A review of empirical evidence on the costs and benefits of rent control

Bengt Turner and Stephen Malpezzi*

Summary

■ We survey selected literature on costs and benefits of rent controls, with a particular emphasis on empirical literature. Much of the empirical literature focuses on measuring welfare changes to individual households from regulatory regimes. The majority of such studies are based on North American data; some, but surprisingly few, studies have been carried out in Europe. Empirical innovations include treatment of selection bias, estimation of Hicksian rather than Marshallian welfare measures, and analysis of the effects of controls on related uncontrolled markets. Other empirical studies examine effects of controls on mobility; few studies address potentially important supply effects directly. Cost-benefit results, notably transfer efficiency, vary somewhat with empirical technique but even more so by location, reflecting differences in market conditions and type of rent control regime. Recent theoretical models of the effects of rent control based on imperfect information have yet to be thoroughly tested.■

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Bengt Turner and Stephen Malpezzi*

Perhaps 40-50 percent of the world's urban population lives in rental housing of one kind or another. Most of these households live in units subject to controls on rent paid, often with additional related regulations. Until a decade or two ago, economists as a group had no trouble reaching a consensus on the qualitative effects of rent control on housing markets.

> The theoretical analysis of rent control rests on some principles which are quite elementary, indeed distressingly so. They are so obvious that one would feel the greatest reluctance to repeat them in a professional journal were it not that a great public policy has been erected upon either ignorance or a repudiation of them.[1]

This previous consensus was founded on the analysis of rent control as a simple effective price control or tariff. However, a more recent review by Arnott (1995) puts things rather differently:

> In recent years, however, there has been a wave (or at least a swirl) of revisionism among housing economists on the subject of rent control. While few actually advocate controls, most are considerably more muted and qualified in their opposition. Perhaps a majority, at least among the younger generation, would agree with the statement that a well-designed rent control program can be beneficial.

What is the nature of the "revisionism" of which Arnott speaks, and is it (more specifically, under what conditions is it) well founded? From the analytic perspective, the economic analysis of rent control, and related regulations, has advanced significantly since the simple (though sometimes fruitful) textbook partial equilibrium, static analy-

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[1] Grampp (1950), cited in Block and Olsen (1981). One oft-cited study of the views of professional economists revealed that only two percent of those surveyed disagreed with the proposition that "a ceiling on rents reduces the quantity and quality of housing available" (Kearl et al., 1979).

sis of rent control as a tax on rental housing. There has in fact been an interesting interplay over time between (1) observations by policy-oriented economists of important differences between the simple assumptions of the early models, and the way regulations are actually designed and implemented; and (2) the development of new theoretical and empirical models that are crafted to take account of the most important real-world features of regulatory systems and housing markets.[2]

There are many different kinds of rent control regimes. For example, one key feature is whether controlled rents are adjusted for changes in costs (with cost pass-through provisions or adjustments for inflation); how close the adjustment is to changes in market conditions; how it is applied to different classes of units; or whether rents are effectively frozen over time. Other key provisions which vary from place to place include breadth of coverage, how initial rent levels are set, treatment of new construction, whether rents are reset for new tenants, and tenure security provisions. Rent control's effects can vary markedly depending on these specifics, and on market conditions, as well as enforcement practices.

Our review is selective; we have an associated bibliography comprising over 500 rent control studies, theoretical and empirical, in addition to a considerable number of related papers in housing and microeconomics.[3] Studies such as Arnott (1981), Anas (1997), and Olsen (1988) have provided a range of new theoretical models that can be applied to control regimes of various types. We include this literature, but very selectively and briefly; Arnott (1995, 2002) provides more detailed discussion. While we will discuss some theory *inter alia*, our intention in this paper is to focus more on empirical papers—on the world "as it is" rather than "as it could be." What are the most common rent control regimes, and how well does the expanded theoretical toolkit provided by Arnott and others match up with the kinds of systems we observe in practice?

The plan of this paper is as follows. First, in Section 1 we present some preliminary information on the nature of rent control regimes in

[2] For example, Arnott (1988, 1995), Lind (2001), Malpezzi (1993), Olsen (1969, 1988), Rydell et al. (1981) and Turner (1988), among others. In addition to other articles in this issue, see also two earlier special journal issues devoted to rent control, *Journal of Real Estate Finance and Economics* 1(3), 1988; and *Regional Science and Urban Economics*, 28(6), 1998.

[3] The longer bibliography is available upon request.

selected countries, and in Section 2 some background information (and unresolved issues) about the nature of housing markets. Next, we survey some empirical research on controls, especially recent papers that address specific features of regimes. We divide this into two main sections, one focusing on costs and benefits to individual tenants and landlords (Section 3); and one focusing on market wide effects (Section 4). Section 5 concludes.

1. Rent control regimes

1.1. How do governments regulate rental housing?

Government intervention in the housing market is broad and deep; all the major types of government interventions can be found at play: the definition and enforcement of property rights and contracts; taxation; subsidy; direct public provision; and of course, regulation. A growing literature examines the effects of a wide range of regulations, including land use, finance etc. Representative studies of such regulations pertaining to individual countries include Fischel (1990), Barlow (1993), Malpezzi (1996), Monk and Whitehead (1999), and works cited therein. Comparative studies have also been undertaken (Angel, 2000; Angel and Mayo, 1996; Evans, 1999; Malpezzi, 1999b, and so on). These issues are also addressed within broader reviews of housing markets, e.g. Hårsman and Quigley (1991).

Housing markets are governed not only by rent controls, but also by planning processes, and zoning regulations; restrictions on conversion of land from rural to urban uses; other land use regulation such as those governing road widths, set backs, and floor area ratios; building codes; impact fees; financial regulations; and numerous regulations affecting the provision of infrastructure and the transport network necessary for real estate development. If there is a consistent finding from studies of these disparate regulations, it is this: regulation per se is neither good nor bad. What matters are the costs and benefits of specific regulations under specific market conditions.

1.2. How rent controls vary

A variety of mechanisms are available to governments attempting to place controls on the rental market. The strength of these mechanisms vary in a spectrum between the complete control of prices in

the rental housing market formerly seen in some socialist economies,[4] to government sponsored landlord/tenant arbitration boards which merely facilitate the price negotiation. It is possible to rank mechanisms according to the degree to which prices are controlled. The specific mechanisms may also be classified according to their type and effect. Actual rent control regimes usually combine several mechanisms.

In its simplest form, rent control can take two approaches. The first is the actual "control of rents"—that is, the fixing of a "fair rent" for every unit and the establishment of enforcement mechanisms to ensure that these rents are in fact charged. Such a regime would fix the rent according to some rule and may or may not allow for future changes. The second form of control is the "control of rent increases"; no effort is made to change current rents, but future increases are regulated. Our survey suggests that it is more common for nations to regulate rent increases than rents themselves; about twice as many countries adopt the former approach, although as will be seen below their are significant differences between the behavior of very low income countries and others.

The fixing of rent levels

How do countries which do so fix rent levels? Many nations rely on a central authority, a "rent controller." Nearly all rent control regimes establish some central organization charged with administering and enforcing the regulation, but the "fair rent"[5] concept empowers this authority to, theoretically, determine the actual rent which should be charged for every unit. For example, Great Britain, India and Pakistan rely heavily on such an authority, giving it the power to authorize rents for individual units on a case-by-case basis.

