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ESSAYS ON INTERAREA WAGE DETERMINATION

BY

JOHN V. WINTERS

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
Doctor of Philosophy
in the
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of
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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ABSTRACT

ESSAYS ON INTERAREA WAGE DETERMINATION

By

JOHN V. WINTERS

AUGUST 2009

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This dissertation consists of two essays concerning the determination of wages across areas. The first essay investigates the equilibrium relationship between wages and prices across labor markets. Of central interest is the extent to which workers receive higher wages to compensate for differences in the cost of living. According to the spatial equilibrium hypothesis, the utility of homogenous workers should be equal across labor markets. This implies that controlling for amenity differences across areas, the elasticity between wages and the general price level across areas should equal one, at least under certain conditions. We test this hypothesis and find that the predicted relationship holds when housing prices are measured by rents and the general price level is instrumented to account for measurement error. When housing prices are measured by housing values, however, the wage-price elasticity is significantly less than one, even using instrumental variables. Rents reflect the price paid for housing per unit of time and are arguably the superior measure. Thus, findings in this essay provide support for the full compensation hypothesis. These findings also have important implications for researchers estimating the implicit prices of amenities or ranking the quality of life across areas.

The second essay uses a national level dataset and a spatial econometric framework to examine the effects of teacher unions and other school district characteristics on teacher salaries.

The results confirm that salaries for both experienced and beginning teachers are positively affected by salaries in nearby districts. Investigations of the determinants of teacher salaries that ignore this spatial relationship are likely to be misspecified. We find that union activity increases salaries for experienced teachers by as much as 16-21 percent but increases salaries for beginning teachers by a considerably smaller amount. This result is consistent with predictions from a median voter model.

ESSAY I: WAGES AND PRICES: ARE WORKERS FULLY COMPENSATED FOR COST OF LIVING DIFFERENCES

1. Introduction

A number of studies have shown that wages differ across labor markets even after control for observable individual characteristics.[1] Such wage dispersion across markets can in part be attributed to differences in prices and amenities across areas. If a city has higher prices for goods and services providing a given level of utility, workers will require higher wages to work there.[2] Similarly, if a city has nicer amenities, all else the same, workers will be willing to accept lower wages to work there. In order for a spatial equilibrium to occur, utility must be equal across areas for workers with identical skills and preferences. In previous literature, this is sometimes referred to as the competitive hypothesis or the law of one wage. Many studies have attempted to test the competitive hypothesis (e.g., regional wage gap studies), but they are often hindered by limited information on area prices and amenities.

Several studies interested in interarea wage differentials have used an interarea price index to fully adjust wages for price differences by dividing nominal wages by the price index.[3] Other studies have used fully adjusted wages to measure the implicit prices of amenities across cities (e.g., Rosen 1979; Greenwood et al. 1991; Glaeser and Tobio 2008).[4] DuMond, Hirsch, and Macpherson (1999), however, suggest that full adjustment for prices may be inappropriate to measure interarea wage differentials, say by region or city size. They instead advocate using a partial adjustment whereby the log of the price index (and potentially higher order terms) is

[1] See Dickie and Gerking (1989) for an early review of the literature on interarea wage differentials in the United States.
[2] In this paper, we often use the term city to refer to metropolitan areas.
[3] See for example, Coelho and Ghali (1971, 1973), Bellante (1979), Gerking and Weirick (1983), Johnson (1983), Sahling and Smith (1983), Dickie and Gerking (1987), and Farber and Newman (1987).
[4] See Gyourko, Kahn and Tracy (1999) for a review of the literature on amenity valuation and quality of life.

included as an independent variable in a log wage equation. The coefficient on the log of the price index can be interpreted as the wage-price elasticity. One hypothesis is that the elasticity between wages and the general price level is equal to one. We refer to this as the full compensation hypothesis. Researchers who fully adjust wages for prices implicitly assume that the full compensation hypothesis holds, but few studies have explicitly tested the full compensation hypothesis.

Two studies that have estimated the elasticity between wages and prices are Roback (1988) and DuMond et al. (1999). Roback (1988) uses a now discontinued cost of living index produced by the Bureau of Labor Statistics and estimates a wage-price elasticity of 0.97, both with and without controls for amenities, which would seem to lend support for the full compensation hypothesis. As discussed below, a reexamination of Roback (1988), however, suggests that her measurement of prices is inappropriate and biases her estimates. DuMond et al. (1999) use a price index based on the ACCRA *Cost of Living Index* and find a wage-price elasticity of 0.46 controlling for amenities and 0.37 absent amenities. Thus, the magnitude of the wage-price elasticity and validity of the full compensation hypothesis are still open questions.

