INU-24

THE WORLD BANK

POLICY PLANNING AND RESEARCH STAFF

Infrastructure and Urban Development Department

Report INU 24

Measuring the Costs and Benefits of Rent Control: Case Study Design

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June 1988

Discussion Paper

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The authors would like to thank Professor Edgar Olsen of the University of Virginia and Margery Turner of the Urban Institute for comments on an earlier draft. They are not responsible for remaining errors.

This paper has been produced under a World Bank research project on Rent Control in Developing Countries (RPO 674-01), directed by Stephen Malpezzi. The project is described in the companion paper:

Stephen Malpezzi and C. Peter Rydell, Rent Controls: A Framework for Analysis (Water Supply and Urban Development Department Discussion Paper No. 102, 1986).

That paper also includes a detailed survey of previous literature on rent controls. This paper presumes some familiarity with that literature.

The World Bank

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ABSTRACT

This paper has been written as part on an ongoing research project on Rent Control in Developing Countries. That project has three related objectives:

(1) Survey rent control laws across countries, with special emphasis on developing countries. Focus on rent control in practice, and on details such as which units are controlled, how units are appraised, how rents are adjusted over time, and related laws (e.g. occupancy laws).

(2) Produce case studies of particular markets, which will estimate the costs and benefits of rent control to landlords, tenants, other households, and governments. Study the effect of different types of controls on the supply of housing, in the short and long runs.

(3) Integrate the results from the case studies in an internally consistent model of market behavior. The costs and benefits to different agents (landlords, tenants, "uncontrolled" households, governments) from different sources (changes in price, changes in consumption, changes in mobility) are all includied. Use the model to study alternative methods of decontrol for different stylized rent control regimes, under different market conditions.

The purpose of this paper is to provide a convenient summary of analytical techniques which can be used in the design of the case studies (Task 2, corresponding to the second objective above). Each case study will focus of issues specific to that market, but this paper defines the common analytical core of the studies, and outlines possible extensions in some markets. The paper may also serve as a guide for analysis of rent control and related policies in other housing markets.

This paper is aimed at a limited audience: researchers working on the rent control case studies, and those contemplating similar work in other markets. Its primary distribution is among staff of the project and other researchers, and is designed to suggest approaches to practical research problems. It is not, in general, about controls and does not present empirical results or policy analysis. These will be forthcoming from the project.

MEASURING THE COSTS AND BENEFITS OF RENT CONTROL: CASE STUDY DESIGN

I. INTRODUCTION

A. Overview of the Research Project

This paper has been written as part of an ongoing research project on Rent Control in Developing Countries. That project has three related objectives:

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This paper is designed to complement the previous paper Rent Control in Developing Countries: A Framework for Analysis.[1/] That paper includes, inter alia, a more detailed treatment of the other tasks (Task 1 overview of rent control practices, Task 3 analysis of decontrol options and synthesis), and a review of existing literature. This paper presumes a familiarity with the previous paper.

This paper, then, is a guide to case study design. Each case study will address every topical issue as defined in the list of core elements (below), within limits imposed by the data. Each case study will include estimation of the simple version of the cost-benefit model described in Chapter.[2/] But not every case study will include explicit estimation of every model in this paper. Specifics on which models will be estimated in which markets can be found in Annex 2. The general strategy, similar to that followed in the Housing Demand and Finance Research Project,[3/] is to estimate simple and comparable models in each market studied, and to also estimate more "state of the art" models where the data support it. Comparisons between simple and "better" models yield information on the size of likely biases and the general robustness of results. Comparisons of simple models across countries yields information on differences and similarities in effects of different rent control regimes under different market conditions.

Another purpose of the paper, and the most important reason for making it more widely available, is to assist other researchers working on rent controls, or housing market regulation more generally, particularly in developing countries.[4/] Malpezzi (1984 c) and Malpezzi, Bamberger and Mayo (1985) present more general introductions to housing market analysis.

B. Overview of Case Studies

The purpose of the case studies is to provide descriptions of rent control systems and related regulations as they exist, in real-world complexity, and to provide empirical estimates of their effects under different market conditions. They are of interest in themselves, and will also feed into a study which describes systems across countries and analyzes the effects of changes in the system, including alternative methods of decontrol.

1/ Malpezzi and Rydell (1986), hereafter referred to as A Framework for Analysis or the Framework.

2/ The model will not be estimated in Zimbabwe because of data limitations.

3/ See Malpezzi and Mayo (1985), Ch. 2 which presents simple comparable models estimable in all cities studied, then evaluates models where the data support the better models.

4/ For example, the Urban Institute has independently prepared an analysis of the costs and benefits of rent control in Jordan. See Struyk (1988).

This section will briefly describe three aspects of the proposed case studies: the design of the case studies, the outputs from case studies, and the choice of markets to be studied.

Case Study Design

Each case study will be tailored to the particular market under study; after all, a maintained hypothesis of the project is that local conditions matter, and are in fact the focus of the study. Still, each case study will contain a set of core elements:

1. An institutional analysis of the rent control legislation and enforcement mechanisms.

2. Analysis of the political economy of rent control, including a description of political alignments leading to controls, how/if these have shifted over time, and the implications for the political feasibility of reform.

3. A literature review of previous housing market research in that market, with emphasis on rent control.

4. Describe important features of the market, including recent price behavior, population change, income changes, vacancy rates, new construction, and the role of the informal sector. This will include an explicit evaluation of the quality and usefulness of the data.

5. Where time series data permit, relate variation in the above measures to the introduction and major changes in rent control laws and enforcement practices.

6. Estimate the effects of rent control on housing expenditures, using household survey data.

7. Estimate the costs and benefits of rent control based on consumer's surplus and cash equivalent value models, similar to that of Olsen (1972).

8. Estimate the distributive impacts of rent control, using these cost/benefit estimates. Relate these estimates to the costs and benefits as perceived by different interest groups.

9. Analyze the likely supply responsiveness of the market to changes in controls. Evaluate alternative methods of decontrol in light of the market's responsiveness.

10. Provide recommendations on the most feasible methods of decontrol or modification of the current law. This should include discussion of local proposals for reform, where appropriate.

11. Estimate the effects of the current regime on property tax revenue.

The actual design of the case studies will be set out in more detail below. A Framework for Analysis provides more details on the how the case studies fit into the rest of the project. Also, a preliminary outline for the synthesis paper is attached as Annex 1 to help ensure that those undertaking the case studies understand the overall direction of the project. Other papers provide more detailed treatments of previous research.[5/]

Each case study will go beyond the core questions to take advantage of particular features of available data. For example, in Cairo, the project will produce estimates of the effects of rent control on household location, mobility, and tenure choice. However, as already noted a set of simple models will also be estimated, so that they may be compared to more complete models from the same market. This will enable us to judge their reliability across markets and hence type of rent control regime and market conditions.

Choice of Case Study Markets

Five case studies are currently underway, in Rio de Janeiro, Brazil; Cairo, Egypt; Kumasi, Ghana; Bangalore, India; and Harare, Zimbabwe. Table 1 summarizes the current status of the case studies.

Table 1: LIST OF CASE STUDY MARKETS
(Phase One)

Country	Current Status
Brazil (Rio de Janeiro)	Consultant identified. Descriptive paper being prepared. Data being collected.
Egypt (Cairo)	Data in hand. Descriptive statistics and cost/benefit model complete. Additional work to be undertaken on location/mobility and depreciation.
Ghana (Kumasi)	Data in hand. Additional household survey data has been collected. Descriptive paper and analytical papers have been drafted.
India (Bangalore)	Institutional analysis completed. Descriptive statistics and cost/benefit model have been drafted.
Zimbabwe (Harare)	Data in hand. Descriptive paper being drafted.

5/ Malpezzi and Rydell (1986); Thibodeau (1981); Malpezzi (1987).

Two principles guided the choice of markets for study: obtaining needed variation in rent control regimes and market conditions (a benefit), and data availability and timing (a cost). Fortunately a number of high quality data sets are available for analysis, and they represent a good cross section of rent control laws and market conditions. Table 2 presents some information on controls and market conditions in each of the markets under study. Note that we have been fortunate to acquire data from markets with reasonable variation in the types of rent control regimes in place, and in market conditions.

Table 2: INTERACTION BETWEEN MARKET CONDITIONS AND RENT CONTROL, CASE STUDY MARKET

	Rent Adjustment		Treatment of New Construction	
Inflation	Indexed	Frozen	Relaxed	Strict
High	Brazil	Egypt Ghana	Egypt Brazil	Ghana
Moderate	Zimbabwe	India	India	Zimbabwe

Comments on Case Study Markets

Rio, Brazil	Significant variation in laws and market conditions over time. Particularly good data for estimating supply response to controls.
Cairo, Egypt	Excellent data in hand. Preliminary cost/benefit work is well under way. Best chance to model effects of location and mobility.
Kumasi, Ghana	Very high inflation, strict rent control regime. Have some data over time.
Bangalore, India	Good data from one market already (Bangalore). Rent controls vary markedly from one city to another, so other markets may be included. Keen interest in property tax issue.
Harare, Zimbawe	Strict law but relatively low inflation. Some adjustment in rents has been permitted.

Primary responsibility for the Brazilian case study will rest with Ricardo Silveira of the Infrastructure and Urban Development Department assisted by Stephen Malpezzi and Eduardo Neto (consultant). The Egyptian case study will be carried out by Stephen Malpezzi and other project staff at the Bank. The Ghana case study will be undertaken by Graham Tipple and Ken Willis of the University of Newcastle upon Tyne. Vinod Tewari of the Indian Institute of Management (Bangalore) is in charge of the Bangalore study; and Marja Hoek-Smit of the University of Pennsylvania will carry out the Phase One research on Zimbabwe.

Data Collection

The guiding principle of data collection in this project is to rely on existing data as much as possible. Much of the data to be used was gathered or generated during the earlier stages of RPO 672-46, Housing Demand and Finance in Developing Countries. For that study we assembled high quality household level data sets from eight countries. Additional data from several countries are about to come "on line." The philosophy during that project and during this project is to exploit available opportunities to develop new data sets when they exist at a modest cost, developing a "data pipeline."[6/]

Most of the household survey data required for the cost/benefit analysis is already in hand and ready for computer analysis. Ghana and Brazil are the two exceptions.

A household survey from Kumasi is in hand, which was collected by Graham Tipple in 1981. Anecdotal evidence suggests that given the freeze in controlled rents since that time, despite rapid general price inflation since that time, a re-survey of those households would yield valuable information on the dynamics of the market, including changing key money practices. The re-survey has just been carried out by the University of Science and Technology (Kumasi), under the direction of Dr. Tipple, and the new data are now being analyzed.

The Brazil case study will make use of existing data, but it has to be prepared for analysis at the Bank. The consultant has prepared analysis datasets from the Brazilian Census and other sources; analysis of the data has just begun.

In most of the case study markets additional collateral data will be collected where available, especially time series data such as starts, vacancy rates, and other aggregate supply measures. These are especially important in studying lags in supply side responses to changes in rent regulation.

6/ For example, during RPO 672-46 the project designed data collection efforts for two governments (Korea and Kenya), and funded a survey in Manila, the Philippines. The Bank (and the countries) received three high quality data sets at a total cost to the project of three trips and $10,000.

In addition, a key piece of information for each case study is what quantity of housing each sample household would consume in the absence of rent controls. The cross-country model of Malpezzi and Mayo (1985, Ch. 3) can be extended, using more countries and an improved specification. The first estimates from this improved cross-country model are presented in this paper. This permits improved imputation of long-run competitive rents for case study households. In some markets these will serve as a check for other methods of imputation; in a few markets they may be the primary method of imputation. In addition, the improved precision of the cross-country estimates will have an important spillover effect, as these estimates are currently being used to evaluate past Bank shelter projects and improve the design of current projects (see The Urban Edge, 1985).

Outputs

A separate report will be produced for each market studied, integrating findings on the 11 elements enumerated above.[7/] In addition, the project will produce a synthesis report which will compare results across case study markets, with special emphasis on relating the outcomes (elements 6,7,8, and 11, above) to political conditions, market conditions, and variation in the laws (elements 1, 2, and 3).[8/] In addition, the synthesis report will contain a detailed analysis of the effects and feasibility of decontrol options.

The organization of the rest of the paper is as follows. The next chapter describes in some detail the models to be used for the case studies. References are included for more detailed technical treatment of the issues that arise. Chapter 2 is, then, the basic roadmap for the case studies. Chapter 3 describes some issues that arise in empirical implementation. Chapter 4 describes the proposed synthesis of case study results, to make the overall direction of the project clear.

7/ Each case study will be prepared for the Department's Discussion paper series. Selected papers will also be considered for publication in the Bank's technical paper series, and as journal articles.

8/ The synthesis paper will be accessible to nonspecialists, at a technical level similar to "Shelter Strategies for the Urban Poor" (Mayo et. al. 1986). The first preliminary outline is to be found in Annex 1. A longer version will be submitted as a technical paper; a shorter version submitted to one of the Bank's journals.

II. METHODOLOGY FOR THE CASE STUDIES

This chapter discusses, in turn, analysis of institutional features of controls; issues in the political economy of controls; how to estimate welfare impacts of controls; and methods for studying dynamic effects of controls on supply.

A. Institutional Analysis

The best starting point for any institutional analysis of rent controls is a straightforward yet thorough description of rent control legislation and enforcement mechanisms. Be sure to describe related laws and regulations where relevant (e.g. tenure security, land tenure). However, a description of the legal framework is incomplete without a description of enforcement mechanisms and practices.

Where possible, a series of semi-structured interviews should be carried out with different classes of individuals affected by controls, including:

1. Government officials--rent control officers, housing officials, revenue department officers. It is particularly important to get a good understanding of how controls are actually administered.

2. Tenants--especially those in controlled rental housing, but also where possible, those who may be affected indirectly, including owners and renters in non-controlled sectors (where applicable). Find out whether those in the uncontrolled sector wish to be in the controlled sector; why or why not; and how they perceive their chances.

3. Developers/investors/landlords--many questions suggest themselves, but especially why they invest in rental housing if they continue to do so.

4. Real estate agents, journalists who have written on the subject, others knowledgeable.

The checklist found in the Framework paper, and Mayo et al. (1981) can be used as guides for designing semistructured interview questionnaires.

Further Reading: Mayo et al. (1981), pp. 12-15,259-268.
Malpezzi and Rydell (1986), Annex 1.

Issues in Political Economy

Understanding how rent control systems have come about is the key to understanding how to modify or remove them. In many countries, controls were first imposed as a response to wartime shortages. Controls can also be responses to periods of prolonged inflation or other market disruptions, including natural disasters and temporary economic dislocations (such as the Alaska pipeline construction project in the United States). In developing

countries, adjustment to such housing market shocks are made difficult by rapid urbanization and problems in input markets.

Some of the "micro" political economy questions to be answered in each market include the following:

1. Why were controls originally applied? Response to shortages, price increases, wartime? Were they originally temporary? If so, why still in place?

2. How do controls fit in with other government programs and policies in urban and housing? Are they seen as primarily housing policy, income redistribution policy, or just the result of a political calculus?

3. What classes of individuals have been in favor of the retention/abolition of controls? What formal groups (lobby groups, pressure groups, political parties) have been the principal actors in the political debate?

4. Have the results of polls or studies had any impact on the evolution of controls? In light of empirical evidence, do actors correctly perceive the gains/losses?

5. What are the current prospects for change, in either direction? What specific kinds of changes have/are being discussed?

6. Describe the interaction between controls and other housing market programs/policies. Has government tried to respond to a rent control induced shortage with public programs? Have controls been applied to public units?

7. What are relative orders of magnitude of distortions due to controls compared to other regulations (e.g. land use), finance, infrastructure? What are the outstanding timing issues (especially those that ensure reasonable market supply response to relaxation of controls)?

Some of the larger topics to be addressed in the synthesis paper include the following:

1. What are the stated policy objectives behind controls? Are these consistent within and across countries? Hidden agendas, if any?

2. Are there systematic differences in responses in democracies, socialist systems, other systems? Describe interest group interaction in simple terms. Size of groups versus pressure exerted. Are models of voter choice fruitful in explaining controls? "New" models of public choice? How to apply to a wide range of political systems?

3. Some writers trace the political economy of rent control to "effective" tenant organization; conversely believe defeat of controls, decontrol are linked to strong landlord organization. Is this an oversimple view--why?

4. Rent control as a transfer of property rights--a theme to carry throughout the synthesis paper.

5. Who is perceived to win and lose from rent control? Who actually wins and who loses? Illustrate how broad policy adapted to address one of many shelter problems of a (usually) narrow group affects a large segment of the market. Better targeted policies are/should be available.

Suggested References: Dahl (1963) is one of many general introductions to political theory; Dreier (1979), Kochanowski (1980), Niebanck (1985), and Thibodeau (1981) are among the few published papers which discuss political economy of rent control; Ricketts (1982) discusses political models of government housing policy; Kahn (1978) and Posner (n.d.) discuss the economics of regulation generally; and Furbotn and Pejovich (1972) survey economic analysis of property rights.

B. Descriptive Statistics

Careful and straightforward use of descriptive statistics can flesh out the descriptive part of the case study, and will also set the stage for econometric work. Such statistics are often more readily comprehensible to important audiences. Often available data will not support more sophisticated models, and the simple models underlying the descriptive statistics serve as a check on econometric work. The descriptive statistics and the other empirical work should be checked for consistency, and any qualitative discrepancies should be explainable to ourselves and to our readers.

Basic Market Conditions

These can be usefully incorporated in the introductory section of the report. Some can be derived from household survey data, and others may be available from secondary sources.

Note that in general, order statistics such as medians and quartiles are greatly preferred to parametric statistics such as arithmetic means and standard deviations, because of their robustness and because they give a better picture of distributions which can be highly skewed (Malpezzi 1984, pp. 2-8). In addition to measures of central tendency, namely medians, it is useful to include some measure of dispersion, such as first and third quartiles within the cells. Malpezzi (1984) p. 7 gives an example of one possible format.

Suggested statistics, and the questions they help answer, include:

1. Percentage of population in each tenure group. How widespread is the impact of rent control? Include separate estimates for households under different forms of controls, where appropriate.