[4] In the extreme, for part of the 1980s, Burkina Faso outlawed the payment of rent for housing. Estonia may represent a former planned economy, where rents in private rented sector was kept at EEK 4 per sq. m. and month and in municipal sector EEK 2.5 per sq. meter and month in 1994, (EEK 1000 corresponded to USD 80)—see Jaffee, Turner and Victorin (1995).

[5] While usage varies, many countries which set rent levels refer to them as "fair rents", and since the underlying rational for setting *levels* is usually an appeal to equity, we use the terms somewhat interchangeably.

The regulation of rent increases

Another approach to rent control is to explicitly contemplate future increases in the legislation but to put limits on the extent to which rents can be increased. Unlike the fair rent approach, this format does not attempt to say what the rent should be, but only to limit how much it can increase in a given time period. The simplest method for controlling rent increases is simply not to allow them, that is, to institute a rent freeze. In other countries rent increases are explicitly set. Some systems allow landlords to cover some or all cost increases. These may include tax increases, operating costs, or even increases in financial charges due to refinancing. A return on capital system may also allow landlords to increase rents if the system is tied to a benchmark interest rate. Others index rents to inflation or some cost index. Even in the most restrictive system landlords are usually allowed to amortize the costs of substantial improvements to the unit. Regimes that are characterized by more-or-less generous indexation and adjustment of some kind are often termed "second generation" rent controls, although Lind (2001) notes that even within this category, regimes are extremely heterogenous.

Enforcement mechanisms

None of the mechanisms for controlling either rents or rent increases can function effectively in the absence of an effective enforcement mechanism. Unfortunately, this is also the area which is most difficult to analyze. Information about the efficacy of enforcement systems tends to be uneven, at best. In some countries, regulations are widely flouted; for example, in cities as disparate as Cairo and New York, tenants commonly pay large deposits for strictly regulated apartments, even though such payments are illegal.

1.3. The "industrial organization" of the housing market

Rent control is usually thought of as a policy applied to private markets, but publicly provided housing is also normally subject to rent controls,[6] where rents in most cases are set on a historic cost basis. As

[6] It is interesting to notice that the rent control literature is mostly concerned with private markets and not public markets, even though the degree of rent constraint ought to be the decisive factor. This may be an effect of a US lead research, where

with rent controlled private markets, the rent controls on public markets implies some of the attendant problems like reduced revenue and maintenance. For example, even after recent market-oriented reforms, much urban housing in China is owned by the state or state enterprises. Tolley (1991) found that as transition was underway, rents were typically 5 yuan per month or less (less than US USD 2). As a consequence, housing subsidies were about 25 percent of the state budget. Even after a decade of reform, most households rent state or enterprise-provided housing (Li, 2000), and rents remain low; for example, Wang (2000) reports unit rents of around 120-150 yuan per month, or roughly 4 percent of income (see also Chen, 1996).

In China, and in many formerly socialist countries of Central and Eastern Europe, and of the Former Soviet Union, many units were under-maintained because of lack of financing (Renaud, 1995a,b). Severely controlled prices can cause financial problems for public as well as private housing, and this is particularly problematic when such public units are a large fraction of the stock.

At the other extreme, in the US less than 2 percent of the housing stock is publicly owned, and perhaps 5 percent of households/housing units participate in a federal low income housing program.[7] Most other OECD countries are between these two extremes, although most South European countries, like Italy and Greece, rely mainly on the private market. A brief summary of current European systems can be found in de La Morvonnais and Chentouf (2000); discussion of recent changes, some in response to European integration, can be found in Maclennan, Muellbauer and Stephens (1998) and in a recent study by the European Central Bank (2003).[8]

2. Two competing views of the rental housing market

2.1. Are Housing Markets Competitive?

As we discuss below, and as emphasized by recent contributions by Arnott (1995, 2002) and others, in many respects an economist's view

the public sector is not seen as a key actor on the rental market, as typically is the case in many other countries around the world.

[7] In the US virtually all housing receives some subsidy; most subsidies accrue to upper-income households through the tax code and, to a lesser degree, the financial system.

[8] An overview from the report is reproduced in appendix.

of how to model controls will hinge on whether she sees the housing market as largely competitive; or, rather, as characterized by monopolistic competition or some other type of market power, or characterized by asymmetric information, or some other type of market failure. Competitive models of housing markets, including their application of such a model to rent controls, are well represented by Bentzel, Lindbeck and Ståhl (1963) or Olsen (1969b). Examples of models characterized by the existence of market power or an information asymmetry include Molho (1995) and Arnott and Igarashi (2000). The choice of model should be pragmatic: when simple competitive models are useful abstractions, we argue for their use; when market power or asymmetric information are central to understanding a policy issue, they should be brought to bear. It is in many contexts an empirical issue which model works best.

Thus it is somewhat surprising that, given the centrality of the issue, so few direct tests have been made of the competitiveness of the housing market. Cherry and Ford (1975) and Cronin (1983) have estimated pricing models that suggest large institutional landlords in the US charge higher rents than small-scale landlords. These have been interpreted by some as evidence of market power by large landlords; but on the other hand they could be explained by Anthony Downs' observation (1983) that small-scale landlords are "turnover-minimizers" as much as profit maximizers, because of the inherent difficulty in managing small numbers of units. Another direct test of the competitiveness of US housing markets (albeit the owner-occupied market) was carried out by Landis (1986), who found evidence suggestive that the regulatory environment in several California cities explained whether the market was competitive, contestable (in Baumol's, 1983, sense), or characterized by high barriers to entry.

Given the wide range in housing market institutions and practices across countries, this is surely a fruitful area for future research. Ball (2003) presents an interesting comparative review of the industrial organization of the construction market, primarily in Western Europe; he argues that the IO of the housing market does vary significantly from country to country, and that much of this can be laid to differences in government approaches, institutions, and regulations. Renaud (1995a,b) makes similar arguments when considering the case of formerly socialist economies of Eastern and Central Europe and the former Soviet Union.

In addition to studies that directly tackle the organization of the housing market, there is important indirect evidence from basic studies of supply. If the housing market is competitive, and if there are no rising supply prices of important inputs, then the price elasticity of supply of housing should be large, at least in the long run. Early studies of US markets, such as Muth (1960) and Follain (1979) suggested such a large elasticity. Later studies, such as Topel and Rosen (1988) and Poterba (1991) found lower elasticities, on the order of 2 to 3.[9] Malpezzi and Maclennan (2001) found a high long run elasticity for the US; found evidence that the different results from the other studies just cited might be partly due to different time spans of data; and suggested that the long run in housing markets might be well over a decade.[10] Following Whitehead (1974), Malpezzi and Maclennan also found significantly lower elasticities for the UK; this tended to confirm Malpezzi and Mayo's (1997) finding that, while basic demand parameters were very similar from one country to the next, supply parameters were highly variable and not readily transportable across borders. In fact, given differences in regulatory environments and natural constraint, these supply elasticities would surely vary across metropolitan areas within a country as well.

To summarize, it is fair to state that there is something of a disconnect between recent theory and recent empirical work on controls, and at least for now the disconnect is probably growing. Many of the interesting studies surveyed in Arnott's excellent (1995) review, and later papers such as Anas (1997), Arnott and Igarashi (2000), Basu and Emerson (2000), Epple (1998) and Skelley (1998) apply modern microeconomic theory and simulation, often assuming some sort of market power and assigning a central role to market failures, including information asymmetries. While it is unlikely that any market for any good ever has or ever will pass the strictest tests of textbook competitiveness, we believe that it is important that future research more fully

[9] As Topel and Rosen and particular make clear, these authors' work can support a range of elasticity estimates, depending partly on assumptions about demand parameters; Topel and Rosen, p. 735, have a particularly clear exposition of this fact. However our reading of these papers is that they propose supply elasticities in the 2-3 range as their preferred estimates.

