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THE EFFECTS OF ZONING AND EXTERNALITIES ON THE PRICE OF LAND: AN EMPIRICAL ANALYSIS OF MONROE COUNTY, NEW YORK

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I. Introduction

The use of urban land is elaborately regulated in the United States. The layout of streets and lots is regulated, typically, by requiring governmental approval of subdivision plans; development is regulated in detail by requiring governmental approval of urban amenities like sewers, water, and utilities; actual building on the land is regulated, also in detail, by building codes enforced by governmental inspectors and engineers; and finally, when land use is fully developed, permanent regulation is provided in the form of zoning codes, property conservation codes, fire codes, even sometimes rent control ordinances—all with bureaucracies to enforce them.

The motive for the regulation of any economic activity, including land use, is to adjust and modify market outcomes. But a market allocation, in which supply and demand are equilibrated by price, has behind it an extremely potent force, namely the agreement and advantage of all those participants who improve positions by trading. This force tends to counteract the modifying effects of regulation.

In this study we analyze some of the effects of zoning in the urbanized area of Rochester, New York in order to determine whether zoning significantly modifies outcomes in the urban land market, or whether market forces negate the forces of regulation. While the study concerns only one locality containing only one market for urban land, still, by analogy, it may apply to other markets in other localities that have roughly similar ordinances and roughly similar market conditions.

II. The Purposes and Methods of Zoning

In order to measure the effectiveness of zoning, it is first necessary to state precisely what it is intended to do. Zoning, which operates by allocating neighborhoods to particular land uses, has several purposes. Historically, its

purpose—probably still the dominant one—has been to prevent offensive uses of land that impose external costs on neighbors. To prevent such negative neighborhood effects, offensive uses of land are grouped together in particular neighborhoods where they are supposed to have the minimum possible external effect on other uses.

The original methods of zoning, which persist almost unchanged in many places, including most of the Rochester area, were such that one can immediately infer from the zoning ordinances themselves just which neighborhood effects officials wished to protect against. The ordinances provided for a hierarchical, or cumulative, system in which any particular use was prohibited in districts reserved for higher uses but was permitted in its own district and in any district established for uses lower in the hierarchy. The typical ordinance placed uses for single-family dwellings at the top, followed by various kinds of uses for multiple dwellings (for example, two-family dwellings, walk-up apartments, etc.), followed in turn by various grades of commerical uses (for example, neighborhood business, shopping center, central business district), and ending finally with various grades of industrial use (for example, light or heavy). From such an ordinance one infers that, in the opinion of its authors, *any* other use of land generates an external cost for owners of adjacent single-family dwellings: apartments generate external costs for owners of adjacent two-family dwellings, and so forth until at the bottom, industrial uses generate an external cost for owners of all adjacent nonindustrial land.

Planners have now developed new concepts of zoning. The previous emphasis on external costs has been relaxed and an alternative argument for zoning has been offered: due to the private sector's short-sighted, narrow perception of the best use of land, certain parcels must be set aside for particular uses in order to improve on the market's allocation of land. In this view, not only must industry be excluded from residential zones, but residences must be excluded from industrial zones. This is exclusive zoning, in the sense that a given district is assigned a single use and all other uses are excluded. Both methods of zoning—cumulative and exclusive—involve grouping together particular uses of land. But it is not clear to what degree this results in a modification of the market, since such grouping often occurs naturally as a consequence of positive externalities that are present in unregulated markets (for example, warehouses are grouped near terminals). Zoning is meant to emphasize and reinforce this market tendency toward specialization of uses and to thwart another market tendency toward intermingling of uses.

III. The Effectiveness of Zoning: Two Hypotheses

The question of whether zoning modifies the allocation of land to various uses probably cannot be answered directly. The best evidence would come

from comparing a map of land use in a zoned city with a similar (hypothetical) map of the same city unzoned. Such a comparison would tell us precisely the modifications due to zoning regulation in both the quantity and the geographic specialization of land in various uses. But the existence of one map precludes the existence of the other, so the comparison can never be made. Instead, we must approach the question of the effectiveness of zoning indirectly by seeking evidence on whether zoning has observable side effects. If the amounts of land in the various zoning categories do not match the unregulated market allocations for uses in these categories, then prices of land in the overallocated categories will be depressed relative to prices in the unzoned market, and prices in the underallocated categories will be elevated relative to prices in the unzoned market. If zoning does not induce significant changes in the quantity of land allocated for various uses, we would expect to observe no such elevation or depression of land prices attributable to zone category. Thus, one way to measure the effect of zoning is to look for price differentials. Such differentials would show that zoning does modify market outcomes by changing the amount of land allocated to various uses. If there is no price effect, then zoning probably does not affect the allocation of land by type of use, though it may affect specialization and location, and it may reduce or eliminate certain transactions costs.

Assuming that the zoners intend to allocate land differently from the market, then the degree to which they succeed will be reflected in and can be measured by price differentials. It cannot be said a priori whether zoning regulation will modify market outcomes or conform to them.[1] Both sides can be argued. For example, consider the supply of land for single-family use relative to the supply for other dwellings. The almost universal preference, as expressed in zoning statutes, for single-family dwellings probably inspires planners to try to overallocate land for single-family use. On the other hand, competition among jurisdictions (central city and suburbs) for uses paying high taxes per unit may lead planners to allocate land in the same way as the market. Or, for another example, consider the special interests connected with commercial and industrial land. Owners of land currently zoned for such use prefer to limit its supply. They may be joined in their efforts to

