Can New York and Los Angeles Learn from Kumasi and Bangalore? Costs and Benefits of Rent Controls in Developing Countries

Stephen Malpezzi
University of Wisconsin

Abstract

This article presents comparative static estimates of the costs and benefits of rent controls for four developing-country cities: Cairo, Egypt; Kumasi, Ghana; Bangalore, India; and Rio de Janeiro, Brazil. The results are compared with those from four U.S. cities: New York; Los Angeles; Santa Monica; and Washington, DC.

Rent control regimes and their effects vary widely across cities and countries. Whatever the "average" cost or benefit of controls, all markets have large variances about the average, and the variation is rarely strongly related to income or other household characteristics in a way most people would find desirable. In the countries for which there was direct evidence, landlords were richer than tenants on average, but not remarkably so, and there was considerable overlap in the two income distributions. In Cairo, where there was direct evidence, key money made up much of the difference between controlled rents and market rents. In very strict regimes, substantial reductions in rents were sometimes largely or completely offset by losses in consumer's surplus (Kumasi). Controls that give differential treatment to favored groups like civil servants were found to distribute substantial benefits to those groups without much benefit to others (Bangalore). In countries willing to incur substantial transactions costs of indexation and renegotiation of rents for leases beyond market periods, control may make little difference on average, although a wide variance of benefits remains (Rio).

Introduction

According to a recent survey by the U.S. Department of Housing and Urban Development (HUD) (1991), about 10 percent of the U.S. rental market is under some form of rent control. In many other countries, the fraction is much higher (Malpezzi and Ball 1991). Economists as a group have no trouble reaching a consensus on the qualitative effects of rent control on housing markets. An often-cited study revealed that only 2 percent of economists surveyed disagreed with the proposition that "a ceiling on rents reduces the quantity and quality of housing available" (Kearl et al. 1979). This consensus rests on the analysis of rent control

as a simple, effective price control or tariff. However, only recently has much empirical research been done to test this proposition, to measure the actual magnitudes involved, or to analyze real-world rent control regimes, which often diverge from the simple textbook model.

Even if the consensus view is correct in the long run, little policy advice is available from the simple price control model other than that immediate blanket decontrol will restore equilibrium after some unknown lag. For practical policy applications, quite specific information is needed—about the magnitudes of the costs and benefits of alternative policies, about their distribution, and about various methods of decontrol. This article aims to contribute to the literature by reviewing recent research comparing costs and benefits of controls in four cities in developing countries: Cairo, Egypt; Kumasi, Ghana; Bangalore, India; and Rio de Janeiro, Brazil. For the benefit of U.S. readers, the article also compares these findings with selected research on four U.S. cities: New York; Los Angeles; Santa Monica; and Washington, DC.[1] For U.S. analysts, international results are of interest because there is more variation in rent control regimes and their effects, and there are data on things like key money and landlord incomes, which are hard to come by in this country. For international analysts, besides providing more "degrees of freedom," the United States is a laboratory containing different rent control regimes in a reasonably homogeneous macroeconomic and cultural environment.

This article focuses on selected comparative static estimates of the costs and benefits of controls, and their distribution, in the eight cities mentioned. Other estimates do exist, for example Struyk's (1988) estimates for urban Jordan, Peña and Ruiz-Castillo's (1984) estimates for Madrid, and Marks's (1984) estimates for Vancouver, to mention only three.[2] This article also has little to say about effects on profitability or on rent control dynamics or about alternative methods of decontrol. These are important issues as well, and the reader is referred to Arnott (1981), Malpezzi and Ball (1991), HUD (1991), and references cited therein for broader reviews of relevant literature. But the cost-benefit measures reported here are the starting point for

[1] The review is very selective; one or two readily available studies were chosen for each U.S. market. See HUD (1991) for a more thorough survey of literature on U.S. rent controls.

[2] A bibliography of approximately 400 studies of rent control around the world is available from the author. Only a minority of the studies include empirical estimates of costs and benefits such as those described here.

any policy analysis. These estimates address the following vital questions:

1. What is the size of today's rent reduction—that is, the income current landlords lose because of controls on the rents of current units?
2. How much do tenants value the reduction in rent, compared with landlord losses?
3. How are these gains and losses distributed?
4. Are these results robust, or do they vary greatly from market to market? Are there underlying patterns in the variation?

Rent control regimes in eight cities

The four developing-country cities—Cairo, Kumasi, Bangalore, and Rio de Janeiro—were chosen as case study markets for a World Bank-financed research project on rent controls in developing countries directed by the author.[3] These cities were chosen in order to study four cities with significant variation in type of rent control regime and variation in market conditions, subject to data availability. Criteria for choosing the four U.S. cities—New York, Los Angeles, Santa Monica, and Washington—are (1) variation in regimes and market conditions and (2) the availability of published studies that yield at least roughly comparable results. I begin with a brief description of the various rent control regimes, by city.

Cairo

Cairo is, of course, the capital and largest city of Egypt, with a population of about 12 million, growing somewhat less than 4 percent per year. Rent control was introduced in Cairo in 1944. At that time, controls were applied only to houses built before 1944, to avoid discouraging housing production. The first major changes in the law took place gradually between 1952 (the Egyptian revolution) and 1965, as rent controls were extended to cover newer units, and previously set rents were further reduced, until by 1962 the law was extended to cover new construction as well as the existing stock.

---

[3] Malpezzi and Ball (1991) present an overview of the project. The individual case studies are Malpezzi (1986), Cairo; Malpezzi, Tipple, and Willis (1990), Kumasi; Malpezzi and Tewari (1991), Bangalore; and Silveira and Malpezzi (1991), Rio.

During recent years, legal rents were fixed at 8 percent of the assessed value of the land and 5 percent of the assessed construction cost of the structure—at the time of completion for units as constructed and at estimated construction costs for units built before the effective date. Since 1981 the legal rate of return has been fixed at 7 percent of the combined value of the land and cost of construction of the structure. Luxury and furnished units are exempt from controls, but the number of such units is strictly controlled. In practice, furnished (and therefore uncontrolled) units are rented only to foreigners.

Kumasi

Kumasi is the second-largest city in Ghana, with a population of about 600,000. As with many other countries, rent controls started in Ghana during World War II when the Gold Coast began to suffer the effects of inflation. In response to inflation, the Defence (Rent Restriction) Regulations of 1942 made it an offense for anyone to increase rents above those of September 3, 1939, unless an assessment had been made by a Rent Assessment Committee. Since then Ghana has had a long and varied history of controls, which is described in detail in Malpezzi, Tipple, and Willis (1990). As of the date of their data collection (1986), most residents of Kumasi rent accommodation, usually rooms in compound houses or tenements. The rent per room is fixed on a simple schedule; most tenants rent rooms with controlled rents of 250 to 300 cedis per month (about $3 in 1986). Rents are adjusted infrequently, in a highly inflationary environment.

Bangalore

Bangalore is the fifth-largest city in India, with a population of about 5 million. It is one of the fastest growing cities of its size in the world, with an annual population growth rate of about 6 percent. Like Ghana and Egypt, India has had controls in urban areas since World War II, but in India controls are largely in the purview of the state governments. Bangalore, in the state of Karnataka, has a two-tier system of controls, in which some units are "uncontrolled" (primarily new units, which enjoy a five-year holiday from controls, and very low rent units), some are under "ordinary" controls (increases are regulated), and some are under "strict" controls ("fair rents" are set by the rent controller, and in many cases tenants are allocated to the unit by the controller). Fair rents are fixed by the rent controller when a

unit has been allotted (if fair rent was not fixed earlier) or upon application by a tenant. Once fixed, a fair rent remains frozen until the landlord files a petition with the rent controller for an increase. Possible grounds for an increase include additions or improvements to the unit and increases in property taxes. In practice, reassessments are rare.

Newly constructed units have their rents set by negotiation rather than administrative fiat; after five years the unit then comes under the rent control acts. Eviction clauses are tilted heavily in favor of tenants, and there are procedural delays even when landlords have legal grounds for eviction.

Rio de Janeiro

Rio's population of about 6 million puts it second in Brazil only to São Paulo's 10 million. Brazil has had a long and complicated history of controls. The first attempt at regulating the private rental market goes back to 1917. Silveira and Malpezzi (1991) detail the laws and their history; for the present purpose the important point is that the controls currently in force in Brazil are generally less stringent than those in other countries studied in the comparative research project. In particular, in Brazil rent *levels* are not controlled directly by legislation; rent *increases* are. The increases are indexed to increases in inflation and are reset by negotiation every fifth year or when tenants change.

New York

New York City's rent control system is quite complex, but I will present a greatly simplified review (see Stegman 1985 for a detailed description). The U.S. Congress enacted national rent control during World War II. After the war, rent control powers devolved to the states, and over several years most states removed controls. By the 1950s most jurisdictions had removed controls, except for those remaining in New York City. About a dozen significant revisions to the system have been enacted since then. There are three main classes of rental housing: controlled, stabilized, and uncontrolled. Controlled rental housing comprises mainly pre-1947 apartments, whose rents are set on what is roughly a (financial) cost-plus basis. Since 1969, units built after 1947 (and some pre-1947 units that had been decontrolled) have been subject to "rent stabilization," under which a board comprising landlord, tenant, and "general public" representatives set annual guidelines for percentage increases. Since 1971, both

controlled and stabilized units are removed from the system whenever tenants turn over, but since 1974, once new tenants negotiate rents, the units come under stabilization again.

