

Do Growth Controls Matter?

A Review of Empirical Evidence on the Effectiveness and Efficiency of Local Government Land Use Regulation

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Abstract

“Do Growth Controls Matter? A Review of Empirical Evidence on the Effectiveness and Efficiency of Local Government Land Use Regulation.”

Empirical evidence shows that the effects of local land use regulations aimed at reducing growth are evident in land values and housing prices. In areas having growth controls, the value of undeveloped land tends to decrease, while that of developed land increases. While the efficiency of land use controls is unclear, they are likely to be inefficient due to major costs such as wasteful decentralization and increased travel time and distance. The paper also outlines how to conduct an empirical test of whether a growth control ordinance is efficient.

Numerous empirical studies are reviewed and critiqued, including: studies that conclude zoning is not justified on efficiency grounds, and that spillovers and land use constraints do not affect property values; studies that show spillovers and zoning do make a difference; studies that examine whether land use controls themselves are a constraint by looking at both undeveloped land values and values of single-family residences; research on intermetropolitan housing price differentials due to growth controls or to monopoly government structure; and studies that evaluate the efficiency of land use controls.

The author concludes that empirical economic research is ambiguous regarding arguments that land use controls are ineffective and unnecessary, but that most growth controls do in fact impose a net cost on society.

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—William A. Fischel

TABLE OF CONTENTS

§1 Summary and Introduction.....	1
§2 A Note on Methods	5
§3 The Zoning-Does-Not-Matter Studies	9
§4 Capitalization of Amenities and Disamenities.....	15
§5 Capitalization of Regulations in Land Values	21
§6 Growth Controls and Housing Costs.....	29
§7 Efficiency of Zoning and Growth Controls	43
§8 Land Value Maximization and Efficient Zoning.....	47
§9 The Causes and Consequences of Growth Controls	53
Bibliography	59

Do Growth Controls Matter?

A Review of Empirical Evidence on the Effectiveness and Efficiency of Local Government Land Use Regulation

§1 SUMMARY AND INTRODUCTION

Summary of Findings

The answer to the title's question is yes: Empirical evidence shows that local land use regulations aimed at reducing growth do matter. The effects are evident in land values and housing prices. Growth controls tend to raise the value of existing housing and sites on which development is still allowed and lower the value of undeveloped land and properties that might be profitably redeveloped. Local spillover effects, which economists traditionally use to justify land use controls, also affect property values, although these effects are complicated and difficult to detect in conventional research designs.

The aforementioned findings do not by themselves imply that growth controls are efficient or inefficient, however. The few analyses that attempt to measure both benefits and costs of land use regulation indicate that growth controls are likely to be inefficient. The major costs are probably wasteful decentralization of firms and too much commuting by households. Higher housing prices are seen as benefits by their owners. Higher prices could emerge from increased attractiveness of a well-planned community, or they could emerge from monopolistic restrictions on supply. There is evidence that both effects exist.

Introduction

Growth control in the 1970s was regarded as an environmental issue. The 1980s interest in growth controls was sparked by concerns that traffic was overwhelming the suburbs of metropolitan areas. While this essay does not attempt to distinguish growth control motivations, I have two observations about the suburban traffic issue. One is that at least some of the traffic problems have resulted from the antigrowth movement of the 1970s. Highway construction slowed

during that time, and environmental activists urged that fixed-rail transit systems (e.g., subways) be built instead.

The problem is that fixed-rail systems are even less efficient for carrying suburban commuters than highways. Economic studies of the Bay Area Rapid Transit (BART) system of San Francisco, which has a setting ideally suited for a commuter rail system, found that BART had little effect on highway congestion. The reason is that the dispersal of homes and jobs in modern metropolitan areas is already too great for any new rail system to be less costly than a highway system that accommodates both cars and buses (Gordon and Richardson 1989).¹

The second cause of suburban gridlock is the baby boom of 1946–64. This abnormally large cohort jammed the schools in the 1950s and 60s (and left a stock of unused elementary schools thereafter), crowded colleges a few years later, and began congesting the labor force by the middle 1970s. The baby boomers have now gotten married—at a later age than previous cohorts—and by the early 1980s were settling into the suburbs.

Consider the largest cohorts of the baby boom, those born in the late 1950s. These people are now in their early 30s, which is slightly after peak childbearing years. Their demand for suburban homes is now at its peak. They need two earners to afford these homes—because of the size of their cohort, their relative wages are lower than those of their parents at the same age—and that normally requires two cars. Baby boomers have to do a lot of commuting, which explains why automobile traffic has increased so much in the last ten years or so.

These problems will abate in a few years. The current entrants to the labor force were born in the smaller cohorts after 1964. Their entry into the job and housing market may create some pressure for a few years, but the growth rate will be slower, and by that time, one hopes, the stock of houses and highways will have at least partly caught up.²

The focus of this survey is local government control of development, not national environmental policy. Growth control devices include the tightening of traditional zoning laws as well as moratoriums on the extension of water and sewer lines, nonprice rationing of building

¹ The key study of BART and alternatives is Keeler (1975), which is summarized in Mills and Hamilton (1984, pp. 246-50). Mills and Hamilton conclude that “none of the subways constructed in the 1960s and 1970s operates with a cost even approaching that of a well-designed bus system” (p. 249).

² Mankiw and Weil (1988) project (by an admittedly naive model) that the lower demand due to smaller cohorts from the post baby-boom era will cause real housing prices to decline after 1990.

permits, and tying development permits to the provision of new public facilities.³ Research design, sample selection, and interpretation of the results are the principal areas reviewed. Data from the papers surveyed are samples drawn from U.S. and Canadian metropolitan areas. Most of the studies were published since 1970. Theoretical papers are mentioned only when especially relevant to empirical work and then mainly in footnotes. My narrative method is to select representative empirical articles for close scrutiny. Related articles are indicated in footnotes and parenthetical references.

This survey is informed by a point of view first propounded by Otto Davis (1963) and expounded in my book (Fischel 1985). I regard the public officials who adopt growth controls and the voters who elect them—and sometimes overrule them—as rational people. Regulations are adopted to create some kind of benefit for someone. I am thus skeptical of empirical work that is guided by the assumptions, which are usually implicit, that growth controls are solely a cost-creating device or are imposed exogenously to the preferences of citizens of the jurisdiction. The zoning-as-earthquake theory has influenced research design, and, as I will show, it has induced dubious inferences from many empirical results.

³ Porter (1986) contains descriptions of a variety of growth management techniques in several communities and states. The common thread of these devices is that they are police-power regulations applied to land. (The police power is one of the three traditional legal powers of government; the other two are eminent domain and taxation.) The police power is broad, and judges are deferential to its exercise, so that one type of land use control merges in effect and purpose with other types. For this reason, I decline to make a sharp distinction between growth management and traditional zoning. Judges who have ruled on the legality of growth management have taken a similarly holistic view.

§2 A NOTE ON METHODS

Nearly all of the studies reviewed below rely on regression analysis of hedonic price models. The sale price of a complicated commodity such as a house can be seen as the sum of the values of a bundle of characteristics. Each house may indeed be one of a kind, but none of the characteristics themselves, other than longitude and latitude, is unique. Because purchasers have substitute homes from which to choose, each characteristic, such as number of bathrooms, size of lot, and type of roofing, can be said to contribute some measurable portion of the market value of the house. Hedonic price models are used to estimate the value of each relevant characteristic. Thus one could say that, for an average house in the sample, an additional bathroom would add \$7000 to a house's value, an extra thousand square feet of lot an additional \$3000, and substituting slate for shingles on the roof another \$2500.

Such inferences can be made using multiple regression analysis. Usually, the dependent variable in the papers surveyed here is the sale value of individual units, though sometimes it is the average value of units in a census tract or a municipality. This value is regressed on the group of characteristics that the researcher regards as important and (more importantly) for which he or she has some data. Most individual observations come from local real estate sources, and they are often supplemented by special field studies. These data are seldom already systematically tabulated for computer use; they are usually painstakingly collected by individual researchers, which may account for the predominance of multiple authors of studies using such data. U.S. Census data are more easily available, but because they are tract or community averages, individual geographic and physical variations are suppressed and fewer inferences can be made from them.

In addition to data about the structural characteristics of the housing units, variables measuring neighborhood characteristics, municipal services and taxes, and regulatory constraints are often added to the hedonic index. A typical regression might look like the following, in which the minus sign indicates that the coefficient is expected to be negative.

Single-Family House Value = $\beta_0 + \beta_1$ (Square Feet of Interior Space)
 + β_2 (Number of Bathrooms) + β_3 (Square Feet of Lot) + β_4 (Dummy for Garage)
 - β_5 (Distance to Metropolitan Employment Center) + β_6 (Test Scores of Local
 School) - β_7 (Property Tax Rate) - β_8 (Traffic Count on Street) + β_9 (Dummy for
 Being in Single-Family Zone) + β_{10} (Distance to Nearest Nonconforming Use)
 + β_{11} (Distance to Nearest Nonresidential Zone).

An interpretation of the coefficients (β) is that they represent the implicit prices for these characteristics. A check on this interpretation is to see whether the coefficient jibes with the price of construction. If the dummy for a garage is \$15,000, and it is known to cost about that amount to build a garage on a home, the price interpretation seems reasonable. (A situation in which it might not be reasonable is when the main bidders for the house want to convert it to another use, in which case the garage might contribute no value because the buyers intend to tear it down.)

The most important and significant coefficient across all housing regressions is that for the measure of floor space of the house. This is consistent with builders' practice of estimating the cost of residential construction by the square foot. There is, however, a question of just how much can be inferred from the regression coefficients. There is an ongoing and seemingly inconclusive debate about the proper specification and economic interpretation of hedonic price models.⁴

For most of the studies reviewed here, econometric issues of interpretation are not critical. All that most researchers want to know is whether a given variable, such as a dummy for presence of growth controls, is significant. The usual test for this is that the absolute value of the t-ratio of a coefficient (the estimate for β divided by the standard error of estimate for that variable) must be about 2.00 or more. This means that there is only a five-percent chance that the true coefficient has a distribution that is centered on zero. While the five-percent confidence interval is the usual test for acceptability, I prefer to report the t-statistic when it is a little less than that and let the reader decide whether he or she is persuaded by the results.

The overall explanatory power of a regression is indicated by its R-square. An R-square value of, say, 0.89 (which is considered rather high) indicates that the independent variables collectively account for 89 percent of the variation in house prices. Higher R-square values are normally found in regressions on data from individual homes observations rather than averages of communities or census tracts, partly because the data are more accurate than those from census

⁴. See Epple (1987) for general interpretive issues and Schwartz and Zorn (1988) for other problems specific to evaluating growth controls.

data and partly because more quantifiable variables can be obtained from individual appraisals. An abundance of data can have drawbacks; most research ignores Zvi Griliches's tongue-in-cheek law that more than five variables in a cross-section analysis yields garbage.⁵ One regression's numerous independent variables included a count of built-in kitchen appliances, which may well have included the sink.

Regressions using real estate values other than single-family homes are less conclusive in determining the significance of land use constraints. Commercial and multifamily properties and open land are often subject to influences less easily predicted than those that affect single-family homes. For example, Peterson (1978) and Pyle (1985) both show that farmland sales are affected by personal characteristics of the owner as well as by external economic factors. The markets for nonresidential property are also thinner, so sufficient data about prices and characteristics are harder to obtain. This may explain the more frequent focus on single-family homes in the literature.

A final note on methods concerns the reliability of measures of land use constraints. Most studies of growth controls and zoning take the legal rules as if they were binding. A rezoning from one-acre minimum lot size to four-acre minimum lot size is thought to reduce the capital-to-land ratio and hence reduce the value of the rezoned parcels. It may not, for any of these reasons:

- The new zoning law may not be enforced.
- Variances and exceptions may be forthcoming for the asking.
- Bribery may be rampant.
- Above-board exchanges, such as offers to pay for some municipal projects, may easily overcome the rules.
- The zone may be largely developed so that the actual amount of land affected may be small.
- The new larger lot size may have been the norm previously, enforced by subdivision regulations or informal norms, so that the rezoning has no effect other than to bring legal rules in line with longstanding practice. On the other hand, this

⁵ Richard Butler (1982) reports that house value estimates using four independent variables (number of rooms, age of house, and two variables that measure quality of housing) are statistically indistinguishable from estimates of the same sample using eleven independent variables. There is a consensus, however, that the failure to include relevant variables (an example of specification error) can be important, so that very parsimonious equations may also be deceptive.

also suggests that apparently loosely regulated communities may in fact be exclusive; developers know that it may be nearly impossible to build despite the presence of an apparently permissive zoning law.

The aforementioned possibilities can be anticipated by getting to know the regulatory system and the area that one investigates. Hence a serious criterion for evaluating the quality of empirical work on land use controls is evidence that the researcher is well acquainted with the local area, its regulatory climate, and the source of the data. This is sometimes hard to discern, since journal articles underplay descriptions of data sources relative to econometric sophistication. The first rule of empirical work is trite but nonetheless bears repeating: garbage in, garbage out.

§3 THE ZONING-DOES-NOT-MATTER STUDIES

A well-known group of econometric studies concludes that zoning is not justified on efficiency grounds because urban spillover effects are inconsequential. These studies also often find that zoning regulations themselves have no systematic effect on property values. Acceptance of these contentions undermines both positive and normative research on growth controls. If land use regulations have essentially random effects, there is not much a social scientist can say about them. If spillovers are in fact negligible, then even if one accepts that land use regulations are real constraints, the normative conclusion must be that any effects one finds are inefficient. Because the present survey is concerned with both positive and normative evaluations of land use controls, it is appropriate to begin with the studies that call the whole enterprise into question.

