k) = \sum_i \omega_i F(\Theta_{NY}(k)|X_i'\beta_{NY}, e^{X_i'\delta_{NY}}) \tag{7}$$

to define $\Theta_{NY}(k)$. By using $F(\hat{\theta}_i(k)|X_i'\beta_{US}, e^{X_i'\delta_{US}}) = F(\Theta_{NY}(k)|X_i'\beta_{NY}, e^{X_i'\delta_{NY}})$, we now also define $\hat{\theta}_i(k)$ for each subgroup, where $\hat{\theta}_i(k)$ represents the actual marginal valuation implied by actual consumption in New York City.[16] Equation (7) implicitly assumes that within a given demographic subgroup, individuals with greater unobservable demand for the housing attribute always consume more of the attribute. We refer to this assumption as perfect sorting on unobservable personal tastes. If $\beta_{US} = \beta_{NY}$ and $\delta_{US} = \delta_{NY}$, then $\hat{\theta}_i(k) = \theta^*(k)$ for each subroup. If the actual cutoff valuations $\hat{\theta}_i(k)$ vary across different subgroups, housing units must be inefficiently allocated. For example, if there are two subgroups (1 and 2) with $\hat{\theta}_1(k) > \hat{\theta}_2(k)$, then there are members of subgroup 1 who are consuming k units who would value the k+1th unit more than some members of subgroup 2 who are consuming k+1

[16]Alternatively, we could have used the more direct methodology of finding $\hat{\theta}_i(k)$ so that $F(\hat{\theta}_i(k)|X_i'\beta_{US}, e^{X_i'\delta_{US}})$ equals the share of the group that actually consume k or fewer units of the housing characteristic. Our procedure is similar to this, but essentially smoothes the data and accepts that the observed housing allocations are drawn from a particular normal distribution.

units. By reallocating apartments with k+1 units from subgroup 2 individuals with valuations just above $\hat{\theta}_2(k)$ to subgroup 1 individuals with valuations just below $\hat{\theta}_1(k)$, and compensating individuals appropriately, a pure efficiency gain is achieved.

Figure 5 explains graphically how $\hat{\theta}_i(k)$ is found in a hypothetical housing market. It shows the cumulative distribution function of the valuations for hypothetical subgroup i. This valuation distribution is estimated on observed housing demand of members of subgroup X_i in the national sample, but applies also to members of subgroup X_i in New York because valuation distributions are invariant across space by assumption. The right vertical axis shows the fractions of subgroup X_i that occupies housing units of each size. We are assuming that the members of the subgroup are efficiently allocated among themselves, i.e. there is efficient sorting on unobservables. Using this assumption, we find the actual cutoff valuations $\hat{\theta}_i(1)$ and $\hat{\theta}_i(2)$ implied by these fractions. By comparing these cutoff valuations with the cutoff valuations $\theta^*(1)$ and $\theta^*(2)$ which are implied by an efficient allocation among all subgroups combined, we determine the proportion of subgroup X_i that is misallocated as well as the valuations of those individuals who are misallocated.

Indeed, as we have shown how to determine values of $\hat{\theta}_i(k)$, we can calculate cases in New York where $\hat{\theta}_i(k)$ differ across subgroups and as a result there is opportunity for Pareto improving trade. Table IV gives a matrix which illustrates these trades. The first column gives the actual number of bedrooms consumed by a set of individuals and the second column shows the number of bedrooms that should be consumed by these individuals. The third column shows the share of the population that falls within each of these groupings. For example, the first row shows that 6.1 percent of all households should be consuming 0 bedrooms and are consuming 0 bedrooms. As the matrix indicates, we find few cases where misallocation is off by more than 1 bedroom.

In part, finding few major misallocations follows from our assumption of efficient sorting on unobservables, i.e. agents with the same observable attributes who live in larger housing units are assumed to have higher unobserved valuations. We will later discuss accounting for inefficient allocations among agents with equivalent observable but different unobservable characteristics. The fourth and fifth columns show results from our later calibration that gives the dollar deadweight loss per household and the contribution of each class of household to the aggregate deadweight loss from misallocation.

The total deadweight loss from misallocation is:

$$
(8) \quad \begin{aligned}
&\sum_i \omega_i \int_{\theta=-\infty}^{\infty} \big(U(\textit{Efficient Housing},\theta) - U(\textit{Actual Housing},\theta)\big) f(\theta | X_i'\beta_{US}, e^{X_i'\delta_{US}}) d\theta = \\
&\sum_i \omega_i \sum_k \left(\int_{\theta=\theta^*(k-1)}^{\theta^*(k)} V(k,\theta) f(\theta | X_i'\beta_{US}, e^{X_i'\delta_{US}}) d\theta - \int_{\theta=\hat{\theta}_i(k-1)}^{\hat{\theta}_i(k)} V(k,\theta) f(\theta | X_i'\beta_{US}, e^{X_i'\delta_{US}}) d\theta \right)
\end{aligned},
$$

where V(k,θ) represents the utility from consuming housing units k for an individual with tastes θ, i.e. $-\alpha(\theta-k)^2$, where we drop any utility coming from consumption of other goods or housing attributes. Due to the separability and quasi-linearity of the utility function, the misallocation costs are independent of the prices people actually do pay, and we can simply drop all other consumption from the deadweight loss calculations.

The first line of equation (8) is the definition of deadweight loss from misallocation: the difference between the utility received in the efficient allocation and the utility that is received in the actual allocation. The second line defines both the efficient and the actual consumption in terms of the previously derived $\theta^*(k)$ and $\hat{\theta}_i(k)$ terms, where the $\theta^*(k)$ term gives valuation cutoffs that would occur in the efficient allocation given New York's existing housing stock and efficient allocation and the $\hat{\theta}_i(k)$ term gives the cutoffs that explain the observed consumption levels in New York.[17]

Calibrating the Curvature of the Utility Function

[17]The existence of measurement error in the measurement of housing characteristics does not itself lead to a fallacious estimate of deadweight losses, since we have accepted that some fraction of the variance in housing consumed across individuals comes from mismeasurement. The crucial assumption is that the amount of measurement is the same in New York and in the United States as a whole.

As we have calculated $\theta^*(k)$ and $\hat{\theta}_i(k)$ terms, as well as estimates of β_{US} and $e^{X_i'\delta_{US}}$, we only lack estimates of the utility function's shape, i.e. the parameter α. Rather than estimating a value for this parameter directly, we will base α on a range of existing estimates of the elasticity of housing demand. Housing demand elasticities usually refer to the demand for a composite of housing services, whereas for our calibration we need the curvature of the utility function for specific housing characteristics. To derive these curvatures, we assume that the increase in each of the housing characteristics is proportional to the increase in the composite housing services, where the proportions are based on the amount of dispersion in the observed levels of consumption of that characteristic.[18]

More precisely, we start with a hedonic relationship $H=\sum_j p_j H_j$, where H is total housing services measured in dollars, p_j is the cost of each housing attribute and H_j is the quantity of each housing attribute. We assume that an overall change in housing increases consumption of attribute k by $\phi_k = p_k\sigma_k \big/ \sum_j p_j\sigma_j$, where σ_k is the standard deviation of housing attribute k. If we consider an equiproportionate increase in the prices of all components of housing services, so $dp/p = dp_j/p_j$ for all attributes j, and use the fact that the derivative of demand for attribute k with respect its price is $1/\alpha_k$ then:

$$\alpha_k = \left(\frac{dh_k}{dp_k}\right)^{-1} = \left(\frac{dh_k}{dH}\bullet\frac{dH}{dp}\bullet\frac{dp}{dp_k}\right)^{-1} = \frac{p_k}{\phi_k\varepsilon_p^H H}, \tag{9}$$

where the prices are found by running hedonics for the national, non-rent controlled sample, and the standard deviations of the various housing subcomponents are also based on the national sample. For our key attributes, the estimates are $\alpha_{bedrooms}=66.9$ and $\alpha_{maintenance\ problems}=23.5$, when we choose an

[18]Alternatively, one could assume that the proportions are based on the levels of consumption. Unfortunately, this is difficult because some attributes such as bedrooms or total maintenance problems often have zero values. An additional advantage to using dispersion instead of levels is that the calibration is less sensitive to relabeling. For example, it makes no difference whether we define each maintenance problem as a disamenity of whether we define the maximum number of possible maintenance problems minus the actual number of maintenance problems as a positive amenity, while such a change would matter if we used levels to determine the proportions.

overall demand elasticity of .5.[19] Rather than attempt to calibrate the relevant curvature for the rent/own decision, we simply assume parameters that $\alpha_{own} = 100$. We believe that it is reasonable that the curvature in the demand for ownership is slightly higher than the curvature in the demand for one bedroom, but we recognize that this is a somewhat arbitrary assumption. Different demand elasticity assumptions will change these α estimates following equation (9). As deadweight losses are a multiple of the estimated α, different demand elasticity estimates just involve multiplying by a constant.