Los Angeles

Los Angeles has had rent control in place since 1978, but unlike neighboring Santa Monica, the city has a fairly flexible regulatory framework.[4] New construction, "luxury" units, and detached units are exempt; according to HUD, about 63 percent of rental units fall under controls. Rents are reset for new tenants by negotiation, and increases thereafter are limited to the Consumer Price Index (CPI) (until recently they were limited to about 7 percent per year). Evictions are permitted for "just cause." There is a "sunset provision" that allows for the end of controls if the rental vacancy rate reaches 5 percent.

Santa Monica

Since 1979 Santa Monica has had one of the strictest rent control regimes in the United States. The law dates from a voter initiative that was more or less a reaction to landlords' failure to roll back rents immediately after Proposition 13.[5] New units built on vacant land and structures with three or fewer units are exempt, but these are only a small fraction of the rental stock.[6] Increases are limited by an elected rent control board and are supposed to be based on changes in operating expenses. Recent changes have averaged about 70 percent of the CPI. There are no allowances for resetting rents upon vacancy. There are controls on conversion and demolition of units.

---

[4] The Los Angeles regulatory framework was set by the city council rather than by tenant initiative.

[5] Partial-equilibrium models of tax incidence suggest that in the short run landlords bear increases in, and benefit from reductions in, property taxes; in the long run, under conditions of elastic supply, the taxes (and changes in taxes) are passed along to consumers. Since housing supply is not highly elastic over one or a few years, especially in California's regulatory climate, no one should be surprised that rents did not fall immediately after Proposition 13. General-equilibrium models of property taxation suggest that taxes are borne by owners of capital generally, at least in part, and hence rents would not fall by the full amount of a tax reduction. See Netzer (1966) and Aaron (1975).

[6] New construction on vacant land is exempt, but if existing units are demolished, the replacement units are under controls (see Shulman 1981).

Washington

The nation's capital has one of the least restrictive regimes. New and renovated units and units owned by small landlords are exempt. Rents can increase in line with the CPI, and by another 12 percent if the tenant moves. There are also provisions allowing cost pass-through and guaranteeing a 12 percent rate of return on equity.

Comparing regimes in the eight cities

Analyzing the interaction among controls, market outcomes, and economic performance requires summarizing much of the descriptive information discussed above in one or more indices. Any such index will be heavily judgmental and, perhaps, arbitrary in some ways. Here I briefly discuss the construction of an index, similar to Malpezzi and Ball's (1991, 1993) index of regulatory stringency, which they constructed for 51 countries.

For their index, countries that have no controls receive a rating of 0. Other countries are rated on a nine-element scale. For each of the first eight elements, the country receives a rating of 0 (permissive), 1 (medium), or 2 (restrictive). The index is the unweighted sum of the nine elements. The first two elements are

1. *Enforcement*. If controls are not enforced or rarely enforced, the country receives a score of 0. Selective or partial enforcement scores 1 point; strict enforcement, 2.

2. *Coverage*. If coverage is restricted to a very small part of the market, the country receives a score of 0. If a significant part of the market is covered, the country receives a score of 1. If more than half the market is covered, the country receives a score of 2.

If a country has controls that are at least selectively enforced and that cover a significant part of the market, additional points are awarded as follows:

3. *Fair rents*. Countries that do not set rent levels for units receive a rating of 0; those with some units so covered or no information, 1; those with stringent rent setting, 2.

4. *Indexation*. If rents are indexed and closely tied to inflation, the country receives a rating of 0; for partial indexing or no information, 1; if rents are frozen or rarely revalued, 2.

5. *Cost pass-through*. Are upgrading, maintenance, and tax increases passed through to tenants? If often, 0; if some items are passed through or no information, 1; if no or little pass-through, 2.

6. *Treatment of new construction*. If newly constructed units are exempt, 0. If newly constructed units have a temporary exemption or some other differential treatment, or no information, 1. If new construction is controlled as other rental housing, 2.

7. *Rents reset on new tenancy*. If rents reset to market on new tenancy, 0; if revalued but below market or no information, 1; if no change, 2.

8. *Tenure security*. If tenure security is more or less covered by private agreement (leases) and normal grounds for eviction, 0; if more stringent security of tenure or no information, 1; if strict tenure security, 2.

The final element, which is open ended, is the average annual inflation rate from 1965 to 1985, divided by 10 (i.e., a country with a 10 percent inflation rate receives 1 point; with 15 percent, 1.5). Capturing such interaction with market conditions, even crudely, is essential; a rent freeze in Switzerland would reduce real rents much less than indexing rents at even 90 percent of inflation in Argentina.

Malpezzi and Ball (1993) emphasize the preliminary nature of this first index. Other research on the construction of such indices highlights the difficulty of constructing accurate indices (Page and Struyk 1990). But even the preliminary index yielded sensible results. Here I present values of a similar index, with separate indices for each city rather than each country. Values of the index in the full range of 51 countries range from 0 to 21, but the values for the eight cities studied range from 8.7 to 20 (table 1). Even including New York and Santa Monica, the U.S. regimes are rather tame by the standards of developing countries.

Most U.S. systems are more directly comparable to European systems than to systems in developing countries.[7] In particular, no U.S. cities set rent levels; they regulate rent increases. All U.S. cities, even Santa Monica, exempt at least some new

[7] Malpezzi and Ball (1991, 1993) demonstrate that developing countries with controls generally have stricter controls than developed countries.

Table 1. **Rent Control Index for Eight Cities**

	Enforcement	Coverage	Set Rent Levels	New Construction	Rent Adjustments over Time	Rent Adjustments/New Tenancy	Cost Pass-Thru	Inflation	Tenure Security	Overall Index
	0 = none	0 = minimal	0 = none	0 = exempt	0 = indexed	0 = reset	0 = pass-thru		0 = standard lease	
	1 = selective/ NI	1 = significant/NI	1 = some/ NI	1 = some exempt/ NI	1 = partial index/ NI	1 = partial reset/ NI	1 = partial pass-thru	Avg annual rate, 1960–85	1 = some restriction/NI	
	2 = strong	2 = widespread	2 = strict or low	2 = not exempt	2 = not indexed	2 = not adjusted	2 = not passed thru		2 = strong security	See text
Rio de Janeiro	1	1	0	1	1	0	1	59.1	0	11.9
Cairo	1	2	1	2	2	1	2	8.9	2	14.9
Kumasi	2	2	2	2	2	2	2	30.2	2	20.0
Bangalore	1	2	2	1	1	2	1	7.7	2	13.8
New York	2	2	0	1	1	1	1	6.7	0	9.7
Los Angeles	2	2	0	0	1	0	1	6.7	0	7.7
Santa Monica	2	2	0	1	1	1	1	6.7	0	9.7
Washington	2	2	0	0	1	1	1	6.7	0	8.7

Note: NI = no information.

construction, although the credibility of promises to continue such exemptions is debatable, at least in New York, where post-war construction was initially exempt but later became subject to rent stabilization.

Measuring changes in consumer's surplus from rent control

Binding rent controls reduce the rents that consumers pay, which presumably benefits them but at a cost borne by landlords. Natural first questions to ask are, How much do controls benefit tenants, and how does the benefit compare with the costs borne by landlords? Economists measure such costs and benefits using the notion of *consumer's surplus*. The underlying idea is that when consumers purchase a good or service they must logically value it at least as much as they pay for it, but possibly more.[8] Consumer's surplus is a measure of how much more, or how much extra benefit they receive from consuming the good at prevailing prices.[9] Government actions that affect prices and consumption therefore change consumer's surplus; changes in consumer's surplus are measures of the benefits (and costs) of government actions.

Estimating consumer's surplus

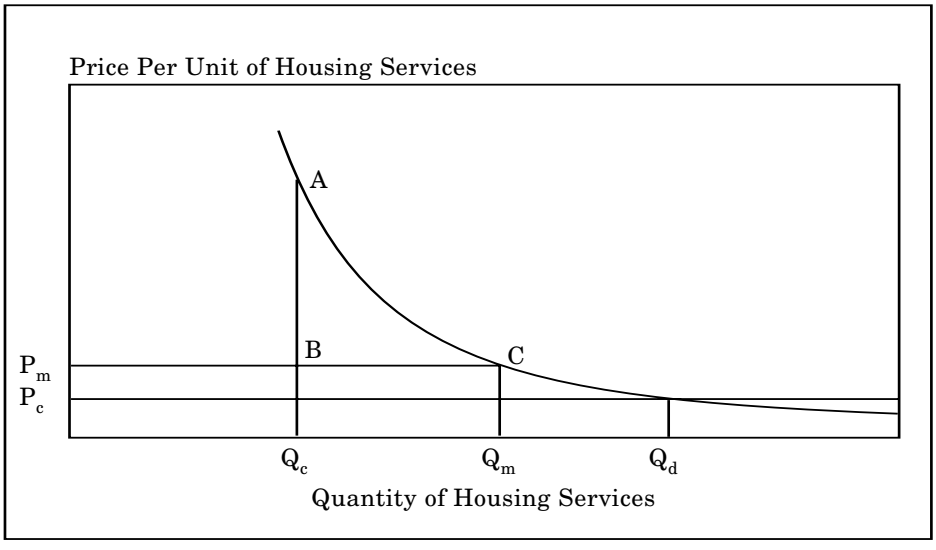
The first published estimates of changes in consumer's surplus from controls were in Olsen's (1972) analysis of New York. Generally such estimates assume that there is an uncontrolled housing market as well as a rent-controlled market (or at least that it is possible to estimate what market rents would be). It is assumed that rents (R) can be decomposed into a quantity of housing services (Q) provided by each unit reflecting all the characteristics associated with the unit: size, amenities, appearance, location and physical features, and a price per unit of these services (P). By definition, $R = PQ$. The changes in consumer's

[8] Briefly, consumer's surplus is a measure of the consumer's willingness to pay more than current market price for at least some units of the good in question, if that were necessary to obtain them. For example, if some coffee drinkers would be willing to pay more than current market price for the day's first few cups of coffee (as this author would), the difference between the amount they would pay (if necessary) and the amount they do pay is their consumer's surplus. This commonsense notion is formalized below.