Among the most persistent researchers along this line have been Jonathan Mark and Michael Goldberg (1986). Their study is among the most thorough of the genre that concludes that zoning and spillover effects do not matter. They explicitly state that their results "call into question those previous findings such as Jud [1980], Peterson-[July 1974], Plosser, Avrin [1977] and Stull [1975]," who find spillovers and land use classifications do matter, while "supporting empirical studies which query the basis for zoning such as Crecine, Davis and Jackson [1967]; Rueter [1973]; Maser, Riker and Rosett [1977]; and Grether and Mieszkowski [1980]" (1981, p. 271).⁶

Mark and Goldberg obtained detailed data from two neighborhoods within the city of Vancouver, British Columbia.⁷ One was affluent (Kerrisdale) and the other poorer (Fraser). The observations were single-family homes that had been sold sometime during the period 1957-80. The data included the usual structural characteristics such as age, number of rooms, and lot size, plus information about land use. The land use data are separated into three categories: (1) the

⁶ See also Ihlanfeldt and Boehm (1987), whose conclusions agree with the zoning-does-not-matter group. John McDonald's (1979) study of land values in Chicago also casts doubt on whether zoning classifications matter for property values. Chicago is special for two reasons: (1) Zoning is surreptitiously bought and sold in that city more than in most others, so that observed classification and actual classification are hard to tell. (2) Illinois courts are regarded as the most prodeveloper among the states, which makes zoning more easily changed by legal payments. Ellickson and Tarlock (1981, p. 76) specifically suggest that McDonald's results may not be extrapolated for the latter reason.

⁷ Mark and Goldberg have in previous work argued the same point with other Vancouver data (1981).

zoning of the home itself (some homes are in nonresidential areas); (2) nearby land uses that might affect the value of the home in question; (3) whether a rezoning had occurred during the 23-year period prior to 1980.

Mark and Goldberg then regressed the sale price of individual homes on these physical characteristics, neighborhood characteristics, and various regulatory classifications. Separate regressions were done for the two neighborhoods on the theory that spillover effects may affect the rich and poor differently. They also did separate regressions for each of the 23 years in their sample because they wanted to know whether the observed land use coefficients were stable over time.

The regressions tested three hypotheses: (1) A home's zoning classification affects its price. Their results provide weak support for this, but sometimes the classification effect was positive and at other times it was negative. (2) Proximity to nonconforming uses adversely affects the values of single-family homes. Mark and Goldberg's results indicate that sometimes there is an adverse effect, but at other times the effect is beneficial. Proximity to nonconformers occasionally raises single-family-home values. (3) A change to more permissive zoning increases a parcel's value. Their results indicate that there is little support for this.

The principal advance of Mark and Goldberg's paper over previous work with similar conclusions was to show that the effects of zoning and land use variables are inconsistent over time. The land use coefficients vary from year to year, significance is often low, and often the coefficients change signs. The authors conclude that urban externalities are not a sufficient basis for zoning because they seem not to be consistently important, and that zoning itself has a most ambiguous effect on parcel value, sometimes raising it, sometimes lowering it, but often having no apparent effect at all.⁸

The chief problem with this study is one that it shares with most of the predecessors with which it agrees. It takes zoning classification, reclassification, and presence of nonconforming uses as events that are exogenous to those affected by them.⁹ Mark and Goldberg's experiments would be valid and their conclusions correct if some bureaucrat in Washington decided what zoning classifications and exceptions were permitted. (I know Vancouver is in Canada; the political

⁸. To the contrary, Chressanthis (1986) found that zoning variables were stable (though sometimes anomalous) determinants of housing values in Lafayette and West Lafayette, Indiana, 1960-80.

⁹. McMillen and McDonald (1990) present evidence that zoning is endogenous to the transportation system in Chicago suburbs. In the same spirit, Barbara Rolleston (1987) explores the determinants of New Jersey suburban zoning restrictions.

remoteness of the decision is the point.) I assume, however, that zoning decisions in Vancouver are made similarly to those in large U.S. cities.¹⁰ Such decisions are local political ones, which elected officials or their appointees make in the face of pressures from such groups as developers, owners of abutting properties, housing advocates, and the construction industry, and sometimes even their own professional planning staff.

Such a political decision is best explained through a political model. Consider a simple one in which all zoning decisions, including whether to allow nonconforming uses to continue, are made at the behest of owners of single-family homes in the immediate area. (It is assumed that all neighboring homeowners agree about the spillover effects of nearby nonconforming uses, and that renters and their landlords have negligible political power.) In this situation, Mark and Goldberg's regressions should not find any significant effects on home values from nearby nonconforming uses. A single-family home next to a store would not be reduced in value because the homeowner would have objected to the existence of the store if it would have reduced the value of her home. She might not have objected because the convenience of a nearby store offsets the disamenities of increased traffic, or because the store owner provides some continuing benefit to his neighbors, such as maintaining a nearby park. Unless such offsetting benefits are accounted for, the fact that the nonconforming use does not reduce nearby home values is consistent with spillover effects being potentially important and zoning working well to internalize them.¹¹

Observed zoning classification would not, in the homeowner-domination model, have any statistically significant effect on home values for the same reason. A neighborhood whose residents wanted the option to convert their homes to stores or apartments would ask the authorities to rezone it for commercial purposes, and the prospect of converting to the more profitable use would be reflected in higher home values.¹² A different, perhaps more affluent neighborhood might insist

¹⁰ I infer that Vancouver's zoning is determined by methods similar to those of U.S. cities from Mark and Goldberg's willingness to tack their results in this article onto previous studies that used U.S. samples. In another paper, Mark and Goldberg (1985) cast some doubt on the appropriateness of comparing U.S. and Canadian cities, though they do not compare zoning administration in the two countries.

¹¹ An exception to the statement in the text occurs when the store owner makes a lump-sum payment to compensate the neighbors. Later buyers of nearby residential property will purchase at a discount reflecting the nuisance, even though the spillover is internalized. In reality, most compensatory payments are made as annual in-kind benefits, so that the residential property is increased to its former value by the prospect of an indefinite flow of benefits (Fischel 1985, chaps. 4 and 11).

¹² This idea is expounded with some empirical evidence supporting it in Grieson and White (1989), as well as in Peterson (July 1974).

on maintaining the residential character of its neighborhood and hence demand zoning that allowed only single-family homes. In both cases, zoning classification should not appear to matter because the classification was chosen by those subject to it.

Such a simple political model of zoning is not entirely realistic for Vancouver. Zoning decisions in large cities are subject to the pressures of other interest groups, such as developers and labor unions. As a result of the occasional prevalence of such interest groups over homeowners' wishes, sometimes we will observe that nonconforming uses or changes in zoning laws adversely affect single-family home values. Lacking a political model of how zoning decisions are made, however, Mark and Goldberg's results appear to be random and inconsistent, when in fact they may be rational and politically predictable.

The ambiguous effects of nonresidential use that Mark and Goldberg detected are consistent with the proposition that zoning in Vancouver works tolerably well. If zoning guides the necessary nuisances to places where they will do the least (but not necessarily zero) harm to nearby properties, and if it allows innocuous exceptions that may even upgrade the neighborhood (e.g., the small neighborhood deli), zoning is a success. Thus Mark and Goldberg's findings are consistent with exactly the opposite of what they infer from them. This is not to say that I believe zoning works so well, as many who have sought a variance for a small deli can attest. It is just that Mark and Goldberg and others have not shown that land use regulation fails to do what it is supposed to do: internalize external costs.

On the other hand, one of Mark and Goldberg's more precisely defined nonresidential categories is consistent with the proposition that single-family homes are adversely affected. The most robust result (as they note on page 270) is the influence of multifamily dwellings on the same or nearby blocks in the affluent Kerrisdale section. The proximity of apartment houses in the same or nearby blocks lowers home values, often significantly, in 25 of 28 reported regressions. (The effects in the poorer neighborhood were ambiguous.) Given the many different types of multifamily dwellings—a luxury co-op and a low-income housing project would both qualify—that

consistency seems remarkable and helps explain the well-known antipathy of single-family homeowners in well-to-do areas toward rezoning for apartment dwellings.¹³

The other challenge to zoning does not claim that it is ineffective. Rather, it asserts that most of zoning's benefits could be provided by private covenants and nuisance laws. Bernard Siegan's (1972) study of Houston seems to support this claim, insofar as covenants are widely used in that city in lieu of zoning.¹⁴

The covenant alternative would be most persuasive if it were easy to establish covenants in developed areas that lacked them. Janet Furman Spreyer (1989) demonstrates that they are not. Spreyer found a rare sample that enabled her to compare a zoned area with an unzoned area. The city of Houston, which still has no zoning, surrounds two municipalities that do have zoning. Within Houston are neighborhoods that are subject to private covenants and some that are not. Using a sample of homes that included all three arrangements—(a) zoned, (b) unzoned but covenanted, and (c) unzoned and uncovenanted—Spreyer was able to determine which types of property were most valuable, after accounting for the usual differences.

Spreyer found that unzoned but covenanted properties in Houston had the same value as properties in the zoned cities, but that properties that were both unzoned and uncovenanted were significantly less valuable than either of the others. In symbols, the value of (a)=(b)>(c). (To be precise, Spreyer found no statistically significant difference between (a) and (b), not perfect equality.) This could provide arguments for both sides: People who favor zoning can say it performs as well as covenants but is more easily altered, while people who dislike zoning can say that covenants do just as well as zoning with none of the governmental coercion.

Both debating points miss the cost of forming covenants. If it were easy to establish covenants in already-developed areas (or in areas in which covenants had expired—courts usually

¹³ Grether and Mieszkowski (1980) also found that New Haven public rental housing had a negative impact on nearby homes, although private garden apartments did not. The effects of public housing seem to depend on how it is done. Nourse (1963) found that replacing slums with (presumably large-sized) public housing did nothing to upgrade the neighborhood, while Rabiega, Lin, and Robinson (1984) found that in Portland, Oregon, new small-scale public housing units actually raised home values in the adjacent neighborhoods. Such frustrating (for researchers) ambiguity extends even to parks. A detailed study by Weicher and Zerbst (1973) found that having a park in the neighborhood is desirable, but being right next to a heavily used urban park may actually lower a residential property's value.

¹⁴ Richard Peiser (1981) attributed Houston's lower housing prices to its lack of zoning. This does not itself support Siegan's proposition, since the lower prices may have been caused by uncompensated nuisances that zoning might have prevented.

limit their duration to less than fifty years), one should observe that uncovenanted land (c) has the same value as that subject to covenants (b). All it would take to convert uncovenanted land to covenanted land would be some legal fees to record the covenant in each deed. Since legal fees per housing unit would presumably be small, the reduction in land value caused by lack of covenants should be trivial. Spreyer's result, then, shows that covenants are costly to establish. This is intuitively plausible. Transaction costs of getting *all* people in an existing neighborhood to agree to restrict potential uses of their property are usually prohibitively high.

Simply to say that some zoning may be justified by the lower transaction costs of establishing it is not to say that any type of zoning is justified. Moreover, Spreyer's exclusive attention to single-family homes precludes any efficiency analysis. Lack of covenants might have permitted multifamily homes or commercial uses with values that could have been much greater than the single-family homes they displaced or devalued. But Spreyer's results do suggest that covenants are a realistic alternative to zoning only in undeveloped areas in which a single developer can acquire enough land and impose restrictions prior to sale of the homes themselves.

§4 CAPITALIZATION OF AMENITIES AND DISAMENITIES

The previous section questioned the interpretation of studies that conclude that spillovers and land use constraints do not systematically affect property values. The reader who is skeptical of my view that land use controls are the result of some rational political process might want evidence that spillovers and zoning do matter. A number of studies show this to be the case, but they leave me in an apparent dilemma. If my previous argument that zoning reduces spillover effects is valid, what am I to make of evidence of the existence of spillovers despite the presence of zoning? Wouldn't it show that my theory is wrong? Not necessarily. Here are some reasons that one may be able to detect spillover effects on single-family homes even in communities with a long history of effective zoning. (I discuss the second horn of the dilemma, that rational zoning should not appear to be a constraint, at the beginning of the next section.)

- (1) Sometimes homeowners do not get their way. This is most common in large jurisdictions in which homeowners are but one of several competing political interest groups, but it is also possible in small suburban communities for municipal authorities to cave in to developer pressures, sometimes from an unfriendly prod from state courts or a too-friendly relationship with the developer.
- (2) Nonconforming uses that predate the establishment of a zoning ordinance are normally permitted to continue. While there is nothing in the common law or the U.S. Constitution that prevents a community from shutting down nonconforming uses, it is not usually done unless the nonconformer is unusually offensive. (Termination of the most offensive uses does, however, tend to censor the observed sample so that only relatively benign ones remain.)
- (3) Some types of nonconforming uses are outside the control of the community. Construction of airports, schools, prisons, hospitals, and some other public facilities may be beyond the power of the community to stop.
- (4) Even in tightly zoned communities, lines between uses have to be drawn somewhere. so there is often a border effect to observe. (But borders may be drawn in such a way as to reduce the spillover effects. Wide physical barriers such as cemeteries, rivers, transportation corridors, and hill ridge lines are often used as zoning district lines.)

- (5) Developers proposing a nonconforming use may sometimes simply pay the residents who are adversely affected to obtain their consent. Cash payments are rare, but barter arrangements, including capitalizing on neighborly behavior, may be common in cases involving special exceptions and variances (as opposed to wholesale rezonings) in which the zoning board's inclination to grant the variance depends very much on what the neighbors think (Tideman 1969). If the developer's compensatory flow of benefits can be isolated from the undesirable spillover effect, then spillover effects will be detectable in current property values. (The exception is when the developer's payment is a lump sum. See note 11 above.)

The effects of local land use spillovers may be examined with a paradigmatic study. Mingche Li and H. James Brown (1980) obtained a sample of 781 single-family homes in fifteen suburban Boston communities in 1971. Observations included detailed geographic data as well as structural characteristics and public sector variables. Their results indicate that public amenities and neighborhood characteristics are important determinants of housing values. This is hardly surprising, of course; nearly all studies of housing values indicate that amenities and disamenities are capitalized into housing prices.