[10] With a well-specified and parameterized dynamic model, it is possible to estimate supply elasticities for a longer run than used to estimate the relationships; when specification and parameterization are imperfect, estimates of long-run elasticities from short time spans are likely to be misleading. See also Bartlett (1989) for a detailed review of supply elasticity literature.

examine when and where data support a competitive market assumption as a useful approximation, and when the noncompetitive model will be more fruitful.

What, then, of the more direct empirical evidence on controls, which is the focus of the rest of the paper? Interestingly, much of the recent empirical studies are based on older, simpler models, of consumer surplus, of market effects of taxes and subsidies, and of household mobility. That does not mean there have not been advances. Many studies, while rooted in models that have been around for some time, have focused on improvements in econometric technique, for example. Also, as will become clear, the implications of the results of many of these empirical studies do not especially hinge on whether one views the housing market as approximately competitive, or characterized by significant market imperfections, although of course these studies will embed their own maintained assumptions.

2.2. Alternative ways to study the empirics of housing markets

Broadly, there are two approaches to empirical studies. The first is to undertake a case study within a single market. Data are drawn from a single metropolitan area or by another definition. Such a study might examine costs, and benefits to individual consumers and producers, as well as (less frequently) other effects, such as government units which face revenue or cost implications; or consumers in related markets facing spillovers.

The great advantage of the case study approach is that, if well done, the study can highlight the richness of individual markets and rent regulation regimes. In studying housing markets, details matter, and case studies generally do best at analyzing details. The great weakness of case studies is the obverse of its strength. Are the results from a single case study generalizable? This problem can be partially obviated by undertaking case studies of several markets chosen to represent a wide range of market conditions and regulatory provisions.

Table 1. Summary results from selected empirical studies of costs and benefits of rent controls

(1) Study	(2) Data	(3) Monthly Controlled Rent, PcQc	(4) Estimated Market Rent for Controlled Units, PmQc	(5) Estimated Household Expenditure without Controls, PmQm	(6) Cost of Subsidy, PmQc - PcQc	(7) Estimated Tenant Benefit	(8) Transfer Efficiency	(9) Annual Household Income	(10) Evidence on Distribution of Benefits	(11) Comments
Olsen (1972)	New York, 1968 (USD)	83	117	123	34	18	0.52	Mean income of controlled renters: $6,229; uncontrolled renters, $9,000	Tenant benefits decrease with income	
Gyourko and Linneman (1989)	New York, 1968 (USD)					203			Tenant benefits increase with income	Dollar amounts not comparable to Olsen (1972), apparently they were inflated to late 1980s dollars.
Ault and Saba (1990)	New York, 1965 and 1968 (USD 1987)				161 in 1965, 130 in 1968	115 in 1965, 65 in 1968 (Gross)	50-75%		Tenant benefits increase with income.	
Caudill (1993)	New York, 1968 (USD)	324	395-409							Data are *annual rent per room*. Uses frontier estimation, compare to PmQc of 608 from OLS.

Note: Column 6 may not equal difference between columns 4 and 5, when paper reports mean of differences (which can differ from difference of means).

Table 1. Continued….

(1) **Study**	(2) **Data**	(3) **Monthly Controlled Rent, PcQc**	(4) **Estimated Market Rent for Controlled Units, PmQc**	(5) **Estimated Household Expenditure without Controls, PmQm**	(6) Cost of Subsidy, PmQc –PcQc	(7) Estimated Tenant Benefit	(8) Transfer Efficiency	(9) Annual Household Income	(10) Evidence on Distribution of Benefits	(11) **Comments**
Glaeser and Luttmer (2003)	New York									$3/mo. Per apt. loss from misallocation of bedrooms Finds little DWL from increased maintenance problems; many other characteristics could be studied.
Nagy (1997)	New York, 1981 (USD)									Examines price difference between stabilized and controlled, but not full CB
Roistacher (1992)	New York, 1987 (USD)				45			Median income for households in controlled units: $10,817; all renters, $16,611	Subsidy highest in Manhattan, lower for higher income households.	

Note: Column 6 may not equal difference between columns 4 and 5, when paper reports mean of differences (which can differ from difference of means).

Table 1. Continued….

(1) **Study**	(2) **Data**	(3) **Monthly Controlled Rent, PcQc**	(4) **Estimated Market Rent for Controlled Units, PmQc**	(5) **Estimated Household Expenditure without Controls, PmQm**	(6) Cost of Subsidy, PmQc –PcQc	(7) Estimated Tenant Benefit	(8) Transfer Efficiency	(9) Annual Household Income	(10) Evidence on Distribution of Benefits	(11) **Comments**
Mildner (1992)	New York, 1987 (USD)				188 (controlled), 24 (stabilized)	122 (controlled), -14 (stabilized)	.65, controlled; NA, stabilized.	Average over all households (controlled and uncontrolled, owner and renter): $20,000.	Net benefits highest in Manhattan, and for old-style controls.	Estimated Hicksian measures; controls for sample selection.
Pollakowski (1997)	New York, 1993 (USD)	525 (Stabilized)			35				Median subsidy in Manhattan is $139, compared to $35 city-wide.	
Olsen (1997)	New York, 1996 (USD)				32				Median subsidy in Manhattan is $132, similar to Pollakowski results for 1993.	
Early (2000)	New York, 1996 (USD)		608 (controlled), 808 (stabilized)	667 (controlled), 885 (stabilized)		-4 (controlled), -44 (stabilized)	NA: welfare *loss*			Finds that controls drive up prices in uncontrolled sector by 12%
Turner (1990)	Washington, D.C., 1987 (USD)				95			Median income $20,000		

Note: Column 6 may not equal difference between columns 4 and 5, when paper reports mean of differences (which can differ from difference of means).

Table 1. Continued….

(1) Study	(2) Data	(3) Monthly Controlled Rent, PcQc	(4) Estimated Market Rent for Controlled Units, PmQc	(5) Estimated Household Expenditure without Controls, PmQm	(6) Cost of Subsidy, PmQc – PcQc	(7) Estimated Tenant Benefit	(8) Transfer Efficiency	(9) Annual Household Income	(10) Evidence on Distribution of Benefits	(11) Comments
Schneider et al. (2000)	Washington, D.C., 1998 (USD)				20			Median tenant income $24,610		Market conditions in 1998 looser than in Turner's earlier study.
Murray et al. (1991)	Los Angeles, 1980s (USD 1978)				287	227	0.79			Dynamic simulation for alternative rules; here we present simulation results for mildest law studied.
Fallis and Smith (1985a,b)	Toronto, 1982 (CAD)	358	408		50					
Marks (1984b)	Vancouver, 1978 (CAD)	233	371		78					
Caudill, Ault and Saba (1989)	Vancouver, 1978 (CAD)	233	466		233					Alternative estimator applied to Marks (1974b) data.

Note: Column 6 may not equal difference between columns 4 and 5, when paper reports mean of differences (which can differ from difference of means).

Table 1. Continued….