[1] James C. Ohls, Richard Chadbourn Weisberg, & Michelle J. White, The Effect of Zoning on Land Value, 1 J. Urb. Econ. 428 (1974), conclude that it is not in general possible using a priori theory to predict the impact of zoning on aggregate land value in a community, regardless of whether the intent of the zoners is to control externalities or to achieve fiscal goals. However, under plausible assumption, they argue that zoning as practiced in the U.S. probably lowers aggregate land values in the communities doing the zoning. For additional theoretical investigations which find that zoning may modify market outcomes, see Michelle J. White, The Effect of Zoning on the Size of Metropolitan Areas, 2 J. Urb. Econ. 279 (19 → William J. Stull, Land Use and Zoning in an Urban Economy, 64 Am. Econ. Rev. 337 (19' → Otto A. Davis & Andrew B. Whinston, The Economics of Complex Systems: The Case of Municipal Zoning, 17 Kyklos 419 (1964); → Otto A. Davis, The Economic Elements in Municipal Zoning Decisions, 39 Land Econ. 375 (1963).

restrict supply by owners of residential land who fear the effects of negative externalities. On the other hand, owners of land which is zoned for residential use but has industry or commerce as its best use constitute a special interest that can profit from an increase in the supply of such land. If that special interest which can best afford to pay prevails, zoning will tend to conform to the unregulated market outcome. But political and economic outcomes diverge often enough to admit the possibility that zoning may modify the market outcome, so the question can be settled only empirically.[2]

In order to determine whether zoning effectively modifies the market outcome, we have drawn samples of real estate transactions, measured characteristics of the parcels that affect the price of land, and used regression analysis to estimate a hedonic price index, one which attributes to the various characteristics of land the prices implicit in the market prices for land. We include the zoning categories to which the parcels belong as independent variables in the regression. If zoning creates artificial scarcities, then it should create rents for owners of land in the scarce categories, and these should appear in the index.

IV. Determinants of Land Prices

The price of land is, of course, dependent on many factors. The precise location of a parcel, with its access to important activities, is probably the main determinant of its price, but there are many subsidiary influences also, including the uses to which neighboring parcels are put. The nature of an urban place is that people live and work close to one another. The advantage of urbanism is this very proximity, the access it offers to other people, and the facility it offers for economic and civil intercourse. The advantage of access is greatest, of course, at the very center of the city, and as one departs from the center the advantage declines. Indeed, on a map of a metropolitan area one could draw an isoaccess curve around the urban center, a curve that passes through all points with a constant degree of access to the center. Then, just as on a physical map the contours around a mountain represent constant vertical distances from the peak, so on the access map the isoaccess contours represent a constant in some measure of access—travel time or transportation cost, for example.

[2] For empirical investigations which conclude that zoning is effective in modifying market outcomes, see Lynne B. Sagalyn & George Sternlieb, Zoning and Housing Costs: The Impact of Land-Use Controls on Housing Price (1973); Bernard H. Siegan, Land Use Without Zoning (1972); ε→ William J. Stull, Community Environment, Zoning, and the Market Value of Single-Family Homes, 18 J. Law & Econ. 535 (1975). But Frederick H. Rueter finds little likelihood that all of the externalities anticipated by the zoning ordinance actually arise in urban property markets: see his Externalitie:→ Urban Property Markets: An Empirical Test of the Zoning Ordinance of Pittsburgh, 16 J. Law & Econ. 313 (1973). See →John P. Crecine, Otto A. Davis & John E. Jackson, Urban Property Markets: Some Empirical Results and Their Implications for Municipal Zoning, 10 J. Law & Econ. 79 (1967).

Assuming all land is equally usable, then the isoaccess curves are, in a rough way, also curves of constant land price, simply because it is access that is being sold with the land. But it is only in a rough way that access and price are equivalent. *Ceteris paribus,* the better the access to the center, the higher the price. But scattered along the isoaccess curve are local centers of activity. An expanding urban area may absorb a village with a retail center, or major employers may locate in a suburban industrial park near an interstate highway interchange. Access to these local centers is itself of some value, and points near it are more desirable than other points on the same isoaccess curve. One can distinguish, then, between access to the main urban center and access to local centers.

Besides access, other locational factors are reflected in price. There are neighborhood amenities which tend to raise prices and negative neighborhood effects which tend to lower them. It is sometimes difficult to predict which locational features raise prices and which reduce them. Proximity to a body of water which has desirable recreational features and aesthetically pleasing vistas might tend to raise prices, while proximity to a busy airport might tend to depress them. These examples seem unambiguous. But does a nearby school raise or lower land values? We know that both amenities and external costs affect prices, but we do not know precisely which features of the city invariably are to be regarded as beneficial and which harmful. Thus, in our regression we will include measures of various neighborhood quality characteristics as well as variables which indicate the presence of externalities. For our purposes a potential externality, either positive or negative, is any adjacent or visible use of land other than single-family homes.

Table A2 in the appendix exhibits the complete list of variables which we employ to account for systematic variation in the price of land.

V. Rochester, New York, as an Example of Zoning

Our investigation is based on a particular example. Whether or not our results have general validity depends, therefore, on whether or not our example is typical of a larger class.

Rochester is a medium-sized city. With 880,000 people, the Rochester Standard Metropolitan Statistical Area (SMSA) was the thirty-seventh largest SMSA in 1970. (The urbanized area of the city and immediate suburbs had 600,000 people, and Monroe County had 710,000.) The Rochester area is large enough to have many of the political and economic complexities found in American cities. It is large enough, for example, to have a somewhat decaying central city ringed by a set of vigorous and mostly new suburbs. In 1969 median family income in Rochester was $11,969, seventh among the fifty largest SMSA's. The source of this considerable wealth is

manufacturing, mainly in optics and photography, office equipment, control mechanisms, machinery and tools, and, generally, the instrument industries. While Rochester's specialty distinguishes it from other cities, specialization itself does not. Other and richer cities are also specialized, some of them even more than Rochester (for example, Hartford in insurance, Washington in government, San Jose in electronics, etc.)