The harder question is whether amenities that can be controlled by local land use regulations are important. Air pollution is known to be a determinant of housing values, but because it spills over municipal boundaries, it is largely beyond the control of local legislation.¹⁵ Housing deterioration also affects nearby properties, but there is little that local ordinances can do to control it. But noise levels, visual amenities, and distance from commercial and industrial establishments are all subject to some measure of local control. Li and Brown find that each of the latter factors does matter.¹⁶ I would point out that the coefficients they observe are probably lower than they would be if the amenities and disamenities they measured were truly random variables,

¹⁵ The seminal air pollution paper is Ridker and Henning (1967), reviewed in context by Myrick Freeman (1979).

¹⁶ Corroborating evidence on localized amenity/disamenity effects of land use decisions normally subject to local zoning is supplied by: Blomquist (1974) on a power plant in Winnetka, Illinois; DeSalvo (1974) on middle-income housing in New York City; Gabriel and Wolch (1984) on rehabilitation centers in Oakland, California; Harris, Tolley, and Harrell (1968) on nonconformers in Raleigh, North Carolina; Kain and Quigley (1970) on industrial uses in a national sample; King (1973) on nonconforming uses in New Haven, Connecticut; Langley (1976) on Washington, D.C., beltway traffic; Nourse (1963) on public housing in St. Louis, Missouri; Poon (1978) on railways in London, Ontario; and Rabiega, Lin, and Robinson (1984) on housing projects in Portland, Oregon.

but their results confirm that the spillover effects that land use regulations are supposed to control are important.

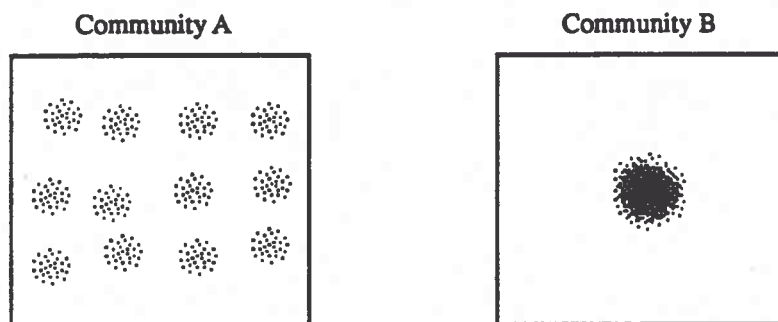
Li and Brown were not specifically interested in the effects of land use controls; their contribution was to add to the evidence that literally construed neighborhood effects are important. An important pair of studies by William Stull (1975) and Ronald Lafferty and Ted Frech (1978) does address the interaction between neighborhood spillover effects and local land use controls. Their purpose was to challenge the claims of the Crecine, Davis, and Jackson (1967) tradition, in which the previously discussed Mark and Goldberg study is firmly rooted.

Using median home values from a 1960s sample of 40 suburban towns in the Boston metropolitan area, Stull found that nonresidential uses did adversely affect home values. Specifically, the proportion of community land devoted to multifamily dwellings and to industrial land uses lowered single-family home values. Stull also found that the proportion of land devoted to commercial uses raised median home values when the proportion was low, but that larger proportions lowered home values. His interpretation of this is that community residents like a little commercial development for the convenience of shopping nearby, but regional commercial centers are regarded as a net cost.

Lafferty and Frech undertook to replicate Stull's study with the same sample of Boston suburbs, but they added an insightful twist. They pointed out that the avowed purpose of traditional zoning was not to exclude entire categories of uses, as Stull seemed to imply, but to concentrate them in contiguous zones. Zoning is, in principle, a municipal good-housekeeping rule: a place for everything, and everything in its place. According to Lafferty and Frech, the Crecine, Davis, and Jackson approach (as described in my discussion of Mark and Goldberg in section 3 above) has too narrow a focus. It only looks at individual property effects, to the neglect of community-wide benefits and costs. Their critique of Stull is that he looked only at community-wide effects, neglecting the adverse effects of nonresidential properties on individual properties.

To rectify this, Lafferty and Frech employed Stull's sample with an additional variable that measured the *dispersion* of nonresidential land uses. If traditional zoning theory is valid, a community that is half nonresidential but with all stores and factories concentrated in a single district will have higher average home values than similar communities that have permitted their nonresidential uses to be dispersed among the housing developments. (In fact, of course, most of the dispersed nonresidential uses might have predated the existing zoning ordinance.) In Figure 1, Community A should have lower average residential values than Community B.

Figure 1



Dark areas are nonresidential uses.

Lafferty and Frech found that their dispersion index was an important overall determinant of housing values. The more dispersed a community's nonresidential land uses, the lower the value, on average, of single-family homes. The good-housekeeping theory of zoning is thus supported although the significance level of several individual variables was doubtful.¹⁷

Inclusion of the dispersion indexes by Lafferty and Frech in the regression also changed the signs of some of Stull's aggregate values. Stull, who did not use any dispersion indexes, found that the proportion of apartments almost significantly ($t=-1.6$) lowered median house values, while Lafferty and Frech found that when dispersion was taken into account, apartments had no significant effect on home values. The same was true for industrial uses and for vacant land: Stull found a significant negative impact on home values; Lafferty and Frech found no significant effects.

While Lafferty and Frech (and the other authors mentioned in this section) did not address the issue of community growth controls, the implication of their results would seem to be that growth controls on nonresidential land uses have negative effects on house values. With the exception of large commercial uses, all of the nonresidential variables they included had a positive (but often not significant) effect on single-family home values. The authors conclude that such uses are beneficial if they are properly concentrated within the community's boundaries (1978, p. 387).

¹⁷. Only two of their dispersion indexes were significant at the conventional five-percent confidence level (t -statistic > 1.95), but all of them had a negative impact on median single-family housing values. (The significant variables were dispersion of institutional and of vacant and agricultural uses; the nonsignificant variables were multifamily, commercial, and industrial.) Burnell (1985) confirmed the Lafferty and Frech study for a Chicago-area sample.

The rationale that undergirds traditional zoning—separating uses by districting—is rational from homeowners' perspectives.¹⁸

One issue raised by the studies of Stull and Lafferty and Frech is the effect of undeveloped land on nearby single-family home values. In Stull's specification, a higher proportion of vacant and agricultural land reduced the average value of housing; in Lafferty and Frech's specification, the dispersion rather than the proportion of vacant land reduced the value of housing. (Dispersion is highly correlated with proportion for this variable, though.) Both findings seem entirely at odds with modern growth control rhetoric, in which preservation of open space is the *summum bonum*. If we are to take these results literally, then arguing against further residential development is irrational in the sense that maintaining open space seems to *reduce* average home values.

The problem with this interpretation (which may, even so, be a correct conclusion) is that, in the samples used, vacant land is a residual category that includes swamps and military bases as well as golf courses, parks, and wildlife refuges. Its negative impact, Stull surmises, is due to the uncertainty of future development rather than undesirability of the current use itself (1975, p. 546). Anxieties that the open space may be developed with uses that adversely affect their properties may outweigh homeowners' current enjoyment of open space. In other studies, permanently established greenbelts in other areas of the country are found to have a positive effect on nearby home values, which comports with the rhetoric of most suburban growth control advocates.¹⁹ Thus one cannot infer that growth control is irrational from the Boston suburban studies, though it seems reasonable to surmise along with Bernard Frieden (1979) that the enthusiasm for open space may often stem from a dislike of a specific proposal than an affection for open space itself.

An important but seldom noticed factor in the zoning and externalities debate is the disparity between studies based on central-city samples and those based on suburban samples. Those that have a hard time finding systematic spillover effects draw almost exclusively on central-city samples. Those that find that spillovers do matter rely mainly on suburban communities. The

18. A study in the same spirit of Lafferty and Frech's is by Theodore Crone (1983), who asked whether separation of homes and apartment houses resulted in higher land values in Foster City, California. He concluded that separation would not raise aggregate property values, so that zoning was not justified. Crone's results are less convincing because Foster City seems to have been a privately planned community, so that its developer would have taken into account the effect each type of use had on aggregate land values. Foster City is thus not a natural test of market failure from decentralized landownership. Lafferty and Frech do not suffer from this because they had a variety of communities in which the distribution of nonresidential uses was at least to some extent determined without any centralized coordination.

19. Correll, Lillydahl, and Singell (1978) found this for Boulder, Colorado, and Nelson (1988) found a similar effect for Portland, Oregon.

difference may be caused by the higher income and higher homeownership rates in the suburbs. If neighborhood quality is not an inferior good, spillover effects should register more clearly in higher-income communities and neighborhoods.²⁰ Studies of the effects of spillovers and zoning can differ according to the geographic area that is sampled.

Mills and Hamilton (1984, p. 208) mention two additional reasons it is often hard to detect spillovers. One is the "different-strokes" argument: The market is large enough to accommodate different tastes, and some people like to live in a mixed-use neighborhood. This is a reasonable proposition. Mixed uses are common in big cities, and people who like them will move there, bidding up the price of housing in mixed-use neighborhoods. Although there are spillovers from such uses, they are not universally disliked. Families with children, on the other hand, often prefer homogeneous residential neighborhoods, and they will move to the suburbs. What nonconforming uses one finds in suburbs are apt to cause at least some reduction in residential property values.

Mills and Hamilton also note that having a nuisance, such as a run-down building, next door is apt to induce a homeowner to reduce maintenance on his own place. Such reduced maintenance may not be separately accounted for by researchers. Mills and Hamilton conclude that reduced quality of housing will explain the variation in housing prices, leaving no evidence of the nuisance that originally caused the quality reduction.

I disagree. There is a difference between purchasing a run-down house in an otherwise good neighborhood and purchasing a run-down house in a run-down neighborhood. In the former case, fixing up the house will restore its value, while in the latter, fixing the house will still leave one with a sound home devalued by nearby nuisances. Hence the latter house is apt to sell for less than the former, and conventional hedonic regressions should be able to pick up the effects of the nuisance, provided one can find a sample with the variations just described.

²⁰ I sketch a political model that explains the suburban and central city difference in my book (Fischel 1985, chap. 10).

§5 CAPITALIZATION OF REGULATIONS IN LAND VALUES

This section reviews studies that ask whether land use controls themselves are a constraint. As I mentioned at the beginning of the previous section, the notion that zoning may be the result of a rational political process suggests that the effects of zoning might not be detectable. Whenever zoning was too restrictive and devalued some land, the owner of the devalued land who wanted to use it in a more valuable manner should be able to (legally) buy his way out of the inefficient restrictions (by providing benefit to those adversely affected by the new use). Hence after such trades have taken place, there should be no apparent premium for land zoned for one use or another.

My answer to this is that the transaction costs of repurchasing rights held by the community are very high. Some trade does go on, but not enough of it to eliminate the premiums for higher intensity uses of land that the studies below show. I discuss this Coasian theory of zoning and the nature of the transaction costs that prevent its full fruition in my book (Fischel 1985, chaps. 4 and 5), so there is no reason to pursue it here, where empirical findings are reviewed.

To narrow the field, only studies with samples that include undeveloped land as a dependent variable are examined here. Undeveloped land should show the cleanest results for land use constraints. (Housing price studies are reviewed in the next section.) The focus on undeveloped land also provides some control on the political model. Owners of undeveloped land are often outvoted by owners of developed property, so there is more reason to assume that observed constraints on undeveloped land will continue to remain in place rather than change at the behest of the owner.

Adams, Milgram, Green, and Mansfield (1968) had some of the earliest and least ambiguous evidence that zoning influences land values. The City of Philadelphia built up its northeast section during the period 1945–62. Adams et al. obtained a sample of more than 1000 transactions of subdivided but undeveloped lots of at least ten acres during that period. Their study

established clearly that land zoned for single-family homes sold for less per acre than that zoned for row houses or apartments.²¹

The Adams et al. study was chiefly aimed at the question of how quickly land values rose in response to expected urban development. They found that although the land had become much more valuable as urbanization occurred, most of the increase in value had occurred prior to the beginning of their observations in 1945. Over the period of their study, they argue that the net return on owning land was no greater than on stocks and bonds and similar alternative assets (1968, p. 254). This is important because it shows that developers did not make extraordinary profits as holders of land.²² Previous landowners who anticipated the growth of the city must have reaped most of the windfall.

The previous research addressed conventional zoning restrictions in single jurisdictions. Two recent studies have looked at the more modern type of restrictions. Vaillancourt and Monty (1985) examined the effect of exclusive agricultural zoning on the value of land in a Montreal suburban fringe area, about fifteen miles from the central city. The agricultural zoning was imposed by provincial law in 1978. Although some exceptions are allowed in negotiations between local and provincial officials, the authors describe the plan as one of the strongest in North America. Using a sample of 1200 sales of land from 1975 (prior to the agricultural zoning) to 1981, they found that the parcels subject to the new restrictions lost between fifteen and thirty percent of their value compared to similar unrestricted land.

Although such a loss may not seem surprising, it does undermine one of the rationales for legislation to preserve farmland. It is argued that without farmland preservation laws, scattered development will impinge on existing farms and raise the cost of their operations.²³ (For example,

21. That larger minimum lot sizes reduce the price per acre of land is also supported by Paul Downing's (1970) study of undeveloped residential land values in Milwaukee in the 1960s. While lot size by itself was not significant in Downing's study, being in a zone that allowed for higher density was highly significant and added considerably to the value of the land. Michael Gleeson's (1979) study of Brooklyn Park, Minnesota, a suburb about twenty miles north of Minneapolis, also found convincing evidence that its 1963 growth management program depressed land values in the restricted area relative to the permissive area.