(1) Study	(2) Data	(3) Monthly Controlled Rent, PcQc	(4) Estimated Market Rent for Controlled Units, PmQc	(5) Estimated Household Expenditure without Controls, PmQm	(6) Cost of Subsidy, PmQc – PcQc	(7) Estimated Tenant Benefit	(8) Transfer Efficiency	(9) Annual Household Income	(10) Evidence on Distribution of Benefits	(11) Comments
Albon (1978)	Canberra, 1976 (AUD)	173	191			187	0.98			Also computes loss in producer surplus, about double the loss from transfer inefficiency.
Albon (1979)	New South Wales, 1960 (AUD)	24	55							
Willis and Cameron (1993)	Newcastle, 1988 (GBP)	89	117	154	28	18	0.65			
Pena and Ruiz-Castillo (1984)	Spain, circa 1980, (ESP)	945	4695		3749					
Berger, Jonsson and Turner (1994)	Sweden, various cities	Stockholm, approx. 400 kronor/sq m (from BJT's Fig. 2)	Stockholm, approx. 750 kronor/sq m		Approx 300-350 kronor/sq m in Stockholm				Rent discounts are large in Stockholm but small or non-existent in smaller cities.	Discounts also vary with age; are about half the average for newer units.
Rodseth and Skogstad (1989)	Oslo, 1985 (NOK)	323,700	564,400		240,700					Sales price control on cooperative apartments.

Note: Column 6 may not equal difference between columns 4 and 5, when paper reports mean of differences (which can differ from difference of means).

Table 1. Continued….

(1) Study	(2) Data	(3) Monthly Controlled Rent, PcQc	(4) Estimated Market Rent for Controlled Units, PmQc	(5) Estimated Household Expenditure without Controls, PmQm	(6) Cost of Subsidy, PmQc – PcQc	(7) Estimated Tenant Benefit	(8) Transfer Efficiency	(9) Annual Household Income	(10) Evidence on Distribution of Benefits	(11) Comments
Willis, Malpezzi and Tipple (1990)	Kumasi, 1986 (cedis)	300	574	1040	274	122	0.45	Mean income of controlled renters: 12,808; uncontrolled renters, 17,554.		
Silveira and Malpezzi (1991)	Rio, 1991 (BRL)	2800	3176	3060	376	372	0.99			
Malpezzi (1998)	Cairo, 1981 (EGP)	12	17		4.9	4.3	0.87			Accounts for key money
Struyk (1988)	Amman, 1986 (LYD)	43	61	54	18	12	0.65	Mean income: 3,265 per annum. .	Benefits accrue mostly to longtime tenants	

Note: Column 6 may not equal difference between columns 4 and 5, when paper reports mean of differences (which can differ from difference of means).

The other broad approach to studying rent control (or other aspects of housing markets) is to undertake cross-market comparisons. Data are collected across metro areas, and/or countries. The analysis typically relies heavily on aggregate outcomes, averages, medians. Thus some richness of detail is lost, but generalizability is gained.

In this paper we review mainly the case study or within-market results. There are more of the latter than the former. Another fruitful way to divide the literature is between studies that analyze the effects on individual households, producers, on the one hand; and on market-wide effects on the other.

3. Costs and benefits to individual landlords and tenants

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. A common procedure, followed for example in Marks (1984a) or Schneider et al. (1999) is to estimate an hedonic price equation on a sample of uncontrolled units (if such a "control group" in the other sense of the term can be found), and to use the coefficient to price controlled units at market prices.[11] More advanced treatments tackle the issue of whether the "uncontrolled" price is itself affected by spillover effects from controls (Marks, 1984b; Hubert, 1993; Malpezzi, 1998; Early and Phelps, 1999), and issues of sample selection (for example, Caudill et al., 1989; see below.) Table 1, which we will refer to throughout much of the paper, lists a selection of static cost-benefit studies.

Let Q_c denote the quantity of housing services produced by the unit a representative household consumes under controls; let P_m denote the market price of a unit of housing services, and Pc denotes the corresponding controlled price. Then the observed controlled rent of the unit can be denoted $P_c Q_c$, and the market rent (estimated using hedonic methods) is $P_m Q_c$. Column 3 of Table 1 presents a representative estimate (usually the mean) rent paid by renters in controlled

[11] Of course the hedonic technique can be applied to other housing policies, including various subsidy programs, as well as rent control. See Büchel and Hoesli (1995), or Berger, Jonsson and Turner (1994), for an example.

units; and Column 4 presents a representative estimate of what such units would rent for in the absence of controls.[12] Column 6 represents the difference between $P_m Q_c$ and $P_c Q_c$, or a first-order estimate of the static cost of controls to landlords. If households in controlled units were in units that approximated their demand in the absence of controls, the difference between $P_m Q_c$ and $P_c Q_c$ would also approximate their benefit from controls.

The first thing to notice about the differences between controlled rents, and estimates of market rents for those units, in Table 1 is that the estimates are all over the map. Estimates from Canada, the UK, and the US find reductions on the order of 10-20 percent of market rent; on the other hand, Pena and Ruiz-Castillo find that 1980s Spanish rent controls reduced average rents from an expected market level of about 4700 pesos to about 950. Albon's two studies imply a much larger proportional rent reduction for New South Wales in the 1960s than for Canberra in the mid-1970s.

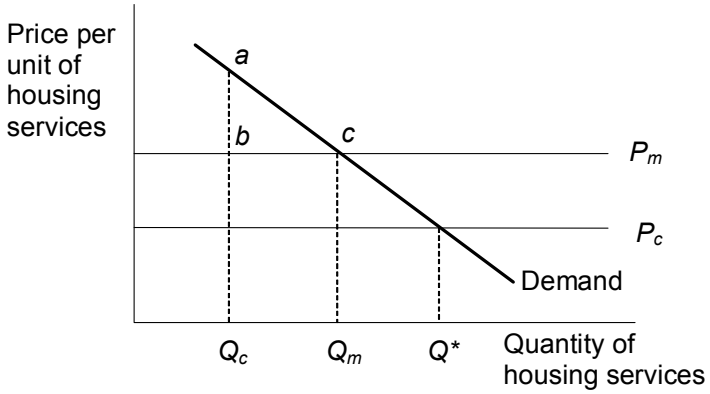
An interesting within-market comparison can be made of two hedonic-based studies of the Washington, D.C. market; the first by Margery Turner and colleagues at the Urban Institute (Turner, 1990), and the second by Stephen Schneider and colleagues at Nathan Associates (Schneider et al., 1999). Note that circa 1987, Turner and colleagues estimated that controls reduced the rent of a typical unit by about USD 95 per month; a decade later, Schneider et al. estimated the typical difference between $P_m Q_c$ and $P_c Q_c$ to be only USD 20 (and this difference would be even smaller in 1987 prices). Schneider et al. discuss differences in methodology, changes in maintenance, and the like; but they argue that the main difference was in market conditions: in 1987 the D.C. rental market was much tighter, whereas in 1998 the District had been undergoing an outflow of population to the suburbs, leading to an increasing vacancy rate. The key result here is that market conditions matter when estimating the effects of controls, perhaps more than is generally realized.

But in any event, hedonic studies that stop after estimating the difference between $P_m Q_c$ and $P_c Q_c$ have an important shortcoming. 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

[12] Table 1 is highly condensed; given differences in methodology and data, quantitative comparisons across studies are indicative at best, and should be made cautiously.

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.

Figure 1. Micro analysis of consumer's surplus: Price control with "too little" housing



Note: Unit of observation: an individual household, and unit.