In population, wealth, social problems, and housing, Rochester is similar to other large American cities, and its method of zoning is typical. The urbanized area, which is the subject of our study, contains fifteen jurisdictions engaged in zoning: all of the city of Rochester, the adjacent northern suburb of Irondequoit, and the three southeastern villages of East Rochester, Fairport, and Pittsford; most of the land of the other adjacent suburban towns (moving clockwise around the city from the east) of Penfield, Brighton, Chili, Gates, and Greece, as well as much of the more distant suburbs (again clockwise) of Webster, Perinton, Pittsford, Henrietta, and a bit of Ogden to the west. In the whole of Monroe County there are thirty such jurisdictions, each with a planning board and zoning ordinances.[3] There is also a county planning board which, however, lacks authority to coordinate local boards. In the three additional counties of the SMSA there are sixty-five more towns and villages, of which forty-nine have zoning ordinances. (Most of the sixteen without ordinances are on the fringe of the SMSA.) It is easily seen, therefore, that the land of the urbanized area (and the larger metropolitan area) is zoned in a highly fragmented way. In this sense, Rochester and Monroe County are typical of most metropolitan zoning systems in the United States.

Most Rochester-area zoning systems are cumulative in the sense that single-family houses can be built in any district, even districts zoned for industrial use. In the city, however, heavy industrial districts are exclusive. As might be expected, the city has quite a bit of land zoned for various kinds of multiple residential use, as well as commercial and industrial use. Most of the land of the suburbs is zoned single-family, including most of the land ultimately intended for other uses. (As a result, most building in the suburbs, except for single-family, can be authorized only after some kind of administrative decision process.)

VI. Measurement of the Effects of Zoning and Externalities on the Price of Land

A. *The Samples*

If zoning is responsible for an allocation of land substantially different from the market allocation, its effect will manifest itself in land price differ-

[3] Genesee-Finger Lakes Regional Planning Board, Regional Housing Report No. 4 (1974).

entials. In order to determine whether such price differences exist, we selected a random sample of about one-sixth of all real estate transactions recorded in Monroe County during each of three years: 1950, 1960, and 1971, the year that ended just before we began our study. Observations were excluded from the sample where we had reason to believe the transfers were not arm's-length transactions, for example, sales involving government agencies, intra-corporate or intra-family transfers, and transactions with no recorded pecuniary exchange. These exclusions reduced the set of transactions for each year by about one-third.

For each piece of land remaining in the sample, we obtained data on physical characteristics and terms of sale from the deed recorded in the Monroe County Clerk's Office; on assessed value, zoning, and variance history from local municipal offices; and on the use on the parcel and its neighboring properties from either visual inspection (1971 only) or Polk's Rochester *City Directory*.[4] The "Rochester Metropolitan Transportation Study,"[5] completed in 1969, had constructed isoaccess contours of average driving time to the central business district. The general neighborhood characteristics surrounding each parcel are available in the Census of Housing Block Statistics.[6]

Details of the sampling procedure and definitions of the variables employed in the analysis are available from the authors upon request.

Altogether, ten samples were analyzed. Nine included only observations from the City of Rochester and the tenth included only observations from the suburban towns. All the samples are relatively homogeneous with respect to the use to which land is put. For 1950 and 1960 there are three samples each: one consisting of single-family properties, one multiple-family, and one commercial and industrial. For 1971 there are four samples, the fourth being single-family suburban properties. Suburban properties could not be analyzed for 1950 and 1960 because block census data covering the suburban towns were not available for those years. For 1971 the city and the suburban towns were analyzed separately in case the assessment of structures in the city differed substantially from the towns. Actually, assessments seem not to differ greatly in the several jurisdictions, and in fact the assessment of

[4] R. L. Polk & Co., Rochester (Monroe County, N.Y.) City Directory; and *id.*, Suburban Directory, for the years 1950, 1951, 1960, 1961, 1971, 1972.

[5] See Planning and Research Bureau, N.Y. State Dep't of Transportation, The 1990 Transportation Plan for Transportation Facilities: Rochester Metropolitan Transportation Study (1969).

[6] U.S. Bureau of the Census, Census of Housing: 1970, Block Statistics, Final Report HC(3)-160, Rochester, N.Y., Urbanized Area (1971); Census of Housing: 1960, City Blocks, HC(3)-281, Rochester, N.Y. (1963); and Census of Housing: 1950, Block Statistics, Rochester, N.Y., vol. V, pt. 160 (1952). We were unable to reduce the number of variables in the equation by substituting a set of principal components for the lengthy list of census characteristics.

TABLE 1
SAMPLES OF RESIDENTIAL REAL ESTATE TRANSACTIONS: DISTRIBUTION ACCORDING TO USE AND ZONE

Sample Description Location, Use, and Year of Sale	Zone			Total
	One-Family	Two-Family	Walk-up	
City of Rochester				
One-Family				
1950	1	251	120	372
1960	39	271	61	371
1971	130	224	39	393
Two-Family				
1950	0	48	66	114
1960	0	68	34	102
1971	9	74	34	108
Suburban Towns				
One-Family				
1971	676	149	165	990
Two-Family				
1971*	4	2	0	6
Walk-up				
1971*	0	8	0	8

* These numbers are given for comparison only. The samples were too small to analyze.

single-family structures both in the city and in the towns is remarkably accurate and consistent.

Within each sample, properties were zoned for several uses but were used only as indicated in Tables 1 and 2. For example, the 1971 one-family City of Rochester sample consisted of 393 sales of single-family houses located on land zoned for single-family use, two-family use, and walk-up use. The object of including only similar uses in a sample is to avoid statistical confounding of the relationship between zone and prices and the relationship

TABLE 2
SAMPLE OF COMMERCIAL AND INDUSTRIAL USE: DISTRIBUTION ACCORDING TO ZONE

Year	Zone				Total
	Residential	Neighborhood Business	Central Business District or Shopping Center	Light Industry	
1950	16	22	3	5	46
1960	21	20	6	8	55
1971 (includes suburbs)	11	25	8	11	55

between use and prices, since use and zone are highly correlated. Our test of the effect of zoning in the single-family use sample pertained only to the three residential zoning categories. We had too few observations of single-family houses zoned for nonresidential use to permit a more inclusive test. Both commercial and industrial uses are included within a single sample from a given year as shown in Table 2.

