The Importance of Local Fiscal Conditions in Analyzing Local Labor Markets

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A new test of the compensating wage differential model is proposed. The logic behind Roback's model, which shows how differences in nonproduced amenities may be reflected in intercity wage differentials, is extended to the case of differences in local fiscal conditions, represented by tax rates and publicly produced services. Results show that differences in local tax rates and services provisions do generate compensating wage differentials across cities. The effects of a particularly large set of taxes and effective services output measures are examined. Differences in local fiscal conditions are shown to play important roles in explaining the variance in intermetropolitan wages.

I. Introduction

The equalizing differences model is at the heart of modern labor economics. Previous empirical studies have tested this model by focus-

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ing primarily on worker- and job-related traits. Some of these studies have produced results that are inconsistent with the model's predictions. One explanation for the relatively poor results is that there exists in micro data sets a significant amount of unobserved heterogeneity in worker productivity that may be correlated with the workers' observed job traits. Thus the coefficients on the job trait variables reflect not only the underlying compensating wage differentials but also the omitted variable bias.

We propose an alternative strategy for testing the equalizing differences model, which includes in the wage specification factors other than job traits that could lead to compensating wage differentials. Specifically, we test for the presence of compensating wage differentials generated by variation in local fiscal conditions across cities. Local fiscal variables may provide a sharper test of the equalizing differences model because they are not likely to be strongly correlated with unobserved heterogeneity in worker productivity. Moreover, it is straightforward to demonstrate on conceptual grounds that controls for the local fiscal climate should be included in a wage equation.

Building on theoretical work by Rosen (1979) and Roback (1980, 1982), we argue that differences across cities in taxes and produced services may be capitalized into wage rates as well as land prices. We empirically verify this point using a cross section of workers from 125 cities drawn from the 1980 *Current Population Survey*. Controlling for detailed industry and occupation, we find that differences across cities in local fiscal conditions (cost of living held constant) explain nearly as much of the variance in intermetropolitan wages as differences in worker characteristics do.

II. The Theory of Equalizing Wage Differences and Local Fiscal Differentials

A major research program since Rosen's (1974) explication of the compensating wage difference model has been to empirically verify its implications. Early work by Smith (1973) and Thaler and Rosen (1976) concentrated on estimating wage differentials associated with hazardous jobs. Wages were found to increase significantly with the mortality risk of a job. Subsequent studies focused on other job characteristics such as required physical effort, working conditions, flexibility of hours, access to job training, and layoff risk.[1] While some of

[1] Effort and working conditions are investigated by Lucas (1977) and Hamermesh (1977). Flexibility of hours and work schedules is investigated by Duncan (1976) and Duncan and Stafford (1977). Layoff risk is investigated by Abowd and Ashenfelter (1981) and Murphy and Topel (1986).

the findings were consistent with the theory, overall the results were not encouraging.

Brown (1980) surveyed the literature and presented a test using longitudinal as opposed to cross-sectional data. This allowed him to difference out the effects of omitted variables that remain constant through time. He found that many of the job characteristics still had either the wrong sign or an insignificant coefficient. More recently, Duncan and Holmlund (1983) attempted to reduce the problem of measurement error in job characteristics by using both panel data and self-reported job characteristics. This allowed them to look at the effect of changes in self-reported job characteristics on changes in a worker's wage. They found that the estimates based on the panel data dominated those based on a single cross section. However, constrained hours and hard physical work still produced inconsistent wage effects.

We propose an alternative empirical approach to this problem. Workers who live and work in the same community consume not only the nonpecuniary characteristics of their job but also the characteristics of the community in which that job is located. Community traits generally should be included in the wage specification on theoretical grounds. Roback (1980, 1982) has shown that land prices alone need not fully reflect differences across communities in their levels of nonproduced (dis)amenities. We extend her work to include produced community traits such as government services. We motivate this specification using a simple model that describes the spatial equilibrium across central cities of urban areas.[2]

A representative worker-resident is assumed to consume some composite traded good Y (which is available everywhere at a constant price—the numeraire) and land services N. By living in the jth community, the worker also consumes some locally provided service package G_j and amenity package A_j. The amenity and service packages are taken as given by all potential residents. The representative utility function is then

$$U\{Y, N; A_j, G_j\}. \qquad (1)$$

The gross-of-tax cost of a unit of the composite good is given by $(1 + s)$, where s is the combined state and local sales tax rate. The gross-of-tax land rental price r is given by $(1 + t)n$, where t is the local property tax rate and n is the local land rental. The budget constraint requires

[2] Blomquist, Berger, and Hoehn (1988) recently have extended Roback's model to allow variation in amenities within as well as across urban areas. Agglomeration effects can be introduced in such a model. The Appendix outlines an expanded version of the model in the text that incorporates within metropolitan area variation in amenities and government tax and service conditions.

that the tax-inclusive costs of the composite good and land consumption do not exceed after-tax wage income (given by $[1 - z]W^g l$, where z is the local wage tax rate, W^g is the gross wage, and l is the assumed single unit of labor supplied) plus any endowment income I:

$$(1 + s_j)Y_j + (1 + t_j)n_j N_j \leq (1 - z_j)W_j^g l_j + I. \tag{2}$$

The indirect utility function is then given by

$$V = V\{W^g(1 - z), r, (1 + s); A, G, I\}. \tag{3}$$

For any given amenity-service package, worker utility is a function of the net wage ($W = W^g[1 - z]$), gross-of-tax land rentals r, and the tax-inclusive price of nonland consumption $(1 + s)$. With zero mobility costs, equilibrium requires that utility must be equalized across jurisdictions:

$$\overline{V} = V\{W, r, (1 + s); A, G, I\}. \tag{4}$$

Firms are assumed to be profit maximizers with the following production function for the composite commodity Y:

$$Y_j = F\{L_j, N_j, R_j; A_j, G_j\}, \tag{5}$$

where L is labor, N is land, and R represents intermediate inputs into production. The firm's optimization problem is given by

$$\max_{R,N,L} \pi = (1 - \tau)[Y_j - (1 + t_j)n_j N_j - W^g L_j - (1 + s_j)R_j], \tag{6}$$

where π is profits, τ is the corporate profits tax, and all other terms are as previously defined. Substituting a firm's factor demand functions into (6) yields its indirect profit function, Π. If we assume that firms are mobile in the long run, equilibrium requires that profits be equalized across cities. Thus

$$\Pi\{W^g, r, \tau, (1 + s); A, G\} = \overline{\Pi}. \tag{7}$$

A reduced-form wage equation can be derived by isolating gross-of-tax land rentals in (4) and (7). Equating these functions results in the following reduced-form wage equation:

$$W^g = W\{(1 + s), z, \tau, A, G, I\}. \tag{8}$$

The intersection of the level sets of (4) and (7) determines this wage rate, as is illustrated in figure 1. The model implies that variations in the effective property tax rate (t_j) are fully capitalized into land prices (n_j). Thus they do not affect wage rates in (8). Recent work we have done (Gyourko and Tracy 1989) confirms this prediction.

