The Structure of Local Public Finance and the Quality of Life

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Differences in local fiscal conditions generate compensating differentials across local land and labor markets just as we have long known amenities to do. Thus the fiscal climate affects the quality of life across metropolitan areas. We present new results showing that intercity fiscal differentials are nearly as important as amenity differentials in determining the quality of life across urban areas. The paper also investigates the sensitivity of the quality-of-life rankings with respect to assumptions about the nature of the marginal entrant. We estimate a random effects model to account for city-specific error components in the housing and wage regressions. Those results indicate that the standard errors of previous OLS-based quality-of-life rankings have been biased downward substantially. More encompassing data on city traits as well as superior controls for worker and housing quality are needed to increase the precision of quality-of-life estimates.

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I. Introduction

Urban quality-of-life rankings date back to work by Liu (1976). Rosen (1979) and Roback (1980, 1982) provided a theoretical foundation for these rankings by identifying market prices for amenities that can serve as weights in the construction of quality-of-life measures. These prices are implicitly generated by the capitalization of interurban amenity differences into local land rentals and wage rates.[1]

In this paper, we relax three important assumptions that traditionally are made in this literature. The first is that all city characteristics used to construct the quality-of-life measure are pure amenities. A pure amenity is a nonproduced public good such as weather quality that has no explicit price. In practice, previous empirical studies include some government services such as education or public safety, with their marginal valuations measured using implicit prices. However, government services are not pure amenities in that they are produced and have explicit tax prices. If a service is fully priced via local taxes, then independent of the actual marginal valuation of the service, no implicit price in terms of wage or land rent capitalization will exist for the service. Estimating the full price for services necessitates adding state and local taxes to the land rental and wage specifications. We find that intercity fiscal differentials have nearly as much influence on the quality-of-life index values and rankings as intercity amenity differentials do.

The second assumption relaxed is that all locational rents are capitalized into land rentals or private-sector wages. This assumption has been employed to ensure that the implicit prices fully reflect marginal valuations of the amenities. However, Ehrenberg and Schwarz's (1986) and Freeman's (1986) reviews of the increasing unionization of the local public sector over the last 30 years cast doubt on the validity of this assumption. Collective bargaining by local public unions may lead to sharing of locational rents between residents and local public workers. This can give rise to explicit tax prices even for pure amenities. Consider an extreme case in which all the locational rents derived from an amenity are captured by the local public union in the form of wage premia. There would be no capitalization of the amenity in either the land or (private-sector) labor markets. The value of the amenity would be reflected in the differential tax burdens the

[1] There has been a renewed interest in this topic, with Blomquist, Berger, and Hoehn (1988) extending the Rosen/Roback framework to include agglomeration economies. Other recent efforts at estimating compensating differentials arising from city attributes include Hoehn, Berger, and Blomquist (1987), Berger and Blomquist (1988), Roback (1988), Gyourko and Tracy (1989*a*, 1989*b*), Leven and Stover (1989), and Voith (1991).

residents must pay in order to finance the union wage premia. This provides a second rationale for including controls for local taxes in quality-of-life measures. Consistent with our earlier work (Gyourko and Tracy 1989*a*), we find that land prices are lower in cities with more highly organized public-sector work forces. However, with taxes and service measures held constant, the independent impact of local public unionization is very small on average.

The third assumption relaxed is that there are no uncontrolled for city-specific group effects present in the housing and wage expenditure hedonics. Our data strongly indicate that the error terms in these equations contain both city-specific and individual-specific components. The city-specific components reflect systematic influences of omitted attributes on land rentals and wages. An important question is whether these city-specific effects should be included in the calculation of the quality-of-life rankings. If they reflect unmeasured housing structure quality or worker human capital, then they should not be included in the rankings. However, if they reflect omitted amenity or fiscal variables that would have been included in the specifications if the data had been available, then they should be priced out and included in the rankings. Because we cannot clearly identify the source of these city-specific group effects, quality-of-life rankings with and without the group effects are reported. The fact that including the group effects materially alters the rankings highlights the need for better worker, housing, and city data in order to pin down more precisely the true urban quality of life.

Section II briefly outlines an expanded Rosen/Roback model incorporating locally provided services and taxes as well as a rent-seeking local public sector. Section III describes the data used in the empirical analysis. Section IV details the econometrics and reports results on trait prices and quality-of-life rankings. There is a short conclusion (Sec. V) and a Data Appendix.

II. An Expanded Rosen/Roback Model

Government services and taxes can easily be included in the Rosen/Roback model, as our two earlier papers show. Because the underlying Rosen/Roback model is now well known, this section reproduces only the essential equations from our previous work.

Workers and firms compete for scarce sites across jurisdictions. Let there be a representative worker-resident consuming a composite traded good Y, land services N, and a package of locally provided services and amenities (G_j, A_j) for each city j. The service/amenity package is taken as exogenous by all potential residents and firms. The service package (G) is financed using one or more of the following taxes: a sales tax of rate s on the composite good whose price is the numeraire, an income tax of rate z on gross wages W^g, and a property tax of rate t per local land rental n. There is endowment income of I. Firms use land services, labor, and intermediate goods in production. The latter are assumed to be subject to the sales tax.

Via the standard utility and profit maximizations, worker and firm evaluations of communities are given by an indirect utility function (V) and an indirect profit function (Π). With perfect mobility in the long run, both worker utility and firm profitability must be equalized across jurisdictions as follows:

$$V = \overline{V}\{(1 - z_j)W_j^g, (1 + t_j)n_j, (1 + s_j), I; A_j, G_j\} \quad \text{for all } j, \tag{1}$$

$$\Pi = \overline{\Pi}\{W^g, (1 + t_j)n_j, (1 + s_j); A_j, G_j\} \quad \text{for all } j. \tag{2}$$

Worker utility in jurisdiction j is determined by the net wage received, $(1 - z_j)W_j^g$; gross-of-tax land rentals, $(1 + t_j)n_j$; what is essentially the nonland cost of living, $1 + s_j$; and the amenity and public-service package, A_j and G_j. Firm profits are determined by similar factors, although firms care about the gross wage paid (W_j^g). We assume that the amenity/service package enters the firm's indirect profit function through its underlying effect on the firm's production function.

These equilibrium conditions can be solved implicitly for the reduced-form wage and land rental equations (with R_j defined as the gross-of-tax land rent):

$$W_j^g = W\{(1 + s_j), z_j, I, G_j, A_j\}, \tag{3}$$

$$R_j = (1 + t_j)n_j = N\{(1 + s_j), z_j, I, G_j, A_j\}. \tag{4}$$

The equilibrium wage and land rental price are given by the intersection of the level sets of (1) and (2) as illustrated in figure 1. The comparative statics for the reduced-form equations are straightforward and are derived in our earlier papers.

As Linneman (1978) implies, if (3) and (4) could be estimated with exact measures of all fiscal variables, then the full prices for amenities and services could be recovered even when rent sharing takes place between residents and local public unions. (However, the marginal impact of local public employee rent seeking could not be directly observed.) Because of the inevitably inexact natures of our tax and service variables, it is helpful to augment the land rent and wage equations with a direct measure of local public union rent seeking (U). That measure should reflect potential residents' perceptions of the union's ability to extract current and future rents. This requires some estimate of the union's long-run institutional strength. We use the extent of local public-sector union coverage in the empirical work

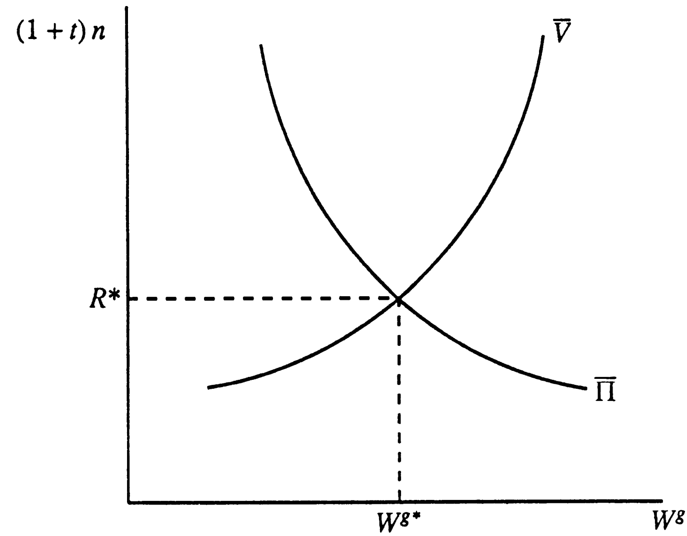

FIG. 1

below. Note that to the extent that the fiscal proxies already incorporate some impacts of rent-seeking activity, the measured influence of U on wages and land prices will not fully capture the costs (or benefits) to worker-residents of local public unionization.

III. Description of the Data

Data were collected for 130 cities throughout the United States. A house value measure is used to proxy for land prices (n_j) because there is no consistent land price series for our sample. Both the housing value and wage data (W^g) come from a 1 percent random subsample of the 1/1,000 public-use A sample of the 1980 *Census of Population and Housing* and pertain to the year 1979. There are 5,263 observations on housing units and 38,870 observations on individual workers.