22. Studies of national housing markets by Guntermann and Smith (1987) and of the Philadelphia metropolitan area by Linneman (1986) suggest that they are efficient in the rational expectations sense, from which they infer efficient land markets. But Hamilton and Schwab (1985) found that this hypothesis could not be maintained during the inflationary period of the late 1970s.

23. Berry (1978) first argued that development hinders farm operations, and it has become a staple of farmland preservation legislation. Contrary evidence is presented by Nelson (1988) and Lockeretz (1989).

the children in the subdivision may annoy the neighbor's cows.) Thus, one rationale for exclusive farmland zoning is that it may raise the value of farmland by eliminating possible urban interferences. This offsets the loss due to restrictions on use of individual properties. The empirical evidence presented by Vaillancourt and Monty makes it clear that if farmers gained legal protection from urban nuisances, it did not offset the loss of development rights.²⁴

Gerrit Knaap (1985) conducted a study of the Portland, Oregon, urban growth boundaries. This is of special interest because it involves a modern program of comprehensive growth controls. Oregon state legislation requires that urban areas define growth boundaries around their perimeters to prevent sprawl and preserve farmland. Portland has two such boundaries: an outer perimeter beyond which no development is allowed until the year 2000 and an inner boundary outside of which development can be forbidden at local option until the land inside has been fully developed.

Knaap's 455 observations (from a sample taken during 1980) were undeveloped sites zoned for single-family homes. They were drawn from inside and outside both boundaries. His results showed that in the two Portland-area counties (Washington and Clackamas), land outside the outer growth boundary sold for significantly less than land inside the boundary. Because Knaap had already accounted for distance from the Portland CBD separately, he inferred that the twenty-year delay in development was perceived as a binding constraint and reduced the land values outside the outer boundary. His results for the effects of the inner boundary were mixed. In Washington County, the more affluent of the two counties studied, the inner growth boundary was shown also to be a constraint, but this was not so in Clackamas County. I think this inconsistency may be explained by the fact that the inner boundary may be breached by local government exceptions, which may have been more readily forthcoming in less affluent and more rural Clackamas County.

Knaap's study establishes that timing controls such as Portland's twenty-year delay can have the same effects on the land market as conventional controls such as minimum lot size. (Lot size zoning was also included in Knaap's regressions, but it was significant only in the Washington

²⁴ Henneberry and Barrows (1990) found some evidence that exclusive farmland zoning did raise the value of large, remote farm parcels in small Wisconsin communities. The conflicting findings may be reconciled by the political differences in the programs. In Quebec, the province required the zoning regardless of local wishes, while Wisconsin's program allowed rural townships, in which farmers were usually a majority, to adopt zoning according to local conditions. As Henneberry and Barrows note, farmers would be loath to devalue their principal asset with unwarranted regulations.

County regressions.) This increases my confidence that growth control policies will be reflected in land values.²⁵

An earlier study of growth controls is a widely cited paper by George Peterson (November 1974). He obtained a sample of almost all vacant, residentially zoned parcels that sold in Fairfax County, Virginia, (near Washington, D.C.) during the period 1969–73. He estimated price per acre of each parcel with and without the zoning constraints included. The addition of zoning constraints raised the explanatory power of the model considerably. The R-square rose from about 0.50 to about 0.60 when the permissible number of housing units per acre and interactions between zoning and other variables were included. This provides evidence that zoning influences land values.

The unusual aspect of Peterson's sample is that Fairfax is a large suburban county of 406 square miles and had half a million people in 1970. It thus controls a large fraction of suburban land supply. Peterson calculated price per acre effects by distance from Washington. He found that on land next to the city limits, there was a seven-fold difference between land zoned for twenty units per acre and land zoned for one unit per acre. On land fifteen miles from the District, this differential shrunk to "only" a three-fold difference between twenty units per acre and one unit per acre.

This answers one objection to zoning studies, which is that even in the absence of zoning, lots would be larger in the suburbs. This is true, but in Peterson's sample, the large minimum lot sizes were still a binding constraint in the farther suburban areas of the county. Moreover, this finding shows that restrictive controls applied to a large fraction of suburban land can have significant effects on urban structure, pushing development to remote locations as close-in development is precluded.

Peterson's observation period overlapped the beginning of Fairfax County's sewer moratorium, which began in late 1972. This was one of the earliest growth control ordinances. Peterson found that by 1973, the sewer moratorium's effects radically changed his model's estimated effects. Having a grandfathered, and thus permissible, sewer connection pushed the value of a lot way up, while the implicit value of other characteristics, such as proximity to the

²⁵ Arthur Nelson's (1988) study of land values outside of Portland's urban growth boundaries confirms Knaap's results (without citing him). Nelson demonstrates that the greenbelt (farms only) boundary established around Portland's suburbs created a marked discontinuity in the trend in land values, with high values closer to Portland, low values in the greenbelt, and then higher values farther out. This suggests that the greenbelt caused some exurban sprawl by forcing development that would have located in the greenbelt still further from the city.

CBD, actually fell. Even his measure of permitted land use intensity, the zoning variable, became much less significant. This suggests an important override effect of growth controls. The existence of new controls may reduce the apparent importance of traditional zoning.

A problem with studies of minimum lot size zoning is that technological as well as zoning constraints may cause larger lots to be lower in value per acre. It is commonly observed that smaller undeveloped lots, especially those near the urban-rural fringe, are worth more per acre (Chicoine 1981; Hushak 1975). If smaller lot size itself raised the value of, say, a 160-acre parcel, farmers could make themselves rich simply by subdividing such parcels into 160 one-acre plots, continuing to farm the whole field as before. Because we do not see farmers routinely doing this even when no zoning law prevents it, we must conclude that the higher value of smaller lots must result from other conditions that make it profitable to subdivide.

One constraint on profitably subdividing property is the cost of infrastructure. If a developer subdivides the 160-acre farm into 640 quarter-acre lots rather than 16 ten-acre lots, more roads, sewers, utilities, and legal fees will be required. It is important to understand that subdivision costs *per acre* rise as the lots are made smaller even though the subdivision cost *per lot* generally falls because there are more lots to spread the fixed costs across.

The upshot of the previous consideration is an identification problem. Is the smaller price per acre of the larger lots caused by the zoning restriction (implying that the developer would like to build on smaller lots but is not allowed to)? Or, is it caused by the subdivision costs (implying that the developer has chosen the size according to profit-maximizing considerations)? This question was addressed by a thorough study of land prices in the famous town of Ramapo, New York, by James White (1988).²⁶

White's sample consisted of over 200 sales of vacant lots zoned for single-family residential use in the period 1977-80. He attempted to separate the subdivision-cost effect from the zoning-constraint effect by two methods. One was to compare transactions of unsubdivided with subdivided land, statistically controlling for other factors that might cause price differences. White concluded that subdividing a parcel into quarter-acre lots rather than into one-acre lots costs nearly three times as much per acre. However, this cost is a minor factor when compared to the zoning

²⁶ The fame resulted from Ramapo's early attempt to limit growth according to its schedule for developing its public service capital facilities. This was upheld legally in *Golden v. Planning Board of Town of Ramapo*, 334 N.Y.S. 2d 138 (1972). The town has since abandoned the plan, but it was in place at the time of White's observations.

constraint. Zoning accounted for about three-quarters of the difference in price per acre between quarter-acre and one-acre lots. That is, if a one-acre lot sold for \$40,000 less than four quarter-acre lots, \$10,000 was accounted for by the greater subdivision costs for the quarter-acre lots, and \$30,000 was accounted for by zoning that generated an apparent surplus of one-acre lots. Because White's sample of unsubdivided land was small, he supplemented it with industry cost standards to estimate the cost of subdivision. These confirmed his previous results obtained from market observations.

White's Ramapo results may, however, underestimate the impact of large-lot zoning on land prices. During his sample period, Ramapo had in effect its famous growth management program, which coordinated development with a capital improvement program. Even appropriately zoned and subdivided lots could not be developed until municipal capital improvements were provided (unless the developer wished to provide the necessary infrastructure at its own expense), and this implied delays of several years or vastly increased expense. As Knaap's Portland study demonstrated, delay in development is an additional cost, which would have reduced the value of all land subject to it. Hence White's estimates may be regarded as lower bounds on the net impact of large-lot zoning on land values. As such, they are among the most convincing results that suburban land use controls are important constraints. Large lots are oversupplied and undeveloped land prices (per acre) are reduced by such zoning.²⁷

Especially important is that Ramapo has no monopoly on available suburban land in the New York City area, which has hundreds of different suburbs. This suggests that Ramapo's zoning must have been typical of other suburbs. If Ramapo were the only suburb to adopt large-lot zoning, no price differences should have been detected because developers of smaller lots would simply have built in other communities. (See also the monopoly zoning discussion in section 6 below.)

Another set of empirical findings that land use controls are capitalized in land values looks at land zoned for commercial use rather than just residential lots. Leroy Hushak (1975) found that vacant land zoned for commercial use on the urban-rural border of Columbus, Ohio, was significantly more valuable than other classifications. Paul Downing's (1973) study of Milwaukee commercial values indicated that land zoned commercial was considerably more valuable than that

²⁷ Larry Orr's (1975) metropolitan simulation study and Nancy Wallace's (1988) econometric study of King County, Washington, also demonstrate that suburban jurisdictions typically oversupply large lots relative to market demand.

zoned residential. Asabere and Colwell (1984) found that land zoned for commercial use in Champaign and Urbana, Illinois, had values eighty percent above the average, while land zoned for single-family residential use had values fifty percent below average. David Chicoine's (1981) Will County (southwest of Chicago) results also support this proposition.²⁸

The commercial premium suggests a shortage of land zoned for such uses. If commercial uses had no adverse impacts on other uses, this would be conclusive evidence of inefficient zoning because of the factor-price equalization principle: If demand for one type of use has the same price elasticity as another type, the profit-maximizing allocation of land between the two uses requires that the price of land be the same for both uses. In a perfectly competitive situation, in which price elasticity is infinite (and so the same) for all uses, all land of similar physical characteristics should have the same price.

The only efficiency reason for restricting one use and favoring others is that one use imposes spillover costs on its neighbors. Commercial uses are widely believed to impose costs on residential uses, so they should be made relatively scarce in residential areas. Since commercial uses often gain financially by being permitted near residential areas, it is necessary to use some form of collective control to limit entry. (This assumes that bargaining of the sort described in section 3 above is forbidden or prohibitively expensive.) With such zoning limitations, the price of commercial land will be higher, and that may be efficient. Of course, we do not know how much higher the price should be. There is certainly casual evidence that commercial zones convenient to residential districts are too scarce: Consider how long one has to travel to buy a quart of milk in most modern suburbs. My point here is only that the commercial zoning premium shows only that zoning is a binding constraint. It does not show that such a constraint is efficient or inefficient.

Because large-lot requirements for homes and analogous restrictions on commercial development have effects similar to growth controls (as indicated by Knaap and Peterson, discussed above), we may conclude that the effects of the growth controls entail some inefficiencies. The problem with such a conclusion is that one needs to measure the potential benefits of growth controls. This will be discussed in the next two sections.

²⁸ Each of these results confutes the conventional belief among planners that communities overzone for commerce and industry in order to attract a larger tax base, for if they overzoned for such uses, commercial zoning would not bring a premium. I showed in my doctoral dissertation (Fischel 1975) that such tax base competition does not necessarily cause overzoning for nonresidential property insofar as communities act as rational suppliers of commercial and industrial sites. Evidence supporting this idea is also contained in Erickson and Wollover (1987).

§6 GROWTH CONTROLS AND HOUSING COSTS

The evidence on the effects of land use constraints discussed in the previous section was taken from studies that examined the effect on undeveloped land values. The greater number of studies, however, have looked at the effects of zoning on the value of single-family homes. As I pointed out in the first section, the effects of conventional zoning on single-family home values are bound to be somewhat ambiguous. They will depend on such things as the nature of the nonconforming uses that are prevented or permitted, and the opportunities for conversion to more valuable uses that may be allowed or denied by zoning authorities. There are, however, some communities in which the effect of new land use controls on existing housing values seems reasonably simple to predict. These communities vote to reduce future residential development substantially below the rate that was permitted under previous zoning laws and below the rate that the community's vacant land inventory can reasonably sustain. The intent of all such policies is to reduce the projected annual rate of residential (and often commercial) development.

Such policies have been most prominent in California, and academic entrepreneurs in that state have not failed to respond to the advantages of having a social science experiment in their backyards. A number of studies have been done, and almost all of them conclude that growth controls raise housing prices. The two groups of studies that are reviewed below were selected because they are of recent vintage, they show a good grasp of the econometric issues, and their authors have done or been associated with previous studies of growth controls, so that they know the territory and most of the institutional pitfalls.²⁹

Seymour Schwartz at the University of California at Davis has been a coauthor of numerous studies of growth controls in the 1970s. His first event-study was Petaluma, California,

²⁹. Other California studies include Case and Gale (1981); Dowall (1984); Dowall and Landis (1982); Elliott (1981); Frieden (1979); Landis (1986); Mercer and Morgan (1982); Stanford Environmental Law Society (1982); and Urban Land Institute and Gruen Associates (1977). None of the foregoing except Dowall and Landis, Elliott, and Landis employ regression analysis, and all conclude that growth controls raise housing prices. A regression study of New Jersey communities by Sagalyn and Sternlieb (1973) concludes that zoning increases housing costs, the same conclusion reached by Pollakowski and Wachter's (1990) study of housing costs in the Maryland suburbs of Washington. Other reviews of growth management studies are Lillydahl and Singell (1987) and Deakin (1989).

which, like Ramapo, New York, had enacted a nationally famous growth control law in 1972.³⁰ It was enacted specifically in reaction to new residential development, which was being built to serve commuters who took Highway 101 south toward San Francisco, forty miles away. The Petaluma Plan limited building permits to a maximum of 500 per year, well below recent and expected demand, and it rationed them with a point system that gave substantial weight to costly design features and developer-provided amenities and services to the community.