It is important to realize that the reduced-form wage comparative statics for (8) can be opposite in sign to the pure compensating differ-

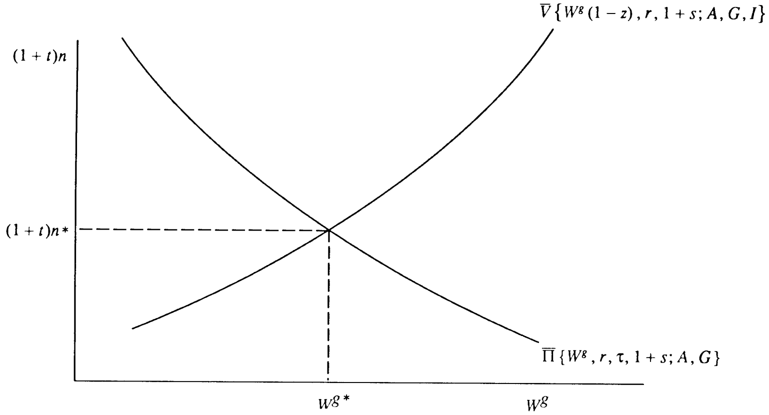

FIG. 1

ential. For example, the impact of an increase in government services from G_0 to G_1 on equilibrium wages (with τ, s, z, I, and A held constant) is given by

$$\left.\frac{dW^g}{dG}\right|_{\tau,s,z,A,I} = \frac{1}{V_w(1-z)}\left[-V_G - V_r(1+t)\frac{dn}{dG}\right]$$

$$= \frac{\left[\frac{1}{V_w(1-z)}\right]\left(-V_G + V_r\frac{\Pi_G}{\Pi_r}\right)}{B}, \tag{9}$$

where

$$B = 1 - \frac{V_r}{V_w}\frac{\Pi_w}{\Pi_r}\frac{1}{1-z} > 0,$$

and V_x and Π_x denote first partials with $V_w > 0$, $V_G > 0$, $V_r < 0$, $\Pi_r < 0$, $\Pi_w < 0$, and $\Pi_G \geq 0$.

Because services can affect both worker utility and firm profitability, the sign of the reduced-form impact on wages is uncertain. With taxes held constant, increasing service quality or quantity makes workers better off. The first term on the right-hand side of (9), $-(V_G/V_w) \times [1/(1-z)]$, is negative and represents the pure compensating wage differential generated by the added service. If the service is not productive on the firm side ($\Pi_G = 0$), the reduced-form impact is unambiguously negative although now a conservative estimate of the true compensating differential because land rents also rise ($dn/dG > 0$), as is illustrated in figure 2. If the service is productive ($\Pi_G > 0$), then $\bar{\Pi}$ shifts to $\bar{\Pi}'$ in figure 3 to maintain firm profits. In the new equilibrium, gross land rentals are unambiguously higher, but the sign of the reduced-form wage effect is ambiguous and depends on

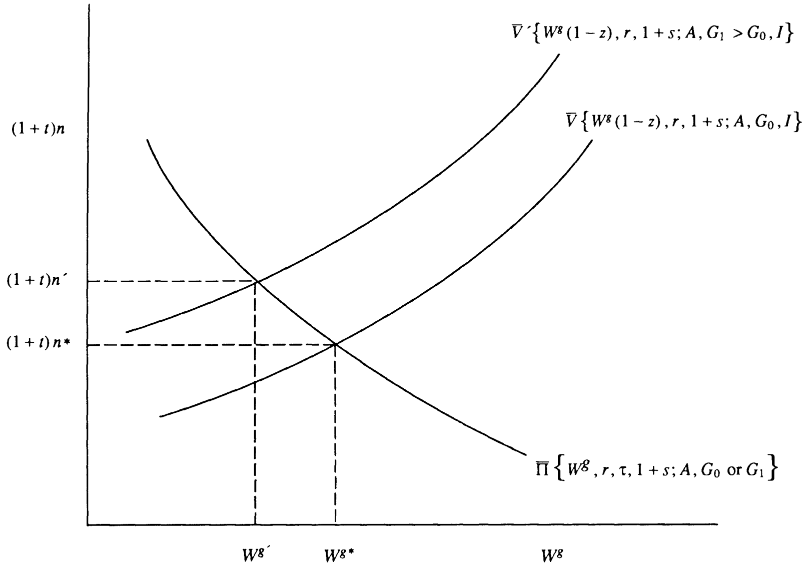

FIG. 2

the magnitude of the shifts in the $\bar{V}$ and $\bar{\Pi}$ curves. Thus if the service is completely nonproductive or if the benefits to firms are of second-order importance, observed wages will likely fall in response to higher service levels.

The empirical results presented below are broadly consistent with the hypothesis that firms receive relatively few or no benefits from added local services and that wages do much of the adjusting. This is also consistent with findings reported in Hoehn, Berger, and Blomquist (1987) and Blomquist et al. (1988). They generally find that the majority of the impact of amenity differences (up to 80 percent) are reflected in wages and not in housing prices. Other findings in the urban economics and local public finance literatures provide additional evidence supporting this view. Most business location studies find that differences in local public services provisions have little or no influence on the location of business activity (see Schmenner [1973, 1982] and the recent review by Newman and Sullivan [1988]).