As a first approximation, the static cost borne 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 short run 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 Pm, paying rent equal to $P_m Q_m$. 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^* units of housing services. But elsewhere we have alluded to the fact that many models predict that under rent control landlords will produce less housing, and/or the transactions costs of moving will rise. In the absence of other actions, 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.[13] As drawn, the representative household consumes Q_c which is less than their equilibrium demand. They receive an implicit subsidy of $(P_m-P_c)Q_c$, whose cost is borne by the landlord. However notice that the consumer has also given up consumer's surplus equal to the triangle abc;[14] 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:

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

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 Rm
P_cQ_c = observed controlled rent, also denoted Rc
b = price elasticity of demand.

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

$$Benefit = P_m Q_m \left(\log\left(P_m Q_c\right) - \log\left(P_m Q_m\right) \right) + P_m Q_m - P_c Q_c$$

The measures in (1) and (2) do not include all possible costs and benefits to tenants. For example, rent control may increase transac-

[13] Note that households can consume "off their demand curve" in uncontrolled markets as well, if there are transactions costs. This key question for analysis of rent control is not whether such disequilibrium in consumption exists, but rather whether I is greater than in the absence of controls.
[14] It is easily shown that a welfare triangle arises whenever Q_c is to the right of Q_m as well.

tions 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 (Loikkanen, 1985; Malpezzi, 1998).

The cost imposed on landlords is straightforwardly approximated by $P_m Q_c - P_c Q_c$, or the difference between controlled and market rents for the unit inhabited by the tenant. This static measure of cost to landlords does not include losses from prior accelerated depreciation of the unit,[15] or losses from the uncompensated transfer of property rights to renters. The true costs to landlords may well exceed these estimates.

This description, and many of the cost-benefit studies undertaken, focus on the Marshallian or money-income-constant surplus measures. Several studies have extended this to Hicksian measures using income-compensated demand curves as the underlying measure of benefit; see Mildner (1992) and Malpezzi (1998) for rent control-related studies, and more generally see Murray (1976), Willig (1976) and Schwab (1985); applications include De Borger (1986) and Mayo and Bornbrock (1986).

It must also be pointed out that the simple model in Figure 1 assumes rent control does in fact effectively reduce the price per unit of housing services. In fact, that can depend on the nature of the control regime, and the time frame of the analysis. For example, several theoretical papers, such as Olsen (1969a), Frankena (1975) and Arnott (1981) point out that if rents (expenditures) are controlled, and landlords have the ability to reduce maintenance and accelerate depreciation, an initial rent reduction may be modeled as an initial price reduction that is accompanied by gradual reduction in housing services; initially $R_c = P_c Q_m$ but over time Q falls to some Q_c such that $R_c = P_m Q_c$, i.e. where the ex ante market price of a unit of housing services is restored. Further discussion of the role played by depreciation is found below.

[15] Note that these costs will be reduced by the amount landlords reduce their maintenance expenditure.

3.1. Measures of static costs and benefits

Studies which calculate the static cost borne by owners of existing rental units, using some variant of the consumer's surplus model in the preceding section, show that the reductions from market rent can be substantial, but that tenants, in general, value the implicit subsidy of controls less than it costs. Table 1, above, includes a number of such studies. Generally in addition to the hedonic estimation of $P_m Q_c$ already partly discussed above, these studies also estimate expenditure relations using uncontrolled households, then use the coefficients to estimate $P_m Q_m$ for controlled households (Column 5 of Table 1), and then estimate cost-benefit measures as above (Column 7, in addition to aforementioned Column 6). We reiterate our note above: the results in Table 1 are from a range of studies with many important differences in details, and we only present some selected (but we believe representative) results. Comparisons across studies can only be indicative.

New York

The classic empirical analysis of the costs and benefits of rent control is Olsen's (1972) paper. Using data from New York City in 1968, Olsen used estimates from a hedonic index of uncontrolled units to predict the uncontrolled rentals of controlled units.[16] In an analogous fashion, he used the data from the uncontrolled portion of the market to estimate the uncontrolled demand for housing services. The average controlled rent for an apartment was USD 999 per annum; for comparison, the average income was USD 6229.[17] The average uncontrolled rent predicted by the hedonic results for those same units was USD 1,405, implying a subsidy of USD 406. The average free market expenditure for the controlled households was USD l,470, indicating that they consumed slightly less housing than they would have in the free market. The average household in the controlled market consumed about four and a half percent less housing than they would have in the free market.

[16] Aficionados of New York City controls will note that most recent studies show a large difference between the effect of the original, stricter controls and subsequent "rent stabilization." At the time of Olsen's original study, rent stabilization was not in force, so there was a reasonably clear delineation between controlled and uncontrolled units.

[17] Data from Olsen's results are in 1968 dollars. Between 1968 and 2003 the implicit price deflator for GDP rose by a factor of about 4.25.

Olsen computed the economic benefit of rent control to each tenant under the assumption of a unitary price elasticity, i.e. using equation (2) from above. Olsen's estimate of the average net benefit is USD 213, little more than half the gross subsidy implied by rent control.

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

A number of broadly similar studies have been carried out in a number of countries; some key results are summarized in Table 1. By far the most studied market is New York. This is in large part because of several related facts: New York is not only the largest city in the US, with long-time rent control; but for over three decades a detailed special-purpose housing survey has been carried out, the New York Housing and Vacancy Survey, which is publicly available and designed expressly for the kind of cost-benefit analysis described above.[18]

Despite the greater difficulty in finding data, a few consumers' surplus based studies of controls have been carried out outside of North America. Albon (1978) worked through a simulation model of the benefits of controls, calibrating it on the basis of data from Canberra in the mid 1970s, with demand parameters derived from other literature. Albon's simulations suggested that Canberra renters were near their demand curve: the transfer efficiency of 98 percent is as high as one ever finds from any sort of in-kind transfer. On the other hand, Willis and Cameron's (1993) econometric estimates of costs and benefits for Newcastle find a much lower transfer efficiency of 65 percent.

Several studies have been carried out in developing countries. In Cairo, Egypt, for example, Malpezzi (1998) found that monthly rents

[18] Details can be found at http://www.census.gov/hhes/www/nychvs.html.

for a typical unit were less than 40 percent of estimated market rents. Key money and other side payments make up about a third of the difference (but mostly for newer units). Benefits to tenants are further reduced because they are not free to choose a unit of appropriate size and location. In Amman, Jordan, Struyk (1988) found that the static cost of controls is about 30 percent of estimated market rent; the benefit to the typical tenant is only 65 percent of cost.

These aggregate statistics may mask large variations in costs and benefits to individual tenants. In their review of developing country experience, Malpezzi and Ball (1991) found that long term tenants of older buildings often receive lower rents at landlords' expense, while recent movers pay large amounts of illegal key money, if they can find a unit at all. Rarely are these circumstances strongly related to income or other measures of need; rent control can be a very inefficient redistributive mechanism.

3.2. Selection 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.[19] Malpezzi (1986) applied a simple estimator due to R. Olsen (1980) which tests for such bias in such a model. In his test for Cairo, Malpezzi (1986) found that the selection bias variable was 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. However, the correction did not affect the predicted rents by much, and the correlation between predictions with and without the correction was .95.

More recent work by Caudill et al. (1989) finds a more economically meaningful effect of correcting for selection bias. Re-estimating

[19] See, for example, Heckman (1979), Olsen (1980), and Maddala (1983, Ch. 9).

variants of Marks (1984) model using market data from Vancouver, they find estimates of the market rent for the average controlled unit are USD 411 without the correction and USD 466 with a correction for selectivity bias.