[9] Thus, if consumers purchase goods for which their total benefit just equals their total expenditure, *net* consumer's surplus is 0.

surplus resulting from controls can be made more clear with the aid of figure 1.

Figure 1. **Rent Control and Consumer's Surplus**



With an uncontrolled (market) rent per unit of housing services, P_m, households would consume Q_m units of housing service and pay a rent P_mQ_m. The immediate effect of rent control is to reduce rent to P_cQ_m, where P_c is the controlled rent per unit of housing services. Thus the consumer spends $P_mQ_m–P_cQ_m$ more on nonhousing goods.

At price P_c the consumer would demand Q_d units of housing services. However, under real-world rent control regimes, landlords have no incentive to increase the flow of housing services to Q_d, and indeed, if landlords reduce maintenance as a response to controls, tenants are likely to end up consuming only Q_c housing units. Households will find it more difficult to obtain and move to suitable units and will systematically consume "off their demand curve."[10]

[10] Strictly speaking, households could end up consuming "too much" housing as well as "too little"; for example, elderly households might stay in large units if giving them up meant moving to market rents and losing their subsidy. Empirically, in the markets under study, the short-side ration ($Q_c < Q_m$) predominated. Also, it is theoretically possible to design a rent control regime in which controlled rents are based on maintenance in such a way as to give landlords an incentive to "overmaintain" units. Malpezzi (1986) and Olsen (1988) demonstrate that this is theoretically possible, but to date no one has found a corresponding real-world rent control regime.

This geometric exposition illustrates the basic method quite well, but an algebraic generalization is better suited for actually estimating the size of welfare gains and losses using a sample. It can be shown that if the price elasticity of demand is constant, the benefit of a program that changes prices and quantities can be written as

$$\text{Benefit} = P_m Q_m - P_c Q_c + \left(\frac{1}{Q_m}\right)^{\frac{1}{b}} \left(\frac{b}{b+1}\right)\left(Q_c^{\frac{b+1}{b}} - Q_m^{\frac{b+1}{b}}\right), \tag{1}$$

where Q_m is the predicted housing consumption in the absence of rent controls, Q_c is the housing consumption under rent controls, $P_m Q_m$ is the estimated rent in the absence of controls, $P_c Q_c$ is the observed controlled rent, and b is the price elasticity of demand.

In the special case for which the price elasticity of demand, b, is equal to -1, the expression $b/(b+1)$ is undefined. But it can be shown that in this special case the benefit can be expressed using natural logarithms as

$$\text{Benefit} = P_m Q_m - P_c Q_c + P_m Q_m \left[\log\left(P_m Q_c\right) - \log\left(P_m Q_m\right)\right]. \tag{2}$$

These two related equations are the centerpiece of the empirical analysis that follows.

These static measures do not include all possible costs and benefits to tenants. For example, rent control may increase transaction costs for tenants, including search costs, and increase waiting time for housing units. These increases will reduce the benefits to tenants, but the full system may also increase the bundle of property rights, such as security of tenure, enjoyed by tenants, thus increasing their benefits in this area. In other words, the measures of consumer's surplus just described are better approximations of true benefits than the often-used simple difference between controlled and market rents, but these measures are static and are still approximations.

The cost imposed on landlords is straightforwardly approximated by $P_m Q_c - P_c Q_c$, or the difference between controlled and market rents for the unit inhabited by the tenant. This measure of cost to landlords does not include losses from prior accelerated

depreciation of the unit.[11] The cost to landlords would also include losses from the uncompensated transfer of property rights to renters. The true costs to landlords may therefore exceed the $P_mQ_c - P_cQ_c$ estimates.

There are many other interesting issues to study, including the effects of controls on maintenance and supply over time, on tenant mobility, and on profitability, as well as the time paths of rents under alternatives for decontrol. These and other issues are discussed in the references cited above.

Empirical implementation

The empirical estimation of the model requires four pieces of information:

1. The rent currently paid for the current controlled unit, P_cQ_c—this is generally directly observable.

2. The rent that the current unit would rent for in the absence of controls, P_mQ_c—this can be estimated using hedonic indices from a "reference group"[12] of uncontrolled units.

3. The amount that the household would spend on housing in the absence of rent control, P_mQ_m—this can be estimated from a demand equation of uncontrolled households.

4. The price elasticity of demand for housing, b—this can be estimated or varied parametrically.

Perhaps the single most difficult empirical problem is choosing a statistical reference group of uncontrolled households and units. It must be reasonable to assume that they are enough like the controlled group that they are comparable—or can be made so statistically. It must be reasonable to assume that rents are not so distorted in the reference group by the presence of controls as to be unreliable guides to rents in the absence of controls—or that a good adjustment can be made for that distortion. Three

---

[11] However, these losses could be partly offset by saving some maintenance expenditures.

[12] Statistical jargon uses the phrase "treatment group" for observations that have something done to them and "control group" for those left alone. So, to a statistician, rent-controlled households or housing units would be the treatment group, and uncontrolled households or units the control group, which I have called the "reference group" to avoid confusion.

points should be noted. First, regression analysis is, in fact, a statistical method that enables analysis of treatment and reference groups that are not identical. Second, while it is possible that households in the two groups are systematically different in their demand for housing, Malpezzi (1986) and Malpezzi, Tipple, and Willis (1990) found that such selectivity bias does not make much difference in Cairo or in Kumasi; Caudill, Ault, and Saba (1989) found that it did make some difference in Vancouver. Third, Fallis and Smith (1984) and several of the developing-country case studies found that rent controls can, under some circumstances, affect rents in the uncontrolled sector. We use the Malpezzi and Mayo (1987) cross-country model of housing demand to test for, and if necessary correct for, such a problem.

Another, somewhat technical issue that must be mentioned is that it is possible to calculate costs and benefits separately for each of a sample of controlled households and then examine sample statistics (e.g., the median net benefit for the sample). Alternatively, one can calculate the characteristics of a typical or "representative" consumer (e.g., by taking medians of relevant variables) and then calculate the costs and benefits for this synthetic individual. There is no particular reason these methods should give the same results, although similar results would be welcome evidence of robustness. Examining sample statistics may understate net benefits if an individual household's true (unobserved) demand is very different from estimated demand. But using a representative consumer may not necessarily accurately represent the costs and benefits of actual households, depending on the joint distribution of characteristics.

Consumer's surplus results from eight cities

Cairo

Malpezzi (1986) presents estimates of the costs and benefits of rent control in Cairo. Controlled units in Cairo rent for much less than estimates of their market rent in the absence of controls. However, as shown in table 2, when account is taken of side payments—including key money, utilities, maintenance and repair, and upgrading by tenants—the discount is greatly reduced for the typical (median) household. When side payments are excluded, the median estimate of the price per unit of housing services is about 38 percent of the estimated long-run equilibrium market price. When side payments are included, the ratio increases to 70 percent of the market price. But it must also be emphasized that there is a wide distribution around this

Table 2. **Cost-Benefit Measures from Cairo, 1981**

	Quantity of Housing Service	Price	Implicit Subsidy	Net Benefit
Q3	29.4	1.46	12.00	8.11
Median	17.0	0.71	3.27	–1.04
Q1	10.1	0.42	–3.76	–27.66
N	312	297	237	237
Prob > |*s*|			.001	.001
Representative consumer	17.0	0.71	4.93	4.30

Source: Malpezzi (1986).
Note: Figures are based on gross rents including annualized key money and other side payments. Prices are normalized at 1 for estimated market price; all other variables in Egyptian pounds per month. One pound was approximately one U.S. dollar in 1981 at unofficial rates. Median renter income is 87 pounds. Q3, Median, and Q1 are third quartile, median, and first quartile, respectively, of costs and benefits calculated separately for each controlled household in the sample. Prob > |*s*|: Probability of observing such a large centered signed rent statistic (not reported) if the population mean is 0. The welfare position of the representative consumer is calculated by setting income, quantity, and price to sample medians.

median. Quite a few Cairo households do receive large discounts, just as some pay very high prices for housing services. That these differences appear to be largely unrelated to tenant characteristics measuring ability to pay raises questions of horizontal equity. Otherwise equal households receive quite different housing deals.