Comparative statics results for the other variables of interest are also easily calculated. The result for a change in amenities A is identical in form to (9) with A substituted for G in the equation. Equation (10) gives the impact on wages from a small change in the local income tax rate z:

$$\left.\frac{dW^g}{dz}\right|_{\tau,s,A,G,I} = \frac{W^g}{1-z} - \frac{V_r}{V_w}\left(\frac{1+t}{1-z}\right)\frac{dn}{dz} = \frac{W^g/(1-z)}{B}. \quad (10)$$

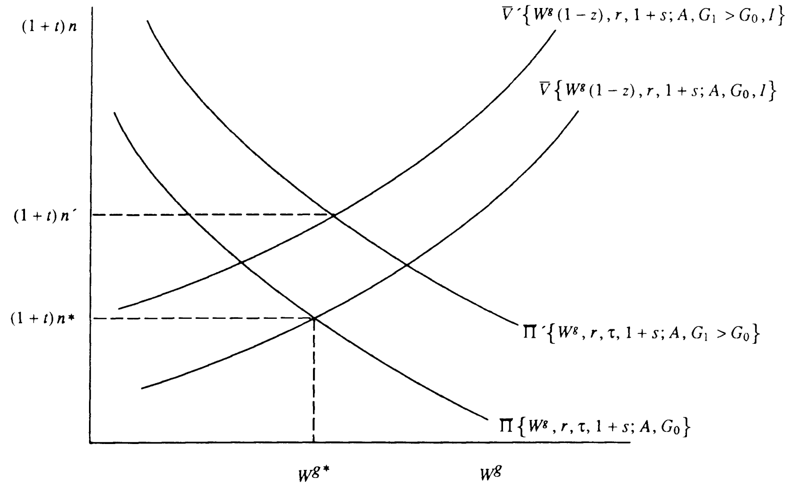

FIG. 3

Once again, the first term on the right-hand side of (10), $W^g/(1 - z)$ represents the pure compensating differential and is positive in this case. If there is some indirect relief through lower land prices ($dn/dz < 0$), (10) is still positive but $\bar{V}$ can be maintained without gross wages rising sufficiently to keep net wages constant.

Equation (11) provides the comparative statics result for a small change in the corporate income tax rate, τ:

$$\left.\frac{dW^g}{d\tau}\right|_{s,z,A,G,I} = \frac{\dfrac{V_r}{V_w}\,\dfrac{\Pi_\tau}{\Pi_r}\,\dfrac{1}{1 - z}}{B}. \tag{11}$$

As long as there is some elasticity to the indirect utility function ($V_r/V_w \neq 0$), the wage falls in the face of a higher corporate profits tax, implying that workers bear at least part of the burden of this tax. In terms of our figures, an increase in τ results in a downward shift of the $\bar{\Pi}$ curve with a fall in wages as long as the indirect utility function $\bar{V}$ is not vertical.

A change in the sales tax on nonland consumption has the following impact on wages:

$$\left.\frac{dW^g}{ds}\right|_{\tau,z,A,G,I} = \frac{1}{V_w(1 - z)}\left[-V_s - V_r(1 + t)\frac{dn}{ds}\right]$$
$$= \frac{\left[\dfrac{1}{V_w(1 - z)}\right]\left(-V_s + V_r\dfrac{\Pi_s}{\Pi_r}\right)}{B}. \tag{12}$$

If firms use intermediate goods subject to the sales tax, land rentals rise unambiguously while the ultimate impact on wages is uncertain. Workers and firms are both worse off initially (services constant). Higher wages and lower land prices will ration workers across scarce sites while lower wages and lower land prices will ration firms across scarce sites in the face of an increase in s. Thus land rentals fall unambiguously while the ultimate impact on wages is uncertain. Changes in the net-of-tax cost of nonland consumption result in wage effects qualitatively identical to those just described for sales tax rate changes.[3]

III. Construction of the Data Set

We test the hypothesis that varying local fiscal conditions generate compensating wage differentials by estimating an expanded wage specification like that in (8) on workers selected from the 1980 *Census of Population.* We started with a 1/100 subsample of workers from the A and B samples of the census. Several selection requirements had to be satisfied before an observation was included in the data set used in the estimation. The individual had to work in the private sector, his or her major activity last week was either working or with a job but not working, he or she had to be a full-time labor market participant, and he or she had to live and work in a central city.

Inman (1981) and others have attempted to model the public-sector bargaining process. With public-sector wages being determined via a budgetary process involving the level of state and local taxation, tax rates and levels of services provisions cannot be considered exogenous determinants of public-sector wages. Consequently, it is difficult to apply Rosen's (1974) compensating differential story with respect to the tax-service package to public-sector workers. Requiring workers to live and work in the central city is important because the tax and service data we collected pertain to central cities only and not to their surrounding suburbs.

The next step involved merging community characteristics into the census data. Data were collected on fiscal and nonfiscal characteristics of the central cities reported in the *Census of Population.* The fiscal traits included various tax liability and service measures. The nonfiscal characteristics included the metropolitan cost of living, the population growth between 1970 and 1980, the city unemployment rate (in

[3] It should be noted that incorporating agglomeration effects introduces an added source of ambiguity into estimated reduced-form wage effects, as Blomquist et al. (1988) have recently shown. With agglomeration impacts, it also is not necessarily true that the reduced-form estimates will tend to be conservative estimates of the true compensating wage differentials. See the Appendix for more details.

1980) as reported by the Bureau of the Census, and amenities such as air quality and weather conditions. We were able to collect complete data on all variables other than the cost of living for 125 of the cities across 46 states covered in the census.

We adjusted the Bureau of Labor Statistics' (BLS) intermediate family metropolitan budget data in order to better approximate the cost of nonland consumption in an area. We deleted the shelter component of the budget data except for costs associated with home maintenance and furnishings. Property taxes and mortgage payments were also deleted. Unfortunately, by deleting these costs, which are clearly associated with land prices, we were forced also to delete some expenditures such as utilities that are associated with normal upkeep. This could not be avoided since the shelter component of the cost-of-living index is divided into only two subindexes. The remaining maintenance/furnishings costs amount to, on average, about 25 percent of the overall shelter budget.

Social security and federal, state, and local income taxes were also deleted. Intercity variation in federal tax burdens reflects differences in income rather than differences in intrinsic cost of living across cities. We control for differences in state and local income taxes separately, as is discussed below. Sales taxes were not purged so that the nonland cost-of-living variable used below incorporates intercity variation in state and local sales taxes.