B. *The Regression Model of Sale Price*

Our tests were performed on regressions of the form,

$$P = \alpha_0 + \alpha_B B + \Sigma\alpha_{Zi} Z_i + \Sigma\alpha_{Vi} V_i + \Sigma\alpha_{Ai} A_i + \Sigma\alpha_{Xi} X_i, \qquad (1)$$

where

P = sale price per acre of land plus structure;
B = equalized assessed value of structure divided by acreage;
Z_i = dummy variables designating zoning category;
V_i = dummy variables indicating that other land, visible from the observed parcel, was devoted to some use that might produce an externality for the given land;
A_i = dummy variables indicating that land on either side of the observed parcel or directly across the street from it was devoted to some use that might produce an externality (A and V are mutually exclusive);
X_i = variables related to the value of land or the value of the structure, or in some way affecting P.

1. *Equalized Assessed Value of Structure Divided by Acreage.* The term B is included so as to eliminate the value of the structure from the total sale price and permit us to test hypotheses about the value of the land only. To determine whether B could be used for this purpose, we estimated ten regressions (for each of the ten samples) of the form,

$$P = \beta_B B + \beta_L L, \qquad (2)$$

where P and B are as previously defined and L is the equalized assessed value of land per acre. If β_B in equation (2) (and α_B in equation (1)) has a very small standard error, then the assessor is consistent in the sense that he is acting as if he is using the same standard in the same way to assess all structures. If the coefficient equals 1, he is accurate as well. To give an example, if the assessor consistently assessed structures at 50 per cent of their market value, the coefficient of B will be 2 and $2 \cdot B$ will correctly measure the market value of the structure. For our purpose, it is only necessary that the assessor be consistent in evaluating B, not that he be accurate.

Table 3 gives estimates of β_B and α_B for the ten samples and the estimated standard errors of the coefficients. The estimates of β_B are from regressions

TABLE 3
ESTIMATED COEFFICIENTS OF EQUALIZED ASSESSED VALUE OF STRUCTURE (AND THEIR STANDARD ERRORS)

Sample	Estimated Values of β_B (from Equation 2)	Estimated Values of α_B (from Equation 1)
City of Rochester		
One-Family		
1950	1.29 (.08)	1.35 (.08)
1960	1.12 (.06)	.93 (.05)
1971	.95 (.07)	.94 (.06)
Two-Family		
1950	.87 (.11)	.83 (.12)
1960	.71 (.15)	.55 (.18)
1971	.84 (.13)	.70 (.12)
Commercial and Industrial		
1950	1.18 (.14)	.99 (.23)
1960	1.06 (.06)	1.05 (.05)
1971	.82 (.11)	.74 (.12)
Suburban Towns		
One-Family		
1971	1.06 (.02)	1.05 (.03)

on equation (2) in which only one other variable appeared, the equalized assessed value of land (see Table A1 in the appendix). The estimates of α_B are all from our final estimates of equation (1), those containing X but from which zoning (Z) or externality (V,A) variables have been eliminated if not statistically significant. On the basis of these estimates, we concluded that variation in the sale price of properties (P) which was due to variation in the value of structure was mostly accounted for by B, the equalized assessed value of structure. We decided, therefore, that B could reasonably be used in equation (1) to eliminate the price of the structure from the total sale price of the land plus structure.

2. *Value-Related Variables.* The term X includes census data pertaining to the city block in which the individual observation is located, such as population density, percentage of blacks, and the average number of rooms in owner-occupied houses. It also includes access variables, certain externalities, measures of structure maintenance, and special characteristics of the sale.

The X variables are meant to account for many factors other than zoning that may affect the value of the land, principally locational advantages at particular sites. Some describe locational advantages directly, such as the set of dummies that stand for distance or travel time to the center of the city.

Traffic density described by the street classification reflects access to local subcenters.

Other variables, like population density, are indirectly related to the value of land. Land near an important employment center is valuable because employees who want to economize on transportation costs (both money and time) will prefer such land. For the same reason, population will be dense and housing units will be small. If the regression equation includes no such variable and if the zoners tend to zone such land for multiple-family use, the high value of such land will be attributed wrongly to zoning. Since many of the attributes of land that affect its value and which may also influence planning and zoning decisions can be measured only at great cost, we substituted proxies that are readily available in census data. Thus, population density and house size are proxies for proximity to centers of employment. Unfortunately, there is no way to know the exact effect of including such proxies in the equation. They could either undercorrect or overcorrect the error we wish to eliminate. As explained below, however, we believe our findings are reasonably free of statistical bias.

The term X also includes some externalities not included in V or A because, strictly speaking, they were due to uses of land that zoning could not control. One dummy variable indicates whether the property is within the 100-decibel ground noise contour of the Monroe County Airport. Another indicates whether the parcel is within sight of a body of water. While zoners can prohibit gas stations or multiple-family housing from appearing in a neighborhood and can attempt to induce these uses to group in a particular area, they cannot influence the lake or the airport.

We account for the possible influence of zoning variances, where they have been granted, on price. The grant of a variance may permit a property owner to capture the value of some advantage not enjoyed by the owners of similarly zoned land. Variances to permit more dense use of land occur in many forms, for example, permission to operate a commercial enterprise in a residential area, or approval to convert a single-family dwelling to a multiple-family building in an area zoned exclusively for single-family use. In principle, variances are similar to "spot rezonings." Yet zoning boards of appeal approach them on a case-by-case basis, frequently with reference to the potential externality problem, but rarely with respect to the broader allocational goals of land use planning. To isolate the possible impact of a variance on sale price from the allocational aims of the zoning scheme, a dummy variable was included in X to indicate such variances. Few of the properties in our samples had ever been granted a variance, and the majority of these were permits to locate a structure closer to a lot line than is allowed by law. Use variances do not occur in our single-family use samples, but they occur in both the two-family and the commercial and industrial use samples.