A central insight of consumer's surplus is that if controls lower prices but restrict the housing choices available, consuming too little (or too much) housing can at least partially erode the gains from lower prices. Analysis of the Cairo renter data suggests that many households have significant welfare losses from under- *and* overconsumption of housing services.[13] Underconsumption appears to dominate, but about a third of the renters consume more housing than predicted by their demand relation. However, as illustrated in table 2, the median of net benefits calculated for each household in the Cairo sample of controlled renters is quite different from the net benefit calculated for the representative consumer.

On distributional issues, Malpezzi also presented evidence from Cairo that median landlord incomes were higher than median

[13] Under- and overconsumption are measured by how far households are from their estimated demand curve (see Malpezzi 1986 for more details).

tenant incomes: 127 versus 87, in 1981 Egyptian pounds.[14] While the difference is not negligible, typical landlords in Cairo are by no means rich. And there was significant overlap in the distributions.

Kumasi

Malpezzi, Tipple, and Willis (1990) analyzed the costs and benefits of controls in Kumasi. Ninety percent of Kumasi's population rent or live as tenants in family houses. Based on 1986 data, typical controlled rents were less than 2 percent of total consumption. A simple cross-country model predicted that the median rent-to-income ratio would be about 0.08 in the absence of controls. Malpezzi, Tipple, and Willis found that renters paid roughly half the estimated market rents.[15] Furthermore, while the controlled rents (P_cQ_c) hardly vary, the estimated market rents (P_mQ_c) vary with size and type of unit. Estimated household expenditure at market prices (P_mQ_m) varies even more.

Key results are in table 3. The median cost of the subsidy implied by these rent reductions is estimated to be about 274 cedis per month in the tenement sector and 291 in the indigenous sector.[16] But households would spend even more on housing in the absence of controls. Median estimated market demand (P_mQ_m) is more than 1,000 cedis per month in both sectors. Comparing P_mQ_c and P_mQ_m, it appears that while units rent for less because of controls, households would spend even more at market prices; that is, consumption of housing services has been greatly reduced under controls.

Rent control imposes a landlord cost (P_mQ_c–P_cQ_c) that exceeds the net benefit to tenants in both sectors. The "transfer efficiency" (ratio of benefits to costs) is therefore low. Under the most favorable assumption (that the price elasticity is –1), the efficiency is 40 to 50 percent; tenants receive net benefits that

[14] At the time of the study, circa 1981, an Egyptian pound was worth about a dollar.

[15] For the great majority of units, rents were fixed at 300 cedis per room per month. Such fixed rents are in some sense a more strict regime than Indian systems, almost all of which permit some variation by type of unit. In 1986, there were about 90 cedis to the dollar.

[16] Kumasi's rental sector comprises mainly individual rooms in indigenous compound housing and rooms in four- to five-story walk-up tenements. Given the physical differences in housing units, we analyzed them separately, although in the event we obtained very similar results for each.

Table 3. **Cost-Benefit Measures from Kumasi, 1986**

	Current Controlled Rent, P_cQ_c	Market Rent for Current Unit, P_mQ_c	Estimated Rent with No Controls, P_mQ_m	Cost of Rent Control Subsidy, $P_mQ_c-P_cQ_c$	Tenant Benefit, $b = -1$	Tenant Benefit, $b = -0.5$	Transfer Efficiency, Percent, $b = -1$
Tenement Sample							
Mean	290	613	1,094	332	106	–264	
Q3	300	580	1,220	287	221	125	
Median	300	574	1,040	274	135	–76	50
Q1	300	570	909	270	14	–415	
N	358	343	328	343	328	328	
Representative consumer	300	574	1,040	274	122	–105	45
Indigenous Sample							
Mean	244	563	1,105	319	72	–382	
Q3	300	580	1,205	370	221	80	
Median	250	574	1,044	291	123	–127	42
Q1	200	513	910	275	–17	–542	
N	322	319	311	319	310	310	
Representative consumer	250	574	1,044	324	169	–61	52

Source: Malpezzi, Tipple, and Willis (1990).
Note: All figures except *N* and transfer efficiency are in cedis per month. In 1986, there were about 90 cedis to the dollar.

are less than half the static cost to landlords. If the price elasticity is on the order of –0.5, net benefits to most tenants are negative; both landlords and (most) tenants are made worse off by controls.

While costs and benefits are large relative to rents paid, they are small relative to income. The cost of the subsidy is usually on the order of 2 to 3 percent of consumption. Net tenant benefits are, at best, small relative to total consumption. This result obtains whether we consider the median of net benefits calculated

separately for each household or the net benefits for the so-called representative consumer.

The bottom line, then, is that rent control reduces the rents households pay, but the benefit of this rent reduction is more or less offset by the welfare loss from underconsumption of housing. We estimate that *existing* units of typical quality would have rented for about twice current rents in 1986, but that households would typically spend more than three times current rents—implying higher housing consumption, if supply was elastic.

Malpezzi, Tipple, and Willis were also able to analyze the income of tenants and landlords. Broadly, the results were similar to those in Cairo. In Kumasi, incomes of landlords were on average about 36 percent higher than those of tenants; but about one-quarter of landlords had incomes below the median renter income, and one-quarter of renters had incomes above the median landlord income.

Bangalore

Malpezzi and Tewari (1991) analyzed controls for Bangalore. A little more than a fifth of controlled households were in the strictly controlled sector. Using a household survey carried out in 1974 by Prakasarao and Tewari (1979), Malpezzi and Tewari found large differences in outcomes for the two controlled groups (see table 4).

Controlled renters paid less than the estimated market rents for their units, but the amount of subsidy is highly dependent on whether the unit is strictly controlled. The median rent paid (P_cQ_c) is 92 percent of the estimated market rent (P_mQ_c) for ordinary controlled units, but only 42 percent for strictly controlled units. The median cost of the subsidy implied by these rent reductions was estimated to be about 7 rupees per month for ordinary units and 26 rupees for strictly controlled units.[17]

The total amount households actually spend on housing was also reduced below uncontrolled levels. Median P_mQ_m is approximately twice the consumption in the presence of controls for both ordinary and strictly controlled units. Moreover, a comparison of P_mQ_c and P_mQ_m for ordinary controlled units shows that while units rent for less because of controls, the actual value of housing consumed has also declined; that is, consumption of housing

[17] In 1974, there were about 10 rupees to the dollar.

Table 4. **Cost-Benefit Measures from Bangalore, 1974**

	Current Controlled Rent, P_cQ_c	Market Rent for Current Unit, P_mQ_c	Estimated Rent with No Controls, P_mQ_m	Cost of Rent Control Subsidy, $P_mQ_c - P_cQ_c$	Tenant Benefit, $b = -1$	Tenant Benefit, $b = -0.5$	Transfer Efficiency, Percent, $b = -1$
Ordinary Controlled Renters							
Mean	96	113	165	13	–42	–215	
Q3	110	120	198	31	22	10	
Median	60	65	133	7	–24	–57	NA
Q1	40	47	90	–15	–72	–179	
N	87	75	75	75	75	75	
Representative consumer	60	65	133	5	–22	–66	NA
Strictly Controlled Renters							
Mean	74	111	103	24	2	–31	
Q3	90	162	112	40	35	36	
Median	45	107	97	26	15	9	58
Q1	25	47	75	–4	–25	–67	
N	25	18	18	18	18	18	
Representative consumer	45	107	97	62	62	61	100

Source: Malpezzi and Tewari (1991).
Note: All figures except *N* are in rupees per month. In 1974, there were about 10 rupees to the dollar. NA = not applicable.

services has been greatly reduced under controls ($Q_c < Q_m$). However, a comparison for strictly controlled units finds that P_mQ_c exceeds P_mQ_m for most renters; these households are consuming more housing under controls.

If the loss from the reduction of housing consumption is subtracted from the subsidy paid by landlords, the net benefit to occupants of ordinary controlled units is negative—both landlords and (most) tenants are made worse off by controls. Such analysis finds that occupants of strictly controlled units do

receive net positive benefits, but that the level received is small. Examining sample statistics on net benefits, if $b = -1$ (the most favorable assumption in terms of controls' efficiency), tenants in strictly controlled units receive net benefits that are typically less than 60 percent of the static cost to landlords. If the price elasticity is on the order of -0.5, net benefits to strictly controlled renters average only 33 percent of the cost to landlords. Thus, a relatively small portion of the renting population is slightly better off (i.e., the majority of the small strictly controlled sector and a minority of the ordinary controlled sector), while the majority of renters, as well as landlords, are worse off. Comparison of these results with the representative consumer results shows that the results for ordinary controlled renters are quite robust but that the estimated benefit for the representative strictly controlled consumer is virtually the same as its static cost.

While costs and benefits are often large relative to rents paid, they are small relative to Bangalore incomes. The cost of the typical subsidy to ordinary controlled renters is about 1 percent of their typical income; few households receive subsidies greater than 5 percent of income. For strictly controlled renters, typical subsidies are around 6 percent of typical incomes. Net tenant benefits are, at best, negligible compared with total income.