The wage variable used is the worker's average weekly wage for 1979. To be included in the sample, the resident had to be a full-time labor market participant in the private sector. Public-sector workers are excluded because tax rates and service levels cannot be considered exogenous determinants of their wages, as Inman (1981, 1982) shows.

The housing expenditure variable is based on a reported interval of housing values in the census data. We use a two-step procedure to impute a specific housing value to each interval. First, we estimate the continuous distribution of housing values that best accords with the observed frequencies for each interval. This estimated distribution is then used to calculate expected housing values conditional on being in each interval. Those results are available on request. The imputed housing expenditure is the conditional expected housing value converted into an annual rent using the 7.85 percent discount rate from Peiser and Smith's (1985) user cost paper.

A variety of other selection criteria were also employed in generating the sample. First, the wage sample is restricted to individuals who live and work in the central city, while the housing sample is restricted to homes located in the central city. This is necessary for matching with city-specific fiscal data collected for central cities of major standard metropolitan statistical areas (SMSAs). Given the heterogeneity in fiscal conditions across any given urban area, it is important that the dependent variables pertain to the same jurisdiction as the right-hand-side fiscal measures. The housing sample is further restricted to units on lots of 1 acre or less in order to narrow the quality range of houses analyzed.

The model in Section II assumes a given quality worker and housing unit. To help control further for quality differences in housing structures, we included in the housing expenditure specification a variety of structural traits reported in the census. The wage specification includes the standard set of human capital proxies, which are also interacted with the worker's sex. Finally, a series of 22 major industry and occupation classifications are controlled for.

A host of city characteristics are merged with the census data. Appendix table A1 more fully describes these variables and their sources. The set of amenities includes the following weather and pollution variables: average annual precipitation, relative humidity, average wind speed, the percentage of days with sunshine, heating and cooling degree days, and an air pollution variable measuring mean total suspended particulates.[2] We also created a dichotomous variable indicating whether the city borders an ocean, the Gulf of Mexico, or one of the Great Lakes. Metro area size, measured by the population of the SMSA, is included as another amenity proxy. Intermediate

[2] Glenn Blomquist provided us with added pollution measures used in Blomquist et al. (1988). These included the number of effluent discharges, metric tons of landfill waste, and the number of treatment, storage, and disposal sites. Blomquist used county-level data, so we sometimes had trouble matching with individual cities. Including these added pollution measures reduced our sample size to under 90 cities. In order to have as much variation as possible in fiscal conditions, we decided to drop the added pollution measures in order to obtain the 130-city sample size that serves as the base for all results reported in the paper. On the smaller sample, we estimated wage and housing expenditure equations that included the added pollution variables. These variables are not statistically significant (individually or jointly) in the random effects specification. Further, the estimated group effects are only marginally smaller than those reported in table 1 below.

family metropolitan budget data from the Bureau of Labor Statistics (BLS), adjusted to remove housing costs, are used to approximate the nonland cost of living. Because of missing observations, this variable had to be imputed for many of the cities in the sample. All standard errors have been adjusted to reflect the imputation. (See the notes to Appendix table A1 for the details.) A final locational amenity controlled for is the percentage of workers in a city that work in another SMSA. This variable is intended to control for access to alternative labor markets. City size held constant, a Connecticut city near New York City is different from a similarly sized city in (say) Iowa because location near a large labor market makes the opportunity set of the Connecticut residents larger than that faced by the Iowa residents.

We collected data on seven fiscal variables. Each of these measures pertains to the central city only and is not a metropolitan area average. The income tax variable is the sum of state and local income tax rates. Data on state corporate tax rates are also included. The effective local property tax rate variable used is identical to that in Gyourko and Tracy (1989*a*) and is the product of the nominal rate times the assessment/sales ratio. We control for four government services: police, fire, health, and education. We attempted to construct output measures for each service since expenditures probably are very poor service proxies for many of the central cities in our sample. For police services, the per capita incidence of violent crimes is used. However, health services are proxied for by an input measure that is the number of hospital beds per thousand people.[3] The measure used for fire services is a rating scheme developed by insurance companies for setting premiums in a city. The ratings range from one to 10, with one being best. The last service controlled for is education. Data problems prevent us from employing standardized test scores as an output measure. Consequently, we use the student/teacher ratio.[4]

[3] In previous work, we experimented with both violent and property crime rates and found no significant effects of property crime, with violent crime held constant. We also investigated alternative input and output health measures including infant mortality rates, the number of physicians per capita, and the number of medical specialists per capita. These variables can be measured only at the county level. Consequently, we chose to employ the city-specific hospital beds measure.

[4] Even test score data have the potential defect of confounding the quality of educational services provided with the ability of students attending the schools. For example, the Scholastic Aptitude Test is taken only by those students applying to college, the fraction of which varies by city and state. Dynarski (1987) and Hanushek and Taylor (1988) demonstrate how to adjust statewide test scores for the selection bias. We were not successful in collecting district test score data via phone or letter surveys. Many districts claim not to have collected or saved data for 1979 or adjacent years. This made it impossible to implement those authors' selection correction procedure with the city district as the unit of observation. While there is little statistical evidence that class size affects student achievement (e.g., Hanushek 1986), Card and Krueger (1990) find a strong correlation between class size and subsequent labor market performance.

Our measure of the institutional strength of public-sector unions used to proxy for the perceived union rent-seeking potential (U) is the percentage of local public workers in the central city who are organized. By the late 1970s, public-sector unionization had leveled off. Differences in public-sector organization strength across cities in 1979 reasonably could be perceived by residents as reflecting long-run differences in union bargaining power.

IV. Econometric Specification and Results

To estimate the reduced-form wage equation given in (3), we assume that the wage for individual i in city j is represented as

$$\ln W_{ij} = \beta_0 + \mathbf{X}_i\beta_1 + \mathbf{Y}_i\beta_2 + \mathbf{Z}_j\beta_3 + u_{ij}, \quad u_{ij} = \alpha_j + \epsilon_i, \tag{5}$$

where $\mathbf{X}_i$ is a vector of individual worker traits, $\mathbf{Y}_i$ is a vector of industry and occupation controls, $\mathbf{Z}_j$ is a vector of community amenity and fiscal attributes, $\alpha_j \sim N(0, \sigma_\alpha^2)$, and $\epsilon_i \sim N(0, \sigma_\epsilon^2)$. To estimate the reduced-form housing expenditure equation in (4), we assume that the housing expenditures for individual i in city j are represented as

$$\ln n_{ij} = \gamma_0 + \mathbf{H}_i\gamma_1 + \mathbf{Z}_j\gamma_2 + v_{ij}, \quad v_{ij} = \delta_j + \eta_i, \tag{6}$$

where $\mathbf{H}_i$ is a vector of housing unit structural traits, $\mathbf{Z}_j$ is a vector of community amenity and fiscal attributes,[5] $\delta_j \sim N(0, \sigma_\delta^2)$, and $\eta_i \sim N(0, \sigma_\eta^2)$. In estimating (5) and (6), we allow the error terms to contain both an individual and a city component. The city component in each equation is common to all workers or housing units in a given city and is assumed uncorrelated across observations from different cities.

To test for the appropriateness of ordinary least squares (OLS) versus random effects in each specification, we calculated the one-sided Lagrange multiplier statistic for the null hypothesis that either $\sigma_\alpha^2 = 0$ or $\sigma_\delta^2 = 0$. In each case, the data strongly reject the null hypothesis of a zero group variance. We also tested for possible correlation between the city-specific error component and the included variables using the procedure described in Hausman and Taylor (1981). The data did not reject the null of zero correlation in either specification.

The results from the underlying wage and housing expenditure hedonics are presented in the first two columns of table 1. We report findings only for the city-specific traits. The results on the individual worker and housing unit traits yielded no surprises and are available

[5] Note that the effective property tax rate is included in the housing expenditure equation but not in the wage equation. The property tax is solved out of the reduced-form wage equation. The equilibrium model in Sec. II implies full capitalization into land prices. The results presented below do imply virtually full capitalization.