This paper builds on earlier work by examining the *equilibrium* relationship between wages and prices, controlling for amenities. We stress the word *equilibrium* because wages and prices are simultaneously determined. While this paper does not provide evidence on the causal effect of prices on wages or *vice versa*, much can be learned from examining the equilibrium relationship between the two. Following Rosen (1979) and Roback (1982), we develop a model that predicts that under certain conditions the elasticity between wages and the general price level should equal one controlling for amenities. In other words, workers should be fully compensated for differences in prices across cities. However, to the extent that the assumptions of the model do not hold, the elasticity between wages and the general price level may differ from unity. The relationship between wages and prices is ultimately an empirical question.

We find that estimates of the wage-price elasticity are sensitive to whether housing prices are measured by housing values or rental payments. Rents are the ideal measure of housing prices, the price paid per unit of time for the use of housing, but in practice housing values are often used to measure housing prices. The preferred specification measures housing prices by rents. Measuring housing prices by rents and using Ordinary Least Squares, we estimate the wage-price elasticity to equal 0.76, but OLS estimates may be downwardly biased due to measurement error in the price index, especially the non-housing price component. Instrumenting for the rent-based price index using rents for the previous year, the estimated elasticity between wages and the general price level is nearly identical to one. Again, if rents are the ideal measure of housing prices, this finding provides strong empirical support for the full compensation hypothesis.

When housing prices are measured by housing values, the estimated elasticity between wages and the general price level is never more than 0.5, even using instrumental variables. The findings of this paper have important implications for researchers estimating the implicit prices of amenities or ranking the quality of life across areas. First, when adjusting wages for prices, housing prices should be measured by rents and not values. Second, it is shown that ignoring differences in non-housing prices, as often done, biases estimates of the implicit prices of amenities.

2. Theoretical Considerations

This section develops a simple model of the equilibrium relationship between wages, prices, and amenities across cities and regions following Rosen (1979) and Roback (1982). Firms produce X_1 and X_2 according to constant returns to scale production functions using labor (N), capital (K), and land (L) given locational differences in productivity due to amenities (Z): $X_i = X_i(N, K, L; Z)$. The marginal products of labor, capital, and land are all non-negative, but increases in amenities can either increase or decrease productivity. The price of capital is determined exogenously in the world market and normalized to equal one, while the prices of labor (W) and land (P_L) are determined competitively in local markets. In equilibrium, firms earn zero profits and the price of each good is equal to its unit cost of production (C_i):

$$C_i(W, P_L; Z) = P_i, \qquad i = 1, 2. \tag{1}$$

Workers maximize utility subject to a budget constraint, where utility is a function of goods X_1 and X_2 and location-specific amenities: $U = U(X_1, X_2; Z)$. Workers are mobile across cities and regions, and in equilibrium utility for identical workers is equal across areas. The indirect utility function can be represented as a function of wages and the prices of X_1 and X_2 given amenities:

$$V = V(W, P_1, P_2; Z). \tag{2}$$

Taking the total differential of both sides of (2), setting $dV = 0$, rearranging, and employing Roy's Identity yields a slight variant of the equation used by Roback to estimate the implicit price of amenities (Eq. 5 in Roback, 1982):

$$dW = X_1 dP_1 + X_2 dP_2 - P_Z dZ.\text{[5]} \tag{3}$$

[5] Alternatively, we could have defined the expenditure function and used Shephard's Lemma to obtain an equivalent result as in Albouy (2008b).

However, instead of solving for the price of amenities (P_Z), the equation is solved for dW. Dividing both sides of (3) by W, converts the equation to logarithmic form:

$$d\ \ln W = (P_1 X_1/W) d\ \ln P_1 + (P_2 X_2/W) d\ \ln P_2 - (P_Z/W) dZ. \tag{4}$$

Equation (4) says that controlling for amenities, a one percent increase in the price of X_1 will require wages to increase by a percentage equal to the share of wages spent on X_1 in order for utility to remain constant. The same is true for increases in the price of X_2, and the result easily generalizes to the case of more than two goods. In other words, the wage-price elasticity for a good should be equal to the budget share of the good, assuming that non-wage income is negligible. Furthermore, if total consumption expenditure is equal to wage income, $P_1 X_1 + P_2 X_2 = W$, then a one percent increase in the prices of all goods will require wages to increase by one percent to maintain equal utility.