Two of the variables in X appear only in our 1971 regressions because the data can be obtained only by a visual inspection soon after the date of sale. These describe the maintenance of the structure involved in the sale.

Finally, two sets of variables contain information peculiar to the details of the transaction. Some parcels in our samples were sold by trustees. Since a trustee's incentives are not identical to a principal's incentives, a dummy variable was entered in the regression to indicate the type of sale. Variables which describe the amount of an assumed mortgage involved in the transaction are included here, but we postpone our discussion of them to Section VII.C.

In summary, our regression model of real estate prices contains basically three categories of independent variables: (1) zoning variables, (2) externality variables, and (3) a broad range of factors which jointly predict land prices in the absence of either zoning or externalities.[7]

C. *The Hypothesis Tests and Results*

Our working hypothesis is that zoning has an effect on land prices. We can test this hypothesis by asking whether or not the predictive power of the regression model in equation (1) is significantly decreased by excluding the zoning variables. If so, we may conclude that zoning has an impact on real estate prices independent of the other forces operating in the market. If, however, exclusion of zoning variables does not lessen the power of the models, then we cannot reject the null hypothesis that zoning has no effect on prices. We may similarly investigate the impact of other variables on land price. For example, we can investigate the impact of neighborhood uses of land that are thought to produce externalities.

To test our hypotheses, we first estimated, for each of our ten samples, a regression of the form of equation (1), except for one modification: instead of treating the effects of zone as simply additive, we treated them as having effects that varied with distance from the center of the city. Zone and access variables, therefore, appeared in the equations as cross products, allowing us to look for the impact of zoning within each of the isoaccess bands surrounding the central business district. In all but one sample, the 1971 suburban towns, our first test consisted of dropping the zone variables from

[7] Because the prices of land may reflect the demand for public services provided by the local municipalities, we collected data on school quality (per pupil expenditure) and property tax assessments. Unfortunately, we had no confidence in the value of their contribution to the explanatory power of the model, largely because they exhibit high collinearity with each other as well as with other variables in the equation. → Wallace E. Oates, The Effects of Property Taxes and Local Public Spending on Property Values: An Empirical Study of Tax Capitalization and the Tiebout Hypothesis, 77 J. Pol. Econ. 957 (1969). Crime statistics disaggregated to the neighborhood level would probably, on the other hand, display considerable explanatory power, but they were not available.

the regression and performing an F-test to determine whether the effect of zone was statistically significant. The null hypothesis was not rejected in eight cases. In one case, the 1960 two-family sample, the F-statistic was significant, so no further test was performed since successive F-tests are valid only so long as the null hypothesis is not rejected.[8]

We cannot a priori rule out the possibility that, in the city, multiple-family land sells at a premium that many one-family users are willing to pay. Our tests are intended to determine whether this is the case. In the suburbs, with 97 per cent of the multiple-family land in one-family use, it is extremely unlikely that a shortage of multiple-family land exists. We do observe that multiple-family suburban land is far more expensive than single-family, but the town planners have zoned for multiple-family-use land that is near expressway interchanges, close to the city, and close to village centers—land that would be especially valuable even in the absence of multiple-family zoning. In the 1971, suburban, one-family sample, we therefore did not test for the effect of zone, and even in testing for externalities we did not test for the effect of adjacent multiple-family uses of land since these seemed clearly to reflect locational advantages rather than negative externalities.

The two groups of dummy variables representing externalities, V and A, were not included in the regressions estimated from the three commercial and industrial samples, so the second set of F-tests, the test of V, was applied to the five remaining residential samples for the City of Rochester and to the 1971 suburban sample. The null hypothesis was not rejected in five of the samples, but in one, the 1960 one-family sample, we rejected the hypothesis that V had no effect.

The third test, the test of A, was applied to the remaining five samples, and in all five the null hypothesis could not be rejected. Table 4 summarizes the results of these tests. The complete analysis of variance for one of the samples (1971 single-family use) is given in the Appendix along with all the estimated regressions on which it is based. Details of the analysis of variance for other samples are available from the authors. Except for the two residential samples from 1960, which we will discuss in Section VII.A, there is no evidence that zoning has a price-distorting effect, or that the uses of land we included in V and A impose external costs on neighbors.

[8] In equation (1), the most general (least restrictive) statement of our model, we recognize that externalities allegedly are an inherent component of the urban real estate model where the relative proximity of neighboring uses is a consequence of the value of access. Because zoning is imposed on this market as a corrective measure, we test first for the contribution of Z to the explanatory power of a model which includes externalities. However, if the null hypothesis that zoning has no effect is not rejected, we cannot subsequently test for the effect of V and A without violating the assumption of stochastic independence between successive F-tests. See E. Malinvaud, Statistical Methods of Econometrics 220 (Stud. Mathematical & Managerial Econ., vol. 6, 1966).

TABLE 4
SUMMARY OF F-TESTS ON Z, V, AND A

	Test of					
	Z		V		A	
Sample	F	$F_{.95}$	F	$F_{.95}$	F	$F_{.95}$
City of Rochester						
One-Family						
1950	1.27	2.40	.78	2.24	.92	2.40
1960	1.30	2.24	3.51	2.40*	—	—
1971	.56	2.40	1.23	2.04	.93	1.78
Two-Family						
1950	1.04	2.72	2.48	2.49	1.09	2.47
1960	6.68	3.97*	—	—	—	—
1971	.98	2.73	1.05	2.06	.43	2.11
Commercial and Industrial						
1950	2.30	2.35	—	—	—	—
1960	.53	2.25	—	—	—	—
1971	1.15	2.14	—	—	—	—
Suburban Towns						
One-Family						
1971	—	—	.68	1.95	1.15	1.95

* The F-value is significant at the .95 level, the null hypothesis was rejected, and subsequent F-tests were not performed.