TABLE 1

REGRESSION RESULTS AND TRAIT PRICES: RANDOM EFFECTS SPECIFICATION

City Trait	Annual Housing Expenditure Hedonic* (1)	Weekly Wage Hedonic† (2)	Annualized Trait Prices‡ Housing (3)	Wage (4)	Full (5)
Precipitation	−.0139	−.0027	−$22.82	−$21.59	−$1.22
	(.0030)	(.0009)	(4.98)	(6.83)	(8.45)
Cooling degree days (thousands)	−.1344	.0031	−7.97	.89	−8.86
	(.0562)	(.0157)	(3.33)	(4.49)	(5.59)
Heating degree days (thousands)	−.0277	.0172	−5.65	16.93	−22.58
	(.0248)	(.0069)	(5.06)	(6.82)	(8.49)
Relative humidity	.0145	.0033	36.53	40.14	−3.61
	(.0053)	(.0015)	(13.36)	(18.67)	(22.95)
Sunshine (percentage possible)	.0079	−.0005	21.84	−6.03	27.87
	(.0056)	(.0016)	(15.38)	(21.97)	(26.82)
Wind speed (mph)	−.0427	−.0192	−18.18	−39.57	21.39
	(.0179)	(.0055)	(7.62)	(11.31)	(13.64)
Particulate matter	−.0019	−.0003	−6.36	−4.34	−2.01
	(.0013)	(.0004)	(4.20)	(−5.79)	(7.15)
Coast	.1345	−.0201	654.15	−435.70	1,089.86
	(.0694)	(.0199)	(360.15)	(429.20)	(560.28)
Cost of living	.6496	.2633	28.89	56.58	−27.70
	(1.6197)	(.4208)	(71.94)	(90.42)	(115.55)
Violent crime	.0574	.0705	2.51	14.91	−12.40
	(.0425)	(.0109)	(1.86)	(2.31)	(2.97)

Student/teacher ratio	−.0107	−.0010	−6.84	−3.09	−3.76
	(.0096)	(.0027)	(6.14)	(8.31)	(10.33)
Fire rating	.0498	.0156	6.93	10.48	−3.55
	(.0255)	(.0078)	(3.55)	(5.28)	(6.36)
Hospital beds	.0031	−.0036	1.82	−10.03	11.85
	(.0037)	(.0010)	(2.12)	(2.82)	(3.53)
Property tax rate	−.1037	. . .	−6.14	. . .	−6.14
	(.0399)		(2.37)		(2.37)
State and local income tax rate	−.0287	.0020	−3.99	1.37	−5.36
	(.0101)	(.0029)	(1.41)	(1.95)	(2.41)
State corporate tax rate	.0208	−.0067	5.97	−9.33	15.30
	(.0100)	(.0029)	(2.88)	(3.98)	(4.91)
Percentage public union organized	−.1646	−.0041	−3.29	−.39	−2.89
	(.1302)	(.0385)	(2.60)	(3.72)	(4.54)
SMSA population (millions)	.0376	.0096	1.27	1.57	−.30
	(.0223)	(.0057)	(.76)	(.93)	(1.20)
Percentage working in other SMSA	1.4693	.1052	5.34	1.85	3.49
	(.6325)	(.1969)	(2.30)	(3.46)	(4.15)
Summary statistics:					
σ_α^2	.0434	. . .			
σ_ϵ^2	.1801	. . .			
σ_δ^2	. . .	.2905			
σ_η^2	. . .	.0023			
Number of observations	5,263	38,870			

* The housing hedonic contains 20 structural trait controls. All results are available on request. Estimated standard errors are in parentheses.

† The wage hedonic contains 11 worker quality variables and controls for 22 major industry and occupation groups. All results are available on request. Estimated standard errors are in parentheses.

‡ The calculations in cols. 3–5 are based on a 1 percent change about the mean of the variables except for the dichotomous coast variable. Its prices are based on a discrete change from noncoast to coastal status. All figures in these three columns are annualized. We assume 1.5 wage earners per household and 49 work weeks per year. These are the sample averages. Standard errors of the implicit prices are in parentheses. They are calculated via the "delta" method.

on request. As Roback (1980, 1982) showed, the compensating differentials estimated from (5) and (6) can be used to compute implicit prices of the amenities and fiscal conditions. The full implicit price of a given city trait is the sum of the land price differential and the negative of the wage differential.[6] Columns 3–5 of table 1 present the full implicit prices as well as the component prices in terms of housing expenditures and wages. Except for the dichotomous coast variable, the calculations are based on an assumed 1 percent change about the mean of each variable. All prices are stated in terms of annual housing expenditures or annual earnings (assuming 1.5 earners per household).

While there are anomalies, the full implicit prices generally have the expected signs in that beneficial traits have positive prices, with the converse being true for traits that are "bads." Consistent with Blomquist et al. (1988) and Gyourko and Tracy (1989*b*), we find that, for many city traits, the full price largely reflects capitalization in the labor rather than in the land market.

As a group, the 11 amenities are highly statistically significant (at better than the 1 percent level) in both the wage and housing hedonics. The same is true for the fiscal variables as a group (seven in the housing hedonic and six in the wage hedonic). Many of the amenity variables have been used in other hedonic estimations, and given this paper's interest in the influence of the fiscal climate, we focus on the taxes and services in our discussion.[7]

The full implicit price for each service proxy has the anticipated sign, with public safety and health services having the largest prices (in absolute value). Most of the impact of these two variables occurs via wage differentials. The property tax coefficient of -0.1037 in table 1 implies that property taxes are virtually fully capitalized into land prices. With services held constant, higher income taxes are viewed as a bad, with most of the compensation coming through the

[6] Roback's equation works with actual land prices. We use housing expenditures in which land rents are bundled with structural traits. As Blomquist et al. note, the problem is easily handled by putting housing instead of land into the utility function and incorporating a housing production function into the model. The full implicit price equation is essentially unchanged, with the quality of housing purchased replacing the quantity of land (see Blomquist et al. 1988, p. 92, eq. 6).

[7] However, two of the individual amenity findings are of unique interest. First, the influence of the size variable (SMSA population) is small. By way of comparison, Roback's (1982) OLS-based findings for the impact of SMSA population are approximately double the magnitude of our OLS and random effects findings. We also experimented with land area and population density measures at both the SMSA and central city levels. Those variables were not found to be influential or to affect other variables' coefficients. Second, there is a particularly strong influence for access to nearby labor markets as indicated by the coefficient on the percentage of residents working in another SMSA. For cities in the San Francisco Bay area and near New York City, where 30–40 percent of residents often work in another SMSA, the land rent coefficient on this variable implies that land prices are 15–30 percent higher, ceteris paribus.

land market. The effect on wages is small and positive, indicating that gross wages do not rise nearly enough to keep net wages constant. Entrants to cities with high corporate tax rates pay higher land rents and accept lower wages. This would be expected if the tax burden were being shifted to nonresidents. This seems unlikely given that many firms in the corporate sector produce for regional or national markets, making forward shifting to consumers difficult. It may be that this variable is picking up beneficial aspects of agglomeration that are being appropriated by the government through the tax. It is also possible that the variable might be proxying for the state's overall economic condition. Finally, cities with higher public-sector unionization rates do have somewhat lower land prices, but the partial correlation is not statistically significant.[8]

The underlying quality-of-life index value (QOL) is created as

$$\mathrm{QOL}_j = \sum_k FP_k \times T_{kj}, \tag{7}$$

where FP_k is the full implicit price of city trait k and T_{kj} is the quantity of that trait in city j. We have standardized on a hypothetical city having the average values of all city traits. The index is measured in 1979 dollars and reflects the premium that individuals are willing to pay to live in city j relative to the hypothetical city with the sample average amenity, fiscal, and public-sector union conditions.

Information on the relative contributions of the amenity, fiscal, and public-sector union conditions to the quality of life is presented in columns 1 and 2 of table 2. The full range of quality-of-life values based on all city traits is $8,227. This band is wide because of a few extreme cities. The interquartile range is only $1,484. The separate impact of the 11 amenity variables (which include the weather and pollution variables, coastal status, cost of living, access to other labor markets, and size variables) can be seen in the second row of columns 1 and 2. All else constant, one would pay $3,979 more to live in the top-amenity city than in the city with the worst amenity set. Note that there is a particularly wide range for the impact of the seven tax/service variables ($6,582 [row 3]). However, restricting attention to the middle of the distribution shows that the fiscal vector makes approximately the same contribution as the amenity vector to the dispersion of quality-of-life index values ($1,188 vs. $1,372).

[8] We also experimented with a dichotomous union coverage variable. The variable was coded as a one if the city had a public-sector coverage rate that was at least one standard deviation above the sample mean (> 67 percent). As in Gyourko and Tracy (1989*b*), the transformed variable generated statistically significant lower land prices of about 10 percent. However, the full implicit price was little changed, as were the resulting quality-of-life rankings.

TABLE 2

AMENITY, FISCAL, AND PUBLIC-SECTOR UNION IMPACTS ON THE QUALITY OF LIFE AND INTERCITY HOUSING AND WAGE DIFFERENTIALS

			Housing		Wage	
Variable Set	Quality-of-Life Range (1)	Quality-of-Life Interquartile Range (2)	Maximum Partial R^2* (3)	Minimum Partial R^2† (4)	Maximum Partial R^2 (5)	Minimum Partial R^2 (6)
All city traits	\$8,227	\$1,484	.55	. . .	.44	. . .
Amenity component	3,979	1,372	.43	.39	.23	.20
Tax/service component	6,582	1,188	.16	.12	.21	.20
Public union component	571	193	.00	.00	.06	.00

* The maximum partial R^2 for the vector of traits in each row is defined to be the second-stage R^2 from the regression containing only the city traits listed in each row.

† The minimum partial R^2 for the vector of traits in each row is defined to be the difference between the R^2 obtained when including all city-specific variables (row 1) and the R^2 from the regression omitting the city traits listed in the relevant row.

For the typical city, the public-sector union coverage variable does not have an economically important independent impact on the overall quality-of-life index (row 4).