Standard Errors and Correcting for Sampling Error

We use the Efron (1979) bootstrap procedure to generate standard errors for the deadweight loss estimates. We draw with replacement from our original sample of U.S. and New York observations a new sample of exactly the same size. This new sample differs only from our original sample due to sampling error, and hence, if we apply our estimation procedure to the new sample, the resulting deadweight loss estimate only differs from our original estimate due to sampling error. Repeating this procedure 25 times and computing the standard deviation of the resulting estimates yields the standard error for our original estimate.

Even if the housing allocation in New York City is completely efficient, the mismeasurement of coefficients due to sampling error would lead one to mistakenly find misallocation in New York. To address this problem, we also use a bootstrapping correction to account for the fraction of the deadweight loss attributable to sampling error.

Using our estimates of the mean and variance of the valuation distribution based on the U.S. sample, we draw a sample of housing valuations from this distribution with the same size and individual characteristics as the New York sample. We efficiently allocate the actual housing supply in New York

[19] If we instead assumed that housing characteristics react proportionately to their levels of consumption we would have found that $\alpha_{bedrooms} = 68.67$ and $\alpha_{maintenance\ problems} = 123$, so for bedrooms which are the primary focus of our results, it makes no difference which methodology is used.

among this sample. These housing consumption bundles are efficiently allocated, but they differ from the consumption bundles predicted by the true U.S. coefficients because we have introduced sampling variation in the overlaps between subgroups. For the U.S. sample, we introduce sampling variation in the overlaps between subgroups by drawing with replacement from our original U.S. sample a new sample of the same size as the original sample. We then calculate the deadweight loss using our basic procedure outlined above by comparing the new U.S. sample and the sample of data that we have just generated for New York with an efficient housing allocation.

The deadweight loss that we calculate is due only to sampling error and not due to any actual misallocation and therefore is a noisy estimate of the artificial deadweight loss due to sampling error. By replicating the previous steps 25 times, we obtain the average deadweight loss that would be found solely due to sampling error, as well as a confidence interval around this average. Finally, we subtract this average deadweight loss due to sampling error from our basic deadweight loss estimate.

V. Results

Table V shows our deadweight loss estimates for bedrooms and maintenance problems assuming a demand elasticity of .5. While we always allow a separate New York intercept for housing prices, we do not generally allow each separate MSA outside of New York to have a separate intercept, rather we allow there to be MSA-specific random effects for the nation as a whole.[20] The first column shows our basic deadweight loss estimates from the misallocation of bedrooms and maintenance problems. As these estimates do not correct for sampling error, they must be positive.

The New York estimates show a loss of 43.6 dollars annually per apartment due to the misallocation of bedrooms. The misallocation due to maintenance problems is much smaller: 11.8 dollars per apartment annually per rental apartment. If we examine misallocation across renters and owners in New York, we see that the rent/own distortion is approximately 15.6 dollars

[20] Appendix Table II shows that these results are robust to MSA fixed effects or having no random effects in the regressions for the U.S. sample.

annually. The second column shows the correction for sampling error. This correction is generally around 10 dollars annually per apartment for the misallocation of bedrooms and around 15 dollars annually per apartment for the misallocation of maintenance problems.

The corrected losses are in the third column. There are still sizable losses due to misallocation of bedrooms: approximately 30 dollars per apartment annually. There is almost no correction to the rent/own distortion which is 13 dollars per apartment annually. Of course, the deadweight loss from the rent/own decision is based on our somewhat arbitrary assumption about the value of α_{own} and therefore should be accepted cautiously. Both of these deadweight losses are quite statistically significant. However, the correction for maintenance problems shows that these losses become insignificantly negative after performing the correction. Therefore, we conclude that rent control causes a misallocation of apartment space but not of maintenance problems in apartments. For the remainder of the welfare loss estimates, we will set the losses due to maintenance problems equal to zero.

In a sense, our findings confirm the value of this methodology relative to the regressions shown in Table III, which suggest misallocation both among bedrooms and maintenance problems. By using an adjustment for city housing supply that does not rely on the assumption of a quadratic utility function and by correcting for sampling error, we find no welfare losses from maintenance problems but the welfare losses from bedroom and renter/owner misallocation remain.

The second panel in the table divides the sample into individuals who have moved into apartments recently and individuals who are long time residents. Following the suggestion of one of the models in Section II, a primary distortion of rent control may consist of individuals being tied to particular apartments. We find that there are losses due to misallocation of bedroom within both groups, but the losses are almost three times as large for long term residents. This supports the idea that the misallocation costs of rent control are in a large part due to the incentives set in place to encourage individuals to stay in the same apartment.

The third panel repeats our procedure for three placebo groups. First, we repeat the exercise splitting the U.S. sample in half and treating one-half of the sample as the free market sector and one-half of the sample as the rent controlled sector. While this exercise does show welfare losses before our correction, after correcting we find negligible misallocation in the false treatment group. Second, we examine misallocation losses among renters in Chicago. Again without the correction there are misallocation losses, but after correcting there are none. Finally, we use the owner sample for New York City and the nation to test the possibility that there is just something unusual about New York that yields these losses. We do find large misallocation losses, but they are not statistically significant for this sample.

Table VI investigates whether the differences in housing allocation in New York that we associate with misallocation due to rent control are actually due to other characteristics of New York, such as population, density or region. The first panel illustrates our approach. We split the U.S. sample based on distance from the east coast.[21] The eastern half of the sample is closer regionally to New York. If our deadweight loss estimates are due to New York being an older, east coast city, then we would expect to see much smaller deadweight losses when we compare New York with the eastern half of the U.S.

As the first two lines in Table VI illustrate, the deadweight losses are slightly lower when the eastern sample is used as the control group. However, the deadweight losses when using the eastern and western groups as controls are quite close in economic magnitude and do not differ statistically. The third line shows that when we use the western sample as a control and the eastern sample as a placebo, we find no significant deadweight loss, which suggests that regional differences alone do not create deadweight losses when using our methodology.

The remainder of the table splits the U.S. sample using a variety of other criteria: share of the households in the MSA living in buildings with 5 or

[21] To increase precision, this division is based on the Census Summary Tape File information not the American Housing Survey. As before, all of our deadweight loss estimates consider only apartments in buildings with 6 or more units.

more apartments (to capture housing stock differences), number of households in the MSA living in buildings with 5 or more apartments, population and density. In all four cases, we measure lower deadweight losses when comparing New York with the share of the U.S. sample that it most greatly resembles, which suggests that some of the deadweight losses may occur because New York is different along many dimensions from the rest of the U.S. However, in all cases there is still a significant and sizable deadweight loss when we compare New York with the share of the U.S. that resembles New York most. Furthermore, when we use the share of the U.S. that differs from New York as a control and the share of the U.S. that resembles New York as a placebo, we never find significant deadweight losses. All in all, we believe that our procedure does not find welfare losses from misallocation among groups where we would expect no such misallocation.

Correcting for Unobservables and Different Elasticities

The first panel of Table VII shows the magnitude of the misallocation losses for different elasticities. As mentioned earlier, changing the demand elasticity only involves multiplying the misallocation losses by a constant. We used a range of housing demand elasticities ranging from .1 to 1. This range generally includes the plausible values estimated by numerous authors.[22] The losses for bedrooms range from 157 dollars, for highly inelastic housing demand to 15.7 dollars for an elasticity of one. The losses from the rent/own decision range from 6.6 to 66 dollars per apartment annually.

The deadweight loss calculations above only measure misallocations across individuals with different observable determinants of housing valuations. The assumption that of efficient allocations among individuals with the same observable characteristics means that our deadweight loss estimate is a lower bound for the actual deadweight loss. As an alternative, we assume that the misallocation among individuals with the same observable

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[22]Hanushek and Quigley (1980) find demand elasticities that are approximately .5 for low income renters using a controlled housing experiment. Polinsky and Ellwood (1979) estimate an elasticity of .7. Rosen (1985) and Poterba (1992) write that the consensus estimate of uncompensated demand elasticities for owner occupied housing is about 1.

characteristics is as severe as the misallocation across individuals with different observables. The fraction of variation in housing demand that can be explained by observable characteristics is given by the R^2 in the regression of housing demand in the control group. This means that unobservable characteristics at most account for a fraction of $1-R^2$ of the variation in housing consumption.[23] If the deadweight loss is proportional to the amount of variation in housing demand, then the loss due to unobservables is $(1-R^2)/R^2$ times the deadweight loss from misallocation due to observables. Therefore our upper bound estimate of the total misallocation deadweight loss is equal to $1/R^2$ times the deadweight loss from misallocation based on observable characteristics.