Schwartz, Hansen, and Green (1981) found that after several years, Petaluma's standard-unit housing prices had risen eight percent above those of nearby Santa Rosa, which had not adopted growth controls during that period and which had formerly had the same prices.³¹ In a follow-up study, Schwartz, Hansen, and Green (1984) found that the fraction of Petaluma's housing stock that was affordable to low- and moderate-income households had dropped significantly below that of a control group. The costly design points offered by the plan overwhelmed the few points that developers got for moderate-income housing.

Schwartz, Zorn, and Hansen (1986) also examined the effects of the less famous but nearly as effective growth controls in Davis, California.³² As in Petaluma, house prices in Davis grew significantly more rapidly than those of a control sample of other Sacramento suburbs after growth controls were implemented. The best estimate given by the study is that growth controls caused prices in Davis to be nine percent higher in 1980 than they would have been without them.

The city of Davis attempted to mandate that the limited number of new units that were built under their controlled-growth program include some units earmarked for low- and moderate-income residents. (As a former resident of Davis, I speculate that they did this to offset the charges that growth management policies are harmful to the poor.) Growth controls were established limiting the number of building permits and requiring builders who did get permits to construct

³⁰. As usual, regulatory fame comes from a landmark court decision, *Construction Industry Assoc. v. Petaluma*, 522 F.2d 897 (9th Cir. 1975), which upheld the plan.

³¹. They did not find any significant difference between Petaluma and nearby Rohnert Park, but that appears to have been because Rohnert Park adopted growth restrictions shortly after as development pressures there increased because of Petaluma's action (Rosen and Katz 1981, p. 334).

³². Davis was also the subject of a court case, *MacDonald v. County of Yolo*, 477 US 340 (1986). MacDonald owned vacant land in Yolo County adjacent to the city of Davis. When he applied to the county for permission to subdivide, the city of Davis successfully petitioned the county to rezone his land for agriculture. The city also refused to annex the land and told the county it would not provide any municipal services for MacDonald's property because of its longstanding controlled growth policy. MacDonald did not succeed in overturning the city's decision in court.

some units earmarked for low-income people.³³ According to Zorn, Hansen, and Schwartz (1986), the Davis program was successful in avoiding the skewness of the Petaluma Plan. The limited growth that did occur contained both low-income and high-income housing.

Apparent success with moderate-income housing was accompanied by an unanticipated offset. Housing built in Davis before growth controls were adopted increased both in price and in quality (more space, better accessories). Zorn, Hansen, and Schwartz expressed some puzzlement at the quality increase. They had expected that older housing in Davis would increase in price as a result of the growth controls, but they did not expect the quality increase. My interpretation of this is that older housing in Davis was filtering up rather than down.³⁴ Since Davis's regulations attempted to suppress construction for the richer side of the market, housing that otherwise would have filtered down was refitted for the more affluent buyers.

The Davis experience shows in a microcosm what econometric studies of public housing have shown at the national level. Construction of a million new public housing units for the poor does not augment the stock of housing for the poor by one million units. Some private housing that would have been built for the poor (e.g., mobile homes) does not get built, and some middle-class units that would have filtered down to the poor are either maintained at a higher level or diverted to other uses.³⁵ The housing market, like other capital markets, is subject to government crowding effects.

The other group of housing price studies was done by Lawrence Katz and Kenneth Rosen at the University of California, Berkeley. A drawback of the previous studies is their focus on two

³³. The tie-in is a frequently employed technique of "inclusionary zoning," and it is the centerpiece of the New Jersey Supreme Court's attempt to promote low-income housing in the suburbs. See *Southern Burlington Co. NAACP v. Mt. Laurel* 456 A.2d 390 (N.J. 1983). A skeptical evaluation of inclusionary zoning, noting that, outside of New Jersey, it is most common in exclusive communities, is Ellickson (1981).

³⁴. An alternative interpretation, suggested by Jon Sonstelie in a letter, is that upward filtering is the result of Proposition 13, which placed a cap on property tax assessments. The cap is removed if one sells one's house. Families with rising income and need for more space have an incentive to stay in their current residence and upgrade rather than sell and move. Proposition 13 probably had only a small effect on the findings of Zorn et al., however, because their "after growth control" period ran from 1975 to 1979, and Proposition 13 did not take effect until 1978. Replications of Zorn et al. would have to account for Proposition 13 effects, though.

³⁵. For empirical evidence supporting benefits of the filtering process to the poor, see Weicher and Thibodeau (1988). The national displacement effect was also found in an earlier study by Michael Murray (1983).

communities, both of which might be sufficiently unusual to make extrapolations unwarranted. Petaluma was a famous test case, and Davis is a university town known for its attention to environmental amenities. Notoriety itself might have sent housing prices up by signalling to house buyers the existence of an exclusive community.

Katz and Rosen (1987) overcame this problem by selecting a sample of over 1600 single-family home sales from 64 communities in the San Francisco Bay Area during 1979.³⁶ Of these sales, 175 occurred in communities that had a growth control program (building permit moratorium or a binding rationing system) in effect for at least one year in the period 1973–79. They found that houses selling in the growth controlled communities were 17 to 38 percent more expensive than those in other communities. Katz and Rosen thus corroborated with a broader sample (but with a larger price differential) the results that Schwartz and his coauthors found for Davis and Petaluma.

Katz and Rosen indicate a much larger price effect from growth controls—from 17 percent to 38 percent—than Schwartz et al.'s estimates of about 9 percent for Petaluma and Davis. Aside from econometric issues suggested by Schwartz and Zorn (1988), Davis and Petaluma are both farther from San Francisco than the communities in Katz and Rosen's sample. As George Peterson's Fairfax County study (November 1974; discussed in the previous section) indicated, locations farther from the center of an urban area are affected proportionately less by the imposition of identical land use controls.

What conclusions can be drawn from the housing cost studies? Their authors imply that they show that growth controls are undesirable. Katz and Rosen conclude: "The spread of these regulatory techniques to metropolitan areas outside of California may have substantial negative effects on the affordability of housing..." (p. 159). The hedge is that the *spread* of growth controls may be undesirable; they do not say that the higher price in the current sample itself is undesirable, though that seems to be implied. (Why else would the spread be undesirable, unless there are some threshold effects?) Schwartz and Zorn's follow-up studies of both Davis and Petaluma evince a concern with the income distribution effects of higher housing costs, and both city's programs attempted to respond to such criticisms, with mixed results. There is no specific charge in these studies that growth controls are inefficient, but, because there is no discussion of the benefits of

³⁶ See Rosen and Katz (1981) for more detailed background on growth controls in the sample. The econometric results are only in their later paper (Katz and Rosen 1987).

growth controls in most articles, a reader might well infer that rising housing prices is evidence of inefficiency: If costs rise with no concurrent benefits, the policy must be inefficient.³⁷

Let me take the role of an advocate for growth controls to show the weakness of the inefficiency implication. We know from the studies reviewed in section 4 above that community and neighborhood amenities raise housing values. Growth controls may create residential amenities, or, for rapidly growing communities, they may prevent impending disamenities. Hence the higher housing prices could be taken as evidence that growth controls do what they are intended to do.³⁸

Furthermore, there is a serious selection problem in the aforementioned studies. None asks why some communities adopt growth controls and others do not. Perhaps only those in which rapid growth threatens to overwhelm the traditional zoning system do so.³⁹ Growth controls thus may give a community in which housing prices were rising anyway (due to demand shifts) an opportunity to adjust conventional land use controls to deal with the new conditions. Sometimes a growth moratorium is imposed during this adjustment process; most are lifted after a few years. While new zoning laws adopted during a moratorium are usually more restrictive than the regulations applied previously, this in itself is not necessarily inefficient. With generally rising incomes and income elastic demand for housing, more open space would have been demanded in any case.

As for the regressiveness of growth controls, two effects are alleged. One is that housing costs in general are raised, to the detriment of the poor in particular. The other is that the poor are prevented from living in certain communities or areas of the metropolitan area.

The studies just examined provide no direct evidence that growth controls affect the overall supply of housing. Katz and Rosen's sample of growth-controlled housing sales represented

³⁷. Schwartz, Hansen, and Green specifically assume without evidence that benefits from growth controls are "insignificant" (1981, p. 308). Deakin's (1989) review of growth control studies explicitly links higher housing prices with inefficiency.

³⁸. Katz and Rosen do note that in an open-city model (discussed in note 40), higher housing prices would be evidence of the efficiency of growth controls (1987, p. 151), but one page later they appear to reject the open-city model for interpreting their results. Lillydahl and Singell (1987, p. 67) arrive at a conclusion similar to that in my text: Higher prices in the community enacting controls are not evidence of harm.

³⁹. Deakin (1989) reviews several opinion surveys that conclude that growth controls are adopted for perceived fiscal and environmental problems rather than for elitist purposes. This makes sense; the elitist communities long ago adopted strict zoning restrictions, so that additional growth controls are unnecessary for purposes of exclusion.

only ten percent of their total sample, and this was in a state and at a time in which growth controls were rampant. There were obviously many other communities in which development could and did occur.

It is for this reason peculiar that Katz and Rosen should have decided to interpret their results in a closed-city model, in which there is no household mobility.⁴⁰ The Bay Area has many independent units of government. Even the entire metropolitan area itself would seem closer to the open city (free migration) model than the closed city. The very fact that the area is growing suggests that there are many households and firms that have overcome moving costs.

Zoning the poor out of particular communities doubtlessly motivates many land use controls, but whether the typical American pattern of the rich living in the suburbs is caused primarily by individual decisions or municipal or other discrimination is a matter of some dispute,⁴¹ and neighborhood income segregation has occurred in the past in the absence of public land use controls.⁴²

As I suggested above, the problem caused by the focus on distributional consequences of growth controls has led to the inference that the higher housing prices that follow their imposition are a measure of their costs. In principle, the opposite is more likely true: The higher prices of housing could be a measure of their benefits. The real problem here is to identify what causes the higher prices. Higher prices of housing—or higher prices of anything else, for that matter—can occur either as a result of supply restriction (an inward shift of the supply curve, showing that lower supplies are provided at all prices) or demand stimulation (an outward shift of the demand curve, showing that higher demand exists at all prices). Inward shifts in supply smell of monopoly, and that is one efficiency objection to growth controls.

⁴⁰. Closed and open are formal analytic terms introduced by Polinsky and Shavell (1976) and applied in a zoning context by Courant (1976) and White (1979). Closed means that the population of the metropolitan area is fixed, and hence residents must suffer whatever costs are imposed upon them, since they cannot leave. Utility in a closed city is thus endogenous to local policies. Open cities allow free migration, so that net costs induced by local policies cause people to leave for other areas. As a result, in open cities utility is exogenous. The real world is obviously somewhere in between these two analytic paradigms. Aggregate land values are lowered by inefficient policies in an open city (Sullivan 1986), but the story is more complex in a closed city.

⁴¹. See Mills and Hamilton (1984, p. 102).

⁴². Warner (1971, chaps. 4 and 6) found that Boston suburban neighborhoods in the late nineteenth century were segregated by income groups without any apparent government effort to promote it and before zoning was adopted.

Figure 2

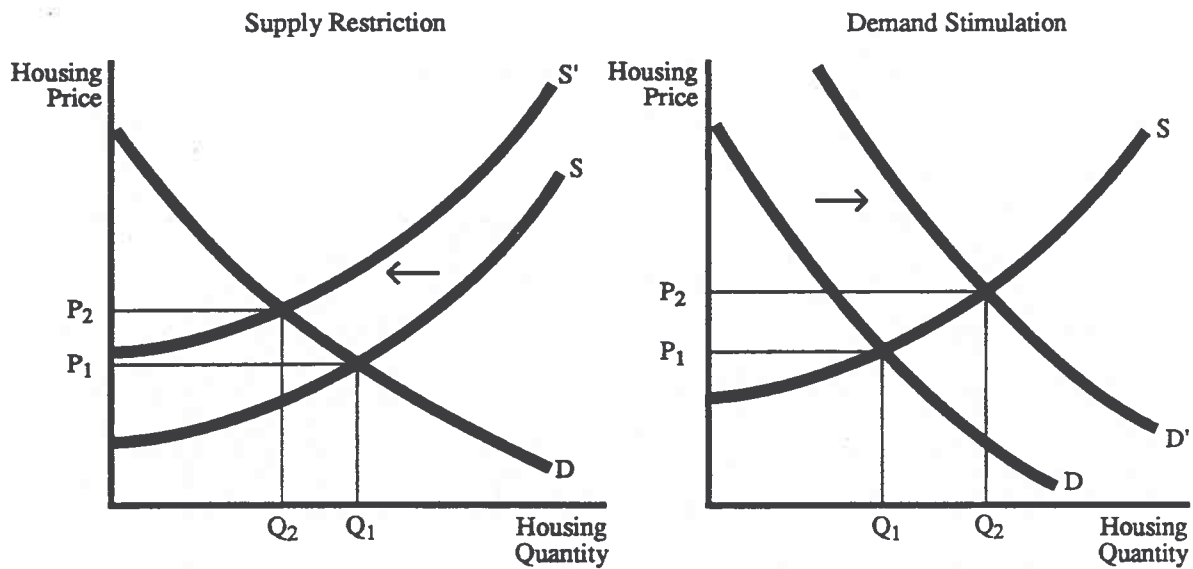


Figure 2 illustrates the problem. The panel on the left indicates a supply restriction of S to S' , which raises the price from P_1 to P_2 . The panel on the right shows the effect of making the community more attractive and thus stimulating demand from D to D' . Just as in the inefficient supply restriction, the demand-stimulating effects of growth controls will make prices increase from P_1 to P_2 . Confronted simply with evidence that prices of housing rose as a result of a public growth management program, we cannot say which scenario is the dominant cause.