2. Median length of stay in each tenure group. Has rent control distorted the moving decisions of households?

3. Median income by tenure group, and measures of dispersion. Are high income people often renters? Will impacts of rent control vary predictably by income class?

4. Vacancy rate by tenure group. Has rent control fueled excess demand, leading to low rates, or has it provided incentives for landlords to hold units off the market? These numbers may have to come from secondary sources, since many household surveys do not collect information on vacant units. If they are not available by tenure group, find and report any available vacancy rates, with notes about coverage and source.

5. Rates of price increase for housing (rents, house values) and for factors of production (land prices, construction wages, materials), compared to the overall movement in prices (consumer price index, GNP implicit price deflator). Have rents lagged behind costs, house prices, and the general price level? Try to find numbers for as many years as possible, and report in a data appendix, if appropriate. Describe coverage and source in as much detail as possible. In particular note whether prices are actually measured through market surveys, or whether official administered prices are used to compute the index are key money and other side payments included?

6. Population, and rates of increase, for the market in question, and for the country (urban and rural). Is demand increasing from population pressures? Rank the market's size within the country.

Descriptive Statistics which Complement the Analytical Models

This section describes several simple plots and regressions which complement the analytical models described later in the paper:

1. Plot rents and house values, by income. Do a separate plot for each tenure group. Include key money and other side payments, if possible. Do the same plots for logarithmic transformations of the variables (see Malpezzi 1984 pp. 9-13).

2. Plot and regress log of rent on length of tenure, for each tenure group. This can be used as an estimate of percentage change in rent (R=PQ) with length of stay. These can be compared to the coefficient from the length of stay variable in a hedonic regression (described below), which are estimates of price changes because the hedonic model controls for the

quantity of housing services produced by the unit (under certain restrictive assumptions).

3. An alternative statistic for (2) is to compute length of stay categories (based on, say, quartiles). Then compute the median rent for each length of stay quartile separately. Do this separately for units under different regimes, where applicable.

4. Some household surveys contain questions which indicate whether respondents are landlords. If possible, compare the median incomes of landlords to the median incomes of renters. Nonparametric tests are preferred.

5. Where data permit, evaluate the impact of rent control on housing quality by comparing structural characteristics, quality measures, and maintenance expenditures (by landlords and tenants, if available) of controlled and uncontrolled housing. See how quality changes with length of tenure, age of the structure.

Further Reading: Malpezzi (1984).

C. Estimating Welfare Impacts: Basics

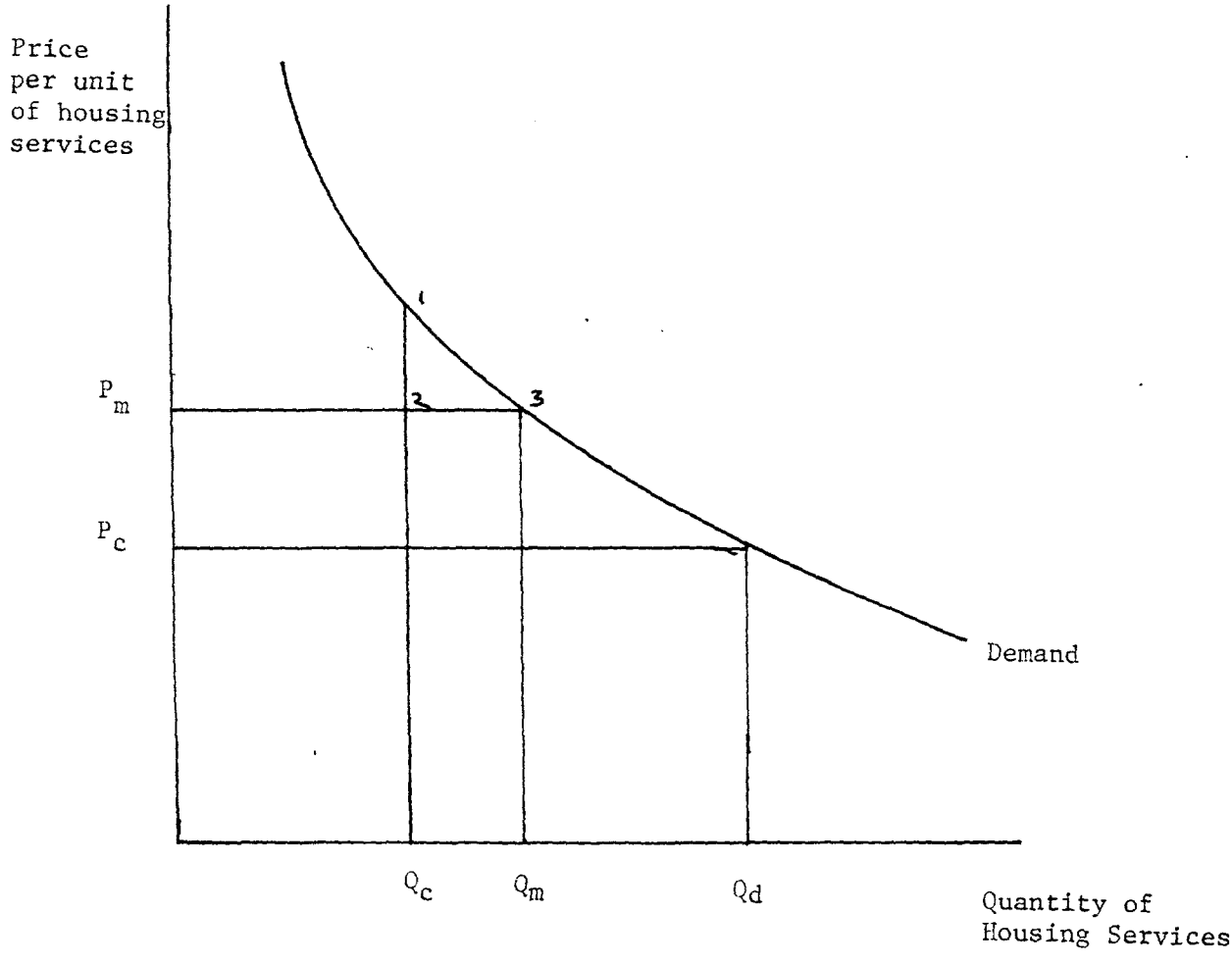
Costs and Benefits to Individual Tenants and Landlords

Perhaps the simplest way to view the costs and benefits of rent control is to estimate how much controlled units would rent for in the absence of controls, and to consider the difference between that estimate and the observed controlled rent as the cost imposed on the landlord and, obversely, the benefit transferred to the tenant. But tenants under rent control are usually not free to adjust their consumption to reflect the new relative prices. Consequently, they do not generally value a reduction in rent for the unit they occupy as highly as they would value an equivalent cash transfer. Costs and benefits of rent control to existing tenants in existing rental units can better be studied by estimating changes in consumer's and producer's surplus resulting from the existence of controls. Figure 1, based on Olsen (1972), illustrates such changes for a representative consumer, and his or her landlord.

Analysis of consumer surplus rests on the fact that demand curves slope downward, i.e. that under quite general conditions the higher the relative price of a good the less of that good a consumer will demand. If the price of housing rises, consumers will consume less housing. An interesting implication of this fact is the following. The "steepness" of the demand curve implies that households would, if necessary, be willing to pay a high price for the first few, essential, units of housing services. They would pay a high price for the first "bit" of housing, a little less for the next "bit," and so on down to the price actually paid for the last "bit" they consume. But in a competitive market, households pay only that last, lowest price for all "bits." They receive a sort of bonus in being able to consume much of

Figure 1

Consumers and Producers Surplus



their housing at a lower price than they would pay if they had to. This bonus is measured by the area under the demand curve but above the horizontal line representing the prevailing price. Changes in the household's welfare from changes in prices and in quantities consumed can be analyzed by measuring these so-called "welfare triangles" before and after the price or quantity change.

As a first approximation, the static cost born by landlords can be estimated as changes in the rectangles bounded by the price line, the vertical line representing the quantity of housing services, and the axes. This rectangle represents the shortrun change in landlord's money revenue.

Suppose that in the absence of controls the representative consumer would choose to consume Q_m units of housing services at the prevailing market price P_m, paying rent equal to P_mQ_m. Suppose that controls are imposed and effectively enforced, so that initially the rental price of one unit of housing services falls to P_c for all rental units. At this price the consumer would demand Q_d units of housing services. But elsewhere we have alluded to the fact that under many real world rent control regimes landlords will produce less housing and the transactions costs of moving will rise. We will return to this point below, but here it is sufficient to note that households may find it more difficult to find and move into a suitable unit. Households may systematically consume "off their demand curve," i.e. they will consume more or less housing than their equilibrium demand at that price. As drawn, the representative household consumes Q_c which is less than their equilibrium demand. They receive an implict subsidy of $(P_m-P_c)Q_c$, whose cost is born by the landlord. However notice that the consumer has also given up consumer's surplus equal to the triangle 123; his net gain is the difference of these two areas.

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 which changes prices and quantities can be written as:

$$\text{Benefit} = \left(\frac{1}{P_mQ_m}\right)^{1/b} \left(\frac{b}{b+1}\right) \left[Q_c^{\frac{b+1}{b}} - Q_m^{\frac{b+1}{b}}\right] + P_mQ_m - P_cQ_c$$

where

Benefit = change in Marshallian consumer's surplus

Q_m = predicted housing consumption in the absence of rent controls

Q_c = housing consumption under rent controls

P_mQ_m = estimated rent in the absence of controls, also denoted R_m

P_cQ_c = observed controlled rent, also denoted R_c

b = price elasticity of demand.

In the special case where the price elasticity of demand, b, is equal to -1, the expression $\frac{b}{b+1}$ is undefined. But it can be shown that in this special case the benefit can be expressed using natural logarithms as:

$$\text{Benefit} = P_mQ_m(\log(P_mQ_c) - \log(P_mQ_m)) + P_mQ_m - P_cQ_c$$

These two related equations will be the centerpiece of the empirical analysis in the next chapter. The benefit may be thought of as composed of two parts. The first is comprised of the two terms to the right of the brackets in equations (1) and (2). This is simply the additional disposable income brought about by paying a rent R_c ($=P_cQ_c$) rather than R_m ($=P_mQ_m$). This simple difference between market and controlled rents, $R_m - R_c$, is often used as an approximation to tenant benefits from the imposition of controls. But this simple benefit measure does not take into account how households value changes in housing consumption in addition to changes in disposable income. The second, comprising the terms in parentheses and brackets in the two equations, depends on the difference in housing consumption with and without rent controls. But whereas in the simple benefit measure $R_m - R_c$ an extra dollar of housing is counted as being worth exactly one dollar to the tenant, in the benefit measures (1) and (2) extra housing is discounted based on the tenant's relative preference for housing vis-a-vis other goods.

The measures in (1) and (2) do not include all possible costs and benefits to tenants. For example, rent control may (as we will argue) increase transactions costs for tenants (reducing the benefit to tenants), but the full system may also increase the bundle of property rights, such as security of tenure, enjoyed by tenants (increasing the benefit). Key money and tenant maintenance expenditures may also reduce tenant benefit. Some additional costs to tenants (e.g. key money, maintenance costs) can be added to rent to estimate costs and benefits with and without side payments, as in Malpezzi (1986).

The cost imposed on landlords is straightforwardly approximated by $P_mQ_c - P_cQ_c$, or the difference between controlled and market rents for the unit inhabited by the tenant. This static measure of cost to landlords does not include losses from prior accelerated depreciation of the unit,[1/] or losses from the uncompensated transfer of property rights to renters. The true costs to landlords may well exceed these estimates.

Estimating these costs and benefits requires four pieces of information for each consumer:

1. the rent currently paid for the current controlled unit, P_cQ_c

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1/ Note that these costs will be reduced by the amount landlords reduce their maintenance expenditure.

2. the rent that the current unit would rent for in the absence of controls, P_mQ_c

3. the rent that the household would pay if they were at their equilibrium demand at market prices, P_mQ_m

4. the price elasticity of demand for housing, b.

The section on empirical implementation below will explain how these will be constructed in some detail. Briefly, P_cQ_c can be observed directly from a sample of controlled households.[2/] P_mQ_c will be estimated using the method of hedonic indexes, described below, which requires information from an additional sample of housing units rented at market prices, or some sample which contains a good proxy for market rents in the absence of controls.[3/] P_mQ_m will be estimated using a demand relation from a sample of households facing market prices. The price elasticity, b, will be a parametric assumption based on other studies. While each of these methods has potential problems, sensitivity analysis will give us some idea of the confidence we can place in these results.

Suggested References: Olsen (1972) for an overview and example of the simple log model; Mayo et al. (1980) for discussion of generalizing to non-unitary elasticities; Malpezzi et al. (1981) for discussion of hedonic indexes; Malpezzi and Mayo (1985) for discussion of demand equations. For one alternative approach to benefit estimation, see Murray (1978).

Distributional Issues

Since the method just described can readily be applied to each household contained in a sample, it is straightforward to examine the correlation between the estimated costs and benefits of rent controls and

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2/ Key money, tenant maintenance, etc. can be added to rent as in Malpezzi (1986) so that P_cQ_c reflects the full user cost of rental housing.

3/ Such as market values or sales prices, or imputed rents for owner occupied units. Note two difficulties. (1) The use of stock measures (value or sales price) requires a capitalization rate be chosen; these are not, in general, constant across housing markets (Phillips, 1988; Muth 1960). Getting the average capitalization right would yield reasonable estimates for the "average" unit, but inferences about the distribution of costs and benefits would be difficult. (2) Rent control can affect the rents paid in the "uncontrolled" market (Fallis and Smith, 1984). The cross-country model described below can serve as an independent check on the reasonableness of the rent estimates, and as alternative, crude, estimator of the effects of controls.

criterion variables of interest, most obviously income. The approach usually taken is to construct auxiliary regressions of costs and benefits against income, length of tenure, and socioeconomic status (e.g. Olsen). There are two problems with such an approach. First, the costs and benefits are calculated only for controlled renters; so strictly speaking, as Olsen (1972) noted, this method only examines the distribution of benefits conditional on being a controlled renter. Conceptually, we could examine equity among controlled renters; we could examine equity between controlled renters and other tenure groups; and we could examine equity between landlords and tenants. To study the second we examine the relationship between income and tenure. To study the third, we can make a simple comparison of the incomes of landlords and tenants. Malpezzi (1986) contains such a test. Note that a non-parametric test is preferred.

Suggested references: Johnson (1951) Lea (19 n.d.); Linneman (forthcoming); Malpezzi (1986); Olsen (1972, 1982), Slack and Amborski (1984).

D. Estimating Welfare Impacts: Extensions

Selectivity Bias

A central assumption of the empirical estimation of the cost/benefit model is that the vector of hedonic prices faced by those in the reference group (uncontrolled renters, or others, see the discussion on choice of reference group below) can reasonably represent the price structure that would be faced by controlled renters in the absence of controls. For example, if the structure of implicit prices of housing characteristics differs between groups, then households with demands for (say) more space relative to quality will tend to choose the group in which the relative price of space is lower.

A large literature dealing with the potential bias from such a self-selected sample has developed, with particular reference to the labor supply decision.[4/] Malpezzi (1986) applied a simple estimator due to R. Olsen (1980) which tests for such bias in such a model.

Suppose there would be in fact separate (semilog) equilibrium hedonic price structures for both groups in the absence of controls, and that the group choice decision depends inter alia on the demand for individual characteristics and their relative prices in the owner and renter submarket. Then for the reference group we observe

$$\ln R = Xb + m \text{ iff } S = 1$$

4/ See. for example, Heckman (1979), Olsen (1980), and Maddala (1983, Ch. 9).

X the vector of housing characteristics, b the hedonic coefficients, m an error term, and

$$S = 1 \text{ if } c < Z\,g$$
$$S = 0 \text{ if } c \geq Z\,g$$

where S is the 0-1 index which denotes whether an observation is in the sample, Z are determinants of inclusion, and g their associated parameters. If S = 0, there is a hedonic price structure but we do not observe it (those in this group are under controls, and their uncontrolled hedonic coefficients are unobservable). Denote the unobserved prices as b'. If b = b', the hedonic price structures are approximately the same. Does the sample contain any information which can be used to test this?

Olsen suggests a simple least squares test.[5/] Estimate the least squares model

$$S = Z\hat{g} + w$$

then estimate

$$\ln R = Xb + a\,(Z\hat{g} - 1) + n$$

If a is significant we can reject the null hypothesis that there is no difference between samples. However, even if the estimated hedonic coefficients change with the inclusion of the correction variable, the prediction for individual households may or may not change. Since the purpose of these hedonics is predicting total rent, the important test is to examine the sensitivity of predictions to inclusion of the correction.[6/]

In his test for Cairo, Malpezzi (1986) found that although the correction factor is significant and that individual hedonic coefficients changed, although not by much. Only the length of tenure coefficient changed by as much as a standard error.

The correction did not affect the predicted rents by much. The regression fits were about the same with and without the new variable. The average difference between log rent predictions using the two sets of coefficients was .07, and the correlation between predictions was .95.

Adding the correction did reduce the sample size, because variables used in the first stage prediction were missing in about a fourth of the observations in the hedonic sample. Hence it was decided to use predictions from a larger sample, without the correction.

5/ Other nonlinear estimators are discussed in Maddala (1983). In practice the least squares correction results are often similar to nonlinear methods, and more robust.

6/ See Butler (1983) and Ozanne and Malpezzi (1985) for details on the robustness of predictions versus robustness of coefficients.

More recent work by Caudill et al (1987) finds evidence to the contrary. Reestimating variants of Marks (1984) model using market data from Vancouver, they find estimates of the market rent for the average controlled unit are $411 without the correction and $466 with a correction for selectivity bias. Such a large difference suggests additional tests of such models would be worthwhile, although none are currently planned in the present project due to resource constraints.

Two other general issues regarding selectivity models bear investigating in any such extensions. One is the interpretation of such methods for hedonic or other non-behavioral models, which has not been worked out. The other is the ad-hoc nature of the specification of the first stage regression. In most of the applications of the selectivity bias models we are familiar with (in applications to other markets as well as housing), the specification of the first stage model is of the "include all variables and their squares" variety, with little basis in theory. Specification is driven by the need to get a first stage regression equation mechanically different from the second.