3.3. Problems with sample statistics vs. representative consumers

All of the analysis of the consumer so far has focused on a single representative consumer. Most studies implicitly assume that all consumers have identical demand functions, or at least identical conditions on known "demand shifters" such as income and demographic variables. But in fact this is not necessarily the case. Olsen and Agrawal (1982), Malpezzi (1986) and Gyourko and Linneman (1990a) show that if similar consumers have different demand curves, and we use statistical techniques to estimate the average demand curve, estimates of welfare losses will be biased upwards.

On the other hand, analysis of a "representative consumer" has its own shortcomings. 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. Glaeser and Luttmer (2003) address the problem by grouping households by demographic factors and estimating consumer surplus measures for these groups, focusing on differences between controlled households' consumption of bedrooms, and estimated demand for bedrooms; and a similar exercise for a measure of housing quality. Their dollar estimates vary depending on assumed elasticities, and other parameters; their estimates of net deadweight loss range widely, from USD 200 to USD 2,000 per apartment per year; the main value of their paper is not a precise estimate of deadweight loss, but an initial and innovative exploration of how one might estimate the deadweight loss from misallocation of housing.

3.4. Recent cost-benefit studies of vacancy decontrol

Recent studies by Olsen (1997) and Schneider et al. (1999) examined the effects of decontrol in New York and Washington respectively. Olsen found that while some individual apartments would experience substantial increases under vacancy control, this simulation estimate

suggested vacancy decontrol would lead to very modest average rent increases, except in parts of Manhattan. This is consistent with Pollakowski's finding that for much of the New York City area, rent control has little effect on rents except for parts of Manhattan. Roistacher (1992) simulates several decontrol options including deregulation targeted first at highest income tenants, deregulation targeted by potential rent levels, and vacancy decontrol. Her results suggest that a combination of income targeted decontrol and vacancy decontrol would deregulate about 400,000 of the roughly million regulated units in a two-year period increasing average rent by about 26 percent. Roistacher points out that in addition to the efficiency gains from aligning the housing market, currently regulated units would become more valuable and property tax revenues would increase, she estimates on the order of USD 100 million per annum.

Pollakowski (1997) estimates hedonic price equations using the New York City Housing and Vacancy Survey for 1987 and finds that most stabilized units and, in particular, most units in boroughs other than some parts of Manhattan actually rent for about the same as similar units in the uncontrolled market. This is so even though Pollakowski uses Marks' (1984b) model to adjust for spillover effects in the uncontrolled market.

The effects of vacancy decontrol may be limited with a typical second generation rent control system as e.g. in a number of European countries. In Sweden, it is generally believed that the regulated rent level is not binding on a number of local housing markets although it is strongly binding in the central parts of Stockholm (see Atterhög and Lind, 2000; and Donner, 2000, for an overview).

3.5. Dynamic 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. Rydell et al. (1981) provides an interesting simulation model of the effects of controls on supply, driven by parametric assumptions of how the supply of each component changes with a change in the relative price of housing.

3.6. Rent control and housing quality 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 (Olsen, 1969a). 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. The direction and size of changes in maintenance will vary with type of law, market conditions, and with landlord and tenant characteristics, as Olsen (1988) has emphasized. In fact, it is theoretically possible to design an ordinance that increases maintenance.[20] Olsen also notes that to the extent a control regime decreases maintenance; tenants simply have an incentive to substitute their own maintenance for the owner's.

The question is not only whether rent control induces deterioration or by how much, but rather if it does, how rapidly it does so, as Olsen (1969a, 1988) points out. There have been a number of studies of housing depreciation, including Malpezzi, Ozanne and Thibodeau

[20] It is often argued that the rent control ordinance in Sweden has lead to an overinvestment in maintenance (Turner, 1998).

(1987), Shilling, Sirmans and Dombrow (1991), and studies cited therein. But these are all measures of net depreciation; since landlords under controls may be incented to lower maintenance, it is also relevant to understand gross depreciation, or the rate at which properties may deteriorate in the absence of maintenance; this places a bound on the effect modeled in Olsen (1969a). To date we are only aware of one direct study of gross depreciation, namely Rydell and Neels (1985). Using iterative techniques on data from the US Housing Assistance Supply Experiment, they estimate a gross depreciation rate of eight percent. In other words, without maintenance, Rydell and Neels estimate 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.

Malpezzi, Ozanne and Thibodeau (1987) have provided estimates of the rate of net depreciation in 59 US markets, and Börsch-Supan (1983, 1986) uses these estimates to test for faster depreciation in controlled market. Some US markets are controlled, but most are not; in those that are, rent control regimes vary significantly from place to place. Börsch-Supan's initial and preliminary (1983) estimates suggested controls may have effects; but a more sophisticated model with improved data in Börsch-Supan (1986) failed to find any significant effects.

Gyourko and Linneman (1990b) examine the effect of controls on housing quality in New York more directly, with a discrete choice model. They find that in Manhattan, controlling for the age and size of the dwelling, units had a 9 percent higher probability of being in unsound condition if under controls. Smaller but significant effects were found in Brooklyn and the Bronx.

3.7. Mobility and household matching

Linneman (1987) develops a two-stage model to analyze the tenancy duration decision and rental payments for controlled, stabilized and uncontrolled housing sectors, and applies it to 1981 data from the New York City Housing and Vacancy Survey.. In the first stage he determines if the length of tenancy varies significantly among stabilized, controlled, and uncontrolled sectors. The second stage involves computing the difference between the unit's market rent within their sector and that unit's hypothetical market rent in the other two sectors.

Linneman's major findings are threefold. First, households' characteristics, notably income, are significantly differ among the three housing sectors. Also, households in the controlled sector lived in their units at least four times longer than their counterparts in the stabilized and uncontrolled sectors. Lastly, low-income renters reap greater benefits from the old style controls, ceteris paribus. These findings imply that the old style sector is less mobile than the new style sector. These results are echoed in the housing market in Stockholm where the turn-over rate is much lower in the central part of the city where the difference between the controlled rent and a hypothetical market rent is large, compared to the outskirts of the city (Lind, 2001).

Gyourko and Linneman (1989) examine the relationship between tenancy duration and rent control. Using implicit market rents from the hedonic function and a tenancy measure to compute the subsidy, they find that tenancy duration and the size of the tenant's subsidy have an inverse relationship such that the larger the subsidy, the less likely a tenant will move.

Rapaport (1992) examines the effects rent control has on the probability of vacancies and occupant turnovers in New York City's rental housing stock. She finds that a rent controlled apartment is about 15 percent less likely to turn over in a three-year period unconditionally and about 8 percent less likely when other determinants of mobility are included in the regression. On balance, she finds that rent regulation does not significantly affect the probability that a unit is vacated but it does decrease the probability of inflow of new tenants into vacant units. Rent stabilization has no effect on either outcome. Ault, Jackson and Saba (1994) find that New York City's rent control reduces mobility and that around 80 percent of the difference in mean expected tenure between controlled and uncontrolled units is attributable to efficiency losses from controls.

Munch and Svarer (2002) study the effects of controls on mobility in Danish housing markets. They begin by measuring the stringency of rent control on each sample unit by comparing controlled rents to estimates of market value by the Danish Tax Authorities. After ranking sample units by the proportionate size of the rent reduction, they apply a proportional hazard model to household moves. They find that the duration of a household's tenure is related to the size of the rent reduction; for example, a typical household in the top decile of

regulatory stringency stays six years longer than a typical household in the least stringently regulated decile.