Finally, the BLS reports direct metropolitan budget data for only 38 of the cities in our sample. To take advantage of the full range of variation in fiscal variables, we chose to retain all 125 cities for which local tax and service data exist and to impute a cost-of-living index when a direct measure was unavailable. The BLS metropolitan budget figures were regressed on three region dummy variables and the log of standard metropolitan statistical area (SMSA) population. The coefficients from this regression were used to impute the missing cost-of-living index values.[4]

The amenity data collected include a measure of mean total sus-

[4] The index used was the log of the nonland cost of living. The specific regression results are

$$\begin{aligned}\text{log nonland cost of living} = {} & \underset{(.006)}{-.023} + \underset{(.011)}{.037} \times (\text{NE}) \\ & + \underset{(.012)}{.009} \times (\text{WEST}) \underset{(.010)}{-.016} \times (\text{SOUTH}) \\ & + \underset{(.004)}{.012} \times (\text{log SMSA population}),\end{aligned}$$

$$R^2 = .537;\ \text{mean square error} = .0006.$$

Population is measured in millions. Standard errors are in parentheses pended particulate matter. This variable comes from data provided in the U.S. Environmental Protection Agency's *Air Quality Data—Annual Statistics* publication for 1979. The Rand McNally *Places Rated Almanac* (Boyer 1983) provides a wealth of weather data for the cities including temperature means and extremes and the average number of clear days.

State and local income tax data were collected for each of the 125 central cities. State income taxes consist of a mixture of flat and progressive rate schedules. Ideally, we would like to have average income tax rates that take into account the extent of income deductibility. Feenberg and Rosen (1986, pp. 154–55, table 6.6) calculate such an average tax rate for hypothetical individuals in each state. We use the average rate for 1979 for a person with $20,000 adjusted gross income. Local income taxes almost universally consist of flat tax rates. The local income tax rate data were collected from *Facts and Figures on Government Finances* (Tax Foundation 1978).

Data on state corporate tax rates were also collected. Many states use progressive corporate tax rate schedules. We would again like to have an effective average rate but were unsuccessful in locating such data. However, for most progressive rate states the highest bracket usually began at a fairly low profit level. Consequently, we generally used the highest marginal rate applicable in each state. Our data are for rates existing as of July 1, 1980, as reported in table 89 of *Significant Features of Fiscal Federalism* (Advisory Commission on Intergovernmental Relations 1981).

The last set of variables to consider are the measures of government services. We attempt to control for police, fire, health, and educational services. The standard approach in the Tiebout literature has been to use per capita expenditures data by service category.[5] While it is questionable whether expenditure measures adequately proxy for service levels across relatively homogeneous suburbs within a single SMSA, it is very doubtful that they adequately proxy for service levels across relatively heterogeneous central cities in different SMSAs. We attempted to construct output measures for each service. For police services, we use the per capita incidence of violent crimes. However, health services are proxied for by an input measure, the number of hospital beds per thousand capita.[6] Both the violent crime rate and

[5] An exception is Rosen and Fullerton (1977). They use school test scores in lieu of education expenditures. Since empirical studies of the Tiebout hypothesis examine land price variation across relatively homogeneous locations within an SMSA, it is less surprising that more effort has not gone into developing alternatives to the expenditure variables.

[6] We experimented with both violent and property crime rates and found no significant wage effects associated with changes in property crime, holding violent crime constant. Consequently, property crime was dropped from the specification.

the number of hospital beds are taken from the 1982–83 issue of *County and City Data Book* and pertain to 1980. 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 the best. These intervals are reported in the International City Management Association's *Municipal Year Book 1976,* which covers the universe of cities with populations in excess of 10,000.

The last service controlled for is education. The natural choice for an output measure is a standardized test score measure. Scholastic Aptitude Test (SAT) scores by district are one possibility. However, even test score data will have the potential problem of confounding the quality of educational services provided with the ability of students attending the schools. Further, the SAT is taken only by students who are applying to college. The fraction of graduating seniors who take the SAT varies widely by city and state. Dynarski (1987) and Hanushek and Taylor (1988) demonstrate how to adjust the SAT scores for this selection bias. We were not successful in collecting individual district test scores data via phone and letter surveys. Many districts did not (or at least claimed they did not) collect or save such data in 1979 or adjacent years.[7] This made it impossible to implement those authors' selection correction procedure with the city district as the unit of observation. For this reason, we were forced to use an input measure as our education proxy.

A standard input measure used to evaluate this educational service is the student to teacher ratio. We computed such a ratio using school district enrollment and full-time equivalent instructional employment data from volumes 3 and 4 of the *Census of Governments* published by the Bureau of the Census. The data we use pertain to the year 1982. Similar data for many but not all of our sample are available for the year 1977. There is a very strong positive correlation between ratios across the two years.

IV. Econometric Specification and Empirical Results

The aim of our empirical work is to produce estimates from the labor market of the implicit prices for community attributes. That is, we are

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Alternative health measures that were investigated included infant mortality rates, the number of physicians per capita, and the number of specialists per capita. Unfortunately, these variables can be measured only at the county level. Consequently, we chose to employ the city-specific hospital beds measure.

[7] We were successful in amassing reading score data on fourth through sixth grades for a much smaller set of cities. It turns out that even those data are badly flawed primarily because of nonrandomness in the selection of students taking the test. See Gyourko and Tracy (1986, pp. 19–20, n. 19) for a more detailed discussion of those data.

trying to empirically trace out, via the reduced form in (8), the price function that describes the wage trade-offs workers face when making their job/location choice. We do not attempt to identify any of the underlying preference or technology parameters that generate this locus. We assume that the wage for individual i working in city j can be approximated as

$$\ln W_{ij} = \beta_0 + \mathbf{X}_i\beta_1 + \mathbf{Y}_i\beta_2 + \mathbf{Z}_j\beta_3 + u_{ij}, \quad u_{ij} = \alpha_j + \epsilon_i, \quad (13)$$

where $\mathbf{X}_i$ is a vector of individual characteristics, $\mathbf{Y}_i$ is a vector of industry and occupation controls, $\mathbf{Z}_j$ is a vector of community attributes, $\alpha_j \sim N(0, \sigma_\alpha^2)$, and $\epsilon_i \sim N(0, \sigma_\epsilon^2)$

The systematic portion of an individual's wage is assumed to be determined by the worker's personal characteristics, by his or her industry and occupation, and by characteristics of the community in which the job is located.[8] The community attributes in the **Z** vector include the city population growth rate from 1970 to 1980 and the city unemployment rate in order to control for supply and demand conditions in the local labor market. The adjusted BLS cost-of-living index described earlier is included to capture intercity variation in the price of nonland consumption. Because this index includes the impacts of state and local sales taxes, separate controls for those variables are not included in **Z.** State and local income tax rate variables are included as well as the state corporate tax rate. The two (dis)-amenities we control for are the quality of the air as measured by the total particulate matter and the quality of the weather as measured by the number of clear days. Finally, the four local services proxies for education, health, police protection, and fire protection round out the right-hand-side variables.