Further Reading: Malpezzi (1986) pp. 209-214; Olsen (1980); Maddala (1983); Caudill et al (1987).

Problems with Sample Statistics vs Representative Consumers

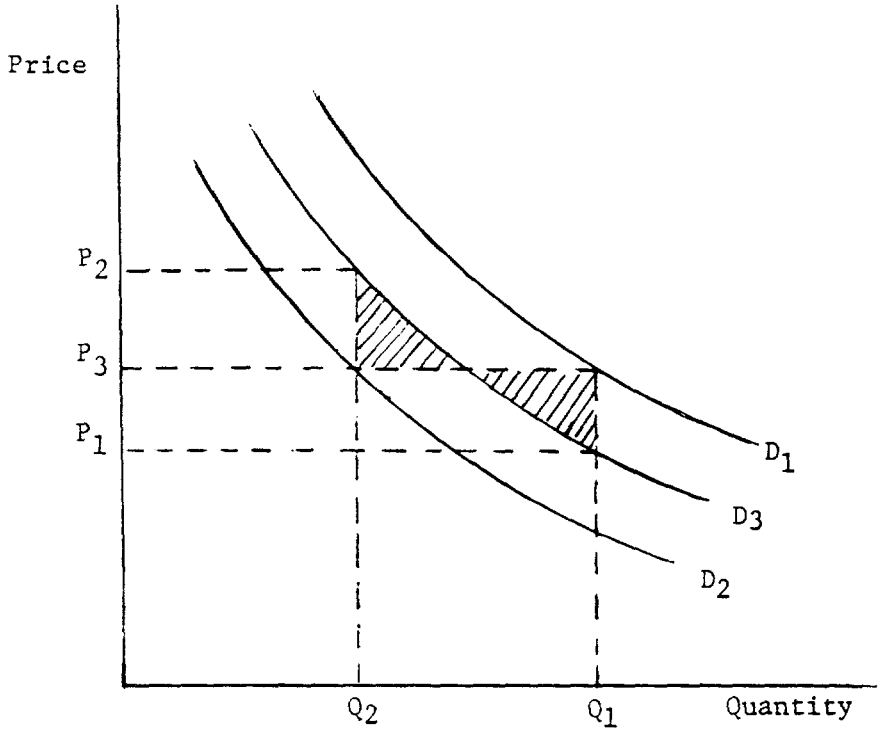
All of the analysis of the consumer so far has focused on a single representative consumer. We implicitly assume that all consumers have identical demand functions, or at least identical conditions on known "demand shifters" such as income and demographic variables. Consider the case of two consumers with different demand curves D_1 and D_2 (Figure 2). Suppose that using this "sample" we "estimate" the average demand curve to be D_3. Assume further that both consumers are on their demand curve, at Q_1 and Q_2, respectively. Our estimate of the shadow prices will be P_1 and P_2, with associated estimated dead weight losses, when in fact neither consumer incurs such a loss. Note in particular that although the two consumers demand curves are above and below the "estimated" or average demand curve, respectively, the errors do not cancel.

The extension to many consumers is straight-forward, and the problem we have just described will arise where ever a stochastic demand relation is used for calculating welfare changes. Estimates of welfare losses will be biased upwards.

Consider the following shortcoming of the "representative consumer." A simple procedure such as using medians of relevant variables, has intuitive appeal as a measure of "representativeness," but there is no guarantee that any consumers with this set of characteristics actually exist. Further, this approach yields no information about the distribution of costs and benefits. Computing costs and benefits separately for each observation solves these two problems. However, consider that this procedure will exaggerate the measured welfare cost of a ration, in the following way. The estimated ration is essentially a transformation of "how far off the demand curve" each household is. Since the true demand relation is unknown, every household is off their estimated demand curve; there is no information

Figure 2

Two Consumers' Demand Compared
to their Average Demand



in the sample to sort out how much of this is due to the stochastic nature of the demand relation, and how much is due to rent control (or even other market imperfections). Even if every household was in long-run equilibrium at market prices, we would still measure some ration, as long as we relied on a stochastic demand relation. Note also that errors in different directions do not cancel, because there are symmetric welfare losses to consuming too much housing as well as too little.

Malpezzi (1986) compared sample statistics to representative consumers. That study found fairly large differences between the median of net benefits computed for each sample household, and the net benefit computed for a "representative consumer" based on medians of other variables. In the former case the benefit of a lower rent appeared to be completely offset by the loss in welfare from consuming "off the demand curve." The median benefit measure was actually slightly negative (a welfare loss of one Egyptain sound). In the latter case the representative consumer retained a net benefit of 4 Egyptain sounds.

Further Reading: Malpezzi (1986); Gyourko and Linneman (1986); Murray (1976); and Olsen and Agrawal 1982).

E. Dynamic Supply Effects

So far most of the discussion has revolved around comparative static analysis of changes in consumer welfare. Of course the cost-benefit analysis described above does generate first order approximations of the cost imposed on landlords. But it was noted above that these estimates understate or ignore several important dynamic supply side effects. In the short run landlords have some latitude to vary the quantity of housing services from the existing housing stock by increasing or decreasing variable inputs (maintenance and repairs). Under certain conditions tenant maintenance might adjust to changes in landlord maintenance. Larger capital investments are also made in existing dwellings; such upgrading is usually undertaken by landlords but has been observed by tenants where occupancy rights are strong. Vacancies can also play an adjustment role in the short run. In the longer run new units are built; old units are demolished or abandoned; owner occupied units are converted to rental, and vice versa. Table 3 presents a summary of these components of inventory change, rough estimates of the size of each component in typical housing markets, and the best current estimates of how the supply of each component changes with a change in the relative price of housing.

At this stage Table 3 is at best incomplete. Most cells have entries, but a fair amount of literature remains to be surveyed and carefully digested, and new empirical work undertaken, before we are confident that components of inventory change are sufficiently understood to predict effects of changes in rent control regimes. Much of the forthcoming work on supply side responses consists essentially of filling out a table like Table 3, with more precise estimates of the parameters, better understanding of the determinants of variable parameters, and finding a way to tie them together (expound further). The case studies will provide some new estimates; but we will also make use of estimates from other studies, including the previous research on housing demand.

Table 3: COMPONENTS OF CHANGE IN HOUSING SUPPLY

Component	Time Horizon	Typical Size Of Component	Output Price Elasticities (Best Estimates)	Selected References	Sources of Case Study Information
Maintenance and Repairs/ Depreciation	Short Run	M&R: 10-15% of rent in non-RC markets Gross dep.: 8% of value Net dep.: ½-3% of value	0.2 to 0.3	Rydell (82) Ozanne & Struyk (76) Helbers & McDowell (82)	Brazil Cairo
Vacancies	Short Run	1-10% of stock (varies w. presence of controls)	0-3.4 (low for RC mkts.)	De Leeuw & Ekanem (71) Rydell (82)	cross country comparison
Upgrading	Long Run	up to 7% of units p.a. (by tenants in some RC mkts).	n.a.	Struyk & Lynn (83) Malpezzi (86) Strassman (84)	Cairo
Demolition & Abandonment	Long Run	less than ½% of stock per annum	.1-.4	Bender (79) Appelbaum (83)	none; will use existing studies

Note: This table is preliminary. Additional literature review is being undertaken in conjunction with case studies.

Table 3
Components of Change in Housing Supply (cont'd.)

Component	Time Horizon	Typical Size Of Component	Output Price Elasticities (Best Estimates)	Selected References	Sources of Case Study Information
Starts	Long Run	1-5% of stock per annum	5-infinite	Muth (60) Smith (76) Follain (79) Rydell (82)	Brazil
Conversions From Rental to Owner-Occ.	Long Run	less than 1% of rental stock	.2	Lea and Wasylenko (83) Crone (86) Weicher et al. (82)	Brazil
Conversions From Owner-Occ. to Rental	Long Run	varies widely (0-40% of rental is previously 0-0)	n.a.	Johnson (85) Weicher et al. (82) Downs (83)	none (rely on previous studies)
Subletting & Doubling Up	Med. Run	varie widely (10-70% of renters)	.4-.9	Malpezzi (86b) Follain et al. (82)	Ghana

Note: This table is preliminary. Additional literature review is being undertaken in conjunction with case studies.

However, as Olsen's recent (1987) survey makes clear, much less is known about the supply side of housing markets, and they are more difficult to study (at least econometrically), partly because of the difficulty in collecting consistent data. It is not likely that we will be able to estimate all or even most of the parameters in Table 3 to our satisfaction given existing data; hence the complementary analysis using present value models (described later).

Keep in mind the fact that rent control's effects on each component will be affected by the particular features of the rent control regime. A regime that, (e.g.), exempts new construction but freezes rents on units produced before a certain date may reduce maintenance of a portion of the stock while leaving new construction relatively unaffected. A law which ties rents to maintenance expenditure may increase incentives for maintenance (Olsen 1987).

In the next few pages each supply component will be discussed briefly in turn. Then ways of further studying these effects with the data in hand will be described. Some indirect methods which can be applied in other case studies where the data support them will also be briefly described.

Rent Control and Housing Deterioration

Rent control ordinances which do not provide separate incentives or sanctions to encourage landlord maintenance offers landlords an incentive to allow their properties to deteriorate. When the rent reduction caused by rent control is 10 percent, landlords can charge the market price for only 90 percent of the housing services they produce. In the long run, landlords will tend to permit the portion of their output that yields no revenue to disappear through deterioration.

However, knowing that in the long run landlords will tend to allow their properties to deteriorate in proportion to the size of the rent reduction tells us little about deterioration in the short or intermediate run. Also, incentives can be created for tenants to invest in or maintain units if an increase in occupancy rights (tenure security) associated with rent control implies that tenants can now capture the gains from such expenditures. Particular ordinances may require landlords maintain units, repay tenant maintenance expenditures, or permit revaluation for a well maintained or upgraded unit. See, for example, Malpezzi (1986), Ch. 4, and Olsen (1987). The direction and size of changes in maintenance will vary with type of law, market conditions, and with landlord and tenant characteristics.

The question is not only whether rent control induces deterioration or by how much, but rather how rapidly it does so. Rydell and Neels (1982) have provided one estimate of a bound on such deterioration. They assumed that in any period, the quantity of housing services are the sum of the last period's maintenance and repair inputs, minus gross depreciation:

$$Q_{t+1} = Q_t + aM_t - bQ_t^L$$

Using iterative techniques on data from the U.S. Housing Assistance Supply Experiment, they find that the best fit is obtained when a gross depreciation rate b of eight percent is assumed (L, the elasticity of housing output with

respect to maintenance, is estimated to be .17). In other words, without maintenance, dwellings will depreciate by eight percent per year. This places a bound on how fast landlords can decrease the quantity of housing services as a response to the imposition of controls.

The rate of gross depreciation is a bound; the rate of net depreciation is how fast units actually deteriorate after maintenance and repairs. Gross depreciation is an essentially technical relation determined by the physical characteristics of the unit and its environment. As such it is unaffected by controls. The observed net depreciation rate may be very much affected by controls, as landlords reduce maintenance and repairs.

Malpezzi et al. 1987 have provided estimates of the rate of net depreciation in 59 U.S. markets. Some are controlled, and some are not; rent control regimes vary significantly from place to place. A simple model explaining the variation in net rates, including different types of controls as explanatory variables, may provide insights into the possible effects controls might have.

Hedonic indexes from the case studies will provide additional information on net depreciation rates. The Cairo data best support additional work on this issue. Other methods of analysis which can be used include present value analysis of alternative maintenance-depreciation strategies by landlords, and analysis of the semistructured interviews from the Egyptian and India case studies. These interviews will provide qualitative information on maintenance and repairs. In addition to this direct approach, the effects of rent control on the housing stock through changes in maintenance and repair could be studied in a production function framework

Further Reading: Rydell and Neels (1985); Malpezzi et al. 1987; Moorehouse (1972); Stout (1984); Skelley (1985).

Rental Housing Unit Losses: Demolitions, Conversions, and Foregone Starts

Regardless of the cause--declining demand or rent control--rent reductions motivate landlords to consider alternate uses for their property. Some remove the property from the housing stock (by either demolishing it or converting it to nonresidential use); others convert their rental units to owner-occupied units. Only a small fraction make such changes in any one year, even in the face of large rent reductions (even in the absence of provisions prohibiting or restricting such responses as are found in some regimes). More serious, especially over the long run, are future starts and conversions foregone. For all these components of inventory change, as in the case of deterioration, the question of rental housing losses caused by rent control becomes a question of the pace at which change occurs.

Note that there are two different kinds of effects controls can have on the numbers of units. Rent control can decrease the total supply of housing, but it can also shift supply from the rental sector into the owner occupied sector; from formal into informal sectors; and it can adversely affect the quality of units through several indirect effects (explain, financing, etc). Certainly decreases in the total supply of housing are potentially the most serious effects. But there can be adverse efficiency and equity effects from changes in tenure and quality, and this can be discussed

further in the case studies (and will certainly be addressed in the synthesis paper).

In principle these effects can be estimated directly where we have time series data from on removals by tenure, starts by tenure, and tenure-conversion rates. (In some markets it will be possible to collect data on starts, etc., but not necessarily by tenure). Such data are being collected for several developed country cities and for the Brazilian case study. Models examining the effects of controls on aggregate investment using time series data will be discussed in more detail below. In other markets we will estimate the effects of observed rental prices on profitability of rental investment. Then, using reasonable assumptions about behavioral responses of landlords to the observed changes, we can estimate losses in the rent-controlled housing supply from removals, conversions, and foregone starts. (See the references in Table 3). The present value model presented in Malpezzi (1988) may be adapted for this purpose.

Modeling Aggregate Housing Investment

The long run qualitative effects of an effectively administered control on rental prices is clear. Long run comparative static models make clear under certain conditions more housing will be produced in the absence of controls.[7/] But there are many possible time paths of adjustment from one long run comparative static equilibrium to another. This adjustment process is not just of academic interest, given the durable nature of housing and the potential lags in response to a policy change.

Better understanding of dynamics is in fact the key to evaluating alternative methods of decontrol. Choosing methods with politically acceptable adjustment costs is essential to initiate and safeguard reform. Under some market conditions, the initial adverse effects of some methods of decontrol can lead to irresistible political pressures to reverse direction. Imposing a method of decontrol which is not politically sustainable would be a Phyrric victory for those concerned with a more efficient and equitable housing market.

Consider generally the initial rental price response to any particular decontrol scheme. If the supply of housing services, perhaps long constrained, changes very slowly, the immediate result will be a sharp increase in rent. This will be followed by decrease to long run equilibrium rents, as additional rental housing comes on the market. The time path of rents will also be affected by the speed with which consumers adjust to the new opportunity set. The time path will also depend upon how decontrol is

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7/ Violations of the assumptions of this model - the existence of key money, tenant maintenance, ineffective enforcement, for example, are discussed elsewhere.

implemented. Further, the speed of adjustment depends upon collateral housing policies.[8/]

The ultimate goals of the research project are to clarify the costs and benefits of different methods of control under different market conditions, compare these to other market imperfections, and to evaluate alternative methods of decontrol. The project aims to make use of existing knowledge about the operation of housing markets,[9/] and develop new information on housing market behavior from these case studies. We need to make more specific the general notions in the preceding paragraph. What are the likely time paths of market adjustment to changes in policy, and what are the implications for decontrol?

This section will present a review of housing market models which will enable us to better understand and predict the market's response to imposition or relaxation of controls. First, we will discuss a series of quite general housing market models, that is, models which explain investment in housing without reference to the existence of owner and renter submarkets. These models include a simple stock adjustment model, and one with an extended lag structure; disequilibrium models; and a class of statistical time series models.

After this survey of general models of housing investment, we will investigate the implications of the existence of different tenure arrangements, and of price controls in one subsector. Then we will discuss data requirements for estimating several of these models.

Dynamics I: A Simple Stock Adjustment Model

Consider first a simple variant of a stock adjustment model for rental housing.[10/] In each time period t households close some fraction D of the gap between their long run equilibrium demand Q_t^* and actual consumption in the previous period Q_{t-1}:

$$dQ_t^D = D(Q_t^* - Q_{t-1})$$

Note that to simplify notation the stock of housing inherited from the previous period is assumed measured net of depreciation.

8/ Mayo et al. (1986) presents an overview of housing policy issues. In general, the policies discussed in that paper are the main collateral actions that will be necessary to ensure a sufficient supply response to relaxation or decontrol. These policies will be discussed further in the synthesis paper.

9/ See Linn (1983), Mayo et al. (1986), and Malpezzi and Mayo (1985) for developing countries, and Maclennan (1982) and Quigley (1979) for developed countries.

10/ The next sections on modeling of the supply side are preliminary and may change as we develop these models further.

Similarly, in each period landlords offer some net change in the supply of housing services:

$$dQ_t^S = S(Q_t^{\ddagger} - Q_{t-1})$$

where $Q^{\ddagger}$ is the long run equilibrium supply. Now Q^* and $Q^{\ddagger}$ are not readily observable. But the long run equilibrium demand Q^* can be readily assumed functionally related to income (Y), the relative price of housing (P), and demographic variables and other demand determinants (denoted Z). $Q^{\ddagger}$ can be readily assumed to be a function of housing prices and input prices (costs).[11/] Substituting, and assuming functional forms are linear in the logs, we are left with a demand function of the form:

$$\ln(dQ_t^D) = a_0 + a_1\ln(P) + a_2\ln(Y) + a_3Z + a_4\ln(Q_{t-1})$$

and the supply function:

$$\ln(dQ_t^S) = b_0 + b_1\ln(P) + b_2\ln(w) + b_3\ln(r) + b_4\ln(Q_{t-1})$$

where

a_i and b_i are regression coefficients,
w and r are the price of labor, and housing capital (including land).

The usual theory leads to a priori restrictions of a_1, b_2 and b_3 to values less than zero, and a_2 and b_1 to values greater than zero. If we further assume that there is a market clearing equilibrium such that:

$$dQ_t^D = dQ_t^S$$

such a two equation model can be estimated using measures of changes in the stock (e.g. rental starts adjusted for depreciation) as the dependent variable.