Columns 3–6 of table 2 summarize the relative importance of the amenity, fiscal, and public-sector union variables in explaining the variation in quality-adjusted housing prices and wages across cities. The minimum and maximum partial R^2's presented are calculated using a two-step procedure. In the first step, city fixed effects are estimated using OLS, controlling for housing or worker quality. The second step involves a generalized least squares regression of the fixed effects coefficients on the city-specific variables.[9] The city-specific variables explain 55 percent of the variation in quality-adjusted house prices and 44 percent of the variation in quality-adjusted wages across our 130 cities. The remaining rows in columns 3–6 document the relative importance of the amenity, fiscal, and union variables. Fiscal differentials clearly are empirically important in that they account for at least 21 percent of the explained variation in housing prices (.12/.55) and at least 47 percent of the explained variation in wages (.21/.44) across cities. However, variation in the degree of public-sector unionization has no ability to explain intercity wage or housing price differentials.

The quality-of-life index values and rankings based on the prices in table 1 are presented in the first two columns of table 3. Recall that the reason for estimating a random effects specification is to control for the influence of omitted traits common to a city-specific group of workers or housing units. If the group effects reflect omitted amenities or fiscal variables that would be included if observable, they should be priced and included in the quality-of-life computation. In contrast, if these traits reflect unmeasured heterogeneity in housing structure quality and worker human capital, then they should not be included in the quality-of-life computation. Unfortunately, it is not readily apparent how to discriminate between these sources.

The correlation between the errors of the two equations might allow some insight into the nature of the city-specific group effects, but only if we know much more about the underlying structure of the model than our reduced-form estimations afford. If both worker and housing quality were systematically underestimated (or overestimated) by city, a positive error correlation would result. However, a negative error correlation would ensue if quality was overestimated

[9] The variance-covariance matrix used in the generalized least squares estimation is the sum of the fixed effects variance-covariance matrix and a diagonal matrix with the estimated group error variance along the diagonal. This particular variance-covariance matrix reproduces the random effects coefficients and standard errors reported in table 1.

TABLE 3

QUALITY-OF-LIFE INDEX VALUES AND RANKINGS

	Random Effects		Random Effects, Group Effects Included		OLS: All Fiscal Variables		OLS: No Taxes/No Union	
City	Ranking (1)	Index Value (2)	Ranking (3)	Index Value (4)	Ranking (5)	Index Value (6)	Ranking (7)	Index Value (8)
Norwalk, CT	1	3,986	23	2,132	1	5,192	1	4,335
	(4.1)	(1,135)	(20.9)	(2,226)	(.4)	(705)	(.8)	(685)
Pensacola, FL	2	2,963	6	3,812	4	3,145	5	2,588
	(4.0)	(714)	(3.8)	(1,601)	(1.5)	(401)	(2.9)	(347)
Gainesville, FL	3	2,819	46	1,019	5	3,115	3	3,026
	(7.3)	(890)	(23.5)	(2,408)	(2.2)	(533)	(2.6)	(512)
San Diego, CA	4	2,574	4	4,474	2	3,586	4	2,971
	(8.4)	(860)	(3.2)	(999)	(1.1)	(299)	(1.8)	(277)
Stamford, CT	5	2,497	38	1,339	17	1,668	58	137
	(9.4)	(875)	(20.6)	(1,808)	(7.4)	(462)	(15.3)	(354)
Columbia, SC	6	2,459	7	3,792	3	3,556	2	4,135
	(14.7)	(1,137)	(9.0)	(1,693)	(2.8)	(677)	(.9)	(653)
Santa Rosa, CA	7	1,955	45	1,024	6	2,443	10	2,309
	(11.4)	(744)	(18.8)	(1,462)	(2.7)	(324)	(2.6)	(284)
Bridgeport, CT	8	1,944	3	4,532	10	2,245	50	305
	(9.3)	(630)	(2.4)	(1,580)	(3.3)	(335)	(9.4)	(209)
Tucson, AZ	9	1,822	17	2,325	9	2,259	22	929
	(13.5)	(780)	(12.6)	(1,182)	(4.3)	(415)	(8.7)	(330)
Shreveport, LA	10	1,802	39	1,318	18	1,619	31	682
	(7.3)	(473)	(11.5)	(1,172)	(3.9)	(232)	(6.3)	(179)
Lancaster, PA	11	1,784	16	2,327	19	1,582	28	762
	(9.0)	(547)	(8.9)	(1,164)	(4.7)	(265)	(7.1)	(198)

Modesto, CA	12	1,678	62	517	11	2,053	12	2,141
	(9.4)	(550)	(15.6)	(1,485)	(3.2)	(249)	(2.5)	(217)
Asheville, NC	13	1,577	32	1,418	20	1,464	7	2,364
	(11.8)	(622)	(21.6)	(3,036)	(6.5)	(346)	(2.9)	(301)
New Orleans, LA	14	1,565	42	1,170	13	1,818	40	506
	(10.8)	(570)	(14.3)	(879)	(4.5)	(304)	(10.0)	(260)
Fall River, MA	15	1,549	85	−327	22	1,417	6	2,536
	(16.5)	(795)	(22.4)	(2,379)	(8.3)	(453)	(3.0)	(416)
Danbury, CT	16	1,498	1	6,662	33	826	9	2,329
	(22.1)	(1,009)	(1.9)	(3,658)	(15.9)	(587)	(4.5)	(540)
Amarillo, TX	17	1,475	59	680	24	1,232	37	551
	(16.9)	(795)	(20.9)	(1,612)	(9.3)	(472)	(15.7)	(418)
Jacksonville, FL	18	1,463	103	−992	27	1,113	30	694
	(13.1)	(630)	(13.1)	(1,003)	(6.4)	(322)	(9.7)	(291)
San Francisco, CA	19	1,416	29	1,578	8	2,296	13	2,046
	(16.4)	(796)	(16.1)	(884)	(2.8)	(291)	(3.0)	(279)
San Jose, CA	20	1,403	75	208	15	1,744	15	1,849
	(16.2)	(740)	(19.1)	(898)	(5.0)	(299)	(3.0)	(281)
New Britain, CT	21	1,389	35	1,395	7	2,335	8	2,345
	(23.1)	(1,003)	(22.8)	(1,931)	(6.2)	(592)	(4.9)	(569)
Lake Charles, LA	22	1,388	113	−1,636	25	1,177	36	588
	(15.9)	(725)	(11.4)	(1,800)	(7.6)	(365)	(9.7)	(252)
New Bedford, MA	23	1,316	51	791	54	179	34	652
	(17.9)	(765)	(20.2)	(1,692)	(14.2)	(387)	(12.1)	(344)
Tyler, TX	24	1,175	11	2,773	35	776	18	1,411
	(14.6)	(605)	(8.3)	(2,030)	(7.8)	(326)	(2.4)	(228)
Odessa, TX	25	1,118	61	577	30	960	42	478
	(17.1)	(671)	(19.4)	(2,953)	(8.9)	(393)	(14.3)	(366)
Erie, PA	26	1,103	18	2,299	23	1,250	64	−30
	(18.4)	(706)	(11.8)	(1,390)	(7.2)	(347)	(12.7)	(291)
Phoenix, AZ	27	1,097	78	59	31	932	20	993
	(26.5)	(1,038)	(24.8)	(1,158)	(10.4)	(424)	(10.4)	(408)
Knoxville, TN	28	1,071	83	−143	28	1,100	26	811
	(10.7)	(412)	(11.1)	(1,024)	(4.5)	(208)	(5.9)	(165)

TABLE 3 (*Continued*)

City	Random Effects		Random Effects, Group Effects Included		OLS: All Fiscal Variables		OLS: No Taxes/No Union	
	Ranking (1)	Index Value (2)	Ranking (3)	Index Value (4)	Ranking (5)	Index Value (6)	Ranking (7)	Index Value (8)
Lafayette, LA	29	930	13	2,615	26	1,164	78	−290
	(15.2)	(548)	(7.6)	(1,709)	(5.8)	(284)	(9.8)	(229)
Monroe, LA	30	905	19	2,287	42	524	60	82
	(11.1)	(404)	(7.3)	(1,740)	(5.9)	(194)	(6.9)	(169)
Wilmington, DE	31	898	72	363	16	1,675	19	1,146
	(19.2)	(666)	(17.9)	(1,141)	(5.4)	(309)	(4.4)	(244)
Waco, TX	32	880	21	2,162	32	870	24	859
	(21.4)	(745)	(13.1)	(1,560)	(10.7)	(430)	(10.9)	(379)
Springfield, MO	33	753	22	2,154	57	151	32	659
	(11.8)	(386)	(7.2)	(1,443)	(6.7)	(184)	(5.8)	(158)
Sacramento, CA	34	703	24	1,832	29	991	29	753
	(18.0)	(564)	(11.3)	(956)	(6.7)	(267)	(8.5)	(235)
Lubbock, TX	35	690	106	−1,107	34	796	46	410
	(20.3)	(650)	(12.7)	(1,400)	(9.5)	(378)	(13.5)	(330)
Los Angeles, CA	36	605	10	2,941	14	1,804	17	1,604
	(15.1)	(930)	(4.6)	(960)	(2.7)	(254)	(1.6)	(244)
Birmingham, AL	37	590	41	1,201	43	507	14	1,962
	(25.8)	(823)	(19.6)	(1,120)	(12.0)	(391)	(3.1)	(308)
Jersey City, NJ	38	573	95	−831	12	1,883	11	2,231
	(29.7)	(984)	(20.7)	(1,407)	(8.6)	(587)	(4.8)	(544)
Fresno, CA	39	542	60	604	21	1,446	16	1,668
	(24.6)	(773)	(20.3)	(1,265)	(7.0)	(365)	(3.9)	(344)
Roanoke, VA	40	518	87	−378	44	434	68	−79
	(16.7)	(490)	(13.4)	(1,415)	(7.7)	(238)	(7.0)	(158)