In estimating specification (13), we allow the error term to contain both an individual and a city component. The city component, α, is common to all workers in the city and is assumed to be uncorrelated across cities. A random effects specification may be appropriate for several reasons. The α's may represent the composite effect of left-out city attributes that affect local wages. Alternatively, the α's could be generated by common demand or supply shocks to the local labor market that are not already captured by our existing controls for local labor market conditions.

Estimating (13) using ordinary least squares (OLS) is equivalent to assuming that $\sigma_\alpha^2 = 0$. If in fact $\sigma_\alpha^2 > 0$, Moulton (1986) argues that OLS, by ignoring the block-diagonal structure to the covariance ma-

[8] The previous section's model assumes a homogeneous set of workers. We include a standard set of human capital variables ($\mathbf{X}_i$) and industry and occupation controls ($\mathbf{Y}_i$) to control for heterogeneity in productivity or jobs that could affect the location of the schedules in figs. 1–3.

trix, can produce downward-biased estimates of the standard errors of the β's. This problem applies in particular to variables that have no variation within groups sharing a common error component. In this application, the standard errors on the city attributes could be biased.

To test for the appropriateness of the random effects specification, we calculated the one-sided Lagrange multiplier test for the hypothesis H_0: $\sigma_\alpha^2 = 0$ versus the alternative H_1:$\sigma_\alpha^2 > 0$.[9] The one-sided Lagrange multiplier statistic is given by

$$\mathrm{LM} = \frac{\sum_j (N_j \bar{u}_j)^2 - \sum_i \sum_j \hat{u}_{ij}^2}{\hat{\sigma}^2 \left[2\left(\sum_j N_j^2 - N \right) \right]^{1/2}}, \tag{14}$$

where $\hat{\sigma}^2 = \Sigma\,\Sigma\,\hat{u}_{ij}^2/N$, $\hat{u}_{ij}$'s are the estimated OLS residuals of (8), N is the total sample size, and N_j is the number of workers in the jth city. Under the null hypothesis, the Lagrange multiplier statistic asymptotically has a standard normal distribution. For our data, LM = 13.89, which has a probability value of zero and strongly indicates the presence of a city-specific error component.[10]

Summary statistics on all variables used in the analysis are reported in table 1. A weekly wage was imputed for all workers using their reported annual earnings and total weeks worked for 1979. In table 2, we report both OLS and random effects estimates of the wage specification given in (13). While both OLS and random effects assume homoscedasticity, the individual-specific residuals may have nonconstant variances. In this case, incorrect inferences could be made using the standard errors reported by either OLS or random effects. Unfortunately, existing tests for heteroscedasticity tend to have low power. In light of this, MacKinnon and White (1985) recommend reporting "jackknife" standard errors, which are fairly robust to different forms of heteroscedasticity.[11] In table 2, we report jack-

[9] The Lagrange multiplier test was derived by Breusch and Pagan (1980). Honda (1985) and King and Evans (1986) proposed using the one-sided version of the test to obtain more power. See Moulton (1987) for a general discussion and other applications.

[10] A two-step estimation procedure can be used as an alternative to random effects. The first step involves using OLS to regress the observed worker wages on worker characteristics, industry and occupation controls, and a set of city-specific intercepts. The second step involves using generalized least squares to regress the city fixed effects on the city-specific variables. As long as the variance term for the error in the second-stage regression is set equal to the group variance estimate, this two-step procedure provides coefficient and standard error estimates that are identical to those of the random effects estimates.

[11] The jackknife standard error is the standard deviation of the distribution of β's generated by estimating the model N times, each time dropping one of the observations from the analysis. In practice, it is not necessary to run N regressions. For details, see Efron (1982) and MacKinnon and White (1985).

TABLE 1

MEANS AND STANDARD DEVIATIONS OF VARIABLES

Variable	Mean	Standard Deviation
Log wage	5.400	.642
Education (yrs.)	12.648	2.811
Experience (yrs.)	18.441	13.702
Male (0-1 dummy; 1 if male)	.535	.499
White (0-1 dummy; 1 if white)	.795	.403
Married (0-1 dummy; 1 if married)	.583	.493
Veteran (0-1 dummy; 1 if veteran)	.191	.393
Population growth, 1970–80 (%)	2.049	16.919
Unemployment rate, 1980 (%)	7.458	2.738
Log nonland cost of living	−.016	.031
Local income tax rate (%)	.649	1.352
State income tax rate (%)	2.026	1.416
Violent crime (no. per 100 capita)	1.181	.663
Fire rating	2.629	1.222
Hospital beds (no. per 1,000 capita)	9.971	5.102
Student to teacher ratio	14.393	2.724
Total particulate matter (μg per cubic meter)	79.050	23.601
Clear days (no.)	110.776	35.325
State corporate tax rate (%)	6.434	3.211

knife standard errors for the random effects model in addition to regular standard errors from that estimation procedure. Comparisons between the two sets of standard errors indicate that heteroscedasticity is not a serious problem.[12]

The impact of the random effects specification on our results parallels the findings reported by Moulton (1986). The unadjusted OLS standard errors of the city-specific variables sometimes were biased downward by a factor of two to three. As expected, the random effects estimation had little or no effect on the coefficients or standard errors of variables with within-city variation. For some of the data examined by Moulton, random effects induced changes in the signs of some significant coefficients. We find no similarly drastic effects in our coefficient estimates. The coefficient most affected was that on the total particulate matter variable. While it was significant at the 1 percent level in the OLS estimation, its standard error rose by a factor

[12] The results reported in table 2 are derived from a specification including controls for 12 major industry and nine major occupation classifications. *All* the results are robust with respect to another specification including controls for 56 detailed industry and 30 detailed occupation classifications. We report the findings on the basis of the specification including the major industry and occupation controls because computer memory limitations prevented computation of jackknife standard errors for the detailed industry and occupation specification, which had over 110 right-hand-side variables. All results are available on request.