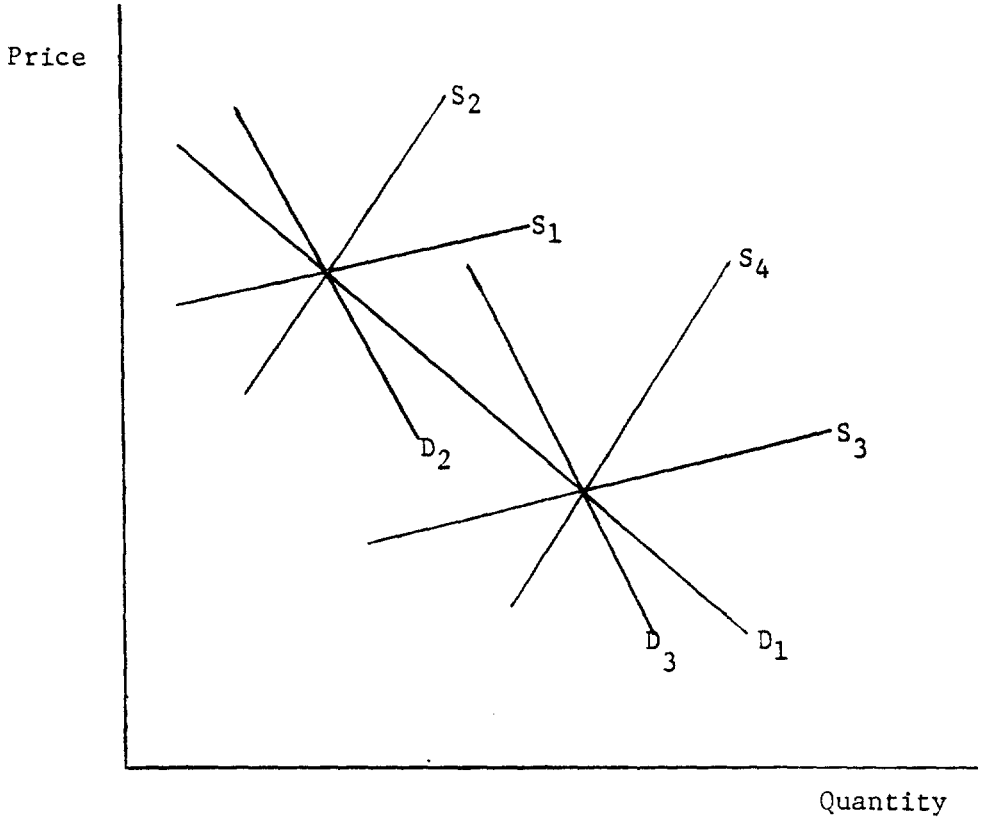
Several operational issues must be dealt with in the empirical implementation of such models. Two will be discussed briefly here: simultaneity, and functional form.

The simultaneous structural model written above has one supply equation, one demand equation, and an equilibrium condition which relates the two. Think of the supply and demand equations as 'processes,' for the moment unknown, which we would like to know. The values of the right hand side (exogenous) variables are readily observed, as is the single outcome, Q_t. In general, without specific assumptions it is not possible to recover the structural processes from knowledge of this outcome. Figure 3 illustrates

11/ Olsen (1987) points out that in a long run competitive market supply can be modeled as a function of output prices or input prices, but one or the other may be excluded.

Figure 3

The Identification Problem



Observed price and quantity combinations are consistent with an infinite number of supply and demand schedules. Additional information is required for identification of supply and demand.

this for the simple two variable case, where supply and demand are each functions of price.[12/]

There are, in general, an infinite number of simultaneous supply and demand equations which could generate the outcomes plotted in Figure 3. Fortunately, it is often possible to identify such a system using prior information, or assumptions, about the model. Full discussion of identification is beyond the scope of this paper. Here we will merely summarize two possible methods of identification:

1. Prior knowledge of one of the processes permits identification of the unknown process. For example, if it is known (or more rigorously a maintained hypothesis) that supply is perfectly elastic (or perfectly inelastic, or if the parameters of the supply equation are at otherwise known) then the data permit identification of the demand equation.

2. If some variables are (theoretically) included in one equation but not another, such exogenous variables permit identification, if the number of exogenous variables excluded from the equation to be identified at least equals the number of endogenous variables included in that equation, minus one.

Other models have implicitly assumed perfectly elastic supply in order to identify demand; but perfectly elastic supply is not a maintained hypothesis for our purposes; the elasticity is what we want to estimate. On the question of functional form, we recommend logs for empirical convenience and ease of interpretation.

Further Reading: Muth (1960) is a basic reference, but his model assumes elastic supply[13/] in contrast to our interest in estimating elasticity directly. DeLeeuw and Ekanem (1973) is one of the few studies which assumes a general form with lags in adjustment on both supply and demand sides. This study is also a model for work to follow the case studies on analysis of decontrol options. Follain (1979) Rydell (1982) and Smith (1976) are other useful references. Olsen (1987) presents an up to date review and critique.

Dynamics II: Explicit Estimation of Market Lags

Note that P, Y, Z, w and r were written without time superscripts. Conceptually long run equilibrium demand and supply would depend on (discounted) expected future values of these variables; P, Y, Z, w and r represent weighted averages of such expectations. In practice, it is

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12/ Generalizations to multivariate models, and further discussion, can be found in any econometrics text.

13/ Muth does test this assumption. His test is unable to reject the null hypothesis of elastic supply, but inability to reject the null does not prove it, and the power of this particular test is small.

customary to add the strong assumption that contemporaneous right hand side variables contain all available information about future prices. Muth (1986) is an interesting counter example of an adaptive expectations model which might be adaptable. But given that data for such econometric work is unavailable in most of the case study markets, we may turn to a simple simulation approach as in De Leeuw and Ekanem (1973).

Further Reading: Muth (1986) presents tests supporting adaptive expectations models of housing markets. Nerlove (1972) is a good general reference on lags. DeLeeuw and Ekanem (1973) model lags explicitly.

Dynamics III: Disequilibrium Models

One further extension to the model is suggested by the work of Fair (1972) and Fair and Jaffee (1972). Figure 4 presents two alternative models of a housing market. The top panel illustrates a market which is always assumed in equilibrium. Note that, in general, each observed pair of price and quantity observations are consistent with any of a number of supply and demand curves. This is the familiar identification problem.

Possible solutions to this problem are treated in all econometrics texts. Here, let us consider how a disequilibrium model might (1) realistically model a housing market and (2) help solve this problem. Relax the assumption that the housing market clears in each period, i.e. that $dQ_t^S = dQ_t^D$ for all t. Suppose, for example, that controls depress the price of rental housing to P_1 such that the market is characterized by excess demand (Figure 1). This so-called "short side ration" implies that that period's observed dQ_t is on the supply curve but not on the demand curve. Conversely, if the price rose to P_2 suppliers would offer more housing than demanded, and the observed point would reveal part of the demand curve D.

Fair and Jaffee use vacancy rates as an indicator of which curve the market is on. In severely controlled markets (where controls do effectively place a ceiling on the price per unit of housing services) we could entertain the maintained hypothesis that we are in a period of excess demand, i.e., on the supply schedule. This simple model may work better than switching regression models, etc. The choice of econometric method will be determined by the type of prior information available. In controlled markets where the regime actually places a binding ceiling on the price per unit of housing services we are estimating a supply equation. Under such conditions time series OLS models can yield good supply information, but little about demand.

Further Reading: Fair and Jaffe (1972); Murray (1983). See Bowden (1978) and Tishler and Zang (1979) for detailed treatments of disequilibrium models.

$$y_t = g(y_{t-1}) + v_t$$

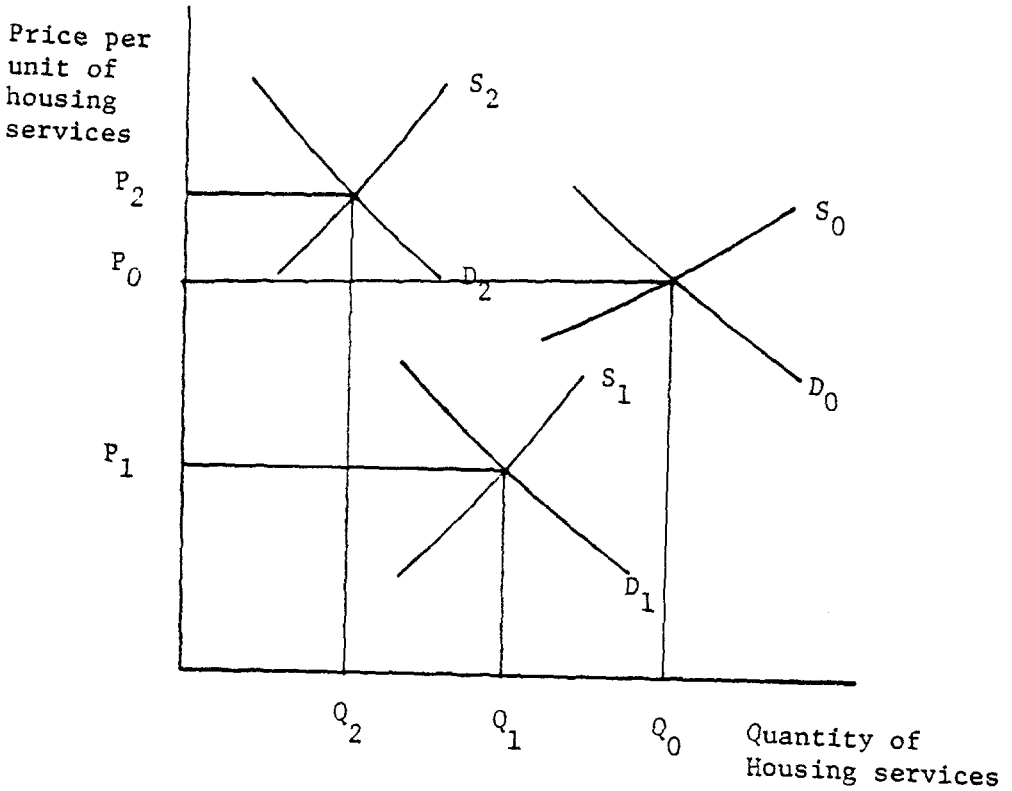
Dynamics IV: Time Series Modeling

The models above are straightforward, estimable, and consistent with current best practice in the analysis of housing markets in developing or developed countries. Yet problems remain--specification, errors in variables,

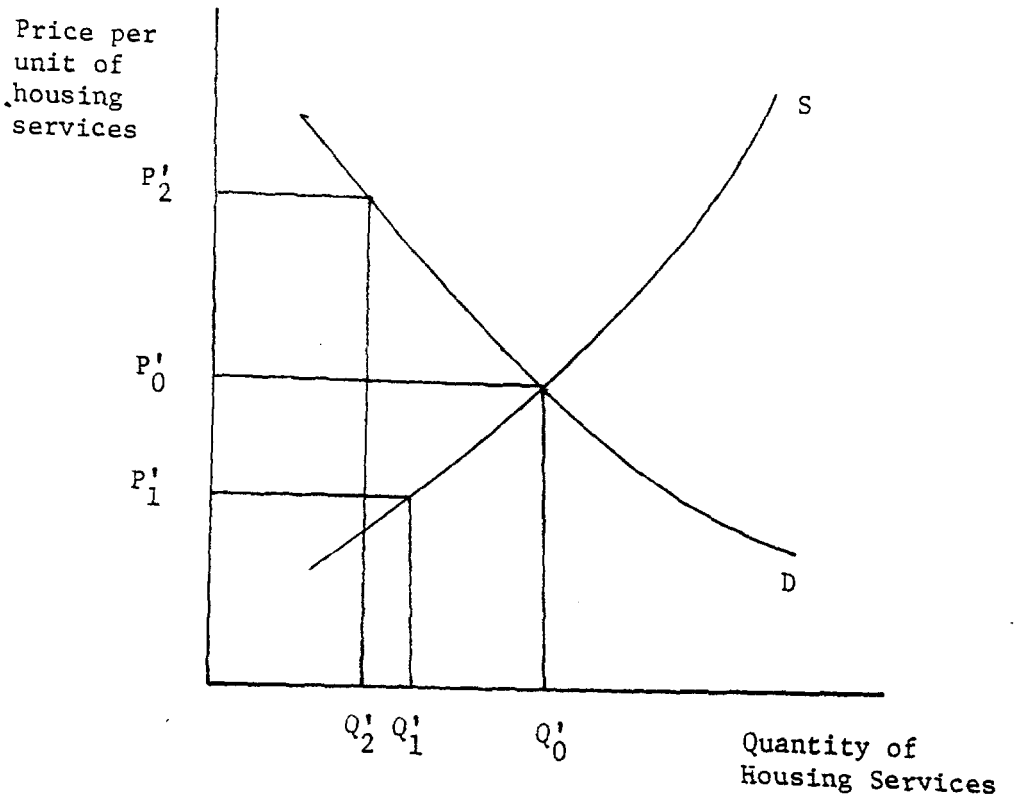
Figure 4

Disequilibrium in the Rental Market

I. Equilibrium



II. Disequilibrium



strong assumptions required for identification, and so on. Another class of models can be estimated, based on what are usually called time series methods.[14/]

The kinds of "structural" or "econometric" models described above are attempts to relate an outcome to past processes. The structural approach makes heavy demands on knowledge of the process, and data. "Time series" models take advantage of the fact that if an outcome is related to past processes, it is also related to past outcomes. A very simple general structural model is of the form:

$$y_t = f(x_{t-i}) + u_t$$

where f, the set of x, and the lag structure must be known, and data available, for reliable estimation. But even if the process f(x) is unknown,[15/] there would still be a realized set of outcomes y_t which can be studied by comparing current and past outcomes, for example by the very simple autoregressive model:

Such models are often used for evaluating structural models. They are even more useful when the state of structural model and data do not permit reliable estimation and forecasting. But simple models of the form above do not admit of changes in outcomes from changes in policy variables (e.g., if y are housing starts this model has no mechanism for exogenous changes in rent control to affect starts). A further refinement is to combine some time series model with policy variables or other structural variables of interest, for example

$$y_t = g'(y_{t-1}, p_t) + v_t$$

where the policy variable p is included in the mixed time series-econometric model.

We will not provide a detailed treatment and specification of these models here.[16/] We will, however, make use of such models with the Brazilian data (and possibly data from other countries) as a check on the other modeling.

14/ The use of the words "time series" to denote autoregressive and moving average models, and "structural" or "econometric" to denote models derived from some specific behavioral process is arbitrary and often confusing. Unfortunately the usage is well entrenched. The classic reference on time series methods is Box and Jenkins (1970); on structural models, Koopmans (1949). Almost all modern econometric textbooks now include sections on time series methods. See, for example, Chow (1983).

15/ In fact the problem might be that more than one process could conceivably yield the set of outcomes Y. This is known as the identification problem in econometrics.

16/ Zellner (1975) provides a detailed treatment of combining the two kinds of models.

Further Reading: Box and Jenkins (1970) is the classic reference on these models, Pindyck and Rubinfeld (1976) and Chow (1983) are two of many textbooks with a general discussion of these models. Zellner (1975) discusses mixed time series-econometric models. Appelbaum (1983) applies such a model to analysis of supply response to controls, although Malpezzi (1986) points out several flaws in the study.

Studying Responses to Changes in the Profitability of Rental Housing Investment Using a Present Value Model

In the absence of direct econometric estimates, supply side effects can be studied with a simple present value model of the profitability of rental housing investment. The key input to such a model is the change in rental prices brought about by controls, which can be readily estimated with the data at hand. Such a model is readily understood by a wide audience, and can be extended to include maintenance, depreciation, and changes in supply from demolitions, tenure conversions, and starts foregone. Different adjustment mechanisms can be studied within this framework.

Simple models of this type have been used to study rental housing policies in Malaysia (Malpezzi, 1988) and the United States (Brueggeman 1985, DeLeeuw and Ozanne 1981). See Malpezzi (1988) for a detailed treatment. The core of the model consists of comparative simulations of the profitability of a rental investment with and without controls. Given additional behavioral parameters including new estimates to be developed from the case studies), the supply implications of alternative changes can be studied.

The model can be used to study the profitability of different kinds of units treated differently under controls. For example, newly constructed units may be treated differently than existing units.

Another advantage of this model is that it can also be used to compare the distortion due to rent control to other housing market distortions. Taxes, finance, inflation, land availability and the presence or absence of alternative investment opportunities all affect the profitability of investment and investor's responses to changes in that profitability. These are all quite general and can easily be built into the model.[17/] The model can also be applied to alternative non-housing investments. Even after the imposition of controls the profitability of rental housing might be favorable relative to other alternative investments, especially in disrupted economies.

17/ Simplifying assumptions will have to be made; for example, the implicit cost of quantitative restrictions on land use have to be estimated or assumed, because the model only handles distortions that can be expressed in price changes.

Further Reading: Malpezzi (1988) sketches out such a model for studying Malaysian rental housing programs. DeLeeuw and Ozanne (1981) and Brueggeman (1985) focus on effects of taxes on rental housing, but the model can easily be extended to include rent controls. See also Downs (1983).

F. Impacts on the Government

Rent Control and Property Taxes

So far we have focused on the effects rent control has on the welfare of landlords and tenants. We have also explained how it can affect the welfare of households living in the "uncontrolled" sector. But rent control also affects government; the most obvious effect is the possibility of reductions in property tax revenue.

The effects of rent control on property taxes can be studied for two cases: when controlled rents are the basis for assessments, and when market values are the basis for assessments.

The first case is the more extreme. India is an oft cited example, where even units in the uncontrolled submarket, including owner-occupied units, are assessed at the controlled rents. Once we are armed with reductions in rents, calculation of rough estimates of the losses are straightforward. Once the rent reduction is further decomposed into price and quantity changes, and information is collected about reassessment practices, more refined estimates of the effects of changes in the rent control law on property tax collections can proceed (see Rydell and Murray).

In the second case rent control also affects tax revenues, because market values are related to the present value of rents. Under some conditions rent control will, for example, depress values of controlled units but increase values of uncontrolled units (Fallis and Smith 1984). Sorting out the net effect on tax revenues correctly requires taking this effect on related markets into account.