The second panel of Table VII gives estimates assuming that individuals are as misallocated on their unobservable characteristics as they are on their observable characteristics. In the case of bedrooms, the correction triples the deadweight loss from misallocation and total losses range from 49 to 490 dollars per apartment annually. In the case of the rent/own decision, observables explain much less of the decision and the factor of multiplication is approximately 9. This correction means that the deadweight loss from allowing the wrong individuals to be renters rangers from 55 to 550 dollars annually.

If we believe that there are other housing characteristics, other than bedrooms, which are also misallocated due to rent control then the overall losses should be multiplied even further. Indeed, a true upper bound estimate might be to multiply the deadweight loss of bedrooms by the extent to which bedrooms explain the total housing value. The R^2 of a regression of rental cost on bedrooms outside of New York City is 17 percent, so such an exercise would yield total deadweight losses of 200 to 2000 dollars per apartment annually. As there are approximately 1.5 million apartments in buildings with six or more units in New York, this implies that total social losses from $300 million to $3 billion. These figures are perhaps unrealistic

[23] Measurement error in housing characteristics could also explain part of the remaining variation. Because we ignore this measurement error, the resulting estimate is an upper bound on the misallocation costs.

but still suggest possibly huge welfare losses from the misallocation of apartments due to rent control in New York City.

V. Conclusion

While our theoretical discussion and empirical work represent a preliminary foray into this topic, we believe that it provides further evidence on the deleterious effects of price controls. Price controls are not simply another means of redistribution with second order welfare losses that can be safely used for relatively small interventions. Instead, price and rent controls eliminate the ability of the price mechanism to allocate goods efficiently across consumers and the resulting misallocation can lead to sizable social losses. Our estimates suggest that this misallocation of bedrooms leads to a loss in welfare which could be well over $500 million annually to the consumers of New York, before we even consider the social losses due to undersupply of housing or rent-seeking behavior. In addition, the misallocation of other housing attributes that we have not estimated could increase this loss significantly.

Because the social costs of price controls are so high, we believe that the policy debate over ending rent control in New York must move beyond the redistribution issues that have long stymied an effective end to rent control. In most discussions, the end to rent control is envisioned as a massive windfall for landlords and a loss for many current tenants. However, there is no reason why this needs to be true. Current tenants could be given the tradable right to rent their apartment at a fixed rate, which they could then sell to the landlord. This would keep the distribution of economic rents as it currently stands, but also end many of the inefficiencies of rent control, especially if most landlords bought back the right to set apartment rents. Any number of possible solutions exist which could end rent control while not taking anything away from current tenants. There is no reason to sacrifice the large social gains from ending rent control because reformers wish to use the end of rent control as an opportunity for redistribution between tenants and landlords.

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Data Appendix

The source of the data for New York City is the New York City Housing and Vacancy Survey from 1993. The NYCHVS is conducted approximately every three years by the Census Bureau and is sponsored by the New York City Department of Housing Preservation and Development in order to obtain data to comply with New York City's rent control laws. The data for all other cities come from the American Housing Survey (AHS) of 1993. Because both these data sources are collected by the Census, many questions have identical wording and coding.

The NYHVS of 1993 has a sample size of 15800 apartments. Eliminating buildings with fewer than 6 units (5848), public housing (1781) and records with missing data (2477), leaves a sample of 741 privately owned apartments, 393 uncontrolled rental apartments, and 4560 rent-controlled apartments[1]. The AHS of 1993 has a sample size of 64998 apartments, of which 49326 are inhabited and interviewed. Eliminating observations outside MSAs (23787), in trailers, tents etc. (444), in New York City (1863), in buildings with fewer than 6 units (18115), with rent-control (231) and in public housing (421), leaves a sample of 559 privately owned apartments and 3906 uncontrolled rental apartments. Summary statistics of these samples are provided in appendix table I.

[1] In the category rent-controlled apartments, we include all apartments that face controls on rent or type of occupant. This includes buildings regulated by Article 4 or 5, the Loft Board, pre 1947 rent stabilization, post 1947 rent stabilization, the Mitchell Lama laws and rent-control. The Mitchell Lama laws also regulate a number of cooperatives in which the initial down payment and monthly carrying charges are made affordable to middle-income families to which these cooperatives are restricted. 166 Mitchell Lama cooperatives are included in the rent-control sample.

	New York City Housing and Vacancy Survey (1993)	American Housing Survey (1993)
Tenure	The values are taken from the variable "Control Status Recode". The Rent Control Status is determined by a two-phase coding procedure performed by the Census. In the first phase the control status is taken from the administrative records of the New York State Division of Housing and Community Renewal. For apartments for which administrative records were missing or were out-of-date, information about the apartment (year built, date moved in, number of units in building, tax benefits and whether the building is a cooperative of condominium) is combined with rent control laws to infer the rent control status. We classified "Owner occupied conventional", "Owner occupied private cooperative" and "Owner occupied condo" as "Owner occupied". We classified "Other rental" as "Free market rental". We classified "Article 4 or 5 Building", "Loft board regulated", "Stabilized pre 1947", "Stabilized post 1947", "Mitchell Lama rental", "Mitchell Lama cooperative" and "controlled" as "Rent-controlled". We classified "Public Housing", "HUD regulated" and "In Rem" as "Publicly owned housing"	The variable "Tenure" identifies owner occupied and rental apartments. Apartments are classified as rent-control if respondent indicated that the apartment was rent-controlled (from the variable "rcntrl"). Apartments are classified as public housing based on the respondent's answering affirmative to the question whether the apartment was "owned by a public housing authority". (from the variable "proj").
Bedrooms	Topcoded at 9 bedrooms in the NYCHVS. For the regressions and DWL estimation framework we topcode it at 3 bedrooms to avoid sensitivity to outliers. This affects less than 1% of the sample.	Topcoded at the 97th percentile in the AHS. For the regressions and DWL estimation framework we topcode it at 3 bedrooms.

Maintenance Problems	This is the number of affirmative answers to the following six questions about maintenance problems. The questions are the same in the NYHVC and the AHS. The 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 of 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?".	
Total Rooms	Topcoded at 8 rooms in the NYCHVS.	Topcoded at the 97th percentile in the AHS
Year Built	Midpoints from the following categories: 1900 and earlier (midpoint: 1890), 1901-1919, 1920-1929, 1930-1946, 1947-1959, 1960-1969, 1970-1979, 1980 and later.	For 1980 or later, the exact year. Before 1980, midpoints from the following categories are used:1919 or earlier (midpoint 1910), 1920-29, 1930-39, 1940-49, 1950-59, 1960-69, 1970-74, 1975-78.
Rent	This is the monthly contract rent. Rents above $2200 are assigned to a value of $2700, which is to the median of rents above $2200.	This is the monthly contract rent. Rents are topcoded at the 97th percentile.
Value	The respondent's estimate of how much the apartment would sell for if it were for sale. Any non-residential portions of the property are excluded from the estimate. The question is only asked if the unit was acquired within 5 years of the survey. Topcoded at $1 million.	The respondent's estimate of how much the apartment would sell for if it were for sale. Topcoded at the 97th percentile.
Persons in Unit	Number of persons in the household. Topcoded at 7 to avoid outliers in the regressions and DWL estimation.	Number of persons in the household. Topcoded at 7 to avoid outliers in the regressions and DWL estimation

Number of Children	Number of persons younger than 18 in the household. Topcoded at 6 to avoid outliers in the regressions and DWL estimation.	Number of persons younger than 18 in the household. Topcoded at 6 to avoid outliers in the regressions and DWL estimation.
Age	The mean age of the head of the household and his/her spouse, if a spouse is present.	The mean age of the head of the household and his/her spouse, if a spouse is present.
Education	The maximum education of the head of the household and his/her spouse, if a spouse is present. Education in the NYHVCS lists degrees obtained, and is easily classified into "HS Dropout", "HS Graduate", "Some College" (which includes Associate Degree) and "College or more".	The maximum education of the head of the household and his/her spouse, if a spouse is present. Education in the AHS is coded in terms of grades attended. Persons with the highest grade attended of 11 or less are coded as "HS Dropout". Those with a highest grade attended of 12 are coded as "HS Graduate", those with a highest grade attended of 3 years of College are coded as "Some College" and the remainder is coded as "College or more".
Household Income	The income of all household members (including non-relatives) in the calender year prior to the survey. Topcoded at \$ 1 million.	
Moved In	Year head/reference person moved in.	