TABLE 2

OLS AND RANDOM EFFECTS WAGE REGRESSIONS (Dependent Variable: Log Wage)

Independent Variables	OLS: Major Industry and Occupation Controls* (1)	Random Effects: Major Industry and Occupation Controls* (2)
Intercept	3.8842	3.7402
	(.0580)	(.1159)(.1105)
Education	.0502	.0493
	(.0021)	(.0021)(.0023)
Experience	.0370	.0368
	(.0013)	(.0013)(.0014)
$(\text{Experience})^2$	−.0006	−.0006
	(.00003)	(.00003)(.00003)
Male	.3070	.3044
	(.0113)	(.0112)(.0121)
White	.0535	.0594
	(.0118)	(.0118)(.0120)
Married	.0597	.0683
	(.0099)	(.0099)(.0097)
Veteran	.1054	.1008
	(.0135)	(.0134)(.0136)
Log nonland cost of living	−.0307	−.1875
	(.2173)	(.5918)(.5950)
Population growth, 1970–80	−.0002	.0008
	(.0004)	(.0010)(.0009)
Unemployment rate, 1980	−.0004	.0061
	(.0021)	(.0050)(.0051)
Local income tax rate	.0107	.0246
	(.0050)	(.0160)(.0174)
State income tax rate	.0189	.0168
	(.0044)	(.0103)(.0096)
State corporate tax rate	−.0107	−.0096
	(.0019)	(.0047)(.0045)
Violent crime	.0521	.0404
	(.0097)	(.0223)(.0225)
Fire rating	.0145	.0223
	(.0054)	(.0149)(.0144)
Student to teacher ratio	−.0017	−.0009
	(.0019)	(.0051)(.0048)
Hospital beds	−.0068	−.0045
	(.0011)	(.0021)(.0021)
Clear days	−.0002	.0003
	(.0002)	(.0005)(.0005)
Total particulate matter	.0008	.0004
	(.0002)	(.0006)(.0006)
R^2	.3828	
F	208.37	
Mean square error	.5053	
σ_i^2		.2464
σ_α^2		.0125
Number of observations	12,805	12,805

NOTE.—Standard errors are in parentheses. In col. 2, the second number in parentheses is the jackknife standard error.

* Twelve major industry and nine major occupation classification controls were also included. Estimated coefficients are available on request.

of three in the random effects estimation. The local and state income tax variables as well as the fire rating measure also had estimated wage effects that were significant at the 1 percent level in the OLS specification. The random effects results show their significance levels to range between 10 percent and 14 percent (for the standard null of $\beta = 0$). These results highlight the importance of investigating the appropriateness of a random effects specification whenever there is a group structure to the data.

The remainder of our comments apply primarily to the random effects coefficients for the city characteristics reported in the second column of table 2. The influence of worker traits has been widely discussed in the literature, and our results generally are consistent with previous findings. The industry/occupation results are available on request.

While our findings indicate that differences across cities in fiscal conditions do generate wage differentials, the same is not true for differences across cities in nonfiscal conditions. City population growth, local unemployment, nonland cost of living, total particulate matter, and the number of clear days are jointly (and individually) insignifiicant in the random effects estimation. Recall that the 1970–80 city population growth rate was included to help proxy for local labor market conditions. The insignificant coefficient on city population growth makes it impossible to discriminate between a demand-side effect (i.e., a positive wage coefficient) and a supply-side effect (i.e., a negative wage coefficient). Similarly, the insignificant coefficient on city unemployment makes it impossible to discriminate between a demand-side effect (i.e., a negative wage coefficient) and a risk premium on a supply-side effect (i.e., a positive wage coefficient). Adams (1985) found that wage premiums were generated in the face of long-run but not short-run unemployment differences. We were unable to collect consistent time-series data on local unemployment rates for all our cities (particularly the smaller ones) that would be necessary to calculate the decomposition used by Adams. Our positive coefficient is consistent with the unemployment risk premium story, but the evidence is weak. Further, we cannot reject the null hypothesis that intercity variation in the nonland cost-of-living index generates no compensating wage differential. We included the number of clear days to control for a weather amenity. This variable's coefficient has an unexpected positive sign in the random effects specification, but it was never found to be significant at standard levels. The same generally was true for other weather proxies we experimented with (e.g., rainfall and snowfall). The pollution measure (total particulate matter) is significant with the expected positive coefficient in the OLS specification. However, the variable loses its significance in the random effects specification since its coefficient is halved and its standard error increased by a factor of three.

Turn now to the separate fiscal controls. Positive coefficients on the state and local income tax rates are expected. A zero coefficient is the lower bound. This would result only if the $\overline{\Pi}$ schedule in figure 2 was vertical. As long as there is some trade-off between land rentals and wages for firms, gross wages should rise if income tax rates rise. As noted earlier, gross wages need not rise sufficiently to keep net wages constant. That upper bound results only if the isoprofit schedule is perfectly elastic.

The magnitude of the income tax coefficients that corresponds to net wages' being held constant is derived in equations (15) and (16). Assume that the local income tax schedule is characterized by a flat rate, z. The following relationship exists between net and gross wages:

$$W = \left(1 - \frac{z}{100}\right)W^g. \tag{15}$$

Taking the log derivative of (15) and setting $d \ln W = 0$ yields

$$\frac{d \ln W^g}{d\tau} = \frac{1}{100 - z}. \tag{16}$$

If the local tax rate is 1 percent (about the average for cities with nonzero income tax rates), then (16) implies that $\ln W^g$ must increase by 0.0101 in response to a 1 percent increase in τ in order to keep net wages constant. Note that with a progressive tax schedule a positive coefficient of less than 0.0101 could still imply that net wages remain unchanged. The reason is that with progressive taxation, marginal tax rates exceed average rates so that a 1 percent increase in the marginal rate implies less than a 1 percent increase in one's tax bill.