In the first stages of the work, the analysis will be limited to simple analysis of the direct effect controls might have on revenue. This is likely to be a good approximation of the cost of controls. If these turn out to be large, future work will analyze potential revenue losses, and suggested changes in tax policy, in more detail.

Further Reading: Rydell and Murray (n.d.); Weitzman (1983); White (1983); Amborski (1983)

Cost of Administration

Rent control also has direct administrative costs. In general, these have not been studied. If data on administrative budgets, court queues, etc., are readily available, their presentation would be of interest. It is unlikely that these costs are large relative to other costs of rent control. But if data are available they are of interest.

III. SPECIFICATION AND DATA REQUIREMENTS

This chapter will treat practical specification and data issues which arise in estimating models from the previous chapter. For convenience, we will discuss models estimated with household data and models using collateral data separately. Then we will discuss several purely statistical issues which can arise in any kind of model.

Summary of Models Using Household Survey Data

The previous chapter presented a suggested cost benefit model similar to Olsen's (1972) model, and some extensions. The steps involved in estimating such a model are described in Table 4. We discuss several practical problems in estimating such a model using household survey data in the next few pages. Hedonic indexes, demand equations, and data requirements will be discussed in turn.

A. Specification of Hedonic Indexes

This section summarizes the estimation of hedonic regression models for housing and how they can be used to estimate $P_m Q_c$, the rent that would be commanded by controlled units in the absence of controls. First, we will present an introductory and intuitive explanation of hedonic price estimation. Then we will discuss several practical specification issues.

Theoretical Basis

To a large extent, housing market analysis consists of comparing different dwellings. For example, measuring inflation requires comparing the price of housing today to that of some base period, but often in the interim the housing stock has changed, through new construction, rehabilitation, conversion, and demolition, so that we are actually comparing two different groups of dwellings. Other examples abound, such as comparing the price of housing in different locations, measuring the effects of racial or caste discrimination in housing, and studying the effects of government subsidies and tax policies on how we are sheltered. All require that we compare different dwellings. Estimating $P_m Q_c$ can be reduced to comparing rents for different types of dwellings in the controlled and the uncontrolled sector.

The method of hedonic equations is one way expenditures on housing can be decomposed into measurable prices and quantities so that rents for different dwellings or for identical dwellings in different places can be predicted. A hedonic equation is a regression of expenditures (rents or values) on housing characteristics and will be explained in detail below. Briefly, the independent variables represent the individual characteristics of the dwelling, and the regression coefficients are estimates of the implicit prices of these characteristics. The results provide us with estimated prices for housing characteristics, and we can then compare two dwellings by using these prices as weights. For example, the estimated price for a variable measuring number of rooms indicates the change in value or rent associated with the addition or deletion of one room. It tells us in a dollar and cents way how much "more house" is provided by a dwelling with an extra room.

Table 4: SUMMARY OF ESTIMATION PROCEDURES AND KEY CALCULATIONS USING HOUSEHOLD SURVEY DATA

I. Estimate Hedonic Indexes

Sample: Uncontrolled renters, homeowners, or other reference group.

Model: Log(Rent) = f(Housing Characteristics)

Rent could be actual rent paid (for uncontrolled renters), imputed rent, or capitalized house value. Should be tested for possibility that "uncontrolled" rents are affected by presence of controls in other sector.

II. Estimate Expenditure Equation

Sample: Uncontrolled renters, homeowners, or other reference group, as above.

Model: Log(Rent) = f(Income, Other Demand Determinants)

III. Calculate Welfare Measures

Sample: Controlled Renters.

P_cQ_c is directly observable from the sample.

P_mQ_c is estimated by applying the hedonic coeficients from the reference group to the controlled renters.

P_mQ_m is estimated by applying the expenditure equation coeficients from the reference group to the controlled renters.

Benefits are estimated under alternative assumptions about the price elasticity of demand:

$$\text{Benefit} = \left(\frac{1}{P_mQ_m}\right)^{1/b}\left(\frac{b}{b+1}\right)\left[Q_c^{\frac{b+1}{b}} - Q_m^{\frac{b+1}{b}}\right] + P_mQ_m - P_cQ_c$$

where b is the assumed price elasticity (-.5), and

$$\text{Benefit} = P_mQ_m(\log(P_mQ_c) - \log(P_mQ_m)) + P_mQ_m - P_cQ_c$$

in the special case when b is assumed to be -1.

IV. Distributional Implications

Sample: Controlled Renters

Model: Benefits = f(Income, Length of Tenure, Socioeconomic Status)

Hedonic regressions should be estimated separately in each market, where prices and quantities ideally clear. When studying the effects of rent controls we normally estimate a hedonic model for one or more uncontrolled submarkets. Once we have estimated the implicit prices of measurable housing characteristics in the uncontrolled submarket, we can use these coefficients to estimate market rents for controlled units.

More formally, the hedonic regression assumes that we know the determinants of a unit's rent:

R = f (S, L, C), where
R = contract rent
S = structural characteristics;
L = neighborhood characteristics, including location within the market; and
C = contract conditions or characteristics which affect the price, such as utilities included in rent

Practical Considerations

Here we want to highlight a few important pieces of advice, based on our experience with these models in other countries.

Functional form. Semilogarithmic models seem to work well and are recommended. Such models regress the natural logarithm of rent against linear independent variables. Linear models are often very heteroskedastic and do not fit data quite as well. More elaborate functional forms are expensive to compute and sometimes unreliable for predicting rents for units not in the sample. See Malpezzi et.al. (1981), pp. 24-25.

Independent variables. With many degrees of freedom, dummy variables can be used for variables which have only a few values, such as number of toilets. One thing to be on the lookout for is a lack of variation when almost all sample observations either have a particular characteristic or do not have it, or when everyone who has one characteristic also has another. Suppose every household who had water also nad sewerage, and every household who did not have sewerage did not have water. Then we could not estimate the two effects (water and sewerage) separately because of high or complete collinearity. Also, if everyone has a characteristic, that variable will be collinear with the constant term in the regression.

Sometimes crosstabs can help identify this problem for dummy variables. Look at the "sum of square and crossproducts" (sometimes called SSCP, or X'X) matrix from the regression, for a quick check. Pairwise correlation coefficients and sample means also help us see which variables do not have sufficient independent variation for reliable estimation. If your computer program for regression outputs diagnostic statistics such as the tolerance level of each independent variable or the condition number of the X' X matrix, then these are also useful. When any of these procedures indicates high collinearity, or lack of variation in a variable, some variables will have to be dropped, or collapsed.

Collapsing dummy variables into a more parsimonious model incurs some cost, since the dummy variables are less restrictive (yield more information about the sample) than the linear variables, but this is outweighted by the gain in being able to more reliably predict rents out of sample by making use of the sensible but more comparable specifications. A good strategy for a specification search is the following. Start the specification search with all sensible variables in all subsamples. The decision rules below, if applied to each subsample separately, will result in different specifications. For proper estimation out of sample, a second goal is to make each specification as much alike as possible. Do this by collapsing variables where necessary, and sometimes stretching the decision rules for single samples. If a variable is the correct sign and significant in one sample, but insignificant in another, keep it in both samples.

This brings up an important issue. How do we decide whether to keep or drop a variable? If a variable has the expected sign and a reasonable magnitude and is "significant," clearly we will keep it, but what about others?

When the purpose of regression analysis is to test hypotheses about certain coefficients, it is a problem if we let the data form the model by simply dropping all non-significant variables, then reporting a test result. But this is not the purpose of the present exercise. We are going to use the results to predict rents out of sample, that is, predict market rents for the controlled sample. It is very important that the hedonic predict the overall rent for a unit in another sample as reliably as possible. If some variables have very imprecise coefficients, this could cause a problem because the point estimate of the coefficient will vary widely from sample to sample. If a coefficient has wrong sign because it is correlated with some omitted variable in one sample, there is no guarantee that this correlation exists in the other sample. Therefore we formulate the following rule: drop a variable if it has the wrong sign, and drop it if it has the right sign but an extremely unreasonable magnitude. Examples of unreasonable magnitudes would be situations like the following. Suppose the coefficient of a dummy variable for cooking with electricity was 0.75 in a semilog modal. We expect units with electrical cooking facilities to rent for more than those with the base case (kerosene, charcoal, wood, other fuels), but it is probably not reasonable to expect them to rent for 75 percent more, on average, than otherwise identical dwelling units. Such a statistical result might well be spurious and cause us to make mistakes when predicting out of sample.

This does not mean you have to drop every coefficient which deviates somewhat from expectations; rather, be on the lookout for strange coefficients--especially if a coefficient in a semilogarithmic regression exceeds one (a 100 percent or more difference in rent) it bears thought and investigation.

Another related issue is, what do we mean by "significant?" Significance levels are almost always chosen merely by convention, not with respect to a well-specified loss function, because such loss functions are almost impossible to design in most real world applications. Given that we have to be arbitrary, we prefer to pick "larger" significance levels than is common (t-statistic of, say, 1 rather than 2, corresponding to a probability of Type I error of about .3, rather than the more common .05, but reducing the

chances of Type II error). This is consistent with the purpose of the hedonic: not to test for significant effects, for which the conservative procedure is to minimize Type I error, but to predict rents, for which we are relatively more worried about Type II error.

Recall that a Type I error is to reject a null hypothesis when it in fact true. In this context, to make a Type I error results in keeping a variable in the model when in fact it adds no real predictive power. A Type II error is to fail to reject a null when it is actually false. In this case, a Type II error results in dropping a variable which really belongs in the model.

If the purpose of a statistical exercise is to examine individual effects of variables, Type I error is very important. However, our real purpose here is to predict the dependent variable (log of rent). Since omitting variables biases this prediction but including extraneous variables does not (it does decrease the efficiency of the still unbiased and consistent estimates), we are relatively more concerned about Type II error. There is a tradeoff between the two types of error. Decreasing the "significance level" decreases the probability of Type I error but increases the probability of Type II error. The correct way to choose a cutoff in the Neyman-Pearson hypothesis testing framework is to specify a loss function which numerically specifies the relative weights given to each type of error. In practice this is never done, and significance levels are chosen merely by convention (usually .05 or .01) because such loss functions are difficult to specify explicitly. The "significance level" we recommend is also arbitrary--we specify no loss function--but at least takes qualitative account of our greater concern with Type II error.

Variables Which Do Not Belong in the Hedonic Index.

Hedonic models are models based on characteristics of the dwelling unit such as space, quality, location, and so on. Characteristics of the household which inhabit the unit, such as income, do not belong in this regression, they belong in the subsequent demand equation. Exceptions are demographic variables which affect the price paid for the unit, such as length of tenure and crowding. See M-O-T, pp. 12-13, pp. 78-82.

B. Estimating Housing Expenditure Equations

Housing expenditure equations should be estimated for the reference group following procedures described in the previous section. Here we discuss some general issues of model specification which parallel those of the previous section. See Malpezzi and Mayo (1985), de Leeuw (1971), Mayo (1981) and Olsen (1987) for more detailed treatments.

A typical housing expenditure equation, or Engel relation, takes the form:

$$R = f(Y, D)$$

where R = housing expenditure (rent or amortized value)
Y = household income

D = household demographic variables (e.g. household size, age and sex of head, religion, caste, length of tenure, etc.)

There are six essential issues which must be tackled when constructing models of housing demand. Three are essentially measurement issues: how to measure housing consumption, incomes, and prices. Another issue is how to integrate related behavioral outcomes like tenure choice and mobility into the demand relation. There is the question of alternative functional forms and the choice of estimating technique. Finally there is the problem of choosing a reference group, and testing the hypothesis that the group is comparable to the controlled renters. Each will be discussed in turn.

Measurement Issues

Measuring housing consumption. Ordinary demand analysis begins by postulating a relationship between the quantity of a good demanded, its relative price, the income of the household, and other things that may affect demand such as household size. This model suggests that given household survey data we estimate a model of the form:

$$(1) \quad Q = f(P, Y, D)$$

where Q is the quantity of housing services demanded, P is the relative price of housing, and the other variables are as defined above.

This kind of model is difficult to estimate for housing, because expenditures (price times quantity) are commonly observed but prices and quantities are less so. Often studies directly estimate the so-called income-consumption path, or Engel curve. That is, the regression is (in logs):

$$(2) \quad \log R = \log (PQ) = a + b \log Y + cD = u$$

Studies using U.S. data have suggested that estimates of b from equations like (2) yield biased estimates of the true income elasticity of demand, because the price per unit of housing services and incomes are negatively correlated, particularly for owner occupants. In the United States, there is a well documented tendency for higher income households to locate further from the central business district, where land (and hence housing) prices are lower on a per unit basis. Also, the U.S. tax code provides proportionally larger tax subsidies to higher income households, reducing their after tax housing cost. Malpezzi and Mayo (1985) showed that this correlation was not observed in several developing country markets, and that, in general, simple models like (2) yield remarkably robust estimates of the income elasticity in a range of developing country markets.

Gross versus net rent. Another issue which arises in consumption measurement is the following. Some renters pay for utilities separately, and some have utility charges included in their monthly rent. There are additional charges more common in controlled markets, e.g. key money, rents paid in advance, and tenant maintenance and repair expenditures. Malpezzi (1986) discusses the role of such side payments in some detail. The ideal

rent measure will comprise all of the tenant's expenditure on housing services, including amortized key money and upgrading expenditures. Comparisons between gross and net rent, as in Malpezzi (1986) are of particular interest.

Recommendation for Housing Consumption Measures in Regression Models of Housing Demand:

1. Convert monthly rent, utilities, tenant maintenance expenditures, key money, tenant upgrading expenditures, the opportunity cost of rental advances into a monthly figure. Compare to contract or net rent.

Measuring housing prices. This topic has been partly addressed in the section on consumption, as these two measurement issues are obviously closely related. Hedonic indexes will be estimated using an uncontrolled reference group (as discussed above), and used to estimate market prices for controlled units. The cross country model discussed later in the paper can provide an independent check on the reasonableness of these estimates. This is of particular importance since controls can, under some conditions, distort prices in the uncontrolled sector (Fallis and Smith, 1984).

Recommendation for Price Measures in Regression Models of Housing Demand:

1. Prices vary from city to city. Therefore, estimate separate hedonic equations for each city with sufficient samples (more than 150 degrees of freedom). Estimate separate models for controlled and uncontrolled markets.

2. Prices also vary with location within a city or town. The simplest model postulates that price varies with distance to Central Business District. Therefore, to control for intrametropolitan price differences, include distance to town center if available as an independent variable if there is sufficient variation in the distance variable to yield significant estimates.

Measuring incomes. Since adjusting the consumption of housing services is so costly and undertaken so infrequently, it is commonly postulated that the demand for housing is related to some expectation of the household economic situation over a time period longer than the immediate market period. Commonly researchers try to distinguish between current and permanent income, where permanent income is adjusted to reflect long run expectations about future income. The classic work on the permanent income hypothesis is Friedman (1957). A related hypothesis which yields similar qualitative conclusions for the demand for durable goods is the life-cycle earnings hypothesis (see Ando and Modigliani (1963). In other words, consumption does not change as much from year-to-year as total income. People save in good years and spend their savings or borrow in bad years. Rent changes even less than total consumption, because it is so costly to move.

Since consumption is related to long-run or permanent income, this suggests permanent income rather than current income is the true determinant of housing consumption.

In practice, there are three common ways in which researchers try to proxy permanent income, which is never directly observable. The first, advocated by Friedman in his seminal paper, is to use a weighted average of past incomes as proxy for permanent income, where the weights reflect some market discount rate. This approach requires panel data (a cross section of households surveyed repeatedly over time). Most such panels have data for three or four years at most. The average used could be improved if longer time series were available. Most studies using this approach assume a very high discount rate. Also, note that the empirical implementation of Friedman's theory is somewhat ad hoc, because the theory postulates that consumption depends on future expectations, which may differ from past experience.

A second method is to use a first stage regression of current income against age, education and other determinants of current income, and to use the prediction from this equation as an instrumental variable proxying permanent income. This method implicitly assumes that the relevant permanent income measure varies over a person's lifetime.

The third empirical approach is straightforward. Since households make decisions about consumption largely on the basis of permanent income, and consumption is measurable, why not use consumption as a proxy for permanent income? The assumption of this approach is that changes in transitory income do not affect total consumption or housing consumption.

Of these three approaches, the third is appealing. The first approach requires time-series data which are unobtainable for the households in our cross section data sets. The second is somewhat complicated, and not without theoretical difficulties.[1/] The third approach is the simplest and can be easily implemented. Malpezzi and Mayo (1985) show that this simple approach yields results similar to more complicated techniques.

Recommendation on Measuring Income in Regression Models of Housing Demand:

1. Estimate demand models using total household income. Since consumption is an excellent proxy for the theoretically preferred "permanent income," also estimate using total consumption if available, and compare.

Demographic Variables. Most economic models focus on the role of prices and incomes in determining patterns of demand. The underlying assumption is that other derminants of demand, such as tastes, family composition and size, are "held fixed". Empirical work requires that we include these kinds of variables in our regression models so that this assumption is tenable.

1/ In brief, such an instrumental variable approach yields predicted current income. Interpreting such a result as permanent income has no strong theoretical foundation at the present time. Such an interpretation is at best ad hoc.

The most important single demographic variable affecting housing consumption is household size. Other candidates for inclusion in the analysis are: age of household head; number of children (measured separately from number of adults); and the sex of the head of household. Sometimes it is hypothesized that tastes vary by income class or by tenure, or by religion or caste.

Research has demonstrated that the longer a household stays in a unit, the lower are rents for a given level of housing service even in markets without rent control. The "tenure discount" associated with longer stays is often a rational outcome of landlords wishes to reduce turnover, avoid vacancy losses, and continue leasing to known tenants.[2/]

In order to forecast rent levels in the absence of rent control, is necessary to account for such tenure discounts. Consequently length of tenure should be entered into housing expenditure functions. One potential problem in doing this for the reference group described above is that households in the low-rent portion of the reference group may have occupied their units for a much longer time than households in the new-unit portion, with the result that length of tenure may be collinear with a number of other differences (particularly income differences) such that estimation of the "true" tenure discount becomes difficult and coefficients of other variables in the expenditure equation could be imprecisely estimated.