Figure 1: The Basic Welfare Losses from Rent Control

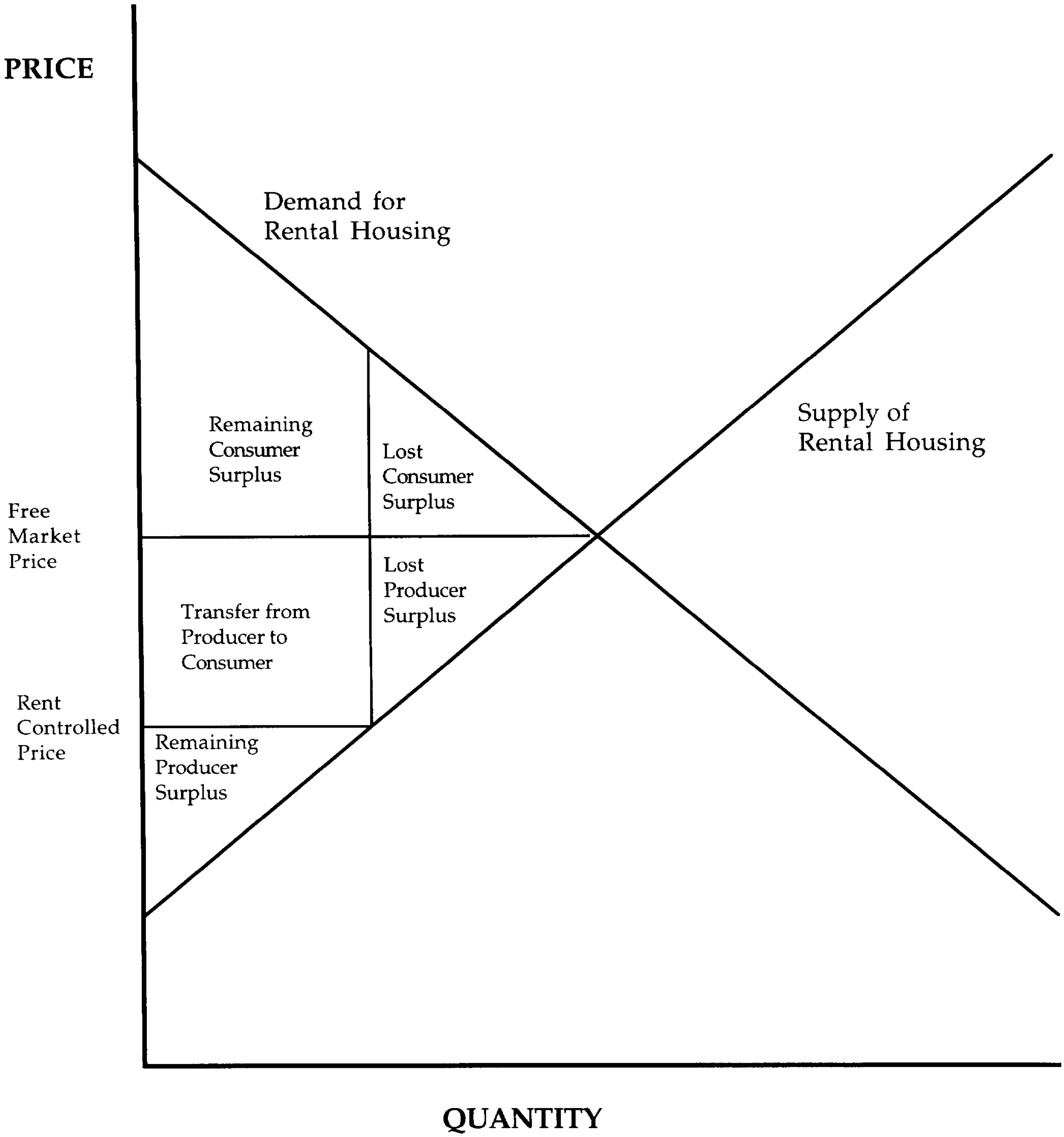
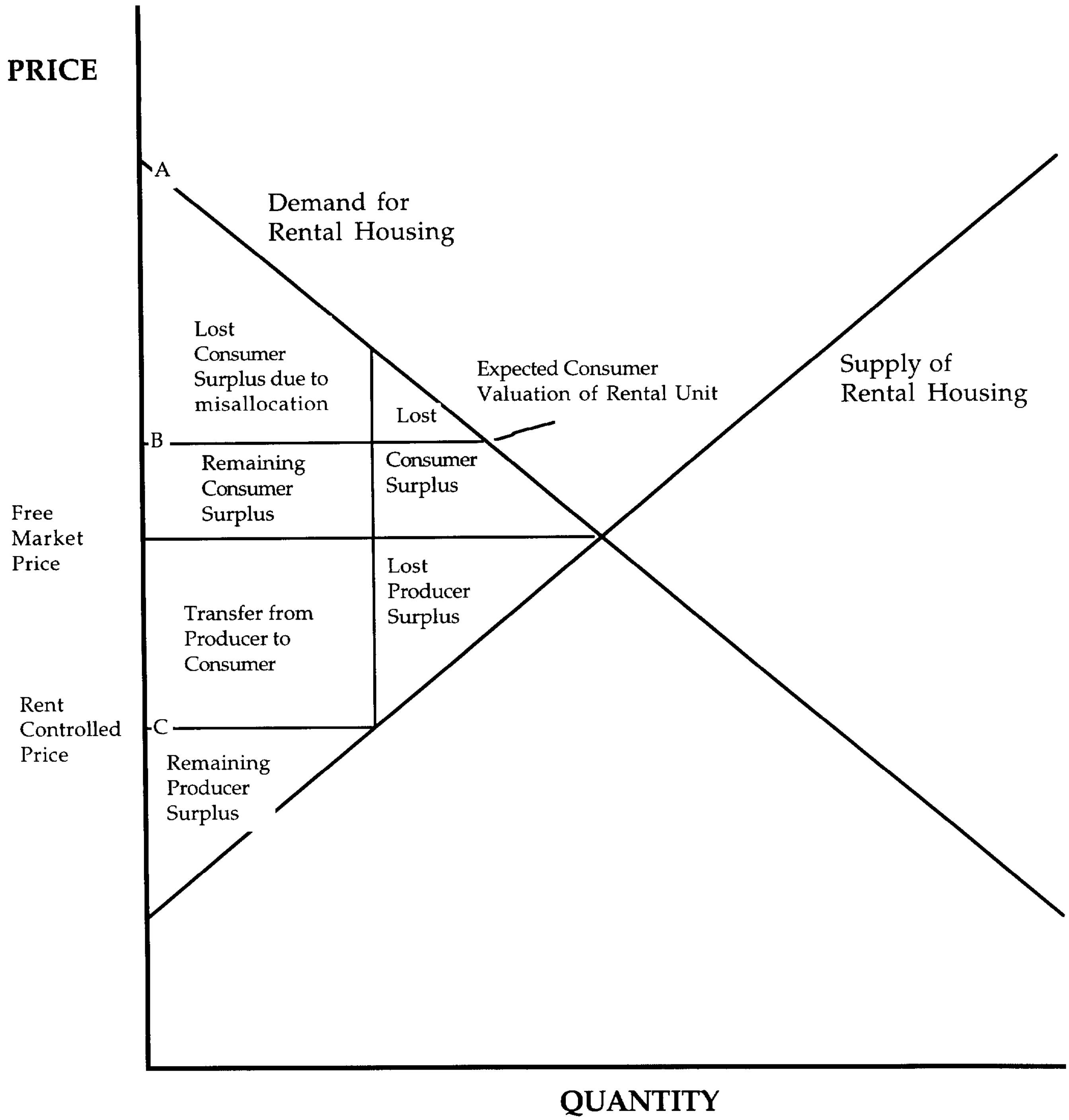


Figure 2: The Welfare Losses from Rent Control when Apartments are Randomly Allocated across Consumers



Point B lies half way between point A and point C on the Y-axis and this point represents the value that the average consumer, who wants an apartment at the rent controlled price, places on getting an apartment.