Again, discussion of the typical tenant and the medians masks the fact that these welfare estimates have wide distributions. Even in the strictly controlled submarket, more than one-quarter of households have negative estimated net benefits. And more than a quarter of ordinary controlled households have positive estimated benefits, even under the lower price elasticity.

Malpezzi and Tewari (1991) also examined the distribution of benefits with respect to income, length of stay, and several other demographic characteristics. Benefits were found to be so weakly related to income, household size, length of tenure, and other potential determinants that the analysis could not statistically reject the hypothesis that benefits are conferred randomly.

Other distributional issues were also studied. The Bangalore survey permitted identification of landlords and contained data on income from property. Some rental tenants are themselves landlords, so Malpezzi and Tewari constructed a three-way classification: (1) tenant, not a landlord; (2) landlords who own their own dwellings; and (3) landlords who are themselves renters. They also constructed a two-way classification: (1) tenant, not a landlord, and (2) all landlords.

Malpezzi and Tewari found that both classes of landlords have higher incomes than tenants who are not landlords, on average; median incomes for the landlord groups are 70 percent higher than those for nonlandlord tenants. They also found that almost one-quarter of the landlords have incomes below the median nonlandlord renter income and that almost one-quarter of the nonlandlord renters have incomes greater than the median landlord income. More than 10 percent of renters (110 out of 1,045), are also landlords, and as a class, they are as rich as homeowning landlords. Most landlords hold relatively few units; the ratio of occupied rental units to landlords is about 4.

Thus it does not appear that rent control does very much redistribution of income from rich to poor, and it almost certainly does some redistribution in the wrong direction. Of course, richer tenants own more units. The data were reanalyzed weighted by income from property. When the data are so weighted, the income disparities between landlords and tenants who do not own other property are accentuated, but the fact that renters who themselves own property are actually the richer class remains unaltered.

Rio de Janeiro

Silveira and Malpezzi analyzed 1980 Brazilian census data for Rio to examine the static costs and benefits of controls. They found that a typical controlled household pays rent that is not very different from what it would pay if market conditions prevailed (see table 5). The rent paid by the median household is about 90 percent of the estimated market rent. This meager discount translates into a net loss of 95 cruzeiros to the average renter (356 cruzeiros assuming the lower demand elasticity of –0.5) once changes in housing consumption are taken into account.[18] For a representative tenant, there is a positive benefit of 374 cruzeiros. However, this benefit is still exceeded by the cost of 376 cruzeiros to a representative landlord.

Rio's rent control imposes a measurable static cost to the landlord of a controlled unit, but this cost as a share of income is smaller than that found in Cairo, Kumasi, or Bangalore. Still, the median cost of the subsidy is estimated to be about 175 cruzeiros per month, or 6 percent of the actual rent. In the case of a representative landlord, however, the loss is more than 13 percent of the actual rent.

[18] At the time of the data collection, there were about 85 cruzeiros to the dollar.

Table 5. **Cost-Benefit Measures from Rio, 1980**

	Current Controlled Rent, P_cQ_c	Market Rent for Current Unit, P_mQ_c	Estimated Rent with No Controls, P_mQ_m	Cost of Rent Control Subsidy, $P_mQ_c - P_cQ_c$	Tenant Benefit, $b = -1$	Tenant Benefit, $b = -0.5$	Net Welfare Change, (Benefit [at $b = -1$] – Cost)	Transfer Efficiency, Percent, $b = -1$
Mean	4,325	3,825	3,503	–447	–850	–1,214	–390	
Q3	5,310	5,141	4,450	979	745	569	–31	
Median	2,800	3,176	3,060	175	–95	–356	–146	NA
Q1	1,500	1,899	2,081	–953	–1,381	–1,762	–445	
N	717	705	666	701	648	648	648	
Representative consumer	2,800	3,176	3,060	376	374	372	–2	99

Source: Silveira and Malpezzi (1991).
Note: All figures except N are in cruzeiros per month. In 1980, there were about 85 cruzeiros to the dollar. NA = not applicable.

While losses are still significant relative to rents paid, they are small relative to income. The cost of the subsidy from landlords is on the order of 1 percent of median tenant income, while tenant losses are 0.5 percent of income. It is clear that static, monetary welfare costs are much lower than in other rent control regimes surveyed by the rent control project.

New York

One of the first published studies of the costs and benefits of rent control is Olsen (1972). Using data from New York City in 1968, Olsen used estimates from a hedonic index of uncontrolled units to predict the market rents of controlled units (table 6).[19]

Table 6. **Summary Cost-Benefit Measures from New York, 1968**

	Current Controlled Rent, P_cQ_c	Market Rent for Current Unit, P_mQ_c	Estimated Rent with No Controls, P_mQ_m	Cost of Rent Control Subsidy, $P_mQ_c-P_cQ_c$	Tenant Benefit, $b = -1$	Transfer Efficiency, Percent, $b = -1$
Means	999	1,405	1,470	406	213	53
Mean/ Income	0.160	0.226	0.236	0.065	0.034	

Source: Olsen (1972).
Note: Olsen reported annual amounts. Currency unit is 1968 U.S. dollars.

In an analogous fashion, he used the data from the uncontrolled portion of the market to estimate the uncontrolled demand for housing services. The average controlled rent for an apartment was $999 per year; for comparison, the average annual income was $6,229.[20] The average uncontrolled annual rent predicted by the hedonic results for those same units was $1,405, implying a subsidy of $406. The average free-market expenditure for the controlled households was $l,470 per year, indicating that the average household in the controlled market consumed about 4.5 percent less housing than it would have in the free market.

Olsen computed the economic benefit of rent control to each tenant under the assumption of a unitary price elasticity—that

[19] At the time rent stabilization was not in force, so there was a reasonably clear delineation between controlled and uncontrolled units.

[20] Data from Olsen's results are in 1968 dollars. Between 1968 and 1992 the implicit price deflator for gross domestic product rose by a factor of about 3.8.

is, using equation (2). Olsen's estimate of the average net benefit is $213 per year, little more than half the gross subsidy implied by rent control.

The benefits are found to be slightly negatively related to income, larger for larger households, and larger for households headed by older people. The annual benefit is estimated to decrease by about 1 cent for every dollar of additional income, to increase by $9 per year of head's age, and to increase by $69 per additional household member. Olsen notes that these results may understate the progressivity of benefits because lower income people are more likely to rent in the controlled market and, hence, to appear in the regression sample. Benefits do not vary significantly by race or sex of head of household. Rent control in New York City in 1968 appears to redistribute income, but very weakly and to an extent in no way proportional to its cost.

Los Angeles

The first careful empirical analysis of the Los Angeles regime was carried out by the Rand Corporation (Murray et al. 1991; Rydell et al. 1981). In 1981 the Los Angeles city council was contemplating several changes to rent control ordinances, and the Rand reports were produced as background analysis. The methodology of the Rand study was somewhat different from that of the studies discussed to this point. The goal was to include dynamic effects of changes in rent control regimes, changes in additions and deletions to the rental stock, and changes in depreciation and maintenance, as well as changes in consumer's surplus. The Rand team therefore built a simulation model that predicted the time path of rents and the quantity of housing services given alternative changes in the regime. Given the narrow comparative static focus of this article, I will not discuss the simulation model and results in detail here but will focus on a few key results that can be roughly compared with those from other studies.

The Rand team analyzed historical data on rents before the imposition of controls and found that "during eight years of 10 percent average rent inflation, rents for units keeping the same tenant would increase by only 8.2 percent, while the rents charged new tenants would increase by 14.3 percent" (Murray et al. 1991, 609).[21] By analyzing data from the Housing Assistance

[21] The existence of length-of-tenure discounts for sitting tenants is well documented (see for example Malpezzi, Ozanne, and Thibodeau 1980).

Supply Experiment (Lowry 1983), they found that if (at one bound) landlords cut maintenance to zero, the quantity of housing services from the existing stock could fall by about 8 percent per year. An aggregate stock adjustment model applied to precontrol data from Los Angeles yielded estimates of changes in the stock itself (removals, conversions, new construction) given changes in price. Given these parameters, the Rand team could simulate how different limits imposed by alternative control schemes would change the time path of rents; could then decompose these changes into changes in price and changes in quantity; and then, given parametric demand assumptions, could measure changes in consumer's surplus over time.