Our suggestion is to estimate housing expenditure equations both with and without length of tenure terms and for both new unit and pooled samples to examine the sensitivity of length of tenure into expenditure equations. Two criteria should be used to judge the best model; first, parameters of variables other than length of tenure should not be greatly affected by the variables inclusion or exclusion and second, the coefficients of length of tenure should be "reasonable." If, for example, adding or deleting length of tenure changes other variable coefficients by more than a standard error this is grounds for using extreme caution in basing estimates of market rents on an equation that includes length of tenure.

"Reasonable" values for length of tenure based on previous research would be on the order of rent discounts ranging from about 0.5 to 2.5 percent per year. Thus in a log-linear expenditure function with log (rent) regressed on length of tenure the coefficient of the latter variable should be from -0.005 to -0.025.

If either of these criteria is violated, then we suggest a somewhat more ad hoc method of accounting for tenure discounts. Specifically, we suggest estimating an expenditure equation with length of tenure included, but then basing estimates of market rents on the estimated equation adjusted for reasonable tenure discounts based on the independent estimates (e.g. Malpezzi et al. 1981, pp. 78-79). Using this procedure the estimated market rent for a household living in a controlled unit would be:

$$R_m = X\hat{B} - \hat{b}L - .01L$$

2/ See M-O-T, pages 78-9.

where R_M = estimated market rent in the absence of rent control

$\hat{XB}$ = estimated market rent based on an expenditure function (with length of tenure included)

L = length of tenure

$\hat{b}$ = the estimated coefficient of the length of tenure variable

and the parameter -.01 is assumed (and may be varied parametrically) indicating a "normal" tenure discount of one percent per year.

Further Reading: Malpezzi and Tewari (1987).

Recommendation on Demographic Variables in Regression Models of Housing Demand:

1. Household size. At a minimum, the estimated demand relations should include a measure of household size. Since the effects of one additional household member are probably different for large families than for small families, an additional quadratic term may be used.

2. Other candidates for inclusion include age of household head (possibly as dummy categories), a dummy for female headed households, and number of children. Some experimentation may be necessary, to determine which variables make a difference in a particular market.

Functional Form. See the discussion of functional form for hedonic indexes, above. Mayo's (1981) survey highlights the fact that qualitatively similar results are obtained using linear and logarithmic models. Log models have desirable properties such as reduced heteroskedasticity, and reducing the influence of extreme rents and incomes on parameter estimates. In addition, ease of interpretation is an advantage; in log models the coefficient is a direct estimate of the elasticity. Hausman (1981) discusses the consumer theory behind such models.

Recommendation for Functional Form

1. Estimate logrithmic demand models. See Malpezzi and Mayo (1985) for additional details.

C. Joint Tenure-Mobility-Housing Consumption Decisions

Housing consumption is actually one part of a joint decision. At any given moment, households can review their housing situation, and make several decisions, which can be modeled in sequential or simultaneous frameworks. Households "compute" their notional current demand, compare it to current consumption, and survey opportunities in the market, including units available, prices, location, transactions costs, etc. At any time households

can do nothing, or change their consumption by moving, increasing or decreasing maintenance on their current unit, or upgrading. If they move they can change tenure. Households may split as children age, or through divorce, among other changes; households may decide to lease part of their current unit.

A growing literature addresses these problems. Lee and Trost (1978) models demand and tenure choice. Weinberg et al (1981) models demand and mobility. Other references are available in Quigley (1979).

Clearly, modeling such joint decisions would put the project at the forefront of the current literature but it also would complicate each case study enormously, and make cross-country comparisons even more difficult. Further, studies such as Lee and Trost, Malpezzi (1986) and Rosen (1979) find modest impacts on basic demand results when more complicated simultaneous models are used. Estimated income and price elasticities from this type of study are usually in line with simple single equation estimates. Our current judgement that the gains from such models would be outweighed by the costs of developing and estimating them, especially in a way which facilitiated cross-country comparisons.

Recommendation for Joint Models:

1. Estimate simple single equation demand models. If possible and resources permit, estimate more complete models (including tenure choice, moving and upgrading) for comparison. Unless important differences are observed, emphasize simple model in rent control modeling.

D. Choice of a Reference Group

Perhaps the single most difficult empirical problem is choosing a reference group. 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 that they are unreliable guides to rents in the absence of controls--or that a good adjustment can be made for that distortion. Malpezzi (1986) presents tests and discussion of these issues in detail (see Chapter 2, above). Here we note the following:

1. Regression analysis is, in fact, a statistical method which enables analysis of "treatment" and "control" groups which are not identical.[3/]

3/ See any intermediate statistics text for a more detailed explanation.

2. One possible problem is that households in the reference and control groups are systematically different in their demand for housing; but Malpezzi (1986) has found such selectivity bias does not make much difference in Cairo.

3. As noted earlier, rent controls can, under some circumstances, affect rents in the uncontrolled sector. We next discuss this hypothesis in the context of Malpezzi and Mayo's (1985) cross country model of housing demand.

E. Cross Country Models of Housing Demand

This section presents a variant of the cross-country housing demand model of Malpezzi and Mayo (1985, 1987a, 1987b) which can be used to estimate market rents in the absence of controls. These estimates can be used to predict market rents in cities where no uncontrolled sector exists for comparison; to test for bias and to adjust rents in the uncontrolled sector if such rents have been affected by controls as in Fallis and Smith's model; and as an independent check on other methods.

First we will describe Malpezzi and Mayo's model generally, then we will present new estimates from a variant of that model.

Until recently few comparative studies of housing demand in developing countries existed. Most of the studies of household demand for housing services in developing countries were based on specialized data bases, not usually collected for the express purpose of estimating housing demand relationships.[4/]

In 1981, a comparative study of housing demand in developing countries was initiated at the World Bank. In that analysis high quality data were collected for 16 cities in 8 countries (Colombia, Egypt, El Salvador, Ghana, India, Jamaica, Korea, and the Philippines) and were used to estimate housing demand relationships using relatively comparable variable definitions and identical functional forms and stratifying variables.

Malpezzi and Mayo first estimated a simple log-linear model of housing expenditures in each of the sixteen cities:

$$\ln R = a + E_y (\ln y) + bH + cH^2 + u$$

where R is rent; y is income; H is household size; E_y is the estimated income elasticity of demand; a, b, and c are regression coefficients, and u is an estimated disturbance. The model was stratified for renters and owners. For renters, rent was defined as net rent, exclusive of separate utility payments. For owners, rent was defined variously, and in order of

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4/ For a review of the pre-1984 literature see Malpezzi and Mayo (1985). Different analyses used different variable definitions, different functional forms, and different stratification variables. This made comparison of results regarding demand parameters across studies difficult.

availability, as owner imputations of net rent, hedonic estimates of net rent based on applying renter-based hedonic price equations to owners' housing characteristics, or imputed rents based on applying a fixed amortization ratio (from one percent to one and one-half percent per month depending on the country) to owners' estimates of housing value. While other functional forms were tried, and other demographic variables were included in alternative estimating equations, results from the simple log-linear model were found to provide adequate fits and robust findings regarding major demand parameters.

Table 5 presents those estimated parameters of housing expenditure functions for renters and owners. In general the results are remarkably consistent with results from developed countries (see Mayo, 1981). The regression fits are typical for this type of equation: typical R-squared statistics are in the 0.1 to 0.3 range (minimum is 0.06, maximum, 0.57). Fits are similar for owners and renters.

The median of all renters income elasticities was 0.49; developing country elasticities ranged from 0.31 (Pusan, Korea) to 0.88 (Davao, the Philippines). Most clustered between 0.4 and 0.6 with estimated U.S. elasticities lower than developing country estimates. The median of all point estimates of owner income elasticities was 0.46, with extremes of 0.17 in Cairo and 1.11 in Santa Ana, El Salvador. The majority of point estimates lie between 0.4 and 0.6. In 9 of 14 cases where comparison was possible, estimated developing country owner income elasticities were greater than those of renters. This finding parallelled findings in the literature for developed countries (Mayo, 1981). Comparing expenditure equations across countries revealed practically no systematic variation of income elasticities with country or city income level or population size, but considerable variation in dollar-adjusted intercepts, which were positively related to average city income. Rent-to-income ratios therefore declined systematically with income <u>within</u> cities, but increased with income <u>across</u> cities.

These relationships are shown graphically in Figure 5 for renters in four representative cities. Relationships for owners are similar, although average rent-to-income ratios are invariably higher at every income level for owners within given housing markets.

The relationships portrayed in Figure 5 are very similar to the consumption patterns within and across countries documented by Kuznets (see Kuznets, 1961 and other works cited therein). Qualitatively, housing consumption is remarkably smaller at various income levels than are between-country differences at different average income levels. Malpezzi and Mayo explored alternative theoretical explanations for these results and then tested a series of long run cross-country housing expenditure models. The simplest cross-country model parallels the log-linear within-country model, but with the addition of a price term, the relative price of housing which was constructed using data from Kravis, Heston and Summers (1982).

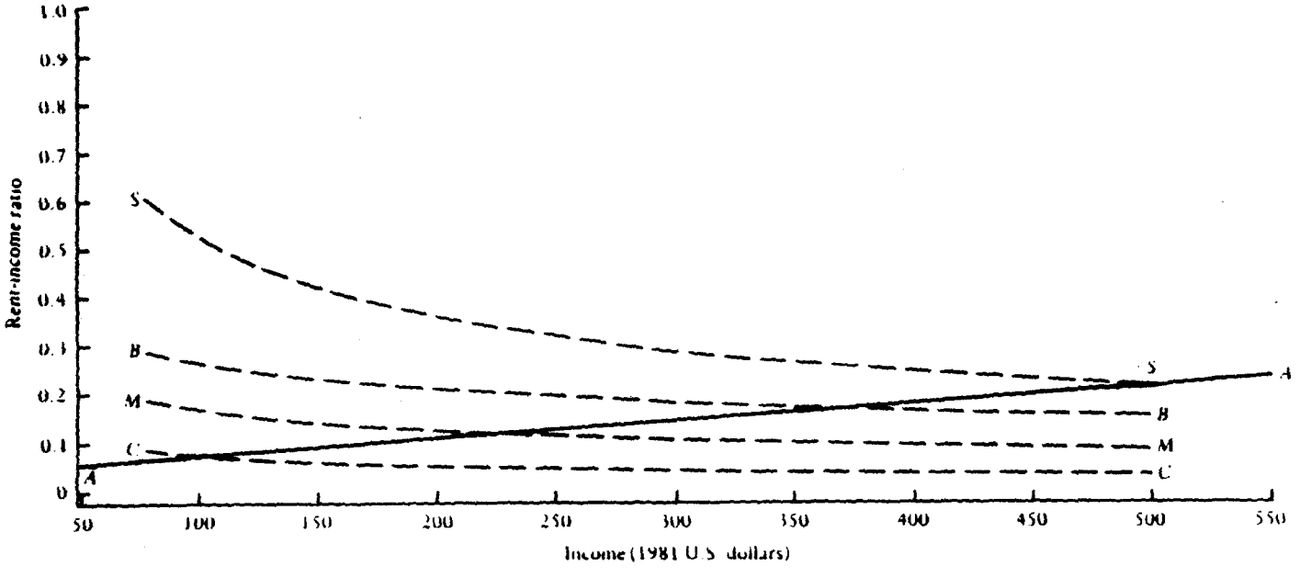
Table 5

Estimated parameters of housing expenditure functions.

			Renters					Owners				
Country	City		Constant	Log income	HH size	HH size squared	*R*-squared *N*	Constant	Log income	HH size	HH size squared	*R*-squared *N*
Colombia	Bogota	(coef)	1.11	0.66	0.09	−0.006	0.40	0.77	0.75	−0.00	−0.003	0.49
	(1978)	(std err)		0.03	0.03	0.003	1016		0.03	0.04	0.003	821
	Cali	(coef)	2.81	0.44	0.13	−0.006	0.27	1.25	0.69	−0.05	−0.000	0.38
	(1978)	(std err)		0.06	0.07	0.007	257		0.06	0.07	0.005	256
Egypt	Cairo	(coef)	0.25	0.46	−0.17	0.010	0.16	0.89	0.17	0.12	−0.009	0.06
	(1981)	(std err)		0.06	0.09	0.008	303		0.12	0.21	0.019	76
	Beni Suef	(coef)	−1.2	0.51	0.38	−0.047	0.25	−0.09	0.42	0.14	−0.003	0.23
	(1981)	(std err)		0.14	0.28	0.029	63		0.13	0.14	0.010	63
El Salvador	Santa Ana	(coef)	0.37	0.48	0.13	−0.014	0.16	−2.5	1.11	−0.06	−0.004	0.37
	(1980)	(std err)		0.11	0.08	0.007	131		0.11	0.12	0.009	169
	Sonsonate	(coef)	0.79	0.50	−0.10	0.007	0.16	0.39	0.79	−0.13	0.001	0.57
	(1980)	(std err)		0.12	0.09	0.007	83		0.15	0.17	0.012	27
Ghana	Kumasi	(coef)	0.82	0.33	0.02	0.000	0.11	–	–	–	–	–
	(1980)	(std err)		0.04	0.03	0.002	814	–	–	–	–	–
India	Bangalore	(coef)	0.66	0.58	−0.08	0.003	0.18	2.84	0.43	−0.17	0.007	0.15
	(1975)	(std err)		0.04	0.04	0.002	1041		0.08	0.06	0.004	205
Jamaica	Kingston	(coef)	−0.12	0.70	0.16	−0.012	0.30	–	–	–	–	–
	(1975)	(std err)		0.08	0.07	0.007	223	–	–	–	–	–
Korea	Seoul	(coef)	5.04	0.45	0.07	−0.004	0.15	6.06	0.44	−0.04	0.002	0.12
	(1979)	(std err)		0.03	0.04	0.005	952		0.04	0.04	0.003	952
	Busan	(coef)	6.26	0.31	0.05	−0.001	0.08	5.93	0.45	−0.05	0.002	0.10
	(1979)	(std err)		0.07	0.06	0.006	508		0.08	0.10	0.011	296
	Taegu	(coef)	4.95	0.44	0.03	−0.003	0.23	6.32	0.47	−0.19	0.011	0.18
	(1979)	(std err)		0.07	0.07	0.008	292		0.08	0.08	0.006	152
	Kwangju	(coef)	2.70	0.62	0.09	−0.002	0.32	7.53	0.41	−0.27	0.018	0.14
	(1979)	(std err)		0.09	0.13	0.014	134		0.11	0.18	0.016	84
	Oth. K. c.	(coef)	3.33	0.54	0.04	0.002	0.17	2.16	0.79	−0.12	0.003	0.26
	(1979)	(std err)		0.05	0.05	0.007	1000		0.05	0.05	0.005	779
Philippines	Davao	(coef)	−1.6	0.88	0.00	−0.002	0.42	−3.2	0.99	0.04	−0.004	0.28
	(1979)	(std err)		0.03	0.05	0.002	1376		0.04	0.04	0.003	1968
	Manila	(coef)	1.27	0.56	0.01	−0.002	0.22	2.46	0.57	−0.02	−0.000	0.31
	(1983)	(std err)		0.04	0.04	0.003	605		0.04	0.05	0.003	390
U.S.	Pittsburgh	(coef)	3.07	0.26	−0.02	−0.002	0.15	3.50	0.18	0.08	−0.005	0.21
	(1975)	(std err)		0.02	0.04	0.005	946		0.01	0.02	0.002	2378
	Phoenix	(coef)	3.68	0.18	0.12	−0.015	0.13	3.62	0.18	0.13	−0.011	0.24
	(1975)	(std err)		0.02	0.03	0.005	918		0.01	0.01	0.002	2284

Figure 5

Rent-Income Ratios by Income for Renters



Key: A average for each city at its average income B Bogota C Cairo M Manila S Seoul

Table 6: ADDITIONAL DATA FOR CROSS COUNTRY MODEL

Country & Source	Survey Year	Market	Rent/ Income	Household Income[1/]	Controlled?[2/]
Colombia (Strassman 1982)	1978	Cartagena	0.16	277	Yes
Indonesia (Shefer 1983)	1978	Jakarta	.19	204	No
		Lg. Cities	.16	110	
		Med. Cities	.16	126	
		Sm. Cities	.13	118	
Ivory Coast (Grootaert and Dubois 1986)	1979	Abidjan	.16	550	No
		Other Urban	.18	386	
Jordan (Struyk 1986)	1986	Amman	.16	655	Yes
		Other Urban	.18	422	
Kenya (Kenya 1986)	1983	Nairobi	.22	71	No
		Mombasa	.16	58	
		Kisumu	.21	89	
		Machakos	.09	42	
Peru (Strassman 1980)	1980	Lima	.12	207	Yes
Poland (Mayo 1987)	1986	Urban	.03	6023	Yes

Notes:

1. Income converted to 1981 U.S. dollars using local CPI and official 1981 exchange rates.

2. Indonesia and Kenya have controls which are not widely enforced.

Defining R as rent, y as household income, and p_H as the relative price of housing, Malpezzi and Mayo originally estimated the following models for renters and owners in developing countries:

Renters

$$\ln R = -5.39 + \underset{(0.18)}{1.60} \ln y + \underset{(0.15)}{0.15} \ln p_H$$

$R^2 = 0.90$
d.f. = 13

(standard errors in parentheses).

Owners

$$\ln R = 3.57 + \underset{(0.35)}{1.38} \ln y + \underset{(0.50)}{0.65} \ln p_H$$

$R^2 = 0.76$
d.f. = 11

where rent, and income are city means converted to 1981 U.S. dollars[5/], and pH is the Kravis-Heston-Summers price index, with the U.S. relative price normalized at one.

The implications of these models, which were confirmed with alternative specifications, are straightforward. In the very long run, housing consumption is income elastic. Price elasticities are smaller in absolute value than income elasticities, although confidence intervals are quite wide for the former. Long-run income elasticities are estimated to be slightly higher for renters than owners. This means that as cities' economies develop over the very long run, that owner and renter consumption patterns increase at a similar pace, ceteris paribus. However, because renter price elasticities are estimated to be higher than owner elasticities, the net effect of both incomes and prices rising with economic development is that owners' consumption increase faster than renters' consumption over most of the range of the data.