Our tests will detect effects that are capitalized, but they will not measure transaction costs associated with externalities. The appearance of a gas station or the construction of an expressway, for example, might be very distasteful to some owners of nearby land. If they choose to move away from the use that is offensive to them, then according to our findings they will not experience a capital loss on the sale of their property. Presumably the reason external costs are not observed to be capitalized into the value of the land is that there exists a sufficient diversity of tastes among potential buyers; that is to say, there are buyers who are indifferent to the offensive use (perhaps even value it). Even so, the seller incurs the costs of consummating the sale and of moving, and to the extent of these transaction costs, affected property owners are motivated to oppose proposed uses distasteful to them.

We believe that our findings are reasonably free of statistical bias resulting from the use of proxy variables. Two arguments can be made for this view. First, in almost every case we find no effect of the zone variables Z on land prices. Were the underlying locational variables not included either directly or by proxy, then part of their effect would be thrown into the zone variables. In principle, the proxy variables can either under- or overcorrect for this. As it happens, the correction is exactly enough in eight cases out of nine

to reduce the effect just to zero. Either this is because there is no effect of Z except for the spurious effect which is corrected by the proxies, or because there is an effect due to Z, and the proxies overcorrect—in eight cases out of nine—by just enough to eliminate the effect. The latter is so unlikely an event that we are encouraged to believe that our findings are free of statistical bias.

A second feature of the result that also encourages us in this belief is that in addition to finding the zoning variables insignificant we also find the externality variables insignificant. Of course, there is no reason to expect the externality variables to be biased in the way the zoning variables might be. But if there are, indeed, no significant externalities, a powerful political motive for restricting land use is absent. This tends to support the finding that zoning has no effect.

VII. Other Determinants of Price: Comments on Value-Related Variables (X)

A. *Negro Population and the Value of Land*

All of the residential regressions included among the X-variables a block census measure of the black population, N. For 1950 and 1960, we used the proportion of all houses in the block occupied by nonwhites, and in the 1971 sample we used the proportion of the block's population that was black in the 1970 Census. We first estimated regressions in which only a linear term was included. In all seven equations, the coefficient was negative and significant, indicating that use by blacks has some depressing effect on land prices.

Doubting that the effect would be constant over the entire range of the variable from 0 to 1, we reran the regression, adding a squared term so as to estimate a quadratic function of the variable. In all seven regressions, the linear term is negative, the squared term is positive, and the two coefficients are roughly of the same magnitude. Though only the two 1971 city samples yield statistically significant coefficients in the final regressions, the fact that linear coefficients, when estimated alone, were significant, combined with the similarity of the coefficients from sample to sample, strongly suggests that the coefficients measure an effect that really is present.

Either of two models might explain our results:[9] (1) blacks may choose to live on cheaper land than whites because they are poorer than whites, or (2) prejudice against blacks may depress the value of the land in neighborhoods in which they live. Most of our findings are consistent with both models, but

[9] For a full discussion of this problem, see John F. Kain & John M. Quigley, Housing Markets and Racial Discrimination: A Microeconomic Analysis (Nat'l Bur. Econ. Res., Urb. & Regional Stud. no. 3, 1975).

there is some evidence in support of the second that is not consistent with the first.

In every sample (except for the suburban sample, in which less than 1 per cent of the population is black), the price per acre falls, but at a decreasing rate over the entire range, $0 < N < .5$. This could be explained by the first model. But in two samples, the 1960 and 1971 two-family samples, N varies enough to permit inferences about the range $.5 < N < 1.0$. In both cases the effect is reversed somewhat between $N = .6$ and $N = .8$. Land that is sold in a predominantly black neighborhood seems to be worth more than the land sold in a neighborhood that is half white and half black. This is not consistent with the poverty model, but it is consistent with some versions of the prejudice model. It is also consistent, however, with a model in which blacks (as well as whites) are averse to living in mixed neighborhoods.

Also consistent with the prejudice model are the results of the F-tests on the 1960 residential samples. Recall that for one-family houses, zoning was not significant but visible externalities were. Of the V-coefficients, one stands out, that indicating a visible, multifamily residence. It reduces price by almost \$12,000 per acre (about \$1,400 per house) and has a t-value of -3.6. For two-family houses, zoning was significant in the 1960 sample. There is a \$52,000 difference per acre (about \$6,000 per house) in the sale price of two-family houses zoned for walkup use and those zoned for two-family use. Thus, in 1960 there was a large premium on houses that could readily be converted to dense use. These two results are consistent with the prejudice model if blacks were then especially likely to occupy multifamily houses.

Presumably, in 1960 there was some limitation by zoning on dwellings for blacks—reflected in the higher price for readily converted two-families—and presumably the motive for the limitation is found in the visible externality. The statistical evidence we offer on this subject is, however, merely suggestive. It would take far more study than we have given it to say anything conclusive.

B. *Externalities Not Directly Controlled by Zoning*

Many variables were measured for the 1971 samples that could not be measured for the two earlier samples. Among these were some externalities not included in V and A: airport noise, nearby (but not adjacent) land recreation, and a visible body of water. These variables were not tested in the analysis of variance. We expected airport noise to be negative in its effect on the residential samples and the other two to be positive. Airport noise is included in the one-family city and suburban samples, but it was not included in the two-family city sample because we had no two-family houses inside the 100-decibel ground contour around the airport. In the city sample

of one-family houses, land within the contour sold for almost $22,000 per acre less than land in quieter parts of the city. In the suburbs, the cost imposed by airport noise was just over $11,000. Both effects are statistically significant, with t-values greater than 2. For the city, this is a cost per house of about $2,500, and for the suburbs, where the number of houses per acre is fewer, the cost per house is probably larger. These figures seem reasonable, given that suburban families earn more than city families and may, therefore, be willing to pay more for quiet.

In the city and suburban one-family samples, the effect of nearby land recreation was positive as we expected, but in the city two-family sample it was negative. Only the suburban effect is significant; it is worth about $16,000 per acre to be near a recreational facility. There may be differences in the quality of facilities or in the way they are used that account for the differences in effect.