Figure 3: Overlap in tastes, θ, between two subgroups of the population

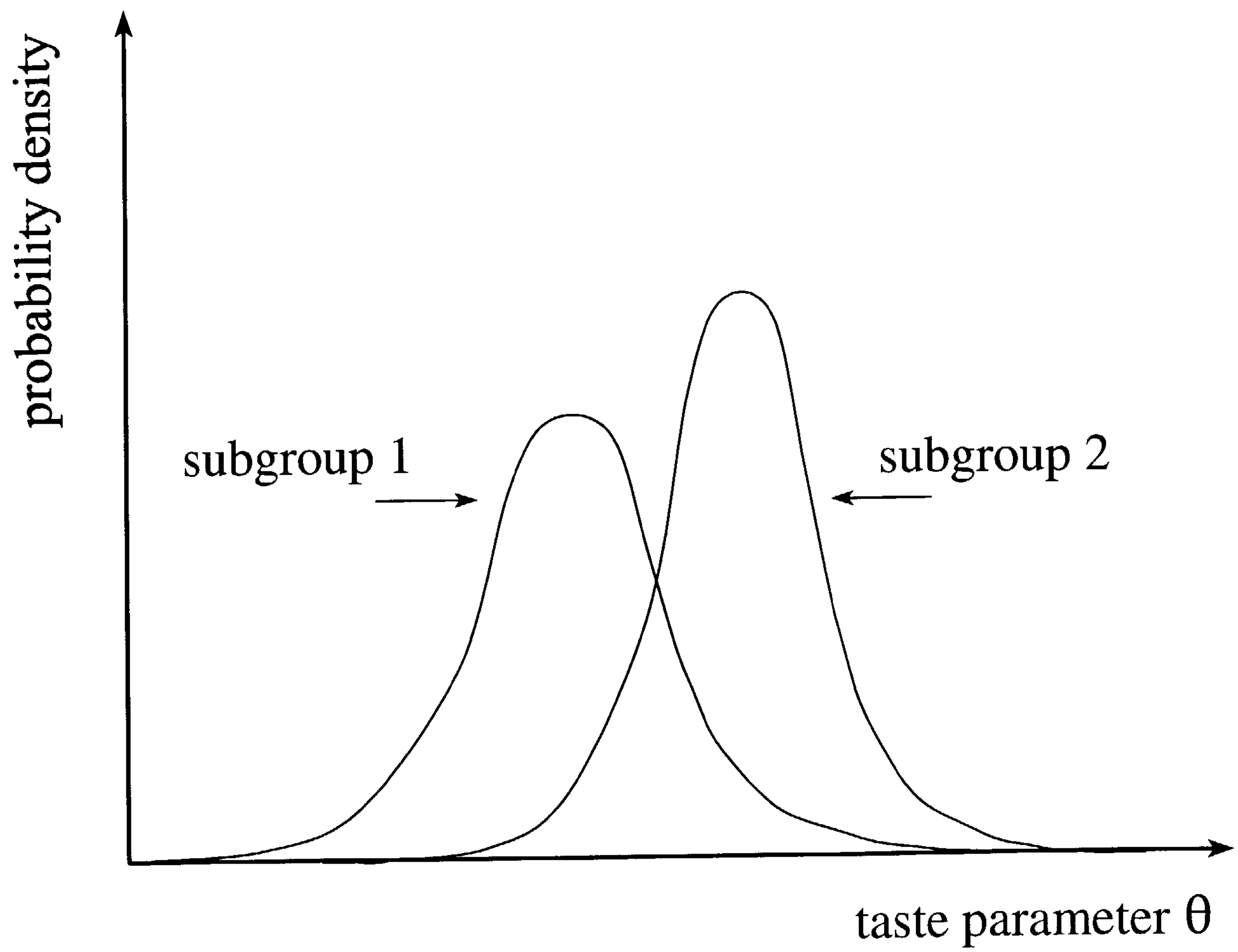


Figure 4: Cutoff valuations $\theta^*(1)$ and $\theta^*(2)$ for three apartment sizes