Columbia, MO	41	464	5	4,155	63	−108	25	844
	(22.5)	(667)	(3.7)	(2,299)	(12.4)	(292)	(8.5)	(261)
El Paso, TX	42	438	8	3,165	36	737	27	810
	(25.8)	(787)	(8.0)	(1,271)	(11.4)	(424)	(11.5)	(375)
Savannah, GA	43	428	52	787	51	294	23	899
	(20.8)	(600)	(16.6)	(1,477)	(9.8)	(288)	(6.8)	(249)
Richmond, VA	44	398	110	1,366	40	548	35	604
	(20.4)	(575)	(9.9)	(1,022)	(8.8)	(288)	(8.4)	(213)
Topeka, KS	45	383	64	478	41	532	44	450
	(14.4)	(392)	(12.1)	(1,508)	(6.2)	(200)	(7.3)	(164)
Baton Rouge, LA	46	376	93	−676	39	562	99	−756
	(18.9)	(540)	(12.2)	(1,176)	(8.1)	(268)	(8.1)	(237)
Albuquerque, NM	47	365	20	2,166	53	183	79	−290
	(23.4)	(673)	(11.9)	(1,197)	(14.4)	(381)	(15.5)	(351)
Memphis, TN	48	325	47	1,014	50	316	71	−156
	(20.2)	(576)	(14.8)	(945)	(10.0)	(296)	(9.8)	(225)
Orlando, FL	49	308	67	420	47	344	57	139
	(20.0)	(545)	(15.3)	(1,021)	(9.6)	(286)	(11.7)	(264)
Fort Wayne, IN	50	303	76	199	49	331	73	−216
	(16.1)	(437)	(12.3)	(1,073)	(7.8)	(215)	(7.7)	(164)
Evansville, IN	51	286	49	891	46	359	47	348
	(16.5)	(455)	(14.0)	(1,709)	(8.4)	(239)	(7.6)	(161)
Pittsburgh, PA	52	275	90	−647	38	589	85	−474
	(27.4)	(846)	(18.2)	(976)	(10.7)	(351)	(13.7)	(330)
Fayetteville, NC	53	274	37	1,357	37	675	55	206
	(19.8)	(543)	(14.2)	(1,727)	(7.5)	(264)	(6.7)	(147)
Mobile, AL	54	250	15	2,346	62	−91	51	299
	(24.7)	(712)	(11.3)	(1,345)	(14.8)	(363)	(13.1)	(314)
Wichita, KS	55	246	54	785	72	−286	74	−225
	(17.7)	(474)	(12.8)	(996)	(11.4)	(250)	(10.9)	(232)
Lynchburg, VA	56	241	30	1,548	69	−211	65	−30
	(16.3)	(439)	(10.6)	(1,505)	(10.5)	(234)	(8.8)	(201)
Worcester, MA	57	216	14	2,599	77	−379	21	969
	(21.6)	(599)	(8.4)	(1,386)	(14.2)	(315)	(7.6)	(278)

TABLE 3 (*Continued*)

City	Random Effects		Random Effects, Group Effects Included		OLS: All Fiscal Variables		OLS: No Taxes/No Union	
	Ranking (1)	Index Value (2)	Ranking (3)	Index Value (4)	Ranking (5)	Index Value (6)	Ranking (7)	Index Value (8)
Austin, TX	58	180	33	1,415	60	−24	41	479
	(23.7)	(666)	(15.6)	(1,124)	(14.2)	(357)	(12.7)	(321)
Lawton, OK	59	178	57	750	59	−20	49	308
	(21.0)	(578)	(22.5)	(2,940)	(12.2)	(308)	(12.7)	(290)
San Antonio, TX	60	110	9	3,069	56	173	45	444
	(25.7)	(740)	(7.7)	(1,025)	(14.1)	(389)	(15.0)	(372)
Waterbury, CT	61	107	73	311	107	−995	104	−914
	(24.1)	(684)	(20.9)	(1,868)	(11.3)	(353)	(8.1)	(303)
Springfield, OH	62	101	27	1,688	66	−184	101	−832
	(14.1)	(363)	(9.0)	(1,484)	(8.1)	(192)	(3.1)	(132)
Jackson, MS	63	18	40	1,237	61	−79	43	477
	(18.7)	(504)	(12.8)	(1,349)	(10.9)	(267)	(8.0)	(192)
Chattanooga, TN	64	−41	66	430	68	−202	89	−540
	(18.9)	(496)	(13.8)	(1,086)	(11.5)	(262)	(10.1)	(255)
St. Joseph, MO	65	−53	12	2,735	76	−374	38	523
	(17.9)	(479)	(7.3)	(1,985)	(10.5)	(237)	(8.1)	(176)
Pueblo, CO	66	−89	96	−861	52	185	39	513
	(21.0)	(564)	(13.1)	(1,935)	(11.3)	(303)	(11.6)	(279)
Manchester, NH	67	−100	53	786	64	−135	61	45
	(26.5)	(765)	(20.6)	(1,758)	(17.2)	(418)	(16.7)	(375)
Terre Haute, IN	68	−112	94	−677	80	−444	88	−491
	(15.4)	(404)	(9.9)	(1,659)	(8.1)	(187)	(6.4)	(176)
Bakersfield, CA	69	−120	112	−1,546	48	341	33	654
	(27.6)	(807)	(13.4)	(1,522)	(11.0)	(321)	(11.3)	(300)

Macon, GA	70	−140	65	463	86	−562	52	259
	(16.9)	(453)	(14.9)	(1,954)	(11.1)	(236)	(6.3)	(156)
Charleston, WV	71	−158	36	1,370	55	177	83	−466
	(23.5)	(647)	(16.1)	(1,598)	(11.7)	(314)	(10.8)	(248)
Decatur, IL	72	−161	107	−1,161	90	−635	72	−207
	(18.4)	(495)	(9.7)	(1,572)	(10.3)	(244)	(9.7)	(228)
Colorado Springs, CO	73	−165	70	384	65	−147	93	−605
	(22.0)	(598)	(16.7)	(1,479)	(13.7)	(329)	(11.9)	(305)
Lincoln, NE	74	−185	25	1,768	91	−638	95	−674
	(18.1)	(470)	(10.3)	(1,327)	(9.7)	(212)	(7.9)	(203)
Altoona, PA	75	−187	34	1,396	105	−963	97	−700
	(27.7)	(820)	(19.4)	(1,896)	(13.5)	(413)	(12.6)	(370)
Huntsville, AL	76	−199	26	1,732	104	−926	81	−411
	(19.1)	(519)	(11.0)	(1,271)	(9.5)	(271)	(9.6)	(231)
Anderson, IN	77	−234	124	−2,951	71	−268	56	170
	(18.2)	(458)	(3.5)	(1,585)	(11.3)	(247)	(11.5)	(238)
Oklahoma City, OK	78	−257	55	769	78	−384	70	−98
	(24.4)	(694)	(18.2)	(1,032)	(15.3)	(354)	(15.0)	(341)
Billings, MT	79	−285	114	−1,649	113	−1,375	109	−1,361
	(26.7)	(786)	(12.9)	(2,137)	(9.3)	(400)	(7.7)	(378)
Syracuse, NY	80	−301	44	1,062	67	−188	87	−478
	(24.8)	(707)	(18.1)	(1,168)	(14.2)	(337)	(11.3)	(272)
Columbus, GA	81	−305	50	808	111	−1,135	54	223
	(22.4)	(634)	(18.1)	(1,974)	(9.3)	(325)	(10.0)	(238)
Buffalo, NY	82	−314	102	−901	73	−287	59	86
	(27.1)	(806)	(16.3)	(1,055)	(15.7)	(368)	(13.7)	(309)
Canton, OH	83	−340	74	274	74	−296	84	−472
	(14.8)	(375)	(11.2)	(1,090)	(8.9)	(195)	(8.4)	(178)
Omaha, NE	84	−379	104	−1,051	94	−700	77	−283
	(12.8)	(337)	(6.7)	(994)	(7.4)	(173)	(7.1)	(150)
Springfield, IL	85	−409	58	747	87	−566	90	−551
	(14.0)	(362)	(12.1)	(1,703)	(8.1)	(162)	(7.1)	(158)
Miami, FL	86	−445	31	1,439	45	411	69	−86
	(29.1)	(925)	(20.7)	(1,114)	(14.5)	(435)	(17.8)	(410)

TABLE 3 (*Continued*)