Key results from these simulations are summarized in table 7. The Rand consumer's surplus calculations differ in several other ways from those reported above. Those above are "snapshot" estimates from a single cross section from observed market data; the Rand estimates are present values of a decade's simulated experience, under alternative assumptions about market conditions and type of regime (although the assumptions are drawn from careful analysis of actual market data). The estimates above are computed for sample and representative households;

Table 7. **Summary Cost-Benefit Measures from Los Angeles, Simulated 1980–90**

Rent Reduction	Tenant Losses[a]	Net Tenant Benefit	Landlord Adjustments[b]	Net Landlord Costs
Regime 1: 7.6 percent annual increase if occupied; no limit on vacancies:				
509.1	–281.8	227.3	222.1	286.9
Regime 2: 5.6 percent annual increase if occupied; 10 percent limit on vacancies:				
1,339.9	–744.0	595.9	428.4	911.5
Regime 3: 5.6 percent annual increase if occupied; no limit on vacancies:				
739.5	–404.1	364.3	299.1	440.3

Source: Murray et al. (1991).
Note: All figures in 10-year present values, aggregates, millions of 1978 dollars.
[a] Tenant losses from housing deterioration, administrative fees, and lost consumer's surplus. In all cases, loss from deterioration is much larger than loss from fees and lost consumer's surplus.
[b] Landlord gain from unspent maintenance expenditures, losses from fees and removals. In all cases, substantial savings from unspent maintenance are partially offset by fees and removals.

the Rand estimates are aggregates for all renter households. While the estimates above focus on the change in consumer's surplus *given* a housing stock, the Rand estimates include administrative fees and also simulate how much the stock might change over a decade from accelerated deterioration and removals.

Results include three key points. First, the magnitude of the effects varies substantially with details of the regime. For example, a 2-percentage-point reduction in the cap on allowable rent increases for occupied units increases the aggregate rent reduction by 45 percent and increases tenant losses from accelerated depreciation and lost consumer's surplus by a similar percentage. Second, numbers presented in the Rand report (but not in table 7) suggest that dynamic losses (not estimated in the studies above) can be substantial; in fact, they outweigh consumer's surplus losses by as much as a factor of 18 (Murray et al. 1991, 622–23). Third, once again tenant benefits are substantially less than landlord costs; the transfer efficiency in the three cases presented ranges from 65 percent to 83 percent.

Several years after the Rand report, a follow-up analysis of Los Angeles's rent control was conducted by the firm of Hamilton, Rabinovitz, Szanton, and Alschuler and the Urban Institute (HRSA-UI 1985). Among other analyses, the study team estimated P_mQ_c with 1977 Annual Housing Survey hedonic indexes, updated to the study period using the CPI. The average difference between P_mQ_c and P_cQ_c was only \$7 per month (\$415 and \$408, respectively; HRSA-UI 1985, exhibit 2-12).[22]

How does this compare with the Rand result? A crude calculation suggests that \$7 per month has a present value of something like \$700, and there are about 600,000 renters in the city of Los Angeles, yielding a total rent reduction of about \$420 million. The Rand calculation closest to the actual Los Angeles ordinance yielded a rent reduction of \$509 million.

Santa Monica

Santa Monica has the strictest regime of the American cities considered in this article, and probably one of the strictest in the country. To date, the empirical analyses of this market have been more rough and ready than those of the other markets studied. Shulman (1981) reports that in the eight years before

[22] An alternative measure of P_mQ_c using data from recently vacated units was \$11 higher (\$426).

the establishment of controls, real rents rose in Santa Monica while they were falling in nearby Los Angeles and Orange Counties.[23] He also reports Society of Real Estate Appraisers data indicating that gross rent multipliers increased from 7.7 to 12.1 during this period (i.e., that asset prices were rising even faster). Surprisingly, the multipliers did not decline after the imposition of controls, at least for the few years analyzed by Shulman. His conjecture is that owners believed that controls would be a temporary phenomenon; unfortunately there are no more-recent *published* data that test the hypothesis that the multiplier has declined as controls have remained in force.

A recent study by Levine, Grigsby, and Heskin (1990) compares telephone surveys of Santa Monica tenants between 1979–80 and 1987. Of 384 units reporting data on rents, 352 are under controls. The median of all rents is $444, and of controlled rents $439. So the Levine, Grigsby, and Heskin study yields an estimate of P_cQ_c; they estimate P_mQ_c using hedonics and find the difference to be $191. The authors are concerned about the reliability of their hedonic estimates, and in fact relegate that result to a footnote without detailed explanation. Instead they compare the distribution of P_cQ_c in 1987 with that from the 1979–80 survey, using the residential rent component of the Los Angeles–Long Beach CPI to inflate the baseline rents. So *if* in the absence of controls rents had risen at the same rate in Santa Monica as in Los Angeles–Long Beach, Levine, Grigsby, and Heskin reckon that the average unit has a rent about $159 less than it would without controls.[24]

Levine, Grigsby, and Heskin also address distribution. While their discussion is somewhat confusing, taking their results at face value it appears that Santa Monica rent-to-income ratios have more or less held constant across the income distribution between 1979 and 1987 and that in fact rent-to-income ratios declined a bit at the very bottom of the income distribution.

Washington

The most careful study of Washington's rent control to date has been carried out by Turner (1988) and her colleagues. As usual,

[23] The median 1978 rent was reported to be $281, an increase of 113 percent between 1970 and 1978. During the same period the CPI rose by 68 percent, and rents in Los Angeles–Orange County rose only 51 percent (Shulman 1981, 44). The changes in median rents are unadjusted for quality change.

[24] While they do not report the median, examination of their figure 1 yields a difference of about $160 at the median rent.

the biggest problem is in locating or constructing a statistical reference group to estimate market rents for comparison. Turner estimates a hedonic index using 1977 data on uncontrolled units before the imposition of controls, prices the 1987 controlled units in 1977 dollars, and then inflates the 1977 estimated market rents using an average of the rental CPI for several large uncontrolled cities. If it is true that rents would have increased at the same rate as the CPI for these cities, Turner finds an average difference between P_cQ_c and P_mQ_c of about \$95 to \$100.[25]

Turner also points out that, generally, long-time renters presumably benefit most from Washington's controls. Since rents are reset for new tenants, recent movers therefore pay at least as much as they would without controls. However, Turner does not actually estimate the benefit to tenants from controls, only the rent reduction, which is the upper bound on benefits. Turner notes that in the District of Columbia more affluent renters move more frequently; so presumably a lower fraction of rich tenants benefit from controls than poor tenants, given that recent movers will benefit less. Turner does not present estimates of the *size* of rent reductions by income.

Synthesis

Summary of studies that measure consumer's surplus of controls

Summary statistics of costs and benefits from the studies above are presented in table 8, expressed in percent for ease of comparison.[26] The discussion emphasized variation in costs and benefits within markets; perhaps the most immediately striking feature of the summary numbers is the wide variation across markets. On average, controls confer discounts that are large

[25] While Turner discusses the difference between P_mQ_c and P_cQ_c at some length, in several places, she does not report either the average rent Washington households pay or the average estimated market rent. For comparisons in table 8, therefore, I obtained estimates of average rent from the American Housing Survey and estimated average market rent from Turner's reported differences.

[26] Of course, the comparisons are still inexact. While attempts were made to compare studies that had similar methodologies, where possible, there are differences between studies. The most comparable studies are the Marshallian consumer's surplus measures used in Kumasi, Bangalore, and Rio and in Olsen's study of New York; Malpezzi (1986) uses a Hicksian measure adjusted for the presence of rationing. The studies of Los Angeles, Santa Monica, and Washington use fairly different methodologies.

Table 8. **Summary of Cost-Benefit Studies**

Study	Rent Reduction as Percent of		Benefit as Percent of		Transfer Efficiency, Percent
	Market Rent	Tenant Income	Market Rent	Tenant Income	
Cairo (Malpezzi 1986)	29	5.8	26	5.1	87
Kumasi (Malpezzi, Tipple, and Willis 1990)	48	2.4	21	1.1	45
"Ordinary" Bangalore (Malpezzi and Tewari 1991)	8	0.8	Negative Benefit	Negative Benefit	Negative Benefit
"Strict" Bangalore (Malpezzi and Tewari 1991)	58	15.1	58	15.1	100
Rio de Janeiro (Silveira and Malpezzi 1991)	12	2.0	12	2.0	99
New York (Olsen 1972)	28	6.5	14	3.4	52
Los Angeles (Murray et al. 1991)	1.7 (approx)	0.4	1.4	0.3	79
Santa Monica (Levine, Grigsby, and Heskin 1990)	27 (approx)	5	NA	NA	NA
Washington (Turner 1988)	20 (approx)	6	NA	NA	NA

Note: Comparisons are approximate only. Los Angeles, Santa Monica, and especially Washington are not strictly comparable with other estimates.

relative to rents in New York, Santa Monica, Cairo, and Kumasi and especially among Bangalore renters under strict controls. While key money reduces the Cairo discount substantially, it does not eliminate it. Ordinary Bangalore renters, Rio renters, and especially Los Angeles renters receive much smaller rent

---

Also, I made some rough adjustments to some numbers to give them a similar metric. To convert the Rand aggregate present values to monthly household figures, I assumed a 1 percent discount rate per month for 10 years and 600,000 Los Angeles renter households. I assumed average Los Angeles rents of $408 and incomes of $1,970 (i.e., assumed the same for Rand as reported in HRSA-UI 1985). Turner reported the size of rent reduction but not average rents or incomes; based on American Housing Survey data, I assumed an annual income of $20,000 and controlled rents that averaged 23 percent of income.

reductions. While the average Los Angeles discount is very small, the other average U.S. discounts are substantial. Patterns are broadly similar when the discount is compared with tenant incomes.[27]

The benefits to tenants of these reductions in rent are not directly related to the size of the rent reduction. Among the markets with the highest transfer efficiency[28] are the markets with one of the largest and one of the smallest total rent reductions (strict Bangalore and Rio, respectively). The relatively relaxed system of controls in Rio yields little rent reduction and little measurable distortion in housing consumption and so is relatively efficient; the very strict regime in Bangalore reduces rent greatly, and households in the strictly controlled units are not, as a class, too far off their demand curves. Most markets have regimes that appear to confer modest benefits in relation to their costs. The meta-median of seven market medians of estimated transfer efficiency is 79 percent.[29]

It is worth reiterating that these measures of central tendency do not tell the full story. The studies that focused on distribution of benefits *within* controlled samples highlighted the extreme variation of costs and benefits; it appears that within markets the averages mask large numbers of winners and losers. For example, in Cairo, Malpezzi (1986) found that if the benefit was calculated separately for each household, the variation in benefits was very large relative to the median benefit or the benefit for the representative consumer. The first quartile of net benefit was 8 Egyptian pounds per month (compared with typical household incomes of about 80 pounds) and the third quartile was –28 Egyptian pounds—for many households, the "disequilibrium in consumption" outweighed benefit from lower rents. Other distributional issues exist, of course, and are discussed below.