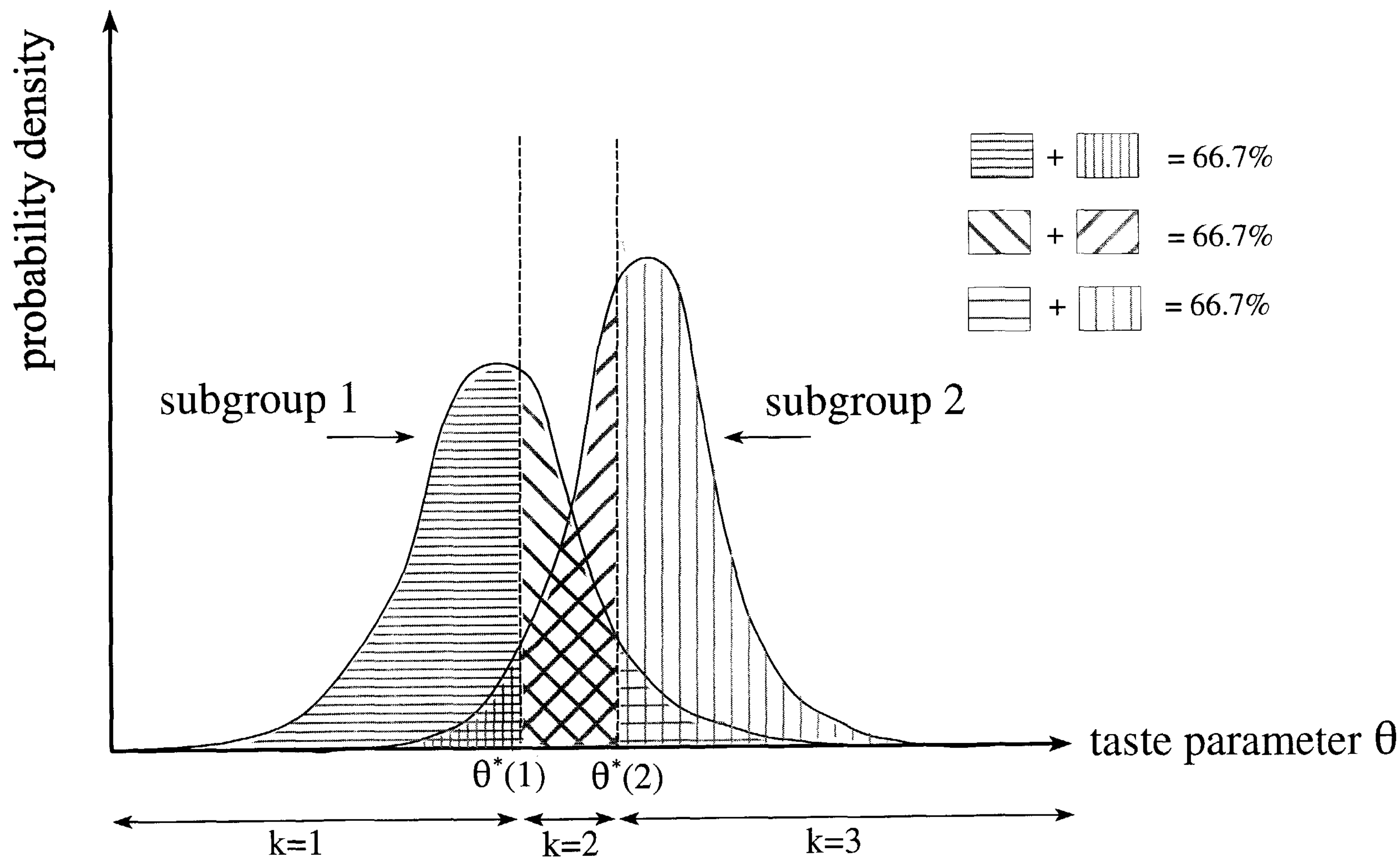


Figure 5: Measuring misallocation for agents of subgroup *i*

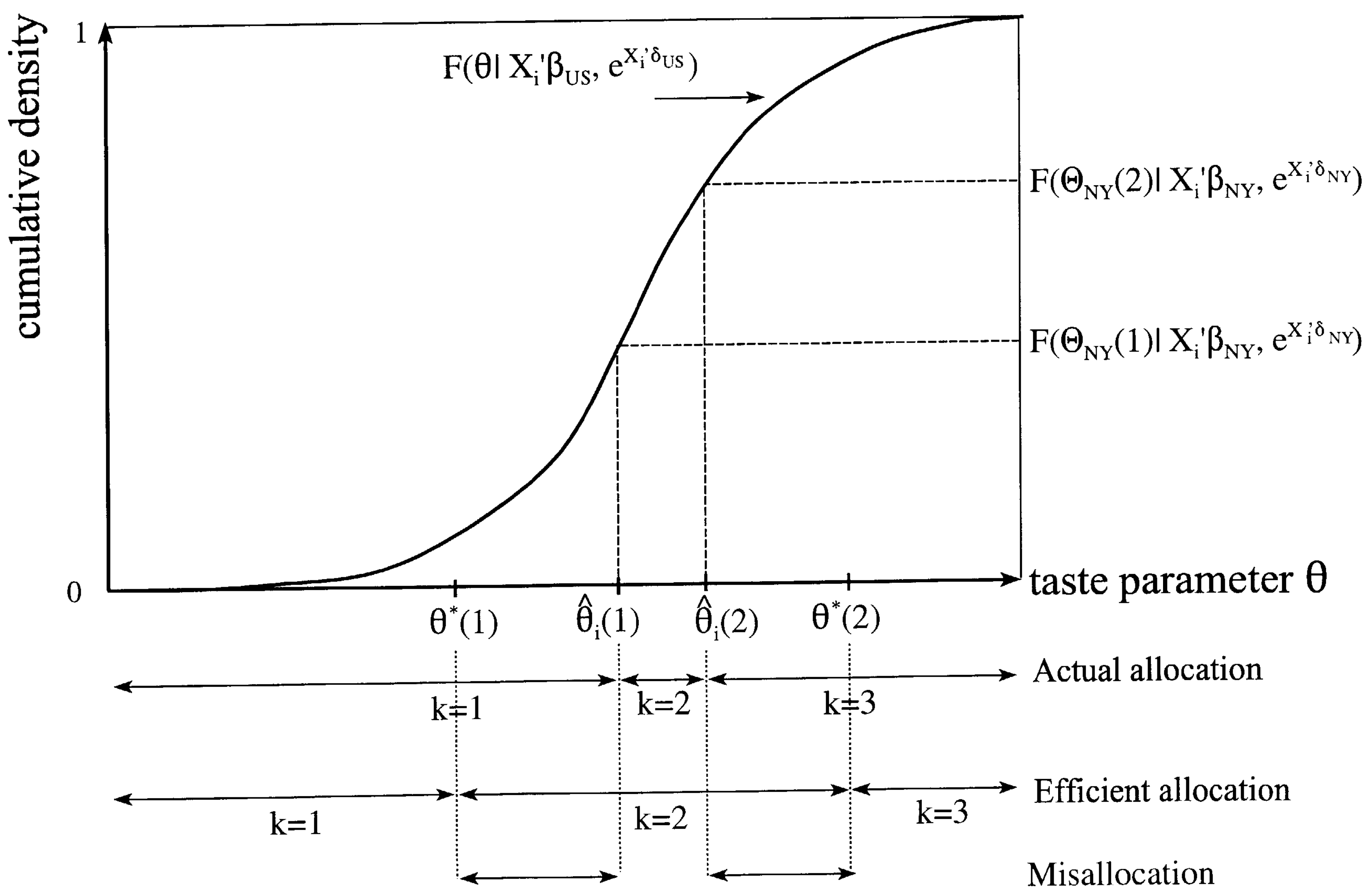


Table I
Overlap in Housing Consumption between Population Groups

		Probability that Bedrooms for group A household > Bedrooms for group B household			
		New York rent-control renters		US free-market renters*	
		Observations	Overlap*	Observations	Overlap*
Group A:	1 person household	1696	.077	1768	.050
Group B:	3+ person household	1596	(.005)	993	(.004)
Group A:	No children	3106	.156	2884	.103
Group B:	Children	1454	(.006)	1021	(.006)
Group A:	Age ≤ 35	1317	.232	1849	.275
Group B:	Age >35 and ≤ 60	2074	(.008)	1362	(.009)
Group A:	High school dropout	1152	.394	593	.288
Group B:	Some college	1314	(.012)	1197	(.014)
Group A:	Per capita income in bottom 1/3	1523	.433	1289	.372
Group B:	Per capita income in top 1/3**	1504	(.011)	1288	(.011)

* Standard errors in parentheses.

** Top and bottom 1/3 of the per capita income distribution are determined relative to indicated sample.

Table II
Means of Regression Variables

	Renters*			Owners*		
	NY	US**	Chicago	NY	US**	Chicago
Dependent variable:						
Bedrooms	1.38	1.47	1.40	1.57	1.82	1.93
	(.791)	(.680)	(.645)	(.807)	(.679)	(.663)
Maintenance problems	1.32	.261	.430	.598	.220	.182
	(1.52)	(.630)	(.918)	(.933)	(.565)	(.611)
Independent variables:						
2 adults	.395	.396	.340	.503	.413	.509
	(.489)	(.489)	(.481)	(.500)	(.493)	(.505)
3+ adults	.132	.064	.050	.097	.028	.109
	(.338)	(.244)	(.218)	(.296)	(.164)	(.315)
1 child	.150	.135	.136	.150	.073	.127
	(.357)	(.342)	(.344)	(.357)	(.261)	(.336)
2 children	.105	.076	.140	.081	.030	.073
	(.307)	(.265)	(.348)	(.273)	(.170)	(.262)
3+ children	.058	.046	.045	.015	.004	.000
	(.233)	(.210)	(.209)	(.121)	(.063)	(.000)
Single parent	.097	.088	.132	.028	.020	.036
	(.295)	(.284)	(.339)	(.166)	(.140)	(.189)
Age	46.2	41.0	44.7	49.4	55.8	53.9
	(17.5)	(17.9)	(19.0)	(15.7)	(18.2)	(19.6)
Max (Age – 35, 0)	13.3	9.80	12.6	15.3	21.8	20.2
	(15.349)	(14.9)	(16.5)	(14.7)	(16.8)	(18.0)
Max (Age – 60, 0)	2.92	2.40	3.33	2.92	5.93	5.76
	(6.56)	(6.33)	(7.35)	(6.45)	(8.25)	(9.46)
Log per capita income	9.16	9.25	9.03	10.2	9.95	9.65
	(1.27)	(1.22)	(1.34)	(1.08)	(1.00)	(1.27)
(Log per capita income)^2	85.6	87.1	83.4	104.9	100.0	94.7
	(21.6)	(20.3)	(21.6)	(20.7)	(18.0)	(20.7)
% High school graduate	.259	.314	.260	.135	.260	.164
	(.438)	(.464)	(.440)	(.342)	(.439)	(.373)
% Some college	.188	.233	.202	.157	.250	.200
	(.391)	(.423)	(.403)	(.364)	(.433)	(.404)
% College graduate	.313	.306	.314	.655	.442	.527
	(.464)	(.461)	(.465)	(.476)	(.497)	(.504)
Observations	**4953**	**3663**	**242**	**741**	**504**	**55**

Table II (continued)

	Renters*			Owners*		
	NY	US**	Chicago	NY	US**	Chicago
Other housing characteristics						
Mean value ($1000)				150.2	111.6	106.9
				(207.3)	(801.5)	(76.5)
Median value ($1000)				90.0	85.0	81.0
Mean rent	578.6	516.7	531.2			
	(351.0)	(192.4)	(174.0)			
Median rent	510	495	502.5			
Mean free market rent	872.5	516.7	531.2			
	(508.5)	(192.4)	(174.0)			
Median free market rent	750	495	502.5			
Mean rent-control rent	552.6					
	(320.9)					
Median rent-control rent	500					
Rooms	3.28	3.75	3.67	3.77	4.37	5.56
	(1.15)	(.963)	(.963)	(1.22)	(1.05)	(.977)
Year built	1934.7	1967.0	1952.2	1946.0	1971.3	1964.3
	(23.8)	(18.9)	(25.0)	(22.5)	(14.9)	(20.0)
Other Personal characteristics:						
White	.659	.742	.607	.815	.909	.855
	(.474)	(.438)	(.489)	(.388)	(.288)	(.356)
Black	.253	.180	.289	.092	.038	.073
	(.434)	(.385)	(.454)	(.289)	(.191)	(.262)
Other race	.086	.078	.103	.093	.054	.073
	(.280)	(.267)	(.305)	(.291)	(.225)	(.262)
Income ($1000)	30.4	26.6	22.9	76.2	44.2	42.7
	(34.0)	(21.3)	(19.3)	(7.8)	(36.5)	(28.8)
Income per capita ($1000)	17.5	16.9	14.6	42.0	29.8	23.1
	(21.5)	(15.4)	(13.1)	(43.4)	(25.1)	(15.0)
Observations	**4953**	**3663**	**242**	**741**	**504**	**55**

* Standard Deviations in parentheses.

** US sample excludes New York and Chicago.

Notes: The data for New York City comes from the 1993 New York City Housing and Vacancy Survey. The New York sample is limited to housing units in buildings with 6 or more units and public housing units are excluded. The data for the rest of the U.S. comes from the 1993 American Housing Survey. This sample is limited to households in metropolitan areas that live in apartment buildings with 6 or more units. Public housing and rent-controlled units are excluded.