City	Random Effects		Random Effects, Group Effects Included		OLS: All Fiscal Variables		OLS: No Taxes/No Union	
	Ranking (1)	Index Value (2)	Ranking (3)	Index Value (4)	Ranking (5)	Index Value (6)	Ranking (7)	Index Value (8)
South Bend, IN	87	−468	105	−1,079	92	−649	107	−1,116
	(15.6)	(430)	(8.8)	(1,357)	(9.5)	(222)	(3.2)	(187)
Salem, OR	88	−488	123	−2,898	110	−1,070	75	−260
	(21.1)	(604)	(4.6)	(1,704)	(8.6)	(288)	(11.3)	(257)
Tulsa, OK	89	−496	80	−31	85	−548	100	−792
	(13.7)	(377)	(10.1)	(807)	(8.9)	(182)	(5.4)	(151)
Portland, ME	90	−498	28	1,659	88	−597	62	15
	(26.5)	(812)	(17.6)	(1,568)	(17.0)	(419)	(16.4)	(381)
Akron,.OH	91	−520	77	173	75	−302	106	−1,036
	(15.8)	(438)	(12.2)	(944)	(9.2)	(205)	(3.7)	(164)
Harrisburg, PA	92	−537	111	−1,408	102	−904	53	253
	(24.3)	(724)	(12.2)	(1,194)	(13.9)	(388)	(15.4)	(352)
Cincinnati, OH	93	−544	56	759	58	68	82	−426
	(16.8)	(484)	(13.2)	(798)	(9.1)	(243)	(9.4)	(220)
Cedar Rapids, IA	94	−544	71	363	99	−823	94	−659
	(18.1)	(529)	(14.9)	(1,460)	(9.4)	(258)	(8.8)	(248)
Indianapolis, IN	95	−600	118	−2,147	106	−983	105	−935
	(16.3)	(477)	(5.1)	(715)	(7.6)	(242)	(4.3)	(188)
Reno, NV	96	−639	119	−2,186	97	−816	48	−315
	(29.1)	(977)	(11.7)	(1,551)	(19.2)	(542)	(17.0)	(412)
Sioux City, IA	97	−675	92	−653	93	−656	91	−582
	(17.9)	(553)	(14.1)	(2,041)	(10.8)	(270)	(9.7)	(262)
Dayton, OH	98	−699	89	−536	81	−484	102	−863
	(18.2)	(532)	(12.5)	(958)	(11.7)	(240)	(6.8)	(208)
Des Moines, IA	99	−700	81	−50	101	−884	98	−707
	(14.0)	(440)	(11.5)	(1,082)	(7.0)	(2,148)	(6.7)	(200)

Trenton, NJ	100	−715	68	415	120	−1,698	67	−68
	(21.7)	(679)	(18.4)	(1,337)	(4.8)	(3,089)	(12.3)	(266)
Philadelphia, PA	101	−736	117	−1,991	109	−1,043	108	−1,343
	(20.7)	(813)	(8.1)	(869)	(7.3)	(269)	(3.7)	(248)
Louisville, KY	102	−794	120	−2,248	79	−433	76	−264
	(13.3)	(429)	(4.4)	(851)	(9.5)	(205)	(8.0)	(181)
Columbus, OH	103	−811	101	−899	83	−514	115	−1,756
	(11.6)	(384)	(7.4)	(722)	(8.4)	(185)	(2.3)	(154)
Seattle, WA	104	−816	82	−58	70	−248	96	−690
	(25.1)	(848)	(20.9)	(969)	(14.9)	(346)	(11.0)	(312)
Rochester, NY	105	−842	122	−2,607	82	−495	80	−298
	(20.8)	(671)	(5.9)	(1,018)	(13.2)	(299)	(11.6)	(262)
Tacoma, WA	106	−846	97	−862	89	−599	66	−50
	(21.7)	(723)	(15.1)	(1,256)	(15.0)	(347)	(13.6)	(289)
Mansfield, OH	107	−965	48	934	103	−920	126	−2,443
	(20.4)	(710)	(19.1)	(1,861)	(12.0)	(349)	(2.2)	(312)
Boise, ID	108	−972	2	5,117	98	−822	92	−596
	(13.6)	(486)	(1.5)	(1,553)	(9.8)	(265)	(8.1)	(207)
Toledo, OH	109	−1,013	91	−647	95	−761	121	−1,974
	(12.9)	(479)	(10.5)	(847)	(9.0)	(237)	(2.6)	(168)
Boston, MA	110	−1,067	63	512	96	−764	103	−908
	(18.3)	(703)	(17.8)	(902)	(12.4)	(309)	(8.8)	(299)
Minneapolis, MN	111	−1,147	43	1,082	84	−520	122	−1,987
	(20.8)	(816)	(19.8)	(917)	(14.2)	(327)	(3.9)	(241)
Chicago, IL	112	−1,209	109	−1,337	112	−1,334	111	−1,486
	(17.3)	(1,031)	(12.2)	(1,061)	(3.5)	(249)	(2.8)	(240)
Tuscaloosa, AL	113	−1,259	99	−879	123	−1,981	86	−475
	(13.7)	(584)	(13.8)	(2,004)	(3.7)	(299)	(9.8)	(235)
Muncie, IN	114	−1,373	126	−3,290	124	−2,021	124	−2,122
	(12.9)	(595)	(3.7)	(2,113)	(3.0)	(306)	(3.5)	(281)
Ann Arbor, MI	115	−1,450	86	−376	126	−2,215	63	7
	(14.9)	(697)	(17.6)	(1,497)	(3.0)	(343)	(12.7)	(270)
Cleveland, OH	116	−1,492	108	−1,218	100	−851	116	−1,833
	(10.9)	(560)	(9.6)	(748)	(9.8)	(267)	(3.8)	(223)
Rockford, IL	117	−1,532	88	−431	122	−1,955	117	−1,845
	(7.0)	(399)	(10.0)	(1,200)	(2.8)	(211)	(3.2)	(176)

City	Random Effects		Random Effects, Group Effects Included		OLS: All Fiscal Variables		OLS: No Taxes/No Union	
	Ranking (1)	Index Value (2)	Ranking (3)	Index Value (4)	Ranking (5)	Index Value (6)	Ranking (7)	Index Value (8)
Peoria, IL	118	−1,634	125	−3,052	121	−1,937	110	−1,396
	(6.5)	(411)	(2.6)	(1,150)	(2.4)	(203)	(2.2)	(175)
Spokane, WA	119	−1,815	84	−273	116	−1,544	114	−1,623
	(11.6)	(728)	(18.0)	(1,324)	(6.0)	(343)	(4.2)	(255)
Portland, OR	120	−1,874	69	388	119	−1,640	119	−1,890
	(8.7)	(607)	(16.3)	(807)	(3.9)	(264)	(3.9)	(249)
Kansas City, MO	121	−1,900	121	−2,523	118	−1,634	113	−1,600
	(5.4)	(441)	(3.7)	(682)	(3.4)	(230)	(3.1)	(203)
Atlanta, GA	122	−1,916	116	−1,980	114	−1,489	120	−1,939
	(9.7)	(671)	(8.1)	(841)	(5.5)	(285)	(4.2)	(251)
Hartford, CT	123	−1,931	79	0	117	−1,631	129	−2,839
	(13.9)	(871)	(22.4)	(1,377)	(7.5)	(434)	(1.5)	(347)
Baltimore, MD	124	−1,934	115	−1,843	115	−1,530	112	−1,524
	(9.4)	(662)	(8.4)	(806)	(5.1)	(274)	(3.8)	(223)
Newark, NJ	125	−2,477	100	−884	108	−1,002	125	−2,176
	(9.8)	(914)	(26.7)	(1,798)	(14.0)	(427)	(4.9)	(395)
Las Vegas, NV	126	−2,832	128	−4,198	125	−2,125	128	−2,637
	(9.0)	(1,027)	(3.4)	(1,403)	(6.1)	(553)	(4.2)	(523)
Grand Rapids, MI	127	−2,947	98	−865	127	−3,908	123	−1,991
	(2.6)	(589)	(12.3)	(1,009)	(.7)	(306)	(3.6)	(245)
Saginaw, MI	128	−3,668	129	−5,273	128	−3,939	118	−1,881
	(1.4)	(646)	(.6)	(1,423)	(.7)	(315)	(3.4)	(225)
Detroit, MI	129	−4,153	130	−5,273	129	−4,188	127	−2,544
	(1.1)	(751)	(.6)	(1,423)	(.6)	(267)	(1.5)	(288)
Flint, MI	130	−4,241	127	−3,537	130	−4,893	130	−2,917
	(1.2)	(786)	(3.5)	(1,251)	(.2)	(407)	(1.1)	(331)

NOTE.—The numbers in parentheses in cols. 1, 3, 5, and 7 are estimated standard errors of the rankings. The standard errors for the rankings were calculated using a sample of 100,000 simulated rankings. Housing and wage coefficient vectors were drawn from the relevant normal distributions implied by the appropriate regression analysis. Full implicit prices and associated quality-of-life rankings were calculated for each set of simulated coefficient vectors. The reported standard error for a city ranking is the standard deviation in the sample of the given city's simulated rankings. The numbers in parentheses in cols. 2, 4, 6, and 8 are estimated standard errors of the index values. They are calculated via the delta method.

in (say) the housing market and underestimated in (say) the labor market. If high-quality workers demand high-quality housing, then we would expect any systematic mismeasurement of quality to produce a positive error correlation. In contrast, if the group effects solely reflect an omitted amenity valued only by the workers, a negative error correlation across equations would be expected. The reason is that an omitted city trait that is beneficial to workers but not firms implies a negative wage residual and a positive land rent residual. However, if firms also value this trait, they might bid wages up, with the resulting error correlation across equations being of indeterminate sign. We simply cannot be certain that the impact from the worker side of the model is dominant, particularly for any omitted fiscal traits.[10] Consequently, the sign of the correlation does not reliably identify the source of the group effects without specific assumptions about the underlying model structure.[11]