However, Malpezzi and Mayo's sample included both controlled and uncontrolled markets. While they tested for rent control's effects, no precise or robust effect was found in their sample.[6/] The cross country price term, which was (unsurprisingly) lower for the controlled markets, seemed to be picking up most of rent control's measured effects. But the sample was too

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5/ Note that in a log-linear expenditure equation the coefficient of price is equal to one plus the price elasticity; thus the price elasticity is the estimated coefficient minus one, or -0.85 and -0.35 for owners.

6/ Malpezzi and Mayo did not report these results in any of their published papers, but details are available from the first author.

small to be particularly confident about this result, and it is singularly inappropriate as a maintained hypothesis for cross country estimates of demand used to evaluate costs and benefits of controls. Therefore we present new estimates here using only uncontrolled markets.

Data from 15 cities in 7 countries are added to Malpezzi and Mayo's original data. The new data are presented in Table 6. All are from secondary sources. There is a gain in degrees of freedom, but there is a loss of data comparability and "quality control" on the estimates.

Data for the original model are, of course, in Malpezzi and Mayo (1985). Controlled markets are those in Egypt, Ghana, El Salvador, India and Jamaica. The Korean market was uncontrolled. Colombia had controls but because they were neither stringent nor widely enforced we have classified the Colombian markets as uncontrolled.[7/]

We make one additional change to the original Malpezzi and Mayo data. At the time of the Korean household survey (1979) the best estimate of the current real discount rate was 3 percent per month, which was quite high. This rate was used to convert chonsei deposits to rental equivalents for renters. Kim (1987) has since suggested that the appropriate long run rate to apply to chonsei from this period is significantly lower, on the order of 2 percent per month. All results reported here use the lower rate.

As noted in the earlier demand work, there are some difficulties with the construction of the price term used in these models. Recall that the price term was derived from rental price indexes from Kravis, Heston and Summers (1982). The value of the index is ceteris paribus lower for controlled markets, hence our finding in the original sample that rent control had no discernible effect in the cross country model (i.e. the effect was picked up in the price term). However here we explicitly want to estimate the effect of controls, and not have the effect be indistinguishable from price effects. One approach would be to construct a price index using instrumental variables and the uncontrolled Kravis-Heston-Summers sample, so as to predict prices in the absence of controls for the estimation of the cross country model. The instrumental variable approach has proven difficult to implement in practice, however. An alternative, simpler approach is to estimate straightforward expenditure (Engel) functions using separate controlled and uncontrolled samples. The price term is omitted. With the enlarged sample there are sufficient degrees of freedom to estimate separate models for controlled and uncontrolled units.

7/ See Edwards (n.d.). In future work we will make some comparisons based on a more detailed classification of type of regime and enforcement. Also, as we develop the cross country model further we intend to construct better price indexes, inter alia separating the effect of different kinds of rent control on the price indexes from other price determinants more carefully.

Table 7: PREDICTED RENT-TO INCOME RATIOS FOR CASE STUDY MARKETS

			Predictions Using Uncontrolled Sample					
			Expanded Sample			Original M&M Sample		
Market	Year	Monthly Income	Rent/ Income	Lower Bound	Upper Bound	Rent/ Income	Lower Bound	Upper Bound
Rio	1980	$500	.15	.09	.26	.16	.09	.29
Cairo	1981	104	.15	.09	.25	.09	.04	.20
Bangalore	1974	81	.15	.09	.25	.09	.04	.21
Kumasi	1980	79	.15	.09	.25	.08	.03	.21
Kumasi	1986		.15	.09	.25	.09	.04	.20
Harare	1982	150	.15	.09	.25	.12	.07	.21

			Predictions Using Controlled Sample					
			Expanded Sample			Original M&M Sample		
Market	Year	Monthly Income	Rent/ Income	Lower Bound	Upper Bound	Rent/ Income	Lower Bound	Upper Bound
Rio	1980	$500	.09	.02	.35	.22	.07	.63
Cairo	1981	104	.08	.02	.33	.07	.03	.17
Bangalore	1974	81	.08	.02	.33	.06	.02	.15
Kumasi	1980	79	.08	.02	.33	.06	.02	.14
Kumasi	1986		.08	.02	.33	.07	.03	.18
Harare	1982	150	.09	.02	.33	.11	.05	.27

Notes: (1) Income in 1981 U.S. dollars using local CPI and official exchange rates, except Ghana 1980 (used exchange rate of cedis 22=$1 because of extreme overvaluation in 1981).
(2) Bounds are plus and minus 1.6 standard errors of the predicted rents.
(3) All income numbers are monthly household income, based on original survey year.
(4) Box denotes preferred estimates of typical rent-to-income in absense of controls.

Reestimating the model above with the additional data yields the following estimates for uncontrolled markets:8/

Uncontrolled Renters

$$\ln R = \underset{(0.466)}{-2.040} + \underset{(0.087)}{1.027} \ln y$$

$R^2 = .88$
d.f. = 17

For controlled markets the model yields the following:

Controlled Renters

$$\ln R = \underset{(1.045)}{-2.689} + \underset{(0.187)}{1.042} \ln y$$

Adj: $R^2 = .72$
d.f. = 11

Table 7 presents the predictions from these regression models for the five case study markets. The table is divided horizontally between estimates from uncontrolled and controlled samples; these results are presented under the heading "expanded sample." Note the surprising result that using the expanded sample, the estimated cross-country income elasticity is approximately one with both the controlled and uncontrolled samples. There is a large difference in the intercept between the two samples, however. In other words, the expanded cross country model predicts that in all uncontrolled markets in the range of our sample, the typical rent-to-income ratio is roughly fixed at .15; in controlled markets, it is fixed at about .08-.09.

Note also that while the point estimates are fixed (because of the unitary elasticity estimates) the interval estimates are quite wide: typically ranging between .09 and .25 for the uncontrolled estimates, and even wider -- from .02 to .35 -- for the predictions from the controlled sample. It appears, not that we have conclusively shown the income elasticity to be one, but rather that the data do not permit us to estimate a precise cross country elasticity using the expanded dataset. Recall that the price of expanding the dataset was to give up quality control over the data used.

8/ Several alternatives models were examined, including a quadratic model, and models incorporating lagged changes in real income, lagged changes in prices, and climate. None was judged superior to the simple model. Extensive regression diagnostics revealed the results were robust (except when the sample was restricted to original data from Malpezzi and Mayo, as discussed below).

Because of this, and because of the strong prior we have that the cross country income elasticity does exceed one[9/], we then reestimated the cross country model using the new imputation for Korea but using the the original M&M sample. Reestimating the model above with the original data yields the following estimates for uncontrolled markets:

Uncontrolled Renters

$$\ln R = -4.017 + 1.355 \ln y$$
$$\qquad\quad (1.733)\ (0.299)$$

$R^2 = .71$
d.f. = 7

For controlled markets and the original data the model yields the following:

Controlled Renters

$$\ln R = -5.934 + 1.709 \ln y$$
$$\qquad\quad (1.412)\ (0.286)$$

$R^2 = .85$
d.f. = 5

As expected, the point estimate of the elasticity from this uncontrolled sample is greater than one, although the limited degrees of freedom reduces the precision of the estimates from M&M's original model. Table 7 presents predicted rent-to-income ratios using these results as well. Because of our strong prior about a long run elasticity greater than one, we recommend using these latter results to predict rents in the absence of control.

For completeness we have also included predictions using controlled units from the original sample. Note in passing that even with this small sample rent-to-income predictions using the model of controlled markets run 10 to 30 percent less than those from the uncontrolled markets. Rio is an exception; because of its high income compared to the estimation sample, and the surprisingly high elasticity (1.7), the prediction is very imprecise.

Of course these estimates from the uncontrolled Malpezzi and Mayo sample in Table 7 are estimates of the long run equilibrium average rent-to-

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9/ Not only because of the previous Malpezzi and Mayo work; related work by Burns and Grebler (1976) and Renaud (1980) also indicates an elastic demand across developing countries, although demand becomes less elastic across developed countries. See Ch. 3 of Malpezzi and Mayo for a review and interpretation of this and related literature.

income ratio. In order to predict the uncontrolled rent[10/] of particular households in a controlled sample--or of representative households at an income level above or below the average--it is necessary to combine an estimate of the average rent-to-income ratio from Table 7, derived from the cross market model, with an assumed within-market income elasticity. Malpezzi and Mayo found most within-market elasticities for renters ranged between .4 and .6. If anything these estimates may have a slight downward bias, so we suggest using an estimate of .6.[11/]

There is no question that these estimates could be improved with a still better cross country model. While these estimates are reasonable and we are confident of their utility for the rent control study, further improving the precision of these estimates can have a high payoff.[12/]

F. Empirical Implementation of the Present Value Model

The sections in Chapter 3 that described supply side modeling was in some ways less specific than the sections describing the demand side models. This is because data required to estimate various supply responses are not all available in each case study market, or in some markets are judged unreliable. We did note that data from the household surveys can yield information about supply, e.g. the current price reduction and subsequent transfer from existing landlords. Starts can be roughly estimated using the age profile from a cross section, and hedonic indexes can estimate the realized rate of depreciation of the rental stock. Table 8 describes other sources of supply side data and their uses. A preliminary list of which data are available in each market is presented in Table 9. In general, Brazil has the best data for estimating such models. But as noted above, much of our supply side work will rely on the present value model, which allows parametric sensitivity analysis of supply side responses.

Present value analysis can be used to examine the economic costs and benefits of disparate government interventions and/or market imperfections. A cash flow model for a representative investment or investments will be constructed, and the present value of each intervention or imperfection (e.g. land subsidies, finance subsidies taxes, regulatory costs, as well as rent control and other rental regulation) is calculated. Present values have the advantage of enabling direct comparisons of the costs and benefits of quite different interventions. Rates of return can be used as inputs to an

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10/ $P_m Q_m$ in the notation of the household model.

11/ Analysis of the individual within-city elasticities from Malpezzi and Mayo was unable to discern any relationship between the elasticities and income, or between the elasticities and the presence of controls.

12/ Particularly since there are many other uses of these estimates, such as evaluating shelter projects and other government housing policies. See Mayo and Gross (1986) and The Urban Edge.

Table 8
Supply Side Data and Methods of Construction

Variable	Measures	Sources/Method of Construction/Comments
dQ	Housing Starts Housing Investment (aggregate, and by tenure)	Starts, construction permits are often available by local govt. jurisdiction. Investment data are ususlly more aggregated. Data are not always broken out by rental/owner-occupied; may have to impute, using independent data on tenure. Note that there are really two questions: effects of RC on rental stock, and effects on total stock. Each can be estimated separately.
Q	Housing Stock (measured in units or real financial flows; aggregate, and by tenure)	If available, only for benchmark years (e.g. census). If no benchmark, can estimate using population data, occupancy measures, typical market values at period. Units of measurment should be consistent with flow measures of dQ (i.e. money or physical units?) Must also make assumptions about depreciation, demolitions. See Grebler, Blank and Winnick for more detailed treatment.
P	Relative Price of Housing	Can be constructed from CPI data. Is housing CPI based on market rents, controlled rents, or asset prices? Should purge general CPI of housing component (need details of weights).
Y	Income	Preferably by tenure as well as aggregate.
Z	Demographic Variables	Population most important in aggregate model. Household size, formation.
w	Relative Price of Operating Inputs	Wages, utilities (if included in rents, or a significant component of operating costs), other inputs.
r	Relative Price of Capital Inputs	Dominated by interest rates, skilled and unskilled wages. In Ghana, must account for foreign exchange component.
Other 1/	Tenure Vacancy rates Demolitions/Removals from Stock Avererage Rents and Values	Tenure Conversions Percent Formal/Informal Costs of Administration Evictions Costs of Administration Court Backlogs

1/ Data which will prove useful for constructing variables, use in switching regression models, etc.

Table 9: SUPPLY SIDE DATA AVAILABILITY 1/

Brazil	In general, best set of data for relating changes in supply to changes in controls. Time series data on starts, input and output prices. Stock data can be constructed.
Egypt	Some data on maintenance and repair expenditures by owners and tenants. May also estimate net depreciation using hedonic indexes.
Ghana	Have data on stock at two points in time. Can be used to investigate effects of strict regime on stock.
India	Some data exist but market and tenure specific model may not be estimable. Will use profitability model, household survey data.
Zimbabwe	Only descriptive tables; modeling not attempted in this Phase of project. Can apply profitability model.
United States	Malpezzi et al. data on market and tenure specific net depreciation rates can be used to study effects of controls. Property Income and Expense data may also be studied.

additional module (to be built) which imbeds behavioral assumptions about landlords and developers to study potential supply side responses to changes in controls.

Malpezzi (1988) describes such a model in more detail. The model is conceptually quite simple. First, analyze the notional cash flows from the investment in efficiency (market) prices. The present value of the costs (at market prices) is the real cost of the investment, and the present value of the benefits (again, at market prices) is the market value of the unit. If the market value exceeds the cost, the unit should be built (according to the criterion of efficiency).

After the "economic" cost-benefit analysis at market prices, each policy intervention is analyzed in turn by examining how the interventions change the prices and corresponding present values. Some interventions confer benefits on the developer/landlords, e.g., land subsidies and financial subsidies. Other interventions exact costs: rent controls, tenure security regulations, and other interventions such as taxes. Table 10 summarizes the data required for such a model.

1/ Note that all case study markets have household survey data which can be used to estimate static costs to landlords. This list is of collateral data which permit estimating dynamic supply effects.

Table 10: ADDITIONAL DATA FOR THE PRESENT VALUE MODEL

In order to analyze the profitability of rental housing, and compare the costs of rent control to other interventions and imperfections we would like to have the following data. Much of it is qualitative, and obtainable from semi-structured interviews landlords, builders, officials, etc.

Costs

Price per unit of land, and typical amount of land used for a rental dwelling. What is current price of land in center of city, and in the periphery? Is price significantly distorted by unusual regulations (e.g. India's Urban Land Ceiling Act)? If so, what would it be in the absence of ULCA (informed guess from real estate professionals? results from simple model of land pricing?) How much land is traded compared to how much would be traded if Act was repealed? (guess or qualitative statement; percentage?)

Similarly, construction costs per square meter, and typical size.

Value of existing units (can be derived from household survey data) Are asset values different for controlled/uncontrolled?

Estimate of what percentage of value would be spent on maintenance in a year (1) with rent control (zero?) and without rent control? Differentiate by type of control (strict or alloted, "normal," uncontrolled).

Financing

How much housing is formally financed (banks, building societies, credit unions, government, cooperatives)? Can be rough estimates. What are terms? (interest rate, length of loan, loan-to-value ratio)

If sales/new units are not formally financed, where to builders and investors get the money? What are common terms?

Taxes

Briefly describe major taxes affecting real estate: acquisition tax, transfer tax, capital gains tax, annual property tax. What are tax rates?

What other taxes apply especially to rental housing? For example, is rental income subject to income tax? What are the rates? Do people actually pay?

Are there any tax breaks (depreciation allowance, investment tax credit, deductibility of interest cost, etc.) that lower the cost of housing investment?

Is housing favored or discriminated against compared to plant and equipment?

The major task ahead is to build in assumptions about supply side behavior. Because independent estimates of (for example) the effects of a change in profitability on starts will not be available for each market, particular attention will be paid to sensitivity analysis.

G. Statistical Issues: Diagnostics and Robust Estimation

This section introduces some purely statistical issues applicable to both household and aggregate data. Specifically, we describe two types of estimators in addition to familiar ordinary least squares (OLS). The first is weighted least squares, with observations whose residuals from OLS are outliers given a weight of zero, other observations weight one. This estimator is denoted OLS2. The second estimator is weighted least squares, where the weights are determined by the influence an observation has on the OLS estimate. This estimator is denoted BIF, for Bounded InFluence regression.

Outliers

Under the maintained hypothesis of correct specification and measurement, errors from a linear model are normally distributed, in the limit.[13/] Unfortunately economic models are not, in general correctly specified and data measured with error other than specified errors. While OLS has appealing properties under the maintained hypothesis, less is known about the properties of misspecified--real world--models.[14/]

Recently statisticians and some econometricians have addressed these issues, and developed so-called robust estimators. These are less efficient than OLS under the maintained hypothesis but are less sensitive to small perturbations in data if some data are measured with unspecified error.

A simple and appealing scheme is to estimate a linear model with OLS and examine the residuals from the regression sample. Since they are unbiased, consistent, and efficient estimates of the true errors under the maintained hypothesis, they should, given a sufficient number, approximate a normal distribution. In particular, if some residuals are much larger in absolute value than expected, these are termed outliers.[15/] Figure 6 illustrates such an observation.

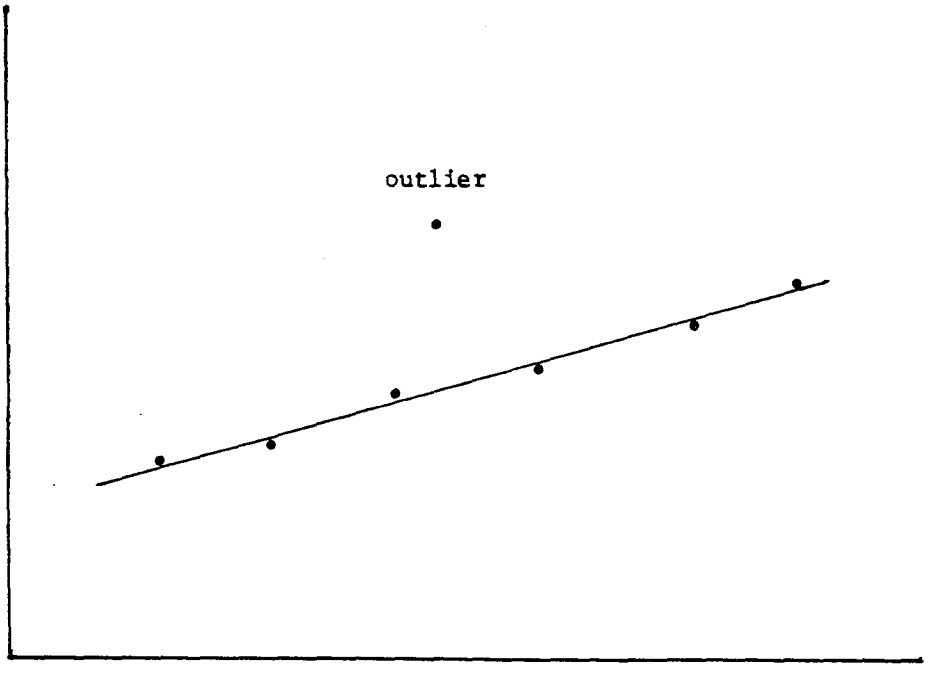
13/ Intriligator (1978).