Of course, the two graphs above are distinguished by opposite effects on the quantity of housing. The supply restriction reduces the quantity of housing, while the demand stimulation increases it. The problem with evaluating growth controls as a monopoly supply restriction is that it is hard to know what the true supply curve looks like when conditions naturally change over time. Simply observing that a community declines to allow the same number of building permits as it did in the recent past is not even proof of monopoly action, much less monopoly power. A private firm with output that has expanded by ten percent per year for the last five years may find that it has reached the limits of its managerial capacities and decline to expand any more. This is not monopolistic as long as other firms can enter the industry to meet the new demand.

Growth controls are especially difficult to assess because restriction in housing supply may actually stimulate the demand for housing for reasons that have nothing to do with monopoly. With rising incomes or other shifts in demand, it may be that low-density, high-amenity communities have become relatively scarce. Growth controls may then be seen as a *positive* supply response to

such demands. This statement seems odd only if one looks at a single community rather than a system of communities in a larger metropolitan market. Viewed in the metropolitan context, growth controls may be no more anomalous a supply response than a market-driven trend toward larger houses.

The key to this apologia for growth controls is the existence of alternative communities to meet other kinds of demands. If housing consumers in general or low-income people in particular do not have alternative communities in which to locate, then the reduced number of sites in the growth control communities will have extraterritorial effects on the housing market. If the supply of sites is inelastic in communities that lack growth controls, then housing prices in the metropolitan area will rise.⁴³ Adverse impacts from growth controls can be tested by looking at housing prices outside the restrictive communities: If they have increased, the impacts are adverse, since, presumably, the benefits of growth controls accrue exclusively to the community that enacts them.

Just such a test was conducted by Henry Pollakowski and Susan Wachter (1990). They examined the growth restrictions adopted in Montgomery County, Maryland, the northern suburb of Washington, D.C. The county had seventeen planning districts. Pollakowski and Wachter found that districts adjacent to more restrictive districts had significantly higher housing prices, though the difference was not large. This implies a spillover effect from zoning that is most probably caused by monopolistic scarcity in the more restrictive areas, since the districts were large enough that amenity effects would not have affected adjacent districts significantly.

An indirect way of testing for extraterritorial effects is to ask whether entire metropolitan areas in which some communities have growth controls have higher housing prices than other metropolitan areas. This is an imperfect measure, because some of the increase in housing prices could be due to communities making themselves more attractive relative to others. Hence the metropolitan area average would rise somewhat even under an efficient system of growth controls. One would expect this effect to be small, however, insofar as growth controls are typically adopted by local governments to provide benefits for their own residents, not for the entire metropolitan area.

Two types of studies have attempted to determine the metropolitan-wide effects of growth controls. One tests directly for the influence of growth controls, and the other tests for monopoly

⁴³ Michael Elliott's (1981) study of California cities and counties shows that geographically isolated communities (e.g., Fresno) that enact growth controls have no effect on housing prices, but when several communities in one area or entire counties adopt controls, the effects are significant.

power. Both types of studies demonstrate some metropolitan-wide housing price effects, with the strong implication that such effects are net costs.

Thomas Black and James Hoben (1985) examined a sample of thirty metropolitan areas in 1980 to determine whether those that had more restrictive land use controls had higher land prices. The variables to be explained were (a) the price of a standard quarter-acre suburban lot, zoned for single-family use and (b) the price of unimproved acreage suitable for a single-family subdivision at the developing fringe of the metropolitan area. Land price is an especially suitable dependent variable because the locus of the effect of restrictions is clear.⁴⁴ Both types of land values were obtained from a special Urban Land Institute survey of local real estate experts in each area.

The independent variable that Black and Hoben employed to measure restrictiveness was a qualitative estimate. The Urban Land Institute asked a panel of eleven experts to rank the thirty metropolitan areas on a ten-point scale according to their perceptions of the restrictiveness of land use controls. While such a subjective ranking is bound to raise eyebrows, I submit that it is superior to most quantitative rankings, such as inventories of minimum lot sizes. The amount of discretion available to local governments is considerable, and the inclination to rezone favorably or unfavorably is not captured by most quantitative measures.

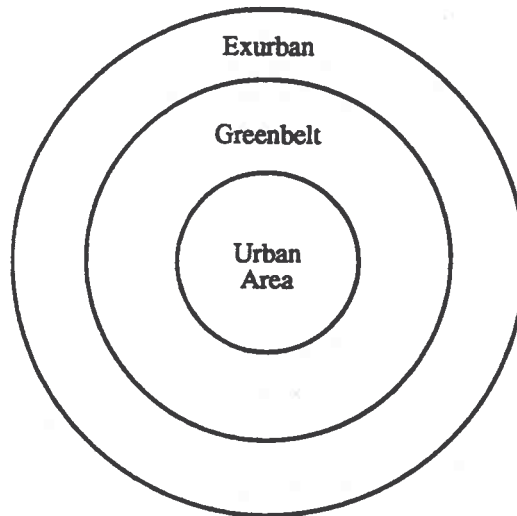
Black and Hoben's stepwise regression analysis found that the index of restrictiveness accounted for a significant amount of the variation in prices of developable land. (The results for increases in land prices from 1975–80 were much less significant, but increments are always harder to account for.) The statistical test they used was hard to interpret, insofar as they did not give results for individual variables but only what each contributed to the regression's R-square. Nonetheless, their study does provide evidence that local restrictive practices can have an effect on metropolitan land and housing costs.⁴⁵

Studies based on the Urban Land Institute's land value survey may actually understate the effects of growth policies on metropolitan fringe land prices. Suppose that the outer suburbs in a

⁴⁴. Both Katz and Rosen and Schwartz, Hansen, and Zorn (discussed above in this section) found that growth restrictions seem to affect developed properties according to several nonland characteristics. This may be accounted for by the difficulty of modifying certain configurations of housing capital after growth controls occur: It may be harder to add extra bedrooms after a new zoning law makes it more profitable to have four instead of three.

⁴⁵. Daniel Chambers and Douglas Diamond (1988) used updated data from the same sample as Black and Hoben to test whether additional measures of restrictiveness, including delay time and general availability of zoned lots, raised land costs. The results were mixed, but they did conclude that delay and zoning did increase land prices.

Figure 3



metropolitan area adopt highly restrictive growth controls in an area labeled “greenbelt” in Figure 3. Prices of land under existing housing will rise in such areas, and prices of land subject to the new restrictions will fall. But another effect will occur as well: developers will move to communities still farther from the CBD, labeled “exurban” in the figure.

Land values in the exurban areas will rise, but because of their remoteness, they will not be as high as the greenbelt area would have been had no growth controls been implemented. The exurban areas will now become the reported fringe land prices in metropolitan areas that are highly restrictive. The prices may appear to be not much higher than in nonrestrictive areas, in which more compact development patterns emerged, because the restrictive areas are already too spread out. If this account can be credited, then Black and Hoben’s results understate the effects of growth controls.

David Segal and Philip Srinivasan (1985) also examined the metropolitan effects of land use controls. They looked not at land prices, but at housing prices, which are less desirable because they must control for intermetropolitan structural differences. Their sample included 51 large metropolitan areas, with data from the period 1975–78. Their econometric techniques were more sophisticated than the others, although the number of control variables that account for other differences among metropolitan area housing prices was lower than most other studies.

Segal and Srinivasan’s crucial independent variable was an index of growth restrictions. They obtained the restriction index by interviewing planners in each metropolitan area to get an estimate of the percentage of developable suburban land that had been withdrawn from the

market by growth controls during the period. (While such surveys are superior to most quantitative data on land use controls, it is regrettable that Segal and Srinivasan provided little information about survey methods.) This variable was highly significant, explaining almost 40 percent of the variation in housing prices among the metropolitan areas. They found that areas that withdrew 20 percent of their suburban land from development had a housing price inflation that was six percent higher than unrestricted cities.

The second type of intermetropolitan studies of land use controls makes no attempt to measure restrictions directly, asking instead whether the structure of government in these areas appears to be competitive or monopolistic. The hypothesis is that metropolitan areas that have relatively few governments will tend to have higher housing prices. This is because when a few jurisdictions do adopt growth controls, a large fraction of available metropolitan area undeveloped land will be removed from the market.⁴⁶

Two studies have found some evidence that metropolitan areas with a more concentrated government structure (e.g., having four suburbs occupy about forty percent of suburban land) tend to have higher housing prices. Bruce Hamilton's (1978) study of housing prices in thirteen smaller northeastern and northcentral metropolitan areas indicated that fewer governments were associated with higher housing prices. My replication of this study (Fischel 1980) found flaws with Hamilton's study, and my attempt to find a monopoly effect with an alternative sample was inconclusive.⁴⁷

Louis Rose (1989) undertook an expanded study of the monopoly zoning hypothesis. Rather than using housing prices, he used developable land prices, which were drawn from the aforementioned Urban Land Institute survey and from Federal Housing Administration data. Rose's principal theoretical improvement was to account for the existence of natural barriers to development, which in practice meant bays, lakes, and oceans, as well as potentially monopolistic government structure. His results "taken as a whole provide moderate support for the monopoly zoning hypothesis" (p. 340).

⁴⁶ Markusen and Scheffman (1977) concluded that there was no evidence that private market power of developers was responsible for Toronto's extraordinary housing price increases in the 1970s; they put the blame on zoning.

⁴⁷ An empirical study of national housing prices by Ozanne and Thibodeau (1983) found some support for the monopoly hypothesis, but their study was not specifically addressed to the issue.

Among the larger metropolitan areas, about ten percent of intermetropolitan land price variation is accounted for by the index of potential monopoly among local governments. This would account for less housing price variation, since land is but one factor of production, accounting for about a quarter of housing cost nationwide. While it is apparent that growth controls can flourish among even competitive government structures, Rose's results warn that if they are adopted at a higher government level, such as by a metropolitan county, the impact on housing prices will be more severe.

Rose's result must be tempered by his finding that natural restrictions accounted for three times as much variation as monopoly power did. This suggests again the difficulty of sorting out benefits from costs in such studies. Take San Francisco as an example. On Rose's reasonable index of natural restrictions, San Francisco ranks as the most severely constrained metropolitan area. The fact that one cannot build on the bay or in the ocean has an effect on housing prices. But no San Franciscan would regard the bay and the ocean as a pure cost; they are among the features that account for its commercial advantages and the desirability of its housing. By the same logic, one could argue that the San Francisco area's severe growth restrictions may contribute to the quality of life in the area. The key to a welfare analysis of such restrictions is not, however, that the homeowners already inside the metropolitan area gain from the controls; the crucial issue is whether their gain exceeds the loss to those who are excluded from living or working in places in which they are willing to pay their social costs.

The reader may have noticed that this survey has not treated impact fees as a form of growth control. Impact fees are monetary exactions that local authorities impose upon developers to pay for the increased expenditures on municipal services occasioned by development.⁴⁸ Developers traditionally provided or paid the community to provide on-site infrastructure such as sewer connectors and subdivision roads. Communities have in recent years escalated their demands for exactions to cover off-site public expenditures such as schools, libraries, police stations, and highway interchanges, which are sometimes only distantly related to the development charged for them. These practices have been attacked as adding to housing costs, and there is some empirical evidence supporting this proposition (Singell and Lillydahl 1990).

My reason for downplaying the role of impact fees is that they are a manifestation of other policies, so one cannot tell whether they are antidevelopment or prodevelopment. A newly

⁴⁸. Impact fees and related devices are discussed in papers in Alterman (1989) and in Babcock (1987). Snyder and Stegman (1986) is a monograph that explores economic issues, though not from the perspective I discuss in the text following this note.

adopted impact fee of, say, \$5000 per developed lot could certainly be read as antidevelopment if the community formerly allowed developers to build for only \$500 per lot. Suppose, however, that a state court subsequently invalidated this fee as an unauthorized tax (something few courts do). The question is, would the community go back to its old ways of cheaply accommodating developers, or would it adopt more strict land use regulations that forestalled nearly all development? If prohibition of fees makes the community opt for more stringent regulations, then it seems to me that the impact fee is progrowth.

A demonstration of the last point was reported by Kate Gurnett in the (Colonie, New York) *Daily Gazette* in issues of October 27, November 9, and November 29, 1989. In a suit brought by the Albany Area Builders Association, the New York Court of Appeals overturned a \$900 per lot impact fee imposed by the suburban town of Guilderland, New York. The fee was earmarked to provide over three-quarters of the financing for a new highway bypass. The town's voters subsequently rejected a bond issue for the highway which, after the court's ruling, would have been financed out of property taxes alone. The town's elected officials then proposed larger minimum lot sizes to reduce growth and the impact of traffic on existing neighborhoods.

My view of fees is governed by my view that growth controls transfer entitlements to develop from the landowner/developer to the community. Regardless of whether one thinks that this transfer is benign or pernicious, it does the landowner little good to prohibit the community from selling development rights back to him once it is accepted that the community can maintain its new regulations. The adoption of a schedule of high fees may be a manifestation of a community's unwillingness to accommodate development, or it may represent a change of heart by a rigidly antigrowth community, allowing some development if it pays its own way (and perhaps a little more). The ambivalence of developers about impact fees supports this view. Developers do not like fees, but if they see them as the only way to get into a community, they reluctantly support them.

A factor that further complicates the analysis of impact fees is that the money generated by them is not thrown away. The public expenditures that they finance may enhance the attractiveness of all property in a revenue-starved community (Helms 1985). This is another explanation for the finding that housing values rise when impact fees are adopted, though it is difficult to separate the expenditure effect from the coincidence of adopting more stringent regulations and adopting higher impact fees. Thus impact fees can be seen as a tax increase or as the outward manifestation of growth controls. In either case, impact fees are the result of something else and should be analyzed in that way.