Because we cannot be sure about the origin of the group effects, columns 3 and 4 of table 3 present the index values and rankings when the group error terms are included. There still is a fairly strong positive correlation between the rank orderings of the indexes with and without the group effects (ρ = .63). However, the rankings of many cities are materially affected by the inclusion of the group effect. The mean absolute change in rankings is 26.8 and the standard deviation of that change is 20.7. Norwalk, Connecticut, drops from being the top-ranked city to being number 23. Lake Charles, Louisiana, suffers the biggest decline, with its ranking falling from 22 to 113. Boise, Idaho, exhibits the biggest rise, increasing in rank from 108 to 2. For the cities exhibiting substantial decreases (increases) in ranks, it typically is the case that observed wages are much higher (lower) than predicted by the right-hand-side variables.[12]

The last four columns of table 3 present quality-of-life index values and rankings based on two OLS specifications, one with and the other without the fiscal/union variables. They provide a base for comparison with previous work that has not controlled for the tax side of local finance or estimated random effects specifications. The results indicate that the rankings are materially influenced not only by inclu-

[10] For example, we do not control for transportation service quality because such measures simply are not widely available. It is easy for us to imagine both the marginal worker and firm highly valuing that attribute.

[11] We do find slight positive correlations across the error terms, although neither is significantly different from zero at standard confidence levels. When the group error terms are weighted by the number of housing observations in the relevant city, ρ = .14. When the number of wage observations serves as the weight, ρ = .10.

[12] We generally have a reasonable number (well into double figures) of individual worker wage observations, making it unlikely that the large group effects found for some cities are the result of a few idiosyncratic worker observations.

sion of the tax and public-sector union variables but also by the econometric method used to estimate the prices. With the full set of amenity and fiscal variables included, the mean absolute change in rankings based on the random effects estimation (excluding group effects, cols. 1–2) versus the OLS-based results (cols. 5–6) is 10.2 with an associated standard deviation of 9.56. The mean absolute dollar change in index value is $391 with a standard deviation of $320.

The impact of including the tax and union vector can be seen by comparing the rankings based on the two OLS specifications (cols. 5–6 vs. cols. 7–8). The mean absolute change in rankings when these variables are added is 16.2 with a 15.2 standard deviation. The mean absolute dollar change in these index values is $603 with a standard deviation of $527.[13]

It is important to note that the influence of the fiscal variables does not appear to be the result of spurious correlation with broader regional forces. For example, when three region dummies are included in the specifications, the joint significance of the fiscal variables continues to hold at very high confidence levels (the same is true for the amenities). When group effects are controlled for, the only significant region effects occur for the West, where housing expenditures are substantially higher (by about 63 percent on average) and wages are slightly higher (by about 8 percent on average). Counting the regional effects as amenities does not materially change the relative aggregate effects of the amenity versus fiscal variables (cols. 1–2, table 2) or the rankings themselves (col. 2, table 3). In the random effects specification, the correlation between the sets of rankings with and without the region dummies included in the underlying hedonics is .97.

It is also noteworthy that our findings are robust with respect to some nonlinear specifications. We estimated a random effects specification in which those city traits with the highest coefficients of variation were entered in quadratic form. Trait prices changed only slightly over the range of city trait amounts found in the data, and rankings and index values were essentially unaltered. With only 130 cities, we opted to include only the linear terms. We also performed a Box-Cox analysis of the specification underlying the results in columns 5–6 of table 3. In addition, we estimated the housing hedonic using *only* the information on the interval of house values to which an observation belonged. While that analysis did reject the log speci-

[13] It should be noted that there is still a moderately strong positive correlation among the sets of rankings. The rankings based on random effects with group effects included (ranking 2 in cols. 3–4 of table 3) are the least strongly correlated with the other sets of rankings. Pairwise correlation coefficients between ranking 2 and the others range from .54 to .63. Pairwise correlations among the other three sets of rankings range from .85 to .94.

fication for both hedonics, this estimation procedure yielded results virtually identical to the OLS-based findings. The simple correlation between the Box-Cox and OLS rankings is .98. The mean absolute change in rankings was about 0.5 ranks. Estimating a random effects model in a nonlinear environment involves an order of magnitude increase in computing difficulty and cost, and our experimentation with this sample clearly shows modeling group effects to be far more important than incorporating nonlinearities.

While the pattern of results appears to be robust with respect to specification and functional form, we close with two important notes of caution about the general reliability of quality-of-life rankings. The reliability issue is starkly illustrated by the large standard errors about the quality-of-life rankings based on the random effects estimates. They are significantly higher than those calculated using the OLS estimates. This is not surprising since Moulton (1986, 1987) has shown that, when group effects exist in the data, the standard errors for the OLS coefficient estimates for variables having no within-group variation are downward biased. In our data, the random effects estimation typically results in a doubling in the standard error of a city's quality-of-life ranking. The average standard error of the rankings based on random effects (cols. 1–2 of table 3) is 16.9 versus an average of 8.6 for the rankings based on OLS (cols. 5–6 of table 3). At standard confidence levels, it becomes difficult to differentiate among many cities unless the comparison is between a very highly rated city and a very lowly rated city (e.g., top 20 vs. bottom 20).[14] The sharp drop in the precision of the full price estimates and the accompanying quality-of-life rankings points out the need for better data in order to reduce the magnitudes of the city-specific error components and to more precisely estimate a city's quality of life.

An added reliability concern arises from the fact that an arbitrary assumption typically is made about who the marginal entrant is. We assumed that the marginal entrant was the household with the sample average number of wage earners (1.5) and spending on housing per year ($4,524). The first four columns of table 4 illustrate how the top- and bottom-ranked cities change as the number of wage earners changes in the assumed marginal entrant household. (Of course, a similar point could be illustrated by varying the amount of housing expenditures.) Note that the rankings and quality-of-life index values do not change because the underlying hedonic coefficients change. Those coefficients are unaltered. However, the trait prices do change

[14] We should emphasize, however, that the random effects estimates of the model do make some progress toward differentiating among cities in our sample. If rankings were randomly assigned to each city, then the implied standard error for a city's rank would be 37.5. This is quite a bit larger than the average standard error of 16.9 produced from the random effects specification.

TABLE 4

RANKINGS FOR HOUSEHOLDS WITH DIFFERENT NUMBERS OF WAGE EARNERS

Rank	Households with 1.5 Earners (1)	Households with 0 Earners (2)	Households with 1 Earner (3)	Households with 2 Earners (4)	Net Subsidy for Retired Household with 0 Earners (5)
1	Norwalk, CT ($3,986)	San Francisco, CA ($4,232)	Norwalk, CT ($3,408)	Norwalk, CT ($4,564)	Gainesville, FL ($3,240)
2	Pensacola, FL (2,963)	San Diego, CA (4,075)	San Diego, CA (3,075)	Pensacola, FL (4,040)	Pensacola, FL (3,230)
3	Gainesville, FL (2,819)	Los Angeles, CA (3,709)	San Francisco, CA (2,355)	Gainesville, FL (3,899)	Columbia, SC (3,163)
4	San Diego, CA (2,574)	San Jose, CA (2,997)	Santa Rosa, CA (2,156)	Columbia, SC (3,513)	Tyler, TX (2,339)
5	Stamford, CT (2,497)	Santa Rosa, CA (2,557)	Stamford, CT (1,993)	Stamford, CT (3,001)	Lake Charles, LA (2,282)
6	Columbia, SC (2,459)	Norwalk, CT (2,253)	San Jose, CA (1,934)	Tucson, AZ (2,445)	New Bedford, MA (2,249)
7	Santa Rosa, CA (1,955)	Chicago, IL (1,762)	Pensacola, FL (1,886)	Shreveport, LA (2,300)	Amarillo, TX (1,950)
8	Bridgeport, CT (1,944)	Sacramento, CA (1,741)	Gainesville, FL (1,739)	Lancaster, PA (2,266)	Tucson, AZ (1,869)
⋮	⋮	⋮	⋮	⋮	⋮
123	Hartford, CT (−1,931)	Oklahoma City, OK (−1,070)	Tuscaloosa, AL (−1,448)	Portland, OR (−2,590)	Saginaw, MI (−2,593)
124	Baltimore, MD (−1,934)	Saginaw, MI (−1,075)	Atlanta, GA (−1,452)	Hartford, CT (−2,711)	Las Vegas, NV (−2,644)
125	Newark, NJ (−2,477)	Tyler, TX (−1,164)	Baltimore, MD (−1,503)	Grand Rapids, MI (−3,659)	San Francisco, CA (−2,816)
126	Las Vegas, NV (−2,832)	Louisville, KY (−1,181)	Las Vegas, NV (−1,951)	Las Vegas, NV (−3,714)	Chicago, IL (−2,971)
127	Grand Rapids, MI (−2,947)	Birmingham, AL (−1,221)	Grand Rapids, MI (−2,235)	Newark, NJ (−3,763)	Los Angeles, CA (−3,104)
128	Saginaw, MI (−3,668)	Mobile, AL (−1,399)	Saginaw, MI (−2,804)	Saginaw, MI (−4,532)	Detroit, MI (−3,320)
129	Detroit, MI (−4,153)	Altoona, PA (−1,478)	Flint, MI (−2,929)	Detroit, MI (−5,259)	Newark, NJ (−3,858)
130	Flint, MI (−4,241)	Tuscaloosa, AL (−1,826)	Detroit, MI (−3,406)	Flint, MI (−5,554)	Flint, MI (−3,939)

NOTE.—The numbers in parentheses in cols. 1–4 are quality-of-life index numbers assuming the marginal household is that listed at the top of the column. The net subsidy number in parentheses in col. 5 reflects the difference between the marginal valuation of a city's amenity, fiscal, and public union traits and the actual prices faced by a retired household with no wage earners. The marginal valuation calculation assumes identical preferences across household types and assumes that the 1.5-earner family is the true marginal entrant.

because they are based on some assumed degree of exposure to capitalization in the land and labor markets. The implicit assumption made here and in other quality-of-life studies is that the assumed wages and rents reflect the preferred degree of exposure based on the marginal entrant's utility function.