The suburban sample also exhibited the only significant effect of a visible body of water—$15,000 per acre. In the city, the visible body of water was usually the Genesee River, while in the suburbs it was either Lake Ontario or Irondequoit Bay.

C. *Mechanics of Land Transactions: Sale by Trustee and Assumed Mortgages*

The variable representing sale by a trustee appears in all ten samples; its coefficient is negative in all ten and is statistically significant in seven. Properties were sold by trustees at prices ranging from 5 per cent to 50 per cent less than properties not sold by trustees. Weighting by sample size and averaging over all ten samples, the average effect is about a 16 per cent price reduction. Frequencies of trustee sales ranged from about 5 per cent to about 10 per cent of the various samples. Apparently trustees have less motivation than owners to work hard on getting a good price.

We also tried to measure the effect on price of the capitalized value of mortgages assumed by the buyer at the time of purchase. If the seller holds an assumable mortgage with an interest rate lower than that available at the time of sale, then *ceteris paribus* the property will be in greater demand and will sell at a premium. We recorded, for each observation, the amount of an assumed mortgage and the interest rate charged to it. We hypothesized that a term $(r_c - r_m) \cdot M$ would have a positive coefficient that would convert the interest savings of the current year into the capital value of all interest savings over the life of the mortgage. In this term, r_c is the current market rate of interest, or ideally, the opportunity rate for people who assume mortgages, r_m is the rate on the assumed mortgage, and M is the amount of the mortgage. Actually, we included three terms in the regression equation: a dummy to indicate assumption of a mortgage, and the term $\alpha \cdot M + \beta \cdot r_m M$.

The dummy was intended to allow the possibility that certain fixed costs are avoided by assumption of a mortgage. We expected $\alpha > 0$, $\beta < 0$, and $\alpha - \beta$ to be approximately the opportunity discount of the buyers. The results are internally consistent in several ways and are, unfortunately, perverse. In seven cases, it was true that $\alpha < 0$ and $\beta > 0$, which is the opposite of what we expected. In all cases, the dummy coefficient is negative and substantial, a result we did not expect. We cannot account for the value of these coefficients, but their similarity from sample to sample suggests that a single model explains them all.

VIII. Conclusions

Our principal conclusion is that comparisons we made reveal no price effects attributable to zoning. These comparisons are limited. We compared the several types of residential land and found no evidence of a shortage of multifamily land. We compared industrial and commercial land and found no evidence that either is scarce relative to the other. We did not compare residential land with commercial and industrial land, so we cannot rule out the possibility that zoning does modify the market allocation across that division. So in this case study at least, and within the limits of our tests, it appears that political forces, however much they originally aimed at modifying market outcomes, did not in fact do so.

We found that—although some external effects from airport noise and nearby bodies of water could be detected—the externalities which zoning is supposed to prevent could not be detected, except in one instance where zoning may have been associated with racial prejudice. The fact that zoning externalities are trivial suggests that the pressure to make zoning effective is likely to be weak and, hence, tends to support the principal finding that zoning is ineffective.

On occasion that pressure may be great, and then zoning probably does just what it was intended to do. It gives zoning officials and neighborhood groups control over egregious events. Landmarks may be preserved or neighborhood areas with architectural or cultural attributes may be maintained, but the amount of control embodied in zoning appears to be both small and episodic. Zoning does not influence prices by altering the total supply of land available for various uses. We have no reason to believe that zoning used to achieve the goals of planning or fiscal policy for a municipality will produce results at variance with our findings.

Yet the institution and operation of zoning is not free. Administrators and planners are employed to run the system; and, on the other side, land owners employ lawyers, architects and other professionals to attempt to minimize the effect of zoning. Nevertheless, the combined salaries and costs of support

services for the planning and zoning officials are but a small fraction of most municipal budgets. And the private costs are no doubt similarly inconsequential. Hence, while zoning costs something, this something is probably not large enough to have a significant effect on land prices.

And as a final note it should be pointed out that, while the externalities said to be prevented by zoning are probably unreal, still there are doubtless significant transactions costs for particular landowners. With this fact in mind we recommend consideration of alternatives to zoning, particularly a greater reliance on the judicial system.[10]

[10] → Robert C. Ellickson, Alternatives to Zoning: Covenants, Nuisance Rules, and Fines as Land Use Controls, 40 U. Chi. L. Rev. 681 (1973).

APPENDIX

TABLE A1

REGRESSION OF SALE PRICE ON ASSESSED VALUES

Sale Price/Acre = Constant + β_B (Equalized Assessment Building/Acre) + β_L (Equalized Assessment Land/Acre) + e

Data Set	Constant	Coefficient β_B	Coefficient β_L	Std. Error of Est.	R^2	n
Single Family Use						
1950 City	$34,198	1.29	0.34	20,052	.48	372
		(15.66)	(1.15)			
1960 City	40,339	1.12	−.50	28,076	.54	371
		(19.27)	(2.26)			
1971 City	63,511	.95	−.08	35,183	.40	398
		(14.61)	(.30)			
1971 Suburb	11,649	1.06	.88	23,777	.79	990
		(46.98)	(8.64)			
Two Family Use						
1950 City	35,929	.86	1.22	20,516	.42	114
		(8.08)	(1.89)			
1960 City	33,264	.71	.88	53,316	.24	102
		(4.83)	(.99)			
1971 City	8,018	.84	1.76	46,925	.42	117
		(6.55)	(3.14)			
Commercial and Industrial Uses						
1950 City	19,695	1.18	1.45	34,552	.73	46
		(8.24)	(6.21)			
1960 City	37,202	1.06	−.07	78,323	.90	55
		(18.79)	(.47)			
1971 Urban Area	51,160	.83	1.04	89,945	.91	55
		(7.23)	(10.98)			