---

[27] The nine observations in column 1 of table 8, of median rent reduction as a percentage of market rent, were regressed against the rent control index of table 1. Stricter regimes had larger reductions, but the result was not statistically significant. Given only seven degrees of freedom, and the roughness of the table 1 index, this lack of significance is not surprising.

[28] It is important to note that transfer efficiency is a simple ratio of static measures (tenant benefit to rent reduction) and is not a measure of total efficiency in any sense.

[29] A simple regression of the seven median transfer efficiencies against the table 1 rent control index yielded a negative coefficient, hinting that stricter regimes may have lower transfer efficiencies, but again the result was not significant.

Distributional effects of controls

The cost-benefit studies discussed above present evidence on the distribution of benefits from rent control within the class of controlled renters and a little evidence on benefit distribution between controlled renters and other households (uncontrolled tenants and homeowners). Since many see rent control as a redistribution of income from landlords to tenants, direct tests of the incomes of each class are of particular interest.

Landlord and tenant incomes compared. Does the implicit subsidy landlords confer on tenants in a rent-controlled market improve the distribution of income? In three of the case-study markets, surveys ascertained whether the respondent owned the house or rented it from the owner or someone else, and whether anyone else in the house rented. Thus these samples can be divided into resident landlords and renters.

The results, which are quite consistent, are summarized in table 9. In all three cases, landlords are richer than tenants, but in all three there is significant overlap in the distribution of landlord and tenant incomes. In Bangalore, the differences between groups are (not surprisingly) accentuated if landlord incomes are weighted by their income from nonresidential property.

Table 9. **Summary of Landlord and Tenant Incomes from Three Markets**

	Cairo (pounds)	Kumasi (cedis)	Bangalore, Unweighted (rupees)	Bangalore, Weighted[a] (rupees)
Renters				
Median	87	11,563	425	425
IQR	71	6,791	420	420
N	252	725	935	935
Landlords				
Median	127	15,668	746	1,182
IQR[b]	94	11,024	825	1,375
N	21	92	258	258

[a] By income from residential property.
[b] Interquartile range—the difference between the first and third quartiles.

Distribution of benefits among tenants. As noted above, the most striking distributional result from Malpezzi's study of Cairo was the tremendous variation in tenant costs and benefits around the

measures of central tendency. While the median net benefit from controls was modest, 25 percent of sample households experienced net welfare gains of more than 9 percent of median household income (see table 2), and another quarter of the population experienced a net welfare loss of about 30 percent of median household income. Long-time residents receive the largest subsidies *and* the largest benefits, but the subsidy increases much faster than the benefits with length of tenure because distortions in consumption also increase with length of tenure. On the other hand, recent movers paid large amounts of key money, while tenants in place paid very small fractions of their incomes for shelter. Looking at the distribution of benefits by other criteria showed no distributive effect of subsidies or total benefits. The coefficients of the logarithm of consumption in auxiliary benefit regressions were statistically zero. Finally, neither benefits nor subsidies were strongly related to household size, although there was a weak tendency for larger households to receive *smaller* subsidies.

In Ghana, the distributional results stem from the fact that (compared with other markets and other rent control regimes) the size and quality of the housing stock, and the controlled rents paid, exhibit little variation. Examining median cost-benefit measures within consumption quartiles, Malpezzi, Tipple, and Willis (1990) found that the median rent paid for each unit (P_cQ_c) remained constant at 300 cedis. The price the housing unit would rent for in the absence of controls (P_mQ_c) was also remarkably stable, since there was not much variation in size and quality of unit. But estimated equilibrium demand in the absence of controls (P_mQ_m) rises with income. So the cost of the subsidy does not vary much with consumption, but higher income households have the largest "disequilibrium in consumption" (i.e., are most constrained by the lack of housing of suitable quality). Richer households have the smallest benefits (or the largest losses, depending on which assumption is made about the price elasticity). Conversely, poorer households receive larger benefits, both absolutely and as a percent of total consumption.

It was found that in Kumasi long-term tenants had the smallest estimated disequilibrium in consumption and the largest benefits. Net benefits were still small relative to consumption. The largest net costs were to recent movers. Even larger unmeasured costs were imposed on households that were constrained from moving at all.

Malpezzi and Tewari's (1991) study of Bangalore also found that discussion of the typical tenant and medians masks the fact that

these welfare estimates have wide distributions. Even in the strictly controlled Bangalore submarket, more than one-quarter of households have negative estimated net benefits. And more than a quarter of ordinary controlled households have positive estimated benefits, even assuming the lower price elasticity.

In Rio, Silveira and Malpezzi (1991) found that typical net welfare changes were small, as discussed above, but that there was some tendency for low-income tenants to have larger gains (assuming a unitary price elasticity) or smaller losses (assuming inelastic demand) than richer tenants. Controls were found to be very mildly progressive, in this restrictive sense.

Effects of controls on the "uncontrolled" submarket

Several papers address the potential effects of a price control on a related, though nominally uncontrolled, market. Fallis and Smith (1984) develop two related models: one for rent control regimes that exempt new units from price controls and one for regimes with vacancy decontrol provisions. Their short-run models predict that under most conditions excess demand spills over into the uncontrolled market and, in the short run, drives up the uncontrolled price. In the long run, Fallis and Smith implicitly assume an elastic supply function that implies a reduction in the quantity of housing services from the controlled sector, and an expansion in the uncontrolled sector, narrowing the gap between prices.

Fallis and Smith also present an empirical test of the model using data from Los Angeles (1969 to 1978). They assume a straightforward relationship between rental rates (R), operating expenses (E), and the vacancy rate (V), estimated as

$$\dot{R}_t = \underset{(3.30)}{-6.25} + \underset{(1.64)}{0.078}\,\dot{E}_t + \underset{(4.12)}{34.09}\left(1/V_t\right) + \underset{(3.10)}{26.49}\left(1/V_{t-1}\right), \tag{3}$$

where dots indicate time derivatives, and standard errors are in parentheses. Rent control was introduced in Los Angeles at the end of this period, in 1978. The estimates are used to forecast what rents would have been in the absence of controls, and the forecast is compared with rents in the controlled and uncontrolled sector. After two years, controlled rents had risen by 10 percent *less* than the forecast, and uncontrolled rents by 22 percent *more*, consistent with the hypothesis that rent control increased prices in the uncontrolled sector in the short run.

Malpezzi (1986) was the first to address this issue for a developing country. He used the cross-country demand model of Malpezzi and Mayo (1987) to predict long-run equilibrium rents in the uncontrolled (furnished) sector in Cairo. There it was found that rents in the uncontrolled sector were much greater than predicted by the model. The average predicted rent-to-income ratio for this group was 0.16; the ratio observed was 0.53, as reported in Malpezzi (1986).

Malpezzi, Tipple, and Willis (1990) followed up with a similar method in Kumasi. They used an improved version of the cross-country demand model to calibrate their model. In contrast to the Cairo case, they found that controls *reduced* the rents paid in the uncontrolled sector in Kumasi in 1986. Predicted rents from the model were 9 percent of income, while actual rents were 5 percent. In India, Malpezzi and Tewari (1991) found that although the prediction from the cross-country model was lower than the value actually observed (predicted rent-to-income of 0.09 versus observed 0.12), the difference was small relative to the standard error of the prediction. Silveira and Malpezzi (1991) found a similar result for Rio. So in summary the effect of controls on the uncontrolled market appears to vary widely with type of control regime, market, and the nature of the uncontrolled sector.

Lessons for housing policy

Despite its limited objective—to examine selected comparative static estimates of the costs and benefits of controls in eight cities—this article has gone on at some length. What have we learned? First, that rent control regimes and their effects vary across cities and countries. We all knew that before embarking, but perhaps we know a little more about *how* they vary. We certainly know that many countries have stricter systems of rent control than any U.S. city, although others are somewhat comparable. We also learned that within a market the distribution of costs and benefits of controls is sometimes "progressive," sometimes perverse, but virtually always poorly focused. Whatever the average cost or benefit of controls, there are wide variances about the average, and the variation is rarely strongly related to income or other household characteristics in a way most people would find desirable. We learned that, at least in the developing-country samples where we had direct evidence, landlords are somewhat richer than tenants, on average, but not remarkably richer, and there is a lot of overlap in the income distribution.