§7 EFFICIENCY OF ZONING AND GROWTH CONTROLS

The notion that higher housing prices may reflect the benefits of growth controls rather than their costs is unsettling to those who suspect that government regulation at best merely redistributes wealth and more likely reduces total output. The research on intermetropolitan housing price differentials due to growth controls or to monopoly government structure, reviewed in the last section, seems to support the idea that higher prices represent wasteful supply shifts due to fewer sites rather than beneficial demand shifts due to more amenities. Aside from the statistical uncertainties of these studies, the problem is that many growth control programs have been established in local government settings in which the fraction of metropolitan land affected by growth controls does not appear to be large. The way to deal with this problem is to attempt to identify both the benefits and costs of land use controls. This section examines studies that evaluate the efficiency of land use controls.

George Peterson's (July 1974) Boston-area study, which is widely cited, was the first to attempt to balance benefits and costs of zoning ordinances. His sample consisted of 1500 single-family homes that sold during 1971 in nine suburban Boston towns near Route 128, the inner circumferential beltway that is the locus of suburban employment. From his regressions, Peterson inferred that being able to convert from one house per acre to four houses per acre raised the value per acre by over thirty percent (\$7980 in his sample), which is consistent with James White's (1988) later estimates for Ramapo, New York. Peterson also showed that an additional sample of 68 vacant land transactions produced results almost identical to the more extensive sample of developed parcels. He concluded that this equivalence demonstrates that land use constraints are nearly fully capitalized in land values, not in the structural characteristics of the buildings. This suggests that changes in land values will reflect the welfare gains or losses from growth controls.⁴⁹

The unique aspect of Peterson's study was his attempt to calculate the *net* price effects of zoning. This is essentially a benefit-cost analysis of zoning, though he does not label it as such. Peterson identified three important effects from large-lot zoning, specifically, one-acre minimum lot size as opposed to quarter-acre lots. The effects are: (1) Larger lots induce more costly homes to

⁴⁹. Much theoretical literature addresses the suitability of using land values to measure welfare gains. See the review in Freeman (1979) and section 8 below.

be built, and it is advantageous to be in a neighborhood with more expensive homes. This external benefit of large-lot zoning contributed an estimated \$1520 to the average value of a one-acre parcel. (2) The larger lots with the more expensive homes also create a community-wide fiscal benefit, presuming that the number of school children and other local service demands remain the same as for homes on smaller lots. The value of this was estimated at \$3240. (3) The cost of one-acre zoning, however, is that quarter-acre zoning allows three more houses to be built. They are less valuable houses, but the value per acre of more intensive use was estimated at \$7980, as noted in the previous paragraph. Hence restraining a parcel from being subdivided from one-acre to quarter-acre size subtracted that value. The net impact on prices caused by larger-lot zoning was thus *negative* \$3211 ($= 1529 + 3240 - 7980$).

The chief drawback of Peterson's study is that it involved an evaluation of conventional zoning laws rather than growth controls. As I have indicated earlier, however, the techniques and motivations for growth controls are often similar, so that Peterson's results may be more general. A more recent study by Ted Frech and Ronald Lafferty (1984) suggests that Peterson's conclusions may be applied to modern growth controls.

In 1972, a ballot initiative, Proposition 20, created the California Coastal Commission to provide "permanent protection of the remaining natural and scenic resources of the coastal zone...." The commission was empowered to veto projects that had local government approval but were located within 1000 yards (0.57 miles) of the ocean. As is the case with most state-mandated controls, the commission was a second layer of regulation, not a replacement for local control.

The intent of the Coastal Commission was to limit development primarily to provide open space and public access to the coast. The area to be regulated was clearly demarcated, and the source of the control was not easily compromised by local politics, since the regional Coastal Commissioners were initially appointed by the state. At the same time, the passage of Proposition 20 by a large majority of the voters strongly suggested that there were perceived benefits from the greater protection afforded by this new layer of regulation. For these reasons, the effects of Proposition 20 form as close to a natural experiment in growth control as one is likely to encounter.

Frech and Lafferty selected a section of the California coast consisting of four cities in and unincorporated parts of Ventura County, about 50 miles northwest of Los Angeles. The sample consisted of 6382 single-family homes that sold in the period 1966–75. In addition to detailed data on house characteristics, Frech and Lafferty located each home on a map. They sorted these

locations by distance from the coast, which was a crucial factor in their test of the effects of Proposition 20.

Their regressions found that houses that were sold after it became known that Proposition 20 would pass (October 1972) were significantly more costly than those that had sold prior to that date. This, of course, is unsurprising, since the Coastal Commission was expected to reduce the number of sites for new housing in that market. But Frech and Lafferty went farther in their reasoning. They asked what would show that there were *benefits* from the establishment of the commission. They proposed two tests.

First, existing single-family homes located in the protected zone itself (within a half mile of the beach) that were surrounded by undeveloped land would presumably benefit more than other homes. Homes in such a situation now had more assurance that the open space near them would remain open or would be developed only in an environmentally acceptable way. The variable they used to test this did turn out to be positive—the more open (agricultural) land surrounding homes in the coastal zone, the larger the increase in price after October 1972—but not significant (the t-statistic was 1.3).

The other test involved the distribution of the housing price increases by distance from the coast. Frech and Lafferty reasoned that the benefits of coastal preservation should be perceived more by those who lived closer to the coast than those who lived farther away. The reason, which they do not discuss, is presumably that those who live farther away have to travel several miles to visit the coast, and so they enjoy it less frequently than those who live within a mile or two. Such an effect is standard in urban economics, and many studies have confirmed that access to a desirable location is capitalized into land and thus into housing values.⁵⁰

Frech and Lafferty found, however, that there was no differential rate of capitalization outside of the coastal one-half mile zone itself. Houses that were located in the band that was between 0.5 and 1.5 miles from the coast rose in value by the same amount as houses in the band between 7.5 and 13.0 miles from the coast.⁵¹ The increase in value of all homes in the study area, in which the farthest observed sale was 13 miles from the coast, was thus taken to be solely the result of reduced supply of building sites rather than spillover effects from the newly preserved coastal zone.

⁵⁰ Milon, Gressel, and Mulkey (1984) show this for access to the Florida coast.

⁵¹ Deacon and Shapiro's (1975) study of the statewide vote likewise revealed no tendency for those closer to the coast to favor Proposition 20 more than those farther away.

The conclusion to be drawn from Frech and Lafferty is that Proposition 20 was inefficient.⁵² Why, then, did it pass? The answer is probably that the distribution of political votes, especially in a plebiscite, is different from the distribution of dollar votes. Frech and Lafferty identify those who bore the costs of reduced supply of housing sites in the coastal zone. They include renters (at least those not protected prior to 1972 by rent control); owners of raw land that might have been profitably developed within the coastal zone; would-be owners of housing in the affected area; and individuals or firms who might have gotten some special benefits from occupying structures within the coastal zone. Beneficiaries of Proposition 20 include those who owned already-developed property that increased in price; owners of undeveloped land near but not in the coastal zone whose site values increased; and California citizens who get special utility knowing that the coast will be less developed than otherwise. While the benefits to the latter group did not show up as a distance gradient in Frech and Lafferty, there may well have been a some option value⁵³ to having an undeveloped coast that appealed to California citizens generally.

⁵². Corroborating evidence from other studies of the Coastal Commission is reported by Frech (1982).

⁵³. Option value is the willingness to pay to reserve the opportunity to purchase a depletable good in the future. Richard Bishop (1982) surveys the theoretical issues of this controversial concept.

§8 LAND VALUE MAXIMIZATION AND EFFICIENT ZONING

In this section I describe how one might conduct an empirical test of whether a growth control ordinance adopted by a majority of community voters is efficient. While this approach is implicit in my previous remarks, it is time to spell it out and indicate its intellectual provenance. It is assumed that the community is small relative to the metropolitan area in which it is located. This permits application of the open-city assumption, mentioned at note 40 above.

There are gainers and losers when a community adopts growth controls. Consider as a paradigm the passage of a voter initiative that imposes an indefinite moratorium on all new building in a town. Among those presumably better-off are the resident homeowners of the community. They would hardly have voted for the regulation if it was not expected to improve their economic position. Their position is improved because they can maintain their town's ambience and protect its fiscal position. These advantages are manifested as increases in existing home values. As the literature discussed in sections 4 and 6 above demonstrates, community-wide amenities and growth controls are capitalized in higher values of existing housing. Because the only nonreproducible, nondepreciable part of housing value is land, its increment in value is the long-run locus of the benefits of growth controls.

A measure of the costs of growth controls is the reduction in the value of land on which development is restricted. In most instances, this land is owned by a distinct minority of the community or by people who do not live there at all. The decrement in the value of undeveloped or other land subject to growth restrictions is the manifestation of the exclusionary effects of growth controls. The studies of undeveloped land values reviewed in section 4 above show that such a decrement is easily detectable.

It is important to distinguish the manifestation from the effect, however. Reduced land value itself has no direct welfare effects. The reason for the reduction is that some people who were formerly expected to live on that land are, after growth controls, not permitted to do so. A crucial assumption of the net land value approach is that losses in undeveloped land values are a reasonable approximation of the economic losses of potential residents or of other land users who must find less satisfactory locations in other places.

Below is a stylized view of how growth controls might manifest themselves in a community that enacted them in 1986.

Figure 4

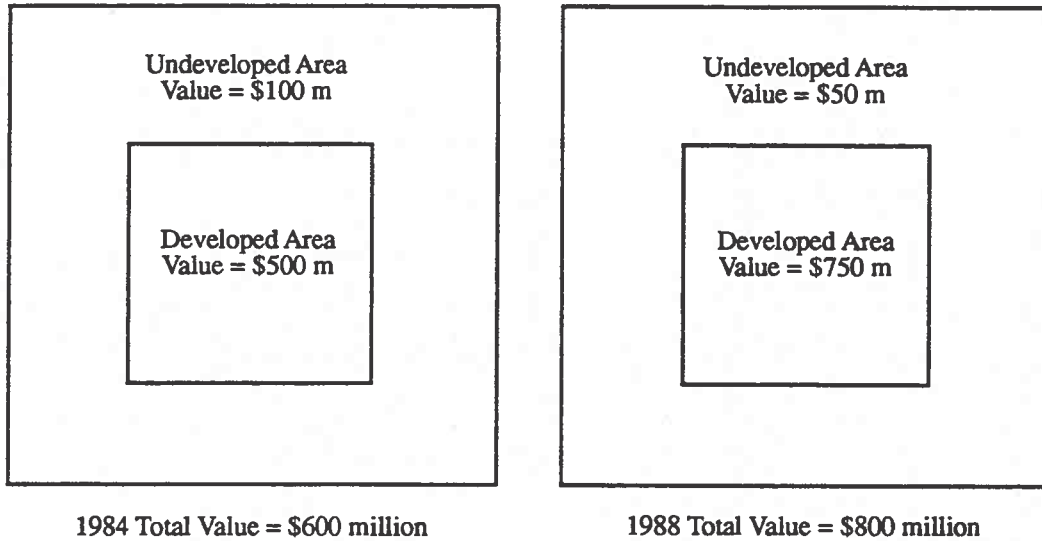


Figure 4 illustrates a \$250 million gain to owners in the developed area and a \$50 million loss to owners in the undeveloped area. Aggregate land values in the community grew by 33 percent between 1984 and 1988. If aggregate land values in otherwise similar communities that did not adopt growth controls during this period grew by more than 33 percent, then my test would imply that the growth control program was inefficient, assuming that other events that affect intercommunity differences can be controlled for. If other communities' aggregate land values grew by less than 33 percent, then the growth control policy was efficient on the Kaldor-Hicks criterion.⁵⁴

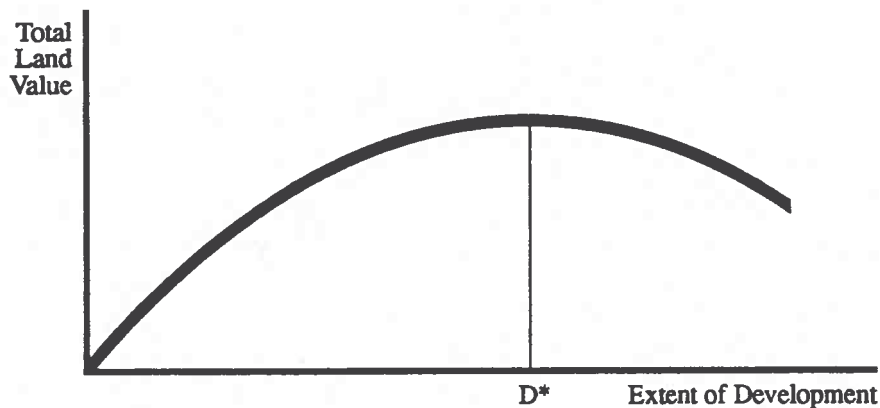
Another way of illustrating this argument is shown in Figure 5, which shows the hypothetical relationship between aggregate land values and the extent of development in the community. At the far left, there is no development and hence no economic value. As the community grows, aggregate land values rise. They reach a peak and decline after congestion becomes so extensive that additional development actually reduces aggregate land value. This does

⁵⁴ The Kaldor-Hicks criterion judges as efficient a project whose dollar gains exceed the dollar losses. The Pareto superiority criterion, which demands that at least one person gains and no one loses, is virtually impossible to meet for public policies such as land use regulation.

not have to involve environmental catastrophe; it may only mean that the inconveniences of greater density induce people at the margin to choose other communities.⁵⁵ The optimal degree of regulation is one that induces the community to reach point D* in Figure 5.

Why would the private market not achieve D*? The principle that underlies growth controls is that an unregulated (or permissively regulated) land market is apt to be to the right of D*. New residents would impose some costs on existing residents that the new people will not have to pay for. The classic example is traffic congestion at a freeway entrance, in which the person who gets in the queue makes her decision based on the number of cars in front of her and does not consider the extra waiting time she imposes on the cars behind her. Some form of public regulation of the queue of traffic or of developers is ordinarily needed to achieve the value-maximizing use of the resource. Thus the economic case for growth controls hinges on the possibility that conventional regulations allow too much development and aggregate land values can be increased by further retarding development. Note that this is an empirical possibility, not a deductive conclusion.