The coefficients on the state and local income tax variables are positive, with the state tax rate coefficient barely significant at the 10 percent level. Both point estimates are nearer their predicted upper bounds than their lower bounds, which is consistent with a relatively flat isoprofit schedule. However, their standard errors are such that at the 5 percent level we also cannot reject that each coefficient is not significantly different from zero. Consequently, we are unable to make any firm statements about the constancy of net wages in the face of income tax differentials across cities.

The sign of the state corporate income tax coefficient is expected to be negative if workers bear at least some of the burden of this tax. Workers would be more likely to bear part of the burden in a situation in which the firm is producing a tradable good for a competitive national (or at least nonlocal) market and in which capital is relatively mobile. The former limits the possibility of forward shifting to consumers, and the latter makes it difficult to shift backward to owners of the firm. Our results are consistent with workers' bearing part of the burden as the coefficient is negative and significant. The random effects result implies that a one-percentage-point (15.5 percent) increase in the state corporate income tax rate from its sample mean value is associated with about a 1 percent fall in annual wage income (or \$106 in yearly wages).[13]

Three of the four local services coefficients yield estimates that are consistent with the equalizing differences hypothesis. The coefficients on the police, health, and fire services all have the expected sign, with the health and police services coefficients significant at the 5 percent and 10 percent levels, respectively. The fire rating variable is significant at the 14 percent level. Unit increases in each service measure are associated with the following changes in annual earnings: police, \$457; health −\$50; and fire, \$250. The relative magnitudes of the compensating wage differentials associated with these three service categories may be seen more clearly by comparing the earnings change associated with a standard deviation change in each variable. Those standardized marginal effects are police, \$303; health −\$255; and fire, \$305.[14] These figures represent 2.7 percent, 2.3 percent, and 2.7 percent of mean annual wage income, respectively. The education service proxy has an unexpected negative sign but is small in magnitude and insignificant in both specifications.

While some of the seven state and local fiscal variables (the four service measures and the three tax rates) are individually insignificant, as a group the fiscal variables are jointly significant at a fairly high degree of confidence. The appropriate F-statistic ($F_{7,12,766}$) is 1.90 with an associated probability value of .06.

We also estimated the model in (13) including the property tax rate in the specification. With imperfect service controls, including the property tax rate may help ensure that the public-sector budget constraint is satisfied. The coefficient was negative as expected but was quite small (in absolute value) and insignificant. Including the propery tax rate has no significant impact on any other variable. Thus the nonexpenditure service controls appear to capture relatively well the locally provided service environments across cities. Finally, we experimented with a version of (13) including three region dummies. The

[13] This figure and the ones that follow are calculated using the mean weekly wage in the sample and an assumption of 50 weeks worked during the year.

[14] In an earlier version of this paper, we estimated the model on a sample of nonunion private-sector workers from the May 1977 *Current Population Survey*. That sample was limited to 31 central cities. We obtained remarkably similar results for the city fiscal variables. In particular, the standardized marginal effects in 1977 earnings for police, health, and fire services were \$420, −\$380, and \$260, respectively.

region coefficients are jointly insignificant in the random effects specification, and their inclusion did not significantly affect the coefficients on the city fiscal variables.

We have demonstrated that variation in local tax and service variables generates compensating wage differentials as predicted by the equalizing differences framework. However, these results do not directly indicate to what extent the variation in average wages across cities is related to differences in city attributes as opposed to differences in worker characteristics or in the industry/occupation composition of local labor markets. To address this issue, we performed an analysis of variance of mean wages across cities in our sample.

We began by calculating the average observed log wage ($\overline{\text{LNWAGE}}$) for each city in our sample. We also computed the average predicted log wage for the following four categories of regressors: (*a*) worker traits (education, experience and its square, married, white, veteran, and male), denoted $\widehat{\text{WT}}$; (*b*) industry/occupation structure (based on the industry and occupation classifications), denoted $\widehat{\text{IO}}$; (*c*) nonfiscal community attributes (population growth, unemployment rate, log nonland cost of living, clear days, and total particulate matter), termed $\widehat{\text{NFISC}}$; and (*d*) state and local fiscal traits (local income tax rate, state income tax rate, state corporate tax rate, violent crime rate, fire rating, student to teacher ratio, and hospital beds), termed $\widehat{\text{FISC}}$. Specifically, variable means for each city were computed and then used as in (17). City *j*'s mean predicted log wage based on (say) nonfiscal traits is given by

$$\begin{aligned}\widehat{\text{NFISC}}_j = {} & (.0008 \times \text{population growth}_j) \\ & + (.0061 \times \text{unemployment rate}_j) \\ & + (-.1875 \times \text{log nonland cost of living}_j) \\ & + (.0003 \times \text{clear days}_j) \\ & + (.0004 \times \text{total particulate matter}_j),\end{aligned} \tag{17}$$

where the coefficients used are those reported in the second column of table 2.

We then regressed $\overline{\text{LNWAGE}}$ on various combinations of $\widehat{\text{WT}}$, $\widehat{\text{IO}}$, $\widehat{\text{NFISC}}$, and $\widehat{\text{FISC}}$. The first two columns of table 3 summarize those regression results by presenting the minimum and maximum partial R^2's for each category of explanatory variables. The minimum partial R^2 for any one category of variables is the marginal increase in explanatory power attributable to adding that category to the other three categories. The maximum partial R^2 for any category is the explanatory power attributable to including only that category in the regression. The R^2 from the regression of $\overline{\text{LNWAGE}}$ on all four

TABLE 3

ANALYSIS OF VARIANCE: WORKER, INDUSTRY/OCCUPATION, AND CITY TRAITS

Variable Category	Major Industry and Occupation Controls: Minimum Partial R^2 (1)	Major Industry and Occupation Controls: Maximum Partial R^2 (2)	Detailed Industry and Occupation Controls: Minimum Partial R^2 (3)	Detailed Industry and Occupation Controls: Maximum Partial R^2 (4)
Worker traits	.0876	.1952	.0525	.1917
Industry/occupation	.0686	.1519	.1674	.2882
City-specific traits:				
Nonfiscal	.0480	.0616	.0029	.0170
Fiscal	.0560	.1413	.0911	.1412

categories of variables is .40. Note that the set of fiscal variables tends to have nearly as much explanatory power as the set of worker trait variables and the set of industry/occupation variables. This result should not be interpreted as implying that the state and local fiscal environment is a key determinant of wage *levels* in cities. However, it does highlight that understanding how individual wages adjust for differences in fiscal traits across communities is an important part of the explanation of how overall wages *vary* across cities.