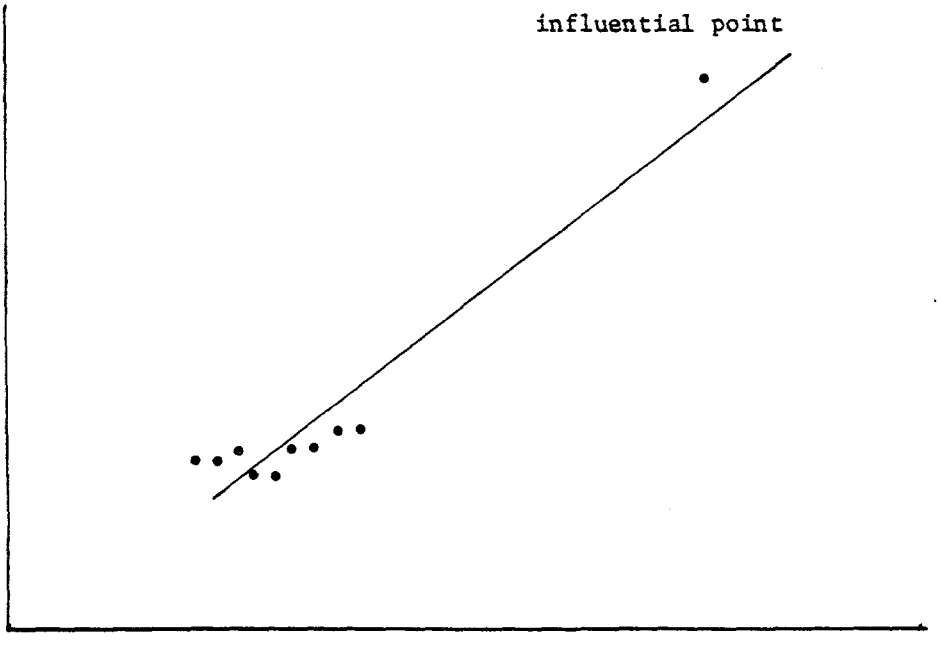
14/ For examples of recent econometric literature on model specification, see Hausman (1978), Leamer (1978) and White (1982).

15/ The shape of the distribution can also be examined in more detail; this was done but is not reported. Results available upon request. In general, residuals from these models were all reasonably bell-shaped; they were truncated (as is common with economic data) and a bit long in one tail, as described below.

Figure 6

Outliers and Influential Points





Order statistics are used to determine outliers, because parametric estimates of location and variance are themselves greatly affected by outliers. An outlier is defined as follows.[16] Compute the quartiles of OLS residuals. Establish an upper and lower cutoff, or fence, 1.5 interquartile ranges[17] above the third quartile and below the first quartile. Whatever the distribution, fewer than 1 in 200 observations would be expected to fall outside these fences.

The weighting rule is simple. If a residual falls outside the fences, that observation is assigned weight zero (is dropped) for the robust estimator OLS2.

Influence

Suppose an observation is so far out of line that it pulls the regression line almost through it. Such an influential observation will not show up as an outlier, but dropping one observation can radically change the estimates. Figure 6 illustrates the difference between outliers and influential data points. The figure highlights two polar cases; with real world data there is often much overlap between the two.

Not every sample observation has equal influence, and to delete all influential points (however influence is measured) would result in an estimator so inefficient as to be useless. However, Welsch (1981) has examined the expected influence of a set of sample observations, and rived a conservative procedure which downweights extremely influential observations but which has 95 percent of the efficiency of OLS under the maintained hypothesis.

A simple summary measure of influence can be derived as follows. Drop each observation and re-estimate the equation, one by one for the entire sample. How much each observation influences the result can be conveniently summarized by the change in the predicted dependent variable when it is predicted from the full sample estimate versus prediction from the sample minus that observation. Appropriately standardized, the measure is known as DFFITS.[18]

Welsch suggests the following weighting rule. If N is the number of observations and P the number of regressions, compute the fences $\pm 2\ (P/N)^{\frac{1}{2}}$. Outside these fences, weight the observation by the inverse of the absolute value of DFFITS. Otherwise, an observation has weight one.

16/ Following Tukey (1977).

17/ The interquartile range (IQR) is the difference between the first quartile (Q1) and the third quartile (Q3).

18/ See Belsley, Kuh and Welsch (1980), Chapter 2. They also show how to compute DFFITS for a sample of N observations without estimating N + 1 regression equations, but it is still computationally burdensome. It can be computed automatically in some regression routines in TROLL and SAS.

An Example

Malpezzi (1986) used these estimators to evaluate the robustness of the hedonic and demand models for his study of Cairo rent control.[19]

He found that the hedonic results were extremely robust. There were no outliers according to the definition above, and the largest coefficient change from OLS to BIF was only half an OLS standard error. Other tests of residuals and other diagnostics confirmed that the simple hedonic models used were well specified.

The demand equation were less robust, though not so much as to cause grave concern. Qualitatively, results were similar; signs and general magnitudes do not change, but the program detected two outliers in the demand equation. The coefficient of income, and the intercept changed by more than a standard error between OLS and OLS2. The differences between OLS and BIF results were smaller, at most two-thirds of a standard error.

It appeared that the demand equation differences were noticeable and statistically discernable but not cause for alarm; qualitative results were robust. Still, given the nonlinear nature of the benefit estimation, a conservative procedure is to compare separate benefit estimates with the full sample results and those without outliers. Given time, further study of robustness could prove fruitful.

19/ Computational details of these and other tests (not reported) are in Malpezzi (1984).

IV. SYNTHESIS OF CASE STUDY RESULTS

A. Links Between Case Studies and Synthesis Paper

The links between the case studies and the synthesis paper can be made clearer by referring to the appended draft outline. The general framework for the synthesis is contained in Malpezzi and Rydell (1986). Among many papers which discuss decontrol options we particularly recommend Arnott (1981), Rydell et al (1981), Olsen (1982), and Muller (1987).

Partial results from the case studies will be use to calibrate a simple general equilibrium, dynamic model of a housing market. The model will be very simple compared to, say, the large computable general equilibrium models familiar to macro economists, and to devotees of models such as the Becker-Mills-Williamson or Kelley-Williamson models (Becker et al. 1984; Kelley and Williamson, 1984). It will be more along the lines of de Leeuw and Ekanem (1973). The synthesis paper will be informed by this model, particularly regarding decontrol options. However, it is important to realize the paper will not be about the model, but about the policy issues. It will be non-technical.

The expected length of the paper is 50-60 pages. The technical level will be similar to "Shelter Strategies for the Urban Poor" (i.e. models should be simple, expounded with geometry; technical issues will be handled largely by reference). The main audience within the Bank are country and sector economists, and their management, in PPR and the regions. Others should be made aware of the main points from a summary of 8-10 pages. Outside the Bank, the intended audience wide, i.e. similar to a policy paper but the synthesis paper is not a policy paper in the formal sense.

OUTLINE FOR SYNTHESIS PAPER

I. BRIEF DESCRIPTION OF DEVELOPING COUNTRY HOUSING MARKETS, LDC RENTAL HOUSING MARKETS, AND THEIR PLACE IN THE ECONOMY

Motivate the paper. Present some stylized facts on housing markets, including the role of housing in the economy, of rental housing, and of the potential importance of rent control.

Present a progression of simple models of housing market behavior: first simple comparative static model, then a dynamic model with costs of adjustment. Discuss equity as well as efficiency in the context of these models. Describe how in principle they can be used to analyze effects of rent controls of different types.

Describe some of the major conclusions of empirical work on LDC housing markets generally. Refer reader to more detailed treatments to save space.

II. WHAT IS THE EXTENT AND NATURE OF RENT CONTROLS?

How widespread is rent control? What are the major types of controls, and how are they enforced? What related regulations exist?

Taxonomy:

Coverage

- residential rental, commercial
- discrimination by vintage
- treatment of new construction
- vacancy turnover
- exemptions by quality class, furnished/unfurnished, etc.

Alternative Adjustment Mechanisms

- accelerated depreciation, demolition
- key money
- foregone starts and conversions
- tenure switches
- crowding, doubling up
- tenant upgrading, maintenance and repair

Interaction with Market Conditions

- tight versus loose markets
- background inflation
- effects on "uncontrolled" submarkets

III. HOW HAVE RENT CONTROL SYSTEMS COME ABOUT?

Understanding this will yield insights into decontrol options.

In developed countries, response to wartime shortages; responses to periods of prolonged inflation; other market disruptions. In developing countries, add rapid growth, problems in input markets.

What are the stated policy objectives? Hidden agendas, if any?

Responses in democracies, socialist systems, other. Describe interest group interaction in simple terms. Size of groups versus pressure exerted. Models of voter choice? "New" models of public choice? How to apply to a range of political systems?

Some writers trace the political economy of rent control to "effective" tenant organization; conversely believe defeat of controls, decontrol are linked to strong landlord organization. Oversimple view -- why?

Rent control as a transfer of property rights -- a theme to carry throughout the paper.

Who is perceived to win and lose from rent control? Who actually wins and who loses? Illustrate how broad policy adapted to address one of many shelter problems of a (usually) narrow group screws up a large segment of the market. Better targeted policies are/should be available.

IV. WHAT ARE THE EFFECTS OF DIFFERENT SYSTEMS?

Standard Cost Benefit: common efficiency and equity arguments, and what the evidence actually shows.

Rent control as a tax on housing capital which violates the benefit principle and the ability to pay principle. Incidence: first order, and general equilibrium (modified for LDCs). Distributional implications.

Supply Side: responses include conversion, accelerated depreciation and demolition/abandonment, foregone new construction and conversion to owner occupancy, ofrgone rental conversions. Relative roles of each. What are the dynamics? How rapid is the response?

Property Taxes: effects in capital value systems, rental value systems.

Size of Distortion/Ranking, (1) in terms of housing market problems, (2) in terms of macroeconomy. What concomitant policy changes are required to make decontrol work? When/how does control/decontrol affect other policy changes (e.g. financial, land). As part of Structural Adjustment. Timing.

The need for relative prices to change (unbalanced growth strategy

is the most explicit model, but explain why prices must change in other frameworks as well).

The key for understanding political economy: what is the net effect of rent control on each agent (landlord, tenant, owner-occupants, government?) How do they perceive these effects?

Role of expectations.

V. WHAT CHANGES ARE DESIRABLE AND/OR FEASIBLE, AND HOW CAN WE BRING THEM ABOUT?

Criteria:

Static Efficiency (the Hicks-Kaldor criterion)
Minimize Costs of Adjustment (Dynamic Efficiency)
Equity (Can/Should/Do we carry out the Hicks-Kaldor compensation?
Political Feasibility

Decontrol/Relaxation Options:

Blanket Decontrol
Phasing out
Indexation
Exempting New Construction
Revaluing for new tenants

Target controls by income
Revalue for Upgraded Units
Permit Landlords to Buy Out Tenants
Vacancy Rate Decontrol
Decontrol by Market Segment

Collateral actions required for housing market adjustment:

Land markets
Finance
Infrastructure
Other housing market problems which must be attacked
still other problems (non-housing)

Issues in the Political Economy of Rent Control: How can we bring these changes about?

What are the objectives of housing policy and specifically of rental policy? Whose objectives are these?

Implications of the "positive" results
First Best Solutions

-- according to comparative statics model
-- according to dynamic model with costs of adjustment
-- with explicit equity objectives

Second Best Solutions: How Close to First Best?

Policy options to make changes palatable to interest groups

stabilize prices at macro level
subsidize tenants
subsidize/grant tax relief to landlords
compensate landlords for loss of property rights

Are there conditions under which controls should be retained? If so, of what type?

Truly temporary (Alaska, Alberta examples)
Natural Disaster
Severe supply constraints (specific guidelines)
Pass through public subsidy (Korea)

What has actual experience been in places which have decontrolled or relaxed controls?

Brazil
Other LAC countries
Portugal (?)
Europe, U.S.

V. SUMMARY AND CONCLUSIONS

Main messages of the paper

Cost-Benefit
Distributional
Dynamic (decontrol)
What is to be done?

DETAILS OF CURRENT CASE STUDIES

A. Brazil

Principal Authors/Consultants

Ricardo Silveira, (World Bank) with Stephen Malpezzi

Will carry out cost-benefit estimation,supply modeling.

Dr. Eduardo Neto
Rua Equador 140, Apartment 304
Belo Horizonte, Brazil
Telephone (031) 225-9104 (home); 201-3253 (office)

Will provide data, background paper.

Synopsis of Rent Control Regime

Brazil has a long history of controls with enormous variation in type of regime. The first rent control law was enacted in 1917, and since then the rental housing market has experienced major shifts in rent control legislation.

The current regime is less stringent than other case study regimes. For example, new rental contracts are negotiated at market prices and rents on old contracts are allowed to be adjusted, yearly, to reflect most of the previous year inflation. The law contains provisions for periodical judicial reviews of old rental agreements.

Notes on Data

Household survey data from 1980 Census. Extract being prepared for Rio de Janiero. Best set of time series data for analysis of effects of changes in regime on housing supply.

Cost-Benefit Notes

Proposed Analysis of Supply Side. Focus will be on time series models relating housing investment to changes in regime over time. May also have some information on maintnenace and repair expenditures by landlords.

Current Status

This is, chronologically, the last case study underway. It was identified after the project had already begun. Data collection and background paper have been prepared by consultant. Draft paper on housing institutions and rent control legislation has been prepared.

Papers Produced

R. Silveria, Review of Brazilian Housing Institutions and Rent Legislation.

B. Egypt Case Study

Principal Authors/Consultants

Stephen Malpezzi and William Stephens
World Bank

Extend previous cost benefit analysis, edit into cogent case study paper.

Synopsis of Rent Control Regime

Rents frozen for past 30 years. Rents for new units set as percentage of appraised capital cost, then frozen. Strong tenancy protection. Well developed key money system.

Notes on Data

Excellent household survey data from Cairo (1981) including data on key money. Collateral data by geographic location (enumeration district). Little time series data.

Cost-Benefit Notes

Best opportunity to study role of key money and other side payments.

Proposed Analysis of Supply Side

Focus on existing stock: comparison of landlord, owner, and tenant maintenance.

Current Status

Cost-benefit model estimated, but paper is not readily accessible to non-specialists. After update on rent control system from Abt Associates, Malpezzi will draft appropriate case study paper.

Papers Produced

Stephen K. Mayo and others, Informal Housing in Egypt, Abt Associates, 1981. Monograph describing Cairo's housing market in some detail. Also basic documentation for data used in this project.

Stephen Malpezzi, Rent Control and Housing Market Equilibrium: Theory and Evidence from Cairo, Egypt, Ph.D. dissertation, the George Washington University, 1986. Cost-benefit analysis of Cairo rent control, analysis of mobility and upgrading, landlord investment model.

C. Ghana Case Study

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Synopsis of Rent Control Regime

Extremely strict regime. Rents frozen with only occasional and minor increases, in highly inflationary environment. Payment of advances is a relatively recent phenomenon.

Notes on Data

New household survey data complements existing survey from 1981. Only panel data available for intertemporal comparisons of changes in distortions over time.

Cost-Benefit Notes

Could focus on changes in occupancy (crowding) as a response to controls. Panel data permit detailed analysis of changes in hosuing stock, but only for two points in time. Changes will be described, but not all change can be attributed to controls.

Current Status

Drafts complete. Currently undergoing internal review and revision.

Papers Produced

A. Graham Tipple, The History and Practice of Rent Controls in Kumasi, Ghana. Discussion Paper (INURD). Describes Kumasi's housing market and rent control regime.

A. Graham Tipple and Keneth G. Willis. Costs and Benefits of Rent Control in Kumasi, Ghana. Discussion Paper (draft). Empirical estimates of costs and benefits.

D. India Case Study

Principal Authors/Consultants

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Stephen Malpezzi (WB)

Synopsis of Rent Control Regime

Rents frozen for existing units but five year holiday for newly built units. Some units have rents set by municipal rent control officer; some units even have tenants allocated by rent controller.

Notes on Data

Primary data is 1973 household survey in Bangalore City. Collateral data collection interviews were carried out by IIM in 1984.

Cost-Benefit Notes

Interesting analysis of redistributive effects. Proposed analysis of supply side will rely on present value model.

Current Status

Descriptive paper complete. Cost-benefit paper in draft.

Papers Produced

Vinod K. Tewari and T. Krishna Kumar, Rent Control in India: Its Economic Effects and Implementation in Bangalore. Water Supply and Urban Development Discussion Paper No. 91, 1986. Descriptive paper and literature review.

Stephen Malpezzi and Vinod K. Tewari, Costs and Benefits of Rent Control in Bangalore, India. Draft cost-benefit analysis.

Government of India, Ministry of Urban Development, National Commission on Urbanization Interim Report. Sections describing rent control and proposing policy reforms written by Vinod K. Tewari.

E. Zimbabwe Case Study

Principal Authors/Consultants

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Synopsis of Rent Control Regime

To be written.

Notes on Data

Some household cross section data, but will not support estimation of full cost-benefit model.

Cost-Benefit Notes

Very simple analysis of rents; econometric modelling not reliable with present data. Analysis of supply side will be limited to mainly descriptive analysis of changes in housing stock.

Current Status

Descriptive paper to be drafted this summer. Further work would require additional data collection and funding.

Papers Produced

Marja Hoek-Smit, Housing Preferences and Potential Demand of Low Income Urban Households in Zimbabwe. Report prepared for USAID which describe's Harare's housing market.

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