Figure 5



⁵⁵ The issue of optimal community size should not be confused with optimal size of metropolitan areas, which in the U.S. are composed of many communities. Although some of the same principles of external cost (e.g., unpriced congestion) are involved in the analysis of both issues, the metropolitan-area question is more complex because making big areas smaller means making small areas bigger or at least more numerous (Tolley 1974). I argue in the final section that inefficient community growth controls are apt to reduce the agglomeration economies that offset the costs of congestion in big metropolitan areas, so that they induce inefficiently small cities.

The test of the efficiency of growth controls, then, becomes simply a matter of determining whether the additional regulations cause aggregate land values to rise or fall relative to an appropriate base point, since rising land values suggest that the preexisting state of regulations was below the optimum. We cannot, however, exclude the possibility of multiple peaks, so this test is a local rather than global test of regulatory efficiency.

The economic literature that supports the previous analysis derives chiefly from the Tiebout hypothesis (1956). Local governments in Tiebout's view were passive suppliers of public goods. Households would migrate to a community based on their selection of the most preferable bundle of public goods. Wallace Oates (1969) placed the Tiebout hypothesis on empirical footing when he demonstrated that prices of residential property reflected the fiscal advantages and disadvantages for a sample of New Jersey communities. This is a necessary condition; if people are not aware of community differences, there can hardly be any sorting by demand for local services. Bruce Hamilton (1975) demonstrated that local fiscal zoning was necessary for the Tiebout process: otherwise unimpeded migration would allow free riders to enter the community and consume local services in excess of their contribution to property taxes.

The step from the Tiebout-Oates-Hamilton model to viewing local governments as rational proprietors of their community's assets was made by Jon Sonstelie and Paul Portney (1978), William Fischel (1978), Oded Hochman and Haim Ofek (1979), and Jan Brueckner (1983). These rational government models differ from the previous Tiebout-inspired literature in that local governments are not seen as mechanically setting up an apparatus of local services, taxes and land use regulations and then passively waiting for things to happen. Local governments instead actively pursue policies that maximize the total wealth of existing residents.⁵⁶ Brueckner (1990) develops a formal model showing that optimal land use controls may increase aggregate land values above market-determined benchmark, but that still further land use restrictions reduce aggregate land values and are inefficient.

Adoption of optimal land use controls by a political mechanism seems unlikely in a world in which voting is determined by one person, one vote, rather than by one acre, one vote. Optimal controls are nonetheless theoretically possible in a community in which a majority of voters owns only, say, ten percent of the land, and that land is already developed. The reason is that the developer may offer side payments to the voters. Developers have been known to offer (or to be

⁵⁶ Sonstelie and Portney (1980) and Brueckner (1982) present some empirical results that are consistent with the idea that communities may choose public expenditure policies that maximize voters' property values.

forced to offer as a condition for rezoning) community-wide benefits such as parks, schools, art museums, additional taxes, and improved roads (Fischel 1985, chap. 4). That such measures are inhibited by judicial and legislative decisions is, in my analysis, one reason that growth controls are apt to be inefficient, but that is an empirical matter.

As I indicated in section 6, monopoly control of land supply by governments would make land value maximization a dubious social indicator. If the community has a monopoly on available building sites, one could not tell whether the increase in total land value was because growth controls created amenities that housing buyers were willing to pay more for, or because of wasteful restrictions on supply that drove up the prices of the remaining land and existing houses. For that reason, the type of government and the market conditions in which growth controls are adopted must be taken into account in any empirical work along these lines.

§9 THE CAUSES AND CONSEQUENCES OF GROWTH CONTROLS

Much of this essay has taken a contrary point of view. To those economists who argue that land use controls are ineffective and unnecessary, I have endeavored to show that empirical economic research does not support either conclusion, and plenty of other research points to the opposite conclusions. To those who criticize growth controls for causing high housing prices, I have pointed out that at least some of the increase in prices could result from amenity creation rather than monopolistic exclusion. On balance, however, I am inclined to believe that most growth controls impose a net cost on society. I believe that the econometric evidence discussed in this paper supports the following three conclusions:

- (1) Land use controls, especially overall growth control programs, are important constraints on the land market. This in turn affects housing values, especially in suburban and exurban communities.
- (2) Land use controls do provide some benefits that would be difficult to obtain under less coercive conditions. Abolition of zoning and related controls would create a demand for alternative controls, and it is not clear that the alternatives are less costly to administer or more efficient in their effects than zoning.⁵⁷
- (3) Growth controls and other aggressive extensions of land use regulations probably impose costs on society that are larger than the benefits they provide. The higher housing prices associated with communities that impose growth controls are more likely the result of wasteful supply constraints than benign amenity production.

The last conclusion is more tentative than the first two because only a few studies have addressed it in a persuasive framework. For this reason, I shall elaborate on the political and institutional considerations that buttress the conclusion that growth controls have been inefficient.

Recent growth management programs occur in two instances. One is in small cities and suburbs that are relatively affluent. They are the typical locus of exclusionary zoning policies (Rolleston 1987). I have argued elsewhere that the growth control movement was in part caused by the judicial and state legislative attempts to limit exclusionary zoning (Fischel 1985, chap. 15).

⁵⁷. While there is little statistical evidence of the relative costs of alternatives, Ellickson's (1973) examination of the legal alternatives argues that at least some of the functions of zoning could practicably be taken over by private covenants and nuisance law.

These limitations may lead the affluent communities that did not want to accept large amounts of low-income housing to adopt a fall-back policy of excluding all new housing. For example, the New Jersey Supreme Court's *Mount Laurel* decision required all municipalities to take affirmative steps to provide new low-income housing, but once that requirement is met, the communities are free to adopt low-density requirements on the rest of their undeveloped land.⁵⁸ Growth controls are seemingly beyond judicial reproach on exclusionary grounds because they democratically exclude everyone. Indeed, many growth management programs go out of their way to mention that what little growth does occur should contain a low- and moderate-income housing component. Such benevolence may not offset the overall effects of restrictions on the housing market.

The other situation that seems to give rise to growth controls is the increasing use of citizen ballot initiatives in some states. Direct democracy allows for little of the compromise and bargaining that goes on in representative government. Measures that provide a small benefit for a large number of voters and impose a large cost on an isolated group of citizens are more likely to pass in a plebiscite than in a legislature (Hanke and Carbonell 1978). Growth controls adversely affect a relatively small number of voters in the jurisdiction—landowners and business interests—while providing financial gains or community amenities to a large number of existing residents.

Courts of law might provide an offset to this political imbalance if they were to respond to developer complaints about such practices by requiring that the community pay just compensation for the devalued land. No state court, however, has intervened solely on the basis of landowner devaluation unless the errant regulation is so extreme that it leaves the landowner with almost no use for his or her land.⁵⁹ The consensus of legal observers is that the California courts have been the most accommodating to community regulation and the least sympathetic to landowner complaints (Ellickson and Tarlock 1981, p. 75), which, combined with the widespread use of

⁵⁸. "Once a community has satisfied its fair share obligation, the *Mount Laurel* doctrine will not restrict other measures, including large-lot and open area zoning, that would maintain its beauty and communal character." *Southern Burlington County NAACP v. Township of Mount Laurel [Mt. Laurel II]*, 456 A.2d 390 at 421 (NJ 1983). The Mt. Laurel doctrine has been eroded by measures that permit communities to buy their way out of their obligations (Rubin, Seneca, and Stotsky 1990).

⁵⁹. The U.S. Supreme Court in 1987 decided two cases that some observers believe might lead the way to just compensation in less extreme cases. For a description and commentary on these cases, see Michelman (1988) and other articles in the same issue. Cordes and Weisbrod (1979) provide evidence that requiring compensation for moving costs does reduce the amount of property that state governments take for highways, providing indirect evidence that compensation requirements would deter government overreaching.

voter initiatives, has made California the undisputed leader in growth controls both in the 1970s and the 1980s.⁶⁰

The result of this institutional setting is that growth controls are apt to go too far. In a situation where some type of controls may be efficient in facilitating reasonable development, affluent suburban communities and communities that adopt controls by referendum will tend to adopt controls that are more extreme.⁶¹ The cost of voting for extreme controls is not brought home to the voters or suburban councils because those adversely affected are either a small fraction of the electorate or not resident in the community at all.

Aside from their adverse effects on the cost of housing, inefficiently restrictive growth controls probably cause metropolitan areas to be too spread out.⁶² This is not to deny that growth controls may make development in *individual* municipalities more compact. My claim is that such local ordinances cause developers to go to other communities. The most likely alternative sites are in exurban and rural communities, where the political climate, at least initially, is more favorable to development. As the more rural communities become partly developed, the newcomers wrest the political machinery from the pro-growth farmers and business interests. Then these communities, too, adopt growth controls, sending development still farther from employment and commercial centers. Eventually, employment and commercial activities also disperse from traditional population centers as they find that employees and customers are harder to find.

A paradigm of modern growth control policy that illustrates the foregoing argument was recently described by Bob Narus (1990). The fifteen-square-mile town of Lincoln, Massachusetts,

⁶⁰. Econometric studies of nationwide housing costs have found that California housing prices are especially high even after fiscal and public amenity (but not land use regulation) considerations are accounted for (Goodman 1988; Ozanne and Thibodeau 1983). While California cities rate highly in Blomquist, Berger, and Hoehn's (1988) sophisticated quality-of-life study, such studies may be flawed because they are based on the proposition that higher housing prices reveal pure amenity benefits. This neglects the possibility that high prices may reflect inefficient land use regulations. At the opposite pole, Philadelphia and Pittsburgh suburban housing prices were significantly lower than those of comparably sized northeastern cities (Fischel 1980). Pennsylvania courts have a reputation among land use lawyers for aggressively overruling local land use restrictions (Ellickson and Tarlock 1981, pp. 76, 763).

⁶¹. Indirect evidence to support this is the presumption by nearly all observers that having to pay for land devalued by regulations would bankrupt the community (e.g., Williams et al. 1984). If the regulations were efficient, the increase in property values should more than offset the losses, and the community would be able to pay the additional tax revenues (to pay compensation) from the enhanced property values of the benefitted land. This is essentially the windfalls for wipeouts plan advocated by the late Don Hagman (Hagman and Miscynski 1978).

⁶². The economics of this are described at length in Fischel (1985, chap. 12).

The more subtle loss from inefficiently dispersed homes and businesses is the loss of agglomeration economies for firms.⁶⁵ The basis for urban economies is the advantages of operating a business in the proximity of many other businesses. Location in a city allows firms to have access to a more skilled and flexible labor force. It also permits the face-to-face exchange of ideas, which promotes innovation. Forces that tend to disperse firms erode such advantages and reduce potential output from the industry. Telecommunications allows firms to deal with dispersion more easily now than in the past, and it is probable that advances in the electronic media have induced at least some businesses to leave urban areas without any loss in efficiency. But such firms are still the exception. Face-to-face contact is an essential ingredient of most growing businesses.

⁶⁵. Fogarty and Garofalo (1988) present evidence that decentralization of manufacturing reduces labor productivity, though they do not attempt to explain the decentralization. Tabuchi (1986) finds that higher density in Japanese cities raises productivity. It should be pointed out, though, that no one knows what the optimal distribution of city sizes is. For a discussion and references to this complicated topic, see Mills and Hamilton (1984, chap. 15).

has for years limited growth by means of regulation, selective purchase of open land, and legal action to block regional road construction. Yet this very affluent enclave (pop. 5200) is among the few to meet the state's requirement that ten percent of the housing stock be affordable to low- and moderate-income households. Subsidized housing was built by developers as a condition for permission to build more profitable commercial buildings. The affordable units were isolated from the affluent parts of town, in which two-acre building lots recently sold for \$350,000.

Its growth controls would have little effect on the housing market if Lincoln were an isolated rural town. Its open spaces, however, are maintained next to the Boston region's major employment corridor, Route 128. The town is only thirteen miles from downtown Boston. If one considers that the average gross density of most new suburbs is about four persons per acre, Lincoln's plan has displaced about 25,000 people.⁶³ Given that Lincoln is not unique, merely the most successful of a group, I would conclude that at least some of the low-density, sprawling development that occurs well beyond the Route 128 corridor is caused by successful growth management by the close-in suburbs.

The long-run effect of this is a lower standard of living. People will commute more than they otherwise would, which reduces their real incomes unless they enjoy commuting. Dispersion of residences and jobs promotes more automobile travel and longer trips, creating more congestion and pollution (assuming, as is realistic, that cars are not charged for their social costs) and eventually requiring more highway construction.⁶⁴

⁶³. The four-persons-per-acre figure is drawn from a highly detailed national study of rapidly growing counties by the U.S. Department of Agriculture, reported in Vesterby and Brooks (1989, p. 99). The figure is for gross density, so it includes acreage in nonresidential uses as well as residential lots and roads in its denominator. Since Lincoln has 9600 acres, its population would be about 38,400 if it had developed at typical suburban densities. This would be less than the density of the nearby town of Wellesley, which is not regarded as unpleasantly crowded.

⁶⁴. Evidence of wasteful commuting in U.S. metropolitan areas is Hamilton (1982) and Blackley and Follain (1987), although M.J. White (1988) finds less waste in the patterns. None of these addresses the causes of these patterns. Theoretical models of urban spatial development that incorporate suburban zoning constraints find that they cause excess suburbanization, though none of these models allows for any benefits that might come from zoning. (See Moss 1977 and Sheppard 1988.) Lizbeth Pyle's (1985) study of farmland preservation regulations in Minnesota suggests that they cause sprawl, and Arthur Nelson's (1988) analysis of Portland, Oregon, indirectly supports her position.

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