( ) = t-value

TABLE A2
CITY OF ROCHESTER ONE-FAMILY HOUSES, 1971 UNRESTRICTED FORM

Variable Name	Regression Coefficient	Standard Error	t-value
Eqlzd Assessment Bldg/Acre	.9435	.0591	15.98
Access: Avg Driving Time			
5-10 minutes	−7,002	9,541	−.73
10-15 minutes	−1,647	9,733	−.17
Access × Zone (Z)			
Two-Family Zone			
5-10 minutes	6,815	5,193	1.31
10-15 minutes	−3,228	6,227	−.52
15-20 minutes	466	17,240	.03
Walk-up Apartment Zone			
0-5 minutes	−19,768	15,452	−1.28
5-10 minutes	7,002	8,286	.85
Visible Uses (V)			
Multi-Family Residential	−6,233	3,888	−1.60
Commercial	126	4,091	.03
Industrial	−9,471	4,551	−2.08
Dump or Slum	6,492	13,002	.50
Body of Water	3,726	15,306	.24
Land Recreation	4,701	7,882	.60
Expressway or Busy Street	3,460	4,152	.83
Public Building	−6,516	5,295	−1.23
Vacant Land	11,712	21,549	.54
Airport	−22,994	8,524	−2.70
Adjacent Uses (A)			
Two-Family Residential	−5,914	3,948	−1.50
Walk-up Apartment	−8,986	7,691	−1.17
Professional Office	13,692	14,808	.92
Neighborhood Business	−4,294	9,830	−.44
CBD or Shopping Center	−9,463	22,775	−.42
Light Industry	−502	11,495	−.04
Heavy Industry	−900	19,178	−.05
Gas Station	−6,510	23,533	−.28
Dump or Slum	−15,872	18,196	−.87
Recreation	−10,812	13,506	−.80
Public Building	15,263	9,797	1.56
Vacant Land	6,331	16,946	.37
Value-Related Variables (X)			
Variance			
Area	−2,450	10,246	−.24
Density	−2,560	18,200	−.14
Commercial	−30,974	18,085	−1.71
Sale by Trustee	−31,976	5,042	−6.34
Mortgage/Acre	.5785	.3927	1.47
Mortgage × Interest/Acre	−.6714	5.221	−.13
Mortgage Dum	−50,887	10,384	−4.90
Type of Street			
Major Arterial	1,591	16,255	.10
Minor Arterial	903	7,010	.13
Collector	6,286	6,946	.91

TABLE A2 (*Continued*)

Variable Name	Regression Coefficient	Standard Error	*t*-value
Maintenance			
Good	.1138	.0649	1.74
Poor	−.0935	.0594	−1.58
Census Variables			
Population Density	−2,439	4,786	−.51
% Negro	−45,206	27,352	−1.65
% Negro Sq	19,478	34,878	.56
% 62 and Over	−5,715	30,899	−.19
% in 10-unit Bldg	−4,850	19,796	−.25
Avg No. Rooms Owner-Occ	30,731	12,567	2.45
Avg No. Owner-Occ Rooms Sq	−3,338	−1,002	−3.33
Avg Rms Own-Occ Missing Info Dum	39,495	50,721	.78
Avg No. Rooms Renter-Occ	738	1,390	.53
Avg Rms Rent-Occ Missing Info Dum	15,138	7,169	2.11

Constant = 21,351
Number of Observations = 398
R^2 = .62
Standard Error of Estimate = 29,950

TABLE A3
CITY OF ROCHESTER ONE-FAMILY HOUSES, 1971 FINAL FORM

Variable Name	Regression Coefficient	Standard Error	*t*-value
Eqlzd Assessment Bldg/Acre	.9450	.0552	17.12
Access: Avg Driving Time			
0-5 minutes	−27,267	13,616	−2.00
5-10 minutes	−6,304	8,126	−.78
10-15 minutes	−7,274	8,346	−.87
Visible Uses			
Body of Water	5,035	14,413	.35
Land Recreation	2,962	7,025	.42
Airport	−21,792	8,232	−2.65
Value-Related Variables (X)			
Variance			
Area	−2,368	9,954	−.24
Density	−924	17,944	−.05
Commercial	−35,173	17,544	−2.00
Sale by Trustee	−32,576	4,954	−6.58
Mortgage/Acre	.5584	.3855	1.45
Mortgage × Interest/Acre	−.5890	5.134	−.11
Mortgage Dum	−49,110	10,061	−4.88
Type of Street			
Major Arterial	−3,413	15,311	−.22
Minor Arterial	−2,143	5,896	−.36
Collector	7,204	6,656	1.08

TABLE A3 (*Continued*)

Variable Name	Regression Coefficient	Standard Error	*t*-value
Maintenance			
Good	.1161	.0641	1.81
Poor	−1054	.0563	1.87
Census Variables			
Population Density	−1,909	4,543	−.42
% Negro	−51,867	25,694	−2.02
% Negro Sq	30,869	32,526	.95
% 62 and Over	8,472	28,757	.29
% in 10-unit Bldgs	−6,762	17,300	.39
Avg No. Rooms Owner-Occ Units	28,733	11,177	2.57
Avg No. Own-Occ Rooms Sq	−3,196	901.2	−3.55
Avg Rms Own-Occ Missing Info Dum	20,992	47,309	.44
Avg No. Rooms Renter-Occ Units	−10.06	1,321	−.01
Avg Rms Rent-Occ Missing Info Dum	11,936	6,956	1.72

Constant = 25,996
Number of Observations = 398
R^2 = .60
Standard Error of Estimate = 29,907

TABLE A4
CITY OF ROCHESTER ONE-FAMILY HOUSES, 1971 ANALYSIS OF VARIANCE

Source of Variance	Sum of Squares	Degrees of Freedom	F	$F_{.95}$
1. Residual	3.0948×10^{11}	345		
2. Z	2.0024×10^{9}	4		
			.56	2.40
3. 1 + 2	3.1148×10^{11}	349		
4. V	7.6912×10^{9}	7		
			1.23	2.04
5. 3 + 4	3.1917×10^{11}	356		
6. A	9.9824×10^{9}	12		
			.93	1.78
7. 5 + 6	3.2916×10^{11}			