This conclusion is robust with respect to a specification that included 56 detailed industry and 30 detailed occupation classifications (see n. 12). The last two columns of table 3 report the minimum and maximum partial R^2's for the four sets of variables using the detailed industry and occupation controls. The R^2 from including all four categories increases to .47. The explanatory power of the $\widehat{IO}$ variables naturally rises. However, controlling for more detailed industry and occupation traits does not markedly weaken the role fiscal differentials play in explaining average intercity wage differentials. The influence of the $\widehat{NFISC}$ vector is markedly lower, as is the minimum partial R^2 for the set of worker traits.

V. Conclusions

In this paper we investigated the question of whether wages tend to equalize differences across cities because of variation in the level of taxation and the provision of basic public services. The results generally validate the equalizing differences model. Moreover, variation in local fiscal conditions appears to be a key determinant of intermetropolitan wage differences. Fiscal differences explain roughly the same amount of the variation in mean wages across cities, as do differences in worker traits on differences in major industry/occupation classifications. Thus a class of variables long known to have an important influence on local land markets is demonstrated also to have an important influence on local labor markets.

Appendix

This Appendix extends the model presented in Section II to incorporate agglomeration effects. It has long been thought that overall urban area size could affect firm productivity either by reducing unit costs because of favorable agglomeration economies or by increasing costs because of added congestion. Blomquist et al. (1988) first introduced agglomeration impacts into a Rosen-Roback model.

Agglomeration effects (AGL) are modeled here as directly influencing firm profits but not worker utility. We assume that these effects are a function of SMSA employment (E). The model in Section II focused solely on the spatial equilibrium across central cities, but SMSA employment obviously is also a function of non–central city conditions. Thus metro area employment is a function of amenities and government services policies in the central city (A and G) and the suburbs (A^s and G^s) as well as other variables (H). The new equilibrium conditions are

$$\bar{V} = V\{W, r, (1 + s); A, G, I\}, \tag{4'}$$

$$\bar{\Pi} = \Pi\{W^s, r, \tau, (1 + s); A, G, \mathrm{AGL}[E(A, G, A^s, G^s, H)]\}. \tag{7'}$$

Note that central city amenities and government services now have an indirect impact on firm profits (and thus reduced-form wage effects) through their influence on agglomeration effects as well as their direct impacts in (4′) and (7′). Unlike the model in the text, suburban conditions also can now affect firm profits in the city via agglomeration effects.

The presence of agglomeration effects can help generate an estimated reduced-form wage effect that is opposite in sign to that generated by a pure compensating wage differential. However, agglomeration effects need not always make the reduced-form effects conservative estimates of the true compensating differential, as is illustrated for the case of a change in central city government services G. Equation (9) from the text reproduces the reduced-form wage effect, while (9′) illustrates the added impact that agglomeration economies can have (with z, τ, s, A, A^s, G^s, I, and H held constant):

$$\frac{dW^g}{dG} = -\frac{\left[\dfrac{1}{V_w(1 - z)}\right]\left(V_G - V_r\dfrac{\Pi_G}{\Pi_r}\right)}{B}, \tag{9}$$

$$\frac{dW^g}{dG} = -\frac{\left[\dfrac{1}{V_w(1 - z)}\right]\left(V_G - \dfrac{V_r\Pi_G}{\Pi_r} - \dfrac{V_r\Pi_{\mathrm{AGL}}\mathrm{AGL}_E E_G}{\Pi_r}\right)}{B}. \tag{9'}$$

Assume that the change in G increases total urban employment and agglomeration effects ($E_G > 0$ and $\mathrm{AGL}_E > 0$). If this creates agglomeration economies valuable to firms, then their costs fall ($\Pi_{\mathrm{AGL}} > 0$). In terms of figure 3, the added impact of the agglomeration economies shifts $\bar{\Pi}$ even further upward, making (9′) more positive (or less negative) than (9) (assuming that the direct effect is such that $\Pi_G > 0$). In this case, the agglomeration effect does make the reduced-form wage impact an even more conservative estimate of the compensating differential. However, there is no a priori reason to assume that increased SMSA employment leads to lower firm costs. Added congestion could cause $\Pi_{AGL} < 0$. In this case, if the indirect agglomeration effect dominates the direct effect of dG on firm costs, it is conceivable that (9′) could be more negative than even the true compensating differential, $(-V_G/W_w) \times [1/(1 - z)]$.

The reduced-form wage equation from (4′) and (7′) is

$$W^g = W\{(1 + s), z, A, G, A^s, G^s, H, I\}. \qquad (8')$$

If agglomeration effects are thought to be important, variables determining them such as non–central city amenities and government traits should be added to the specification. To test if suburban conditions contribute to agglomeration effects that are economically important, one could estimate (8′) with and without the A^s, G^s, and H variables and see if their coefficients are significant.

Blomquist et al. (1988) compiled amenity data at the county level and had A and A^s variables for separate counties within the same SMSA. They estimated wage and rent equations in order to obtain full hedonic prices for use in quality-of-life indexes for each county in their sample. They did not perform a test like the one suggested above to see if amenity conditions outside one's jurisdiction do contribute to economically relevant agglomeration effects. Much of the amenity data we experimented with (besides the pollution measure) did not vary across jurisdictions within an SMSA. Further, it simply is not feasible to obtain relevant local public-service and tax data on many of the suburban areas for many of the 125 SMSAs in our sample. Consequently, we could not estimate an equation like (8′).

Further, recent work by Henderson (1986) suggests that the reduced form given by (8′) may be misspecified. He found that important agglomeration economies for a firm primarily depend on the concentration of its industry's employment or output in the metropolitan area. A test of that hypothesis in our context would require detailed industry employment or value-added data that are not available for many SMSAs in our sample. Finally, Henderson's findings suggest that the underlying model must be modified to include a set of industry-specific profit functions rather than a single representative profit function. The equilibrium wage in a local labor market would be determined by the intersection of the marginal industry's profit function and the indirect utility function. To test the model, one would have to identify the marginal industry in the data and include the appropriate agglomeration proxy variable for the relevant industry.

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