We learned that (at least in Cairo) key money can make up much of the difference between controlled rents and market rents. We learned that in very strict regimes substantial reductions in rents can be largely or completely offset by distortions in consumption patterns (e.g., Kumasi). We learned that systems that give differential treatment to favored groups like civil servants can distribute substantial benefits to those groups without much benefit to others (Bangalore). We learned that in countries willing to incur substantial transaction costs of indexation and renegotiation of rents for leases beyond market periods, controls can make little difference on average although a wide variance of benefits remains (Rio).

Rent regulation is now a particularly hot topic in the reforming socialist economies of Eastern Europe, Russia, and China (see for example Buckley, Dániel, and Thalwitz 1993; Dániel 1983; Renaud 1992; Tolley 1991; Turner, Hegedus, and Tosics 1992). Of course, in those countries the bulk of controlled rental housing is owned by the state or by state enterprises. Prereform rents were very low, ranging from 7 to 8 percent of income in Bulgaria to 1 percent in Russia (Renaud 1992).

The consumer's surplus model presented in this article has yet to be applied to formerly socialist economies, but related work suggests that the distortions would be considerable. For example, Dániel (1983) found that in Hungary rents for flats averaged about 3.7 percent of household income. This was only about 40 percent of maintenance costs, and about 15 to 20 percent of maintenance and depreciation. For comparison, if rents covered full cost, Dániel reckons they would average about 17 percent of income.[30] Thus by this simple measure the average subsidy is 15 percent of income but falls with income (the subsidy is only 10 percent in the lowest decile). She examines the effect on income distribution and finds that "*the rented flat as an allowance in kind does not reduce vertical inequality in society, as it should under the declared intentions, on the contrary it augments it*" (95) (emphasis in the original). Dániel points out that horizontal equity is also violated.

There are many topics related to controls that this article has neglected; some have been studied in the papers cited and elsewhere, and others remain to be studied.[31] Some evidence exists,

[30] Estimating market rents on the basis of recurrent costs is a crude method at best, necessitated by the lack of sufficient market comparators.

[31] On all these points see the references cited above, especially Malpezzi and Ball (1991), HUD (1991), and the references therein.

for example, on the effects of different regimes on property taxes; on controls, profitability, and housing supply; on the interaction between controls and tenure security; and on the political economy of controls. Only skimpy evidence exists on the effects of controls on mobility and tenure choice, or on dynamics generally. While some good work has been done on alternatives for decontrol, as always, more is preferred to less.

Author

Stephen Malpezzi is on the faculty of the Department of Real Estate and Urban Land Economics of the University of Wisconsin.

The author is indebted to Richard Ault, Leland Burns, Ira Lowry, Michael Teitz, and two anonymous referees for comments on a previous draft. They are not responsible for remaining errors.

References

Aaron, Henry. 1975. *Who Pays the Property Tax?* Washington, DC: Brookings Institution.

Arnott, Richard (with Nigel Johnston). 1981. *Rent Control and Options for Decontrol in Ontario*. Toronto: Ontario Economic Council.

Buckley, Robert M., Zsuzsa Dániel, and Margaret Thalwitz. 1993. *The Welfare Cost and Transparency of Housing Policy in Socialist Economies: The Hungarian Experience*. Washington, DC: World Bank, Urban Development Division.

Caudill, Steven B., Richard W. Ault, and Richard P. Saba. 1989. Efficient Estimation of the Cost of Rent Controls. *Review of Economics and Statistics* 71:154–58.

Dániel, Zsuzsa. 1983. Public Housing, Personal Income and Central Redistribution in Hungary. *Acta Oeconomica* 31(1–2): 87–104.

Fallis, George, and Lawrence B. Smith. 1984. Uncontrolled Prices in a Controlled Market: The Case of Rent Controls. *American Economic Review* 74(March):193–200.

Hamilton, Rabinovitz, Szanton, and Alschuler and the Urban Institute. 1985. *The Rent Stabilization System: Impacts and Alternatives*. Los Angeles.

Kearl, J.R., Clayne Pope, Gordon Whiting, and Larry Wimmer. 1979. A Confusion of Economists? *American Economic Review*, 69(2):28–37.

Levine, Ned, Eugene Grigsby III, and Allan Heskin. 1990. Who Benefits from Rent Control? Effects on Tenants in Santa Monica, California. *Journal of the American Planning Association*, 56(Spring):140–52.

Lowry, Ira S. 1983. *Experimenting with Housing Allowances: The Final Report of the Housing Allowance Supply Experiment*. Cambridge, MA: Oelgeschlager, Gunn and Hain.

Malpezzi, Stephen. 1986. Rent Control and Housing Market Equilibrium: Theory and Evidence from Cairo, Egypt. Ph.D. dissertation, George Washington University.

Malpezzi, Stephen, and Gwendolyn Ball. 1991. *Rent Control in Developing Countries: A Synthesis*. World Bank Discussion Paper. Washington, DC.

Malpezzi, Stephen, and Gwendolyn Ball. 1993. Measuring the Urban Policy Environment: An Exploratory Analysis Using Rent Controls. *Habitat International* 17(2):39–62.

Malpezzi, Stephen, and Stephen K. Mayo. 1987. The Demand for Housing in Developing Countries. *Economic Development and Cultural Change* 35(4):687–721 (July).

Malpezzi, Stephen, Larry Ozanne, and Thomas Thibodeau. 1980. *Characteristic Prices of Housing in 59 SMSAs*. Washington, DC: Urban Institute.

Malpezzi, Stephen, and Vinod Tewari. 1991. *Costs and Benefits of Rent Regulation in Bangalore, India*. World Bank, Infrastructure and Urban Development Department Discussion Paper No. 82. Washington, DC.

Malpezzi, Stephen, Graham Tipple, and Kenneth Willis. 1990. *Costs and Benefits of Rent Control: A Case Study in Kumasi, Ghana*. World Bank Discussion Paper No. 74. Washington, DC.

Marks, Denton. 1984. The Effect of Rent Control on the Price of Rental Housing: An Hedonic Approach. *Land Economics* 60(1)(February):81–94.

Murray, Michael P., C. Peter Rydell, C. Lance Barnett, Carol E. Hillstead, and Kevin Neels. 1991. Analyzing Rent Control: The Case of Los Angeles. *Economic Inquiry* 29(October):601–25.

Netzer, Dick. 1966. *Economics of the Property Tax*. Washington, DC: Brookings Institution.

Olsen, Edgar O. 1972. An Econometric Analysis of Rent Control. *Journal of Political Economy* 80(November/December):1081–1100.

Olsen, Edgar O. 1988. What Do Economists Know about Rent Control? *Journal of Real Estate Finance and Economics* 1(3):295–308 (November).

Page, Douglas, and Raymond J. Struyk. 1990. *Measuring the Housing Sector: Results from the International Housing Market Survey*. Washington, DC: Urban Institute.

Peña, Daniel, and Javier Ruiz-Castillo. 1984. Distributional Aspects of Public Rental Housing and Rent Control Policies in Spain. *Journal of Urban Economics* 15:350–70.

Prakasarao, V.L.S., and V.K. Tewari. 1979. *The Structure of an Indian Metropolis: A Study of Bangalore*. New Delhi: Allied Publishers Private Ltd.

Renaud, Bertrand. 1992. The Housing System of the Former Soviet Union: Why Do the Soviets Need Housing Markets? *Housing Policy Debate* 3(3):877–99.

Rydell, C. Peter, C. Lance Barnett, Carol E. Hillstead, Michael P. Murray, Kevin Neels, and R.H. Sims. 1981. *The Impact of Rent Control on the Los Angeles Housing Market*. Santa Monica, CA: Rand Corporation.

Shulman, David. 1981. Real Estate Valuation under Rent Control: The Case of Santa Monica. *AREUEA Journal* 9:38–53.

Silveira, Ricardo, and Stephen Malpezzi. 1991. *Costs and Benefits of Rent Control in Rio de Janeiro*. World Bank, Infrastructure and Urban Development Department Discussion Paper No. 83. Washington, DC.

Stegman, Michael. 1985. The Model: Rent Control in New York City. In *The Rent Control Debate*, ed. Paul L. Niebanck, 29–55. Chapel Hill: University of North Carolina Press.

Struyk, Raymond J. 1988. The Distribution of Tenant Benefits from Rent Control in Urban Jordan. *Land Economics* 64(2)(May):125–34.

Tolley, George S. 1991. *Urban Housing Reform in China: An Economic Analysis*. World Bank Discussion Paper No. 123. Washington, DC.

Turner, Bengt, Jozsef Hegedus, and Ivan Tosics, eds. 1992. *The Reform of Housing in Eastern Europe and the Soviet Union*. London: Routledge.

Turner, Margery Austin. 1988. *Rent Control and the Availability of Affordable Housing in the District of Columbia: A Delicate Balance*. Washington, DC: Urban Institute.

U.S. Department of Housing and Urban Development. 1991. *Report to Congress on Rent Control*. Washington, DC: Office of Policy Development and Research.