Nagy (1997) develops a model in which tenants with longer durations will dominate the controlled market. Using New York City Housing and Vacancy Survey data for 1981, Nagy finds that those in the rent stabilized sector actually paid higher rent than those who occupied similar apartments in the uncontrolled market. He then examined the same tenants six years later and found that they paid less than the real 1981 rent in 1987, as long as they still occupied the same apartments.

Simmons and Malpezzi (2003) employ a two-step estimation process to study mobility with panel data from New York. First, they estimate the gross benefit to rent control (the rectangle in Figure 1), and the gross loss of consumer's surplus due to controls (the triangle in Figure 1). Then they use each of these measures, along with other determinants, to predict the probability of a household move from one period to the next. Both cost and benefit measures are significant, and of the expected sign. Tenants are more likely to move when the triangle becomes large, and will most likely stay when the rectangle is big.

3.8. Rent control, profitability and supply

As noted above, so-called second-generation rent controls (now common in Europe and, where controls operate, in the US) generally do not fix rents per se but rather place limits on the amount of rent increases. For example, the inflation rate can be used to increase the rent ceiling over time. Even if average rents tend to move with inflation, however, the distribution of rent changes must be considered. Understanding the stochastic processes that rents follow turns out to be central to forecasting the effects of controls.

Malpezzi, Pollakowski and Simmons (2002) estimate this volatility of rent changes using the American Housing Survey for a number of large US metropolitan areas. The volatility measures, including the standard deviation, are used to parameterize a simulation model of the profitability of rental housing investment under rent control. They then simulate a large set of possible rent paths for controlled and uncontrolled (benchmark) properties to be held ten years. Next they calculate the capitalization rate that real estate investors use to value a dwelling unit that yields a given net rent or net operating income. The effects that controls will have on prices of existing units can then be found by starting with our initial capitalization rate and

calculating a constrained rate of return. If controls are binding the rate of return will be lower. The "cap rate" that would raise the return back to the unconstrained case by lowering the value of the housing investment is calculated. This yields an estimate of the value of the unit under controls as well as the capitalization rate.

The simulations to date—still under way—suggest that even modest controls could considerably drop capital values on the order of 10 percent or more. Of course, whether there are aggregate market effects—our next topic—depends upon the elasticity of supply.

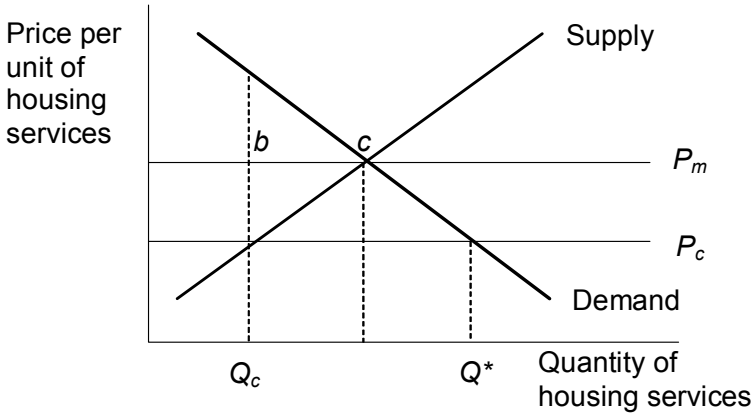
4. Aggregate analysis: Market effects

How does analysis proceed when we have added up a market-full of individual supply and demand curves similar to those of Figure 1? Analysis of market effects begins with the simple model of aggregate supply and demand in Figure 2. The model is quite similar to, say, the partial equilibrium effects of a tariff, or a tax on income from capital generally.

Let the market price of housing be denoted P_m, and initially assume a simple, and effective, rent control that shifts all rents down by a constant amount P_m-P_c. Now tenants pay a lower rent; if unconstrained, in the aggregate they demand Q^* units of controlled rental housing. But at the lower price, suppliers of rental housing are only willing to offer Q_c units.

It is this simple but powerful analysis that lies behind the prior belief of so many economists that, in the absence of countervailing government action (such as the subsidy or provision of social housing) that stringent rent controls which effectively reduce the price of housing services will reduce the supply of housing, or at least the supply of housing in the controlled sector. This prior is widely held despite the relatively modest direct empirical evidence on the point, about which more below.

Figure 2. Aggregate analysis: Market effects of a price control



Note: Unit of observation: an entire submarket.

While Figure 2 is the starting point for thinking about the market effects of rent regulations, it is hardly the final word. First, what do these supply and demand curves actually look like? In particular, while there is a wealth of information on housing demand in markets around the world (Mayo, 1981; Olsen, 1987; Malpezzi, 1999a; Whitehead, 1999) as already noted there is much less information on supply elasticities. A moment's consideration of Figure 2 confirms the result familiar to any undergraduate that the more inelastic supply, the more modest the potential reduction of Qc relative to Qm; and the more elastic the market, the greater the effect on supply.

4.1. Rental housing unit losses: Demolitions, conversions, and foregone starts

Regardless of the cause—declining demand or rent control –rent reductions motivate private landlords to consider alternate uses for their property. In the absence of collateral policies affecting such changes, some would remove the property from the housing stock (by either demolishing it or converting it to nonresidential use); others might 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). Potentially 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 the private market to social housing; and (especially in many developing countries) from formal into informal sectors; and it can adversely affect the quality of units. But despite the frequency with which potential supply effects are pointed out in the theoretical literature, and the substantial literature that demonstrates that, in general, landlords respond to incentives, surprisingly little empirical work has directly tested the effects of controls on supply. Malpezzi and Ball (1993) do find a negative and significant relationship between their cross-country measures of the stringency of controls, and the share of GDP invested in housing; but their model was rough at best.

4.2. Spillovers to related markets

A line of papers of particular interest to this work concerns price adjustments and price determination on markets with two submarkets which provide goods which are close substitutes, and where one submarket is controlled and the other is uncontrolled. The general wisdom was for a long time that the price of the uncontrolled market should be higher if the substitute market is controlled. This wisdom was challenged by Gould and Henry (1967), who built a formal model. They demonstrated that the price in the uncontrolled market could be higher or lower than in the absence of a price control on the substitute market depending on the elasticities of demand and supply, as well as the allocation mechanism with which the rationing of the controlled market was handled. They did not however have the housing market in mind, why they did not include a binary choice constraint in their model, i.e. they did not exclude the possibility to consume housing in the controlled and uncontrolled market at the same time.

In the contribution by Fallis and Smith (1984), a binary choice restriction was imposed. They also introduced a constraint on the allocation mechanism, assuming that controlled flats predominantly are allocated to those households who initially occupy them. Finally, they

deal with the problem of likely quality differences between uncontrolled and exempt dwellings.

Fallis and Smith develop two different decontrol regimes: new or vacated units are exempt. This mirrors the traditional decontrol schemes which are applied in the US and to some extent in Europe as well. Irrespective of exemption method, they conclude from their partial equilibrium analysis that equilibrium rents on uncontrolled units are higher than would have been the case if there was no rent control at all on the rental market. They also support the conclusions by presenting empirical data from Los Angeles.

The paper by Fallis and Smith is built on two rather restrictive assumptions. They assume that the rental market is homogeneous and that the rationing process imposed by the (binding) rent control result in a rather peculiar way: all controlled units are allocated to sitting tenants, why no mobility is assumed.

Hubert (1993) arrives at the same conclusion as Fallis and Smith with the additional twist that the rationing scheme has no importance for the result. However, if a size cap on the controlled units is introduced, the result is ambiguous.