While this interpretation of equation (4) is valid for small changes in prices, it may be less valid for large changes in prices as consumers respond to large price differences by altering their consumption mix. However, if utility is Cobb-Douglas as assumed by Davis and Ortalo-Magné (2008) and others, the elasticity between wages and the price of a good is equal to the expenditure share of the good even for large changes in prices. To see this, let utility take the Cobb-Doulas form: $U = f(Z) {X_1}^{\alpha} {X_2}^{(1-\alpha)}$. Taking a monotonic transformation, the indirect utility function can be written as: $V = C + \ln W - \alpha \ln P_1 - (1-\alpha) \ln P_2 + \ln(f(Z))$, where α is the constant budget share for X_1, $(1-\alpha)$ is the budget share for X_2, and C is a constant. Holding utility constant across areas, $\partial \ln W / \partial \ln P_1$ is equal to α even for large changes in prices. In other words, Cobb-Douglas utility suggests that the elasticity between wages and the price of a good is equal to the good's budget share even for large price changes. Similarly, Cobb-Doulas utility predicts that the elasticity between wages and the general price level should equal one. Workers would, therefore, require full compensation for price differences across cities.

The full compensation hypothesis has considerable intuitive appeal. Suppose there are two cities with equal bundles of consumer amenities, but one city has higher prices for goods and services. If the general price level in the expensive city is 10 percent higher than in the less expensive city, how much higher will wages have to be in the expensive city to keep workers from leaving for the other city? Intuition seems to suggest that a worker would need 10 percent higher wages to compensate for the 10 percent higher price level. In other words, workers would require full compensation for price differences holding amenities constant.

Workers may not be fully compensated for price differences for a number of reasons. If workers are highly immobile or do not have sufficiently good information on wages, prices, and amenities in other cities, then migration may not arbitrage away interarea differences in wages, prices, and amenities. In other words, barriers to migration may cause workers in some markets to have higher utility levels than comparable workers in other markets. In reality though, workers are often quite mobile across markets. Even if some workers are relatively immobile, the movement of marginal migrants between labor markets may result in an equilibrium relationship between wages and prices that yields equal utility across areas for all homogenous workers.

The relationship between wages and prices may also differ from full compensation if utility is considerably different from Cobb-Douglas and prices are very different across markets. Thinking of X_1 and X_2 in the above model as housing and non-housing consumption, a high degree of substitutability between housing and non-housing may cause the true elasticity between wages and the general price level to be less than one.[6] As will be shown later, housing prices are significantly more dispersed across areas than non-housing prices. If workers can easily substitute away from housing consumption in places where it is relatively expensive, they will not have to be fully compensated for differences in housing prices.[7] As a result, a fixed basket price index will overstate the true cost of living in expensive cities and cause the elasticity between wages and the general price level to be less than one.

As hinted above, the wage-price elasticity also depends on the extent to which people save. If consumption is less than wage income ($P_1X_1 + P_2X_2 < W$), the true wage-price elasticity should be less than one. Conversely, if consumption is greater than wage income, the wage-price elasticity may be greater than one. Evidence from the 2005 Consumer Expenditure Survey suggests that average consumer expenditures are indeed quite close to average after-tax wage income. The ratio of average expenditures to average after-tax income in the 2005 CES is 0.94. The CES is a relatively small sample and there could be some misreporting (e.g., of income), but the available evidence indicates that assuming expenditures are equal to wage income may be a reasonable first approximation.

There are, therefore, a number of reasons why the elasticity between wages and the general price level may be less than one. Ultimately, the relationship between wages and prices is an empirical question. We explore this relationship empirically in subsequent sections.

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[6] Cobb-Douglas utility implies an elasticity of substitution equal to one. The limited literature has not reached a consensus on the elasticity of substitution between housing and non-housing. Ogaki and Reinhart (1998) estimate the elasticity of substitution to be 1.17, but not statistically different from one at the 5% significance level. Piazessi, Schneider, and Tuzel (2007) find estimates of 0.77 and 1.24 depending on the time period considered, neither of which is statistically different from one. However, Benhabib, Rogerson, and Wright (1991) and McGrattan, Rogerson, and Wright (1997) estimate the elasticity of substitution to be 2.5 and 1.75, respectively. Davis and Ortalo-Magné (2008) do not explicitly estimate the elasticity of substitution, but do find that the expenditure share on housing is roughly constant over time and across metropolitan areas suggesting that the elasticity of substitution is close to one.