The rankings for households with one, one and a half, and two wage earners are highly correlated ($\rho > .87$ in all pairwise comparisons) as suggested by the fact that their top- and bottom-ranked cities tend to have substantial overlap. However, the rankings for the zero-earner household in column 2 are not nearly as strongly correlated with the other sets of rankings. The smallest correlation of $-.02$ occurs between the rankings for the zero-earner household and the two-earner household. The highest correlation of .38 is achieved with the rankings of the one-earner household.

Finally, it is worth noting that, even with identical preferences among households, the equilibrium prices typically will not make all households indifferent about location. This is a direct consequence of the observed differential capitalization of amenity and fiscal traits in the land and labor markets. As an example, assume that we have correctly identified the 1.5-earner household as the marginal entrant to all cities. In this case, our quality-of-life index values reported in table 3 would correctly measure the marginal valuations for all households. At these prices, a 1.5-earner household would be indifferent across cities. However, a retired-couple household would be inframarginal because the prices it faces are radically different because of its lack of exposure to labor market capitalization. Column 5 of table 4 lists the cities that provide the largest and smallest implied net subsidies to a retired-couple household. The net subsidy is defined to be the difference between the 1.5-earner household's marginal valuation of the city's amenity, fiscal, and public union package and the price the retired-couple household actually has to pay to consume that package. The highest-subsidy cities tend to be southern and western cities with high-value weather amenities whose full prices tend to contain large wage components (see table 1). The lowest-subsidy cities are a more diverse lot, including some northern cities as well as a few western cities with very high land values.[15]

[15] There is some evidence that retired-worker households are sorting into the highest net subsidy cities. Using the 1980 census tapes, we calculated for each city the ratio of retired residents to the sum of full-time labor market participants plus retired residents. Retired residents were defined to be people aged 50 or older who do not work and are not looking for work. We then computed the correlation between this ratio and the net subsidy figures listed in col. 5 of table 4. When the 130 observations are unweighted, $\rho = .15$ (the probability value is .08 for the null of $\rho = 0$). When the city observations are weighted by the total number of full-time labor market participants plus retired residents, the correlation rises to .35 (the probability value is .0001 for the null of $\rho = 0$).

Appendix

TABLE A1

Variable Description and Summary Statistics

Variable	Description	Source	Mean*
Precipitation	Annual inches (multiyear average)	U.S. Dept. Commerce (1983)	36.17 (13.38)
Cooling degree days	Thousands per year (multiyear average)	U.S. Bur. Census (1985)	1.31 (.84)
Heating degree days	Thousands per year (multiyear average)	U.S. Bur. Census (1985)	4.49 (1.90)
Relative humidity	Percentage (multiyear average)	U.S. Dept. Commerce (1983)	55.58 (8.74)
Sunshine	Percentage possible (multiyear average)	U.S. Dept. Commerce (1983)	61.02 (8.31)
Wind speed	Miles per hour (multiyear average)	U.S. Dept. Commerce (1983)	9.36 (1.65)
Particulate matter	Micrograms per cubic meter (1979 data)	U.S. EPA (1980)	73.30 (21.71)
Coast	Dichotomous: 1 if on border of ocean, Great Lake, or Gulf of Mexico; 0 otherwise	Created by authors	.20 (.41)
Nonland cost of living	Logged index number[†]	U.S. Dept. Labor (1977)	.98 (.02)
SMSA population	Millions of persons (1979 data)	U.S. Bur. Census (1985)	.744 (1.122)
Percentage working in other SMSA	Percentage; variable entered as spline; cities with less than 16% of residents working in another SMSA (one standard deviation above sample mean) have a value of 0; all others are coded as their actual percentages minus .16 (16%)	U.S. Bur. Census (1983)	.013 (.042)
Violent crime rate	Per 100 people	U.S. Bur. Census (1985)	.96 (.64)

Student/teacher ratio	Ratio of students to full-time-equivalent instructional employees[‡]	*Census of Governments* (1982, vols. 2, 3)	14.07 (2.46)
Fire rating	Insurance company rating of fire department quality (1 = best; 10 = worst)	Internat. City Management Assoc. (1976)	3.07 (.90)
Hospital beds	Public beds per 1,000 people in the city	U.S. Bur. Census (1985)	12.73 (7.59)
Effective property tax rate	Nominal rate times assessment/sales ratio (1979 numbers)[§]	Assessment/sales and assessed base: *Census of Governments* (1982, vol. 2); city and special district property tax revenues: U.S. Bur. Census (1978–79; 1979)	1.31 (.79)
State and local income tax rate	State: average rate in 1979 after deductibility for person with $20,000 adjusted gross income	Feenberg and Rosen (1986, table 6.6)	3.61 (2.71)
	Local: flat rate typically; if not, highest rate applicable used	Tax Foundation (1978)	
State corporate income tax rate	Highest rate applicable as of 7/1/80 (for most states, the highest bracket began at a fairly low profit level)	Advisory Comm. Intergovernmental Relations (1981, table 89)	6.32 (3.05)
Percentage public union organized	Percentage of local public workers in the central city who are organized[‖]	U.S. Bur. Census (1979)[#]	.44 (.21)
Housing expenditures	Annual housing expenditures; converted using 7.85% discount rate	U.S. Bur. Census (1983)	$4,542 (3,904)
Wages	Weekly wages	U.S. Bur. Census (1983)	$299 (208)

* The summary statistics for all city-specific variables are unweighted (by housing or worker observations) means and standard deviations (in parentheses) over the 130 cities in the sample.

† In adjusting the cost-of-living data, we deleted the shelter component of the index except for costs associated with maintenance and furnishings. Property taxes and mortgage payments were also deleted. Because of the nature of the BLS data, we could not avoid deleting some costs such as utilities that are associated with normal upkeep. The remaining upkeep costs amount to 25 percent of the overall shelter budget on average. Social security and all income taxes were also deleted. Intercity variation in federal tax burdens reflects differences in income more than in the intrinsic cost of living State and local taxes are controlled for separately except for sales taxes, which are reflected in the cost-of-living index. (It was not possible to obtain local sales tax data for many cities in our sample. Further, it is difficult to compute an effective sales tax rate because the base for the tax appears to differ widely across cities.) Finally, the BLS reports direct metropolitan area budget data for only 38 of the cities in our sample. We imputed a cost-of-living index for the other cities. This was done by regressing the adjusted budget data on region dummies and SMSA population density, with the resulting coefficients used to impute the missing index values. The R^2 for the regression was .51. Those results are available on request. All standard errors reported in the tables have been adjusted to account for the imputation of the missing cost-of-living index data (see Pagan [1984] and Murphy and Topel [1985] for the procedure).

‡ The data employed pertain to 1982. Similar data for many of our cities are also available for 1977. There is a very strong positive correlation between ratios across the two years.

§ The nominal rate is calculated by dividing total property tax revenues by the assessed base. It is noteworthy that our property tax revenue includes funds raised by the city municipal government as well as special districts within city boundaries. The nominal rate is then multiplied by the city's median aggregate assessment/sales ratio to obtain the effective rate.

‖ As with the property tax revenue data, these percentages are based on municipal workers as well as employees of special districts.

\# The *Survey of Governments* also provides information on the percentage of workers in bargaining units. That variable is a ratio in which the numerator and denominator come from different surveys conducted by the Bureau of the Census. Various cities were found to have nonsensical coverage ratios far in excess of one when the bargaining unit data were examined. The organization numbers do not suffer from this defect.

V. Conclusions

Differences in the local fiscal climate generate compensating differentials across local land and labor markets just as we have long known amenities to do. Thus they should affect the local quality of life. This paper presents new estimates of the quality of life that highlight the importance of local fiscal conditions. Unlike standard locational amenities, the fiscal climate is under the control of local authorities. Thus the quality of life may be more malleable than we have previously thought. Finally, accounting for city group effects through a random effects estimation is found to have a similarly strong impact on the rankings and their associated standard errors. The presence of strong group effects begs the question of whether they should be included in the quality-of-life index. Because it is not apparent what the proper procedure to follow is, we suggest that rankings with and without the group effects be presented to give the reader some idea of potential problems from omitted variables.

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