The quality diversity is introduced in a paper by Häckner and Nyberg (2000). They divide the rental market in an attractive area and a less attractive area. They also introduce a rationing scheme, giving all households equal chances to acquire a controlled unit.[21] With a partial analysis that resembles the one used by Fallis and Smith, they let the rent control be binding only in attractive areas. The result is an unambiguous correlation between the regulated rent level and the rent level in the uncontrolled sector. However, if the rent control is binding in the whole rented stock, the result is ambiguous. There are two counteractive forces. A low rent level attracts low income households (Häckner and Nyberg allow a continuous income distribution) which will crowd out high income households from the controlled sector (attractive as well as unattractive). On the other hand, the changing relative prices between controlled and uncontrolled housing will induce some better off households to compete over the controlled units. The resulting average income level of tenants in each subsector will thus be ambiguous.

[21] They argue that even if social landlords favour low income households and private landlords favour small, high-income households, this has a "muted" effect on a possible income bias in the allocation of households (footnote 4, p. 312).

In a recent paper by Early and Phelps (1999) the main results in the theoretical papers are backed by an empirical analysis. They find, using an OLS estimation on data from the American Housing Survey, that the introduction of rent control may increase rents in a substitute uncontrolled market. Malpezzi (1993) however presents some evidence that controls lowered prices in uncontrolled markets in mid-1980s Ghana while it raised uncontrolled prices in early 1980's Egypt. A more recent study by Early (2000) found that in New York City, rent control drives up prices in the uncontrolled rental sector, reducing the actual benefits to tenants considerably from estimates made by previous studies which neglected this spillover effect. In fact, Early found that the average benefit to tenants in regulated units was negative.

These papers assume, by and large, that rent control is binding in the controlled market. The paper by Häckner and Nyberg (2000) to some extent marks a development towards a more generalised model, where the rent control is only binding in one submarket. This is interesting, because it highlights a common situation in the housing market, where current, administratively influenced price regime induces both shortage and thus rationing, as well as excess supply and thus vacancies.

Finally, we note the relative paucity of empirical studies of the net effect of controls on aggregate supply. Maclennan (1978) collects data from several sources, including newspaper advertisements and other listings, for Glasgow, and finds a reduction of rental housing on offer that is correlated with the tightening of Glasgow's rent control regime. Malpezzi and Ball (1993) construct an index of the stringency of rent control regimes in some 60 countries, and in a simple cross-country model they find that the more stringent the controls, the less housing investment is obtained. However they note that their stringency index is correlated with other measures of distortion in economic policy, and it is hard to state definitively that it is the effect of rent controls per se that reduces supply.

5. Conclusions

Rent control reminds us of macroeconomics: if you are studying it, and you are not at least a little bit confused, you are probably not yet thinking clearly.

Still, there are some "consensus findings" from empirical research. The transfer efficiency—the ratio of tenant benefits to landlord costs—is generally less than 100 percent (but this is hardly surprising, since it must be less than this by construction). The transfer efficiency varies quite a bit from market to market; one would be hard pressed to use results from, say, New York to accurately predict effects of controls in Los Angeles, much less Paris or Stockholm or New Delhi. One thing that is consistent is that the variance of costs and benefits within a market is almost always very large. Net benefits are very poorly and in some cases perversely targeted. Analysis of individual costs and benefits in the markets mentioned above shows no consistent redistributive effect. Typical private landlords are better off than typical tenants, but in the few markets for which we have data, the differences are not great; in markets with many private tenants and landlords, there are surely some well off tenants benefiting from controls and landlords with modest incomes. In general, even when the cost of controls has not yet largely shifted to tenants, it is not clear why it is desirable to tax such a narrow base as landlords; perhaps, because supply elasticity is so low in short run, there is a Georgist notion that taxing landlords will recoup some monopoly rents. It may be related to political economy issues (tenants are almost by definition more numerous than landlords). But these issues are as yet especially poorly understood.

In second generation schemes, where rents are indexed more or less to inflation, generally rent reductions—and net benefits to tenants—are smaller, and transfer efficiency is somewhat higher on average. However, such schemes neglect the fact that the distribution of individual rent changes in a market has a much larger volatility than the index; hence second generation controls take away the "upside." The effects of this will depend on whether landlords and investors accept a reduction in rates of return, or whether capital values fall accordingly. If the latter effect dominates, the effect on the market depends on the (as yet still poorly understood) elasticity of supply.

5.1. Back to basics, revisionism, or both?

This review suggests that while new models based on contract theory, bargaining under asymmetric information and the like are an advance, there is also life left in the older standard models. Given that so many rent control outcomes seem to vary with market conditions and industrial organization, there is probably as much or more to be gained

by "capital widening"—applying standard models to more markets, and systematically collating results—than by "capital deepening," developing new models. There would also be large gains from more careful studies of the supply side of the housing market more generally. But perhaps the clearest finding of this review is the need for empiricists to develop more direct tests of the "new" theoretical literature of the last two decades.

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Appendix 1. Major reforms of rent regulations since 1980

Belgium	1984: Rent increases linked to CPI. 1985-1987: Indexation temporarily suspended. 1991: Freely negotiated new rental fixed term contracts introduced. 1997: Limits set to new short-term agreements.
Denmark	1990: Condominiums built after 1991 exempt from rent control.
Germany	1983: Introduction of upper limit of 30% in a three-year period on rent increases for sitting tenants, rent escalation clauses and rent contracts linked to a price index permitted. 2001: Upper limit on rent increases in a three-year period reduced to 20%. Period of giving notice for tenants reduced to three months.
Greece	1997: Freely negotiated rents in new contracts. Minimum duration of contracts of 3 years.
Spain	1985: Freely negotiated rents in new agreements. 1995: Minimum lease of five years (at tenant's option); CPI indexation; One-off updating of existing contracts (to be implemented over ten years).
France	1997: New contracts liberalized.
Ireland	No significant controls/regulation on rent contracts.
Italy	1992: Freely negotiated new fixed-term contracts introduced. 1998: Two types of "free" contracts: freely negotiated at the individual level at the start and contracts where yearly rent increases are collectively negotiated by landlords and tenants.
Luxembourg	1987: Increases in the rents of dwellings built before 10 September 1944 and clarification of the meaning of invested capital for those built after this date.
Netherlands	1994: Liberalized more expensive segment of rental market.
Austria	1986: Partial liberalization of new tenancies. 1984: "Indicative value rent system" introduced.
Portugal	1981: Freely negotiated rent contracts for new tenancies introduced (but no indexation allowed in these contracts). 1985: Mechanism of updating all rents with CPI; one-off updating of old contracts (but still remaining very distant to rents in new contract. 1990: Possibility of setting a limit on the duration of rental contracts. 1993: Possibility of introducing different indexation mechanisms under specific circumstances
Finland	1990-1992: Gradual liberalization of rent controls. After 1995: Rents are practically free from public control, they should not be "excessive" (in a legal sense).
Sweden	No major reforms that could improve the efficiency of allocation in the rental sector have been undertaken.
UK	1988: Assured tenancy—eviction easier and initial rent and indexation negotiated.
US	1994: Boston, Cambridge, Brookline Massachusetts rent control ordinances repealed; units decontrolled when existing tenants leave. 1995: Santa Monica, Berkeley California also institute decontrol upon vacancy.
Canada	1992: Most rent controls were removed, with the exception of Ontairo that kept a system of rent increase guidelines and a rent review system in Quebec and British Columbia