Table III
Allocation Regressions

Dependent variable:	Bedrooms*			Maintenance*			Owner**		
Estimation Method:	OLS			OLS			Probit		
Interaction:	Direct effect	New York	Chicago	Direct effect	New York	Chicago	Direct effect	New York	Chicago
2 adults	.537	-.073	-.022	.032	.079	.245	.036	.010	.122
	(22.6)	(-2.14)	(-.246)	(1.25)	(1.39)	(1.53)	(3.27)	(.690)	(2.38)
3+ adults	1.01	-.121	-.226	.124	.220	-.151	-.012	.067	.524
	(23.7)	(-2.15)	(-1.12)	(2.22)	(2.33)	(-.496)	(-.490)	(1.91)	(3.75)
1 child	.226	.045	-.040	.033	.031	.148	.038	.013	-.030
	(6.93)	(.954)	(-.284)	(.807)	(.355)	(.529)	(1.87)	(.550)	(-.670)
2 children	.465	.044	-.071	.037	.244	-.054	.039	.018	-.048
	(11.4)	(.743)	(-.460)	(.693)	(2.17)	(-.236)	(1.39)	(.580)	(-1.03)
3+ children	.763	.061	-.076	.260	.620	.469	.005	.018	.
	(13.6)	(.812)	(-.305)	(3.02)	(4.06)	(1.04)	(.100)	(.310)	
Single parent	.55	-.208	-.058	-.007	.322	-.048	-.016	.025	.054
	(12.5)	(-3.27)	(-.366)	(-.126)	(2.67)	(-.161)	(-.630)	(.670)	(.590)
Age	.005	.002	-.002	-.001	.019	.009	.004	.003	.003
	(2.14)	(.426)	(-.207)	(-.250)	(2.47)	(.540)	(2.63)	(1.44)	(.650)
Max (Age – 35, 0)	.001	.008	.003	-.000	-.021	-.006	.001	-.006	-.006
	(.388)	(1.50)	(.233)	(-.066)	(-2.04)	(-.277)	(.510)	(-2.14)	(-.840)
Max (Age – 60, 0)	-.007	-.016	-.000	-.005	-.016	-.015	-.003	.002	.003
	(-2.20)	(-3.51)	(-.004)	(-1.48)	(-2.32)	(-1.05)	(-2.31)	(1.34)	(.820)
Log per capita income	-.409	.280	.161	.067	.500	-.401	-.151	.27	-.080
	(-6.08)	(3.39)	(.773)	(.724)	(3.26)	(-1.38)	(-5.43)	(.740)	(-.960)
(Log per capita income)^2	.028	-.019	-.011	-.007	-.033	.025	.012	-.002	.004
	(6.87)	(-3.68)	(-.817)	(-1.30)	(-3.65)	(1.33)	(6.99)	(-.750)	(.850)
High school graduate	.135	-.144	.043	-.079	-.132	-.070	.087	-.031	.005
	(4.20)	(-3.36)	(.366)	(-2.00)	(-1.79	(-.361)	(3.87)	(-1.50)	(.090)
Some College	.125	-.088	.158	-.081	-.079	-.080	.135	-.024	.008
	(3.54)	(-1.86)	(1.24)	(-1.97)	(-.967)	(-.408)	(5.10)	(-1.08)	(.140)
College graduate	.147	-.189	-.080	-.089	-.215	-.195	.126	.006	.036
	(4.08)	(-3.91)	(-.665)	(-2.12)	(-2.68)	(-1.10)	(5.31)	(.260)	(.580)
MSA Fixed Effect		Yes			Yes			Yes	
F-test on interaction with NY		9.32			14.6			46.9	
		(.000)			(.000)			(.000)	
F-test on interaction with Chicago			.76			1.19			22.2
			(.715)			(.278)			(.052)
R^2		**.321**			**.241**			**.220**	
Observations		8858			8858			10158	

* T-statistics corrected for heteroscedasticity in parentheses

** Coefficients reflect marginal changes in probabilities evaluated at the mean of the dependent variables. Pseudo R^2 reported for probit. Z-statistics corrected for heteroscedasticity in parentheses. "Bedrooms" measures the number of bedrooms in the apartment, "Maintenance" measures the number of maintenance problems that are present out of a total of 6 potential maintenance problems and "Owner" is a dummy variable that equals 1 if the apartment is owner occupied.

Table IV
Decomposition of Misallocation among Renters in New York

Actual bedrooms	Efficient bedrooms	Percentage of Households	Annual DWL per household	Contribution to aggregate DWL
0	0	.061	0	.000
0	1	.048	239	.262
0	2	.000	1304	.001
1	0	.048	178	.195
1	1	.401	0	.000
1	2	.041	155	.145
1	3	.000	1192	.000
2	0	.000	2992	.003
2	1	.041	199	.186
2	2	.251	0	.000
2	3	.023	211	.112
3	1	.000	1103	.000
3	2	.023	180	.096
3	3	.063	0	.000

Notes: This table is based on the DWL calculation in the first row of Table V. It is a decomposition of the uncorrected DWL estimate for renters in New York assuming an elasticity of housing demand of 0.5 and efficient selection on unobservables. The aggregate uncorrected DWL estimate for this case is $43.60 per apartment per year.

Table V
DWL from Misallocation due to Rent-control in New York

	Observations	MSA Specific Effect	Dependent Variable	Annual DWL from misallocation per apartment*		
				Un-corrected	Correction	Net Estimate
Treatment:						
Renters in New York[a]	4953	Random	Bedrooms	43.6 (4.90)	12.1 (2.34)	31.5 (5.43)
			Maintenance problems	11.8 (4.83)	15.9 (4.04)	-4.15 (6.29)
Renters and owners in New York[b]	5694	Random	Rent/Own	15.6 (5.74)	2.38 (.854)	13.2 (3.68)
			Bedrooms	38.8 (5.74)	11.2 (2.42)	27.6 (6.23)
			Maintenance problems	9.87 (2.90)	13.7 (3.40)	-3.82 (4.47)
Renters in New York – moved prior to 1987[a]	2458	Random	Bedrooms	51.2 (7.27)	13.7 (2.67)	37.5 (7.74)
			Maintenance problems	11.4 (5.32)	16.4 (4.25)	-4.97 (6.81)
Renter in New York – moved after 1986[a]	2458	Random	Bedrooms	31.1 (5.10)	17.4 (2.86)	13.7 (5.85)
			Maintenance problems	16.7 (6.63)	19.0 (5.29)	-2.28 (8.48)
Placebo:						
Random 50% of US (excludes New York)[c]	1831	Random	Bedrooms	11.1 (5.17)	26.4 (5.17)	-15.3 (7.92)
			Maintenance problems	15.5 (4.20)	12.8 (3.37)	2.60 (5.39)
Renters in Chicago[a]	242	Random	Bedrooms	22.7 (13.0)	25.3 (6.38)	-2.58 (14.5)
			Maintenance problems	19.9 (15.6)	26.2 (8.54)	-6.32 (17.8)
Owners in New York[d]	741	Random	Bedrooms	86.3 (43.2)	50.0 (16.5)	36.3 (46.3)
			Maintenance	51.4 (22.1)	25.4 (11.6)	26.1 (24.9)

* DWL is expressed in $ per year per apartment. In the DWL calculation, we assume an elasticity of housing demand of 0.5 and efficient selection on unobservables. Standard errors in parentheses.

[a] Control group (n=3663) is non-rent-control apartments outside New York and Chicago.

[b] Control group (n=4167) is owners and non-rent-control renters outside New York and Chicago.

[c] Control group (n=1831) is the 50% of the US (excluding New York) not in the random sample.

[d] Control group (n=504) is owners outside New York and Chicago.

Table VI
Control Groups Split by MSA Characteristics

	Mean of MSA Characteristic for Observations in: (Standard Deviation)			Net Estimate of DWL from Misallocation of Bedrooms			
MSA Characteristic	MSAs most dissimilar to NYC	MSAs most similar to NYC	New York City	Control Group:	Treatment Group:	DWL Estimate	Standard Error
(1) Longitude	-110.0	-80.7	-73.9	Dissimilar half	New York	38.8	(10.7)
(West is more negative)	(12.4)	(5.4)	-	Similar half	New York	32.3	(7.4)
				Dissimilar half	Similar half	3.5	(9.7)
(2) Fraction of Units	0.197	0.349	0.837	Dissimilar half	New York	37.7	(6.4)
in Buildings with 5 or more Units	(0.040)	(0.055)	-	Similar half	New York	31.4	(8.7)
				Dissimilar half	Similar half	2.7	(5.6)
(3) Number of Units	99.1	512.3	1996.4	Dissimilar half	New York	34.9	(7.4)
in Buildings with 5 or more Units	(57.0)	(304.8)	-	Similar half	New York	26.3	(8.9)
(thousands)				Dissimilar half	Similar half	7.9	(6.6)
(4) Population	1.23	3.44	20.7	Dissimilar half	New York	42.5	(7.5)
Density (thousand	(0.32)	(1.32)	-	Similar half	New York	19.4	(7.5)
persons / km^2)				Dissimilar half	Similar half	8.8	(7.9)
(5) Population	1.16	4.58	8.54	Dissimilar half	New York	38.8	(7.0)
(millions)	(0.55)	(2.39)	-	Similar half	New York	23.7	(8.3)
				Dissimilar half	Similar half	1.0	(7.8)

Notes: The elasticity of housing demand is 0.5. DWL is expressed as $ per year per apartment. There are 3901 observations outside New York City that are evenly split into a group most dissimilar to NYC and a group most similar to NYC, where similarity is measured by the MSA characteristic listed. The treatment group consists of 4953 observations in New York City. Population Density for an MSA is computed as the population-weighted average the population densities in the Census tracts in the MSA. All the MSA characteristics are calculated using the 1990 Census Summary Tape Files in order to obtain a larger sample size.

Table VII
Magnitude of Effect

	Observations	MSA Specific Effect	Dependent Variable	Net estimates of DWL from misallocation across elasticity of housing demand*			
Efficient selection:*				0.1	0.2	0.5	1.0
Renters in New York	4953	Random	Bedrooms	157.3	78.6	31.5	15.7
				(27.2)	(13.6)	(5.43)	(2.72)
Renters and owners in New York	5694	Random	Rent/Own	66.1	33.0	13.2	6.61
				(18.4)	(9.21)	(3.68)	(1.84)
			Bedrooms	137.8	68.9	27.6	13.8
				(31.2)	(15.6)	(6.23)	(3.12)
Similar selection:*							
Renters in New York	4953	Random	Bedrooms	490.7	245.3	98.1	49.1
				(84.7)	(42.4)	(16.9)	(8.47)
Renters and owners in New York	5694	Random	Rent/Own	548.3	274.2	109.7	54.8
				(152.8)	(76.4)	(30.6)	(15.3)
			Bedrooms	463.9	231.9	92.8	46.4
				(104.9)	(52.4)	(21.0)	(10.5)

* DWL is expressed in $ per year per apartment. Efficient selection refers to efficient selection on unobservables. Similar selection refers to selection of unobservables similar to selection on observables. Standard errors in parentheses.

Appendix Table I
Means of Variables

	New York*		New York**		US**		Chicago**	
	All	6+ units	All	6+	All	6+ units	All	6+ units
Entire Sample:								
% Owner	.239	.096	.347	.159	.595	.111	.590	.170
% Market renter	.192	.056	.426	.454	.366	.762	.381	.741
% Rent control	.437	.651	.153	.261	.014	.046	.000	.000
% Public housing	.133	.198	.074	.125	.258	.081	.030	.090
Owners:								
Value ($1000)	193.8	174.5	168.6	132.4	129.0	111.1	134.2	106.9
Bedrooms	2.51	1.59	2.58	1.57	3.03	1.84	2.92	1.95
Maintenance problems	0.43	0.59	0.24	0.21	0.30	0.22	0.32	0.18
Year built	1941	1947	1943	1950	1957	1971	1953	1964
Persons in unit	2.76	2.08	2.65	1.85	2.74	1.67	2.88	2.05
Number of children	0.59	0.35	0.53	0.26	0.68	0.16	0.70	0.27
Age	53.2	49.3	54.1	49.9	51.8	55.6	52.3	53.9
Household income($1000)	56.6	79.7	54.3	58.6	50.2	44.0	54.6	42.7
% High school dropout	0.134	0.048	0.107	0.061	0.110	0.054	0.140	0.109
% High school graduate	0.275	0.122	0.296	0.152	0.295	0.250	0.296	0.164
% Some college	0.198	0.160	0.173	0.171	0.213	0.245	0.206	0.200
% College graduate	0.393	0.671	0.425	0.616	0.382	0.451	0.359	0.527
Free-market Renters:								
Rent	692.3	872.5	602.5	574.2	514.0	517.6	511.0	531.2
Bedrooms	1.78	1.08	1.64	1.43	1.87	1.46	1.77	1.40
Maintenance problems	0.74	0.70	0.60	0.65	0.36	0.27	0.45	0.43
Year built	1936	1948	1935	1935	1958	1966	1945	1952
Persons in unit	2.61	2.03	2.53	2.35	2.40	1.99	2.43	2.07
Number of children	0.74	0.38	0.76	0.67	0.71	0.45	0.79	0.58
Age	41.2	38.2	42.4	43.2	40.6	41.3	43.5	44.7
Household income($1000)	35.0	47.6	30.6	28.7	28.0	26.5	25.7	23.0
% High school dropout	0.199	0.100	0.198	0.224	0.171	0.153	0.219	0.225
% High school graduate	0.271	0.140	0.329	0.310	0.329	0.311	0.317	0.254
% Some college	0.218	0.158	0.163	0.150	0.232	0.230	0.212	0.204
% College graduate	0.312	0.602	0.310	0.316	0.268	0.307	0.253	0.317
Rent-control renters:								
Rent	551.7	552.6	562.0	567.5	587.5	584.6		
Bedrooms	1.41	1.40	1.25	1.24	1.40	1.28		
Maintenance problems	1.38	1.39	0.86	0.88	0.53	0.52		
Year built	1934	1933	1930	1930	1942	1945		
Persons in unit	2.27	2.28	1.98	1.98	2.10	2.17		
Number of children	0.57	0.57	0.38	0.38	0.41	0.46		
Age	47.0	46.8	50.0	50.0	42.4	42.9		
Household income($1000)	28.9	29.0	33.6	33.8	31.9	31.1		
% High school dropout	0.256	0.254	0.121	0.119	0.169	0.170		
% High school graduate	0.266	0.265	0.246	0.242	0.245	0.270		
% Some college	0.190	0.190	0.154	0.149	0.181	0.178		
% College graduate	0.289	0.290	0.479	0.491	0.405	0.383		
Observations	**10351**	**6796**	**1832**	**1030**	**22891**	**5027**	**1153**	**324**

* source: 1993 New York City Housing and Vacancy Survey (NYCHVS).

** source: 1993 American Housing Survey. US sample excludes New York. There are no rent-control units in Chicago.

Notes: Means are unweighted. The rent-control variable in the AHS is self-reported, but rent-control is determined using administrative data in the NYCHVS. This may explain the large discrepancy in % rent-control in NY between the AHS and the NYCHVS. The number of observations for some of the variables may be somewhat less than reported in the last row due to missing observations.

Appendix Table II
Sensitivity to MSA Specific Effects

	Observations	MSA Specific Effect	Dependent Variable	Annual DWL from misallocation per apartment*		
				Un-corrected	Correction	Net Estimate
Treatment:						
Renters in New York[a]	4953	Fixed	Bedrooms	41.3	11.2	30.1
				(6.43)	(2.54)	(6.92)
			Maintenance problems	12.3	15.9	-3.60
				(4.77)	(3.90)	(6.16)
Renters in New York[b]	4953	None	Bedrooms	45.9	19.7	26.1
				(8.00)	(4.35)	(9.10)
			Maintenance problems	12.0	17.4	-5.36
				(6.33)	(4.35)	(7.68)
Renters and owners in New York[c]	5694	Fixed	Rent/Own	14.4	2.06	12.3
				(3.42)	(.563)	(3.47)
			Bedrooms	36.8	12.4	24.3
				(4.36)	(1.92)	(4.76)
			Maintenance problems	9.91	14.5	-4.63
				(5.62)	(3.84)	(6.81)
Renters and owners in New York[d]	5694	None	Rent/Own	17.3	2.35	15.0
				(4.16)	(.756)	(4.23)
			Bedrooms	38.7	16.2	22.5
				(5.27)	(3.62)	(6.39)
			Maintenance problems	9.96	15.4	-5.49
				(4.56)	(2.88)	(5.40)

* Elasticity of housing demand is 0.5. Efficient selection on unobservables. Standard errors in parentheses.
[a] Control group (n=3420) is all non-rent-control apartments outside New York and Chicago with at least 10 observation per MSA.
[b] Control group (n=3663) is non-rent-control apartments outside New York and Chicago.
[c] Control group (n=3941) is all owners and non-rent-control apartments outside New York and Chicago with at least 10 observation per MSA.
[d] Control group (n=4167) is owners and non-rent-control renters outside New York and Chicago.