[7] Consumers may also shift away from consumption of relatively expensive housing toward consumption of local amenities, especially since local residents can often consume natural amenities at very low marginal cost (e.g., climate and coastal location).

3. Empirical Considerations/Previous Literature

The theoretical model suggests that under certain conditions, the elasticity between wages and a composite price index is approximately one. Based on the intuition behind this result, a number of researchers interested in interarea wage differentials have fully adjusted nominal earnings using an interarea price index and estimated log wage equations of the form:

$$\ln\left(W_{ij}/P_j\right) = X_{ij}\beta + \varepsilon_{ij}, \tag{5}$$

where W_{ij} is the wage for person i in city j, P_j is the price level in city j, X is a vector of personal characteristics, β is the corresponding coefficient vector, and ε is an error term with mean equal to zero. Along these lines, Johnson (1983) obtains the seemingly surprising result that fully adjusted wages were more dispersed across cities than were nominal wages, at least for men.[8] DuMond et al. (1999), however, argue that full adjustment may be inappropriate. Instead, they advocate using a partial adjustment where the dependent variable is the log of the nominal wage and the log of the price index is included as an independent variable on the right hand side:

$$\ln W_{ij} = X_{ij}\beta + \theta \ln P_j + \varepsilon_{ij}. \tag{6}$$

Doing so, they find wage dispersion to be considerably lower across markets than with either nominal or fully-adjusted wages.[9]

Theory and empirics also suggest that wages are affected by attributes that make a city a more or less pleasant place to live. Therefore, (5) and (6) can also be modified to include city-specific amenity levels and a corresponding coefficient vector. The parameter θ in (6) can be interpreted as the interarea wage-price elasticity. If θ = 1, (5) and (6) are equivalent. However, if θ is not equal to one, (5) may be misspecified. Thus the value of θ is of considerable interest.

[8] Johnson (1983) uses a pooled cross-section of 34 cities from the May Current Population Survey for 1973-1976 with price data from the BLS for an intermediate standard of living for 1974.

[9] DuMond et al. (1999) use a pooled cross-section of 185 cities from the 1985-1995 CPS Outgoing Rotation Group files with price data from the ACCRA *Cost of Living Index* from the same period.

Roback (1988) estimates equation (6) both with and without amenities and produces estimates of θ equal to 0.97 for both specifications. DuMond et al. (1999), however, estimate a point estimate for θ of 0.46 with amenities and 0.37 without amenities with standard errors small enough in both cases to easily reject the hypothesis that $\theta = 1$. There are a number of differences between the two studies, such as the time period considered, the number of cities considered and the amenities included. However, the most important difference is likely the price indices used and the way they are used. Roback uses a now discontinued price index produced by the Bureau of Labor Statistics from the Handbook of Labor Statistics, and DuMond et al. use a price index based on the ACCRA *Cost of Living Index*. Measurement error may be more significant in the ACCRA price index, and this may explain some of the difference between the estimates of Roback (1988) and DuMond et al. (1999). DuMond et al. reestimate their results using the BLS *Urban Family Budget and Comparative Indexes for Selected Urban Areas* updated from its 1981 value (the last year the BLS produced the index) using the city-specific CPI for a limited number of cities and find that the estimate of θ without amenities increases to 0.526. This price index is much closer to the index used by Roback, but the coefficient estimate it yields is still much less than one.

Closer examination of the two studies reveals a more subtle distinction in the way the price indices are used. DuMond et al. (1999) use the same price index for all workers within a given city. In Roback (1988), on the other hand, the price variable used consists of "low, medium, and high standard of living budgets assigned based on individual family income and number of dependents" (p.41). In other words, Roback assigns persons within a given city a different price value based on their income. Presumably, her intent is to assign to each individual the most relevant price for their particular consumption bundle. This approach creates intra-city variation in prices, and a problem arises if the intra-city variation in prices is spuriously correlated with intra-city differences in wages. In such a case, the coefficient on the price variable in the log equation will be biased. In other words, if the average price index value across cities is greater for the high standard of living price index than for the intermediate index and higher for the intermediate index than for the low index, then the price index is on average increasing with income within cities. Indeed, a separate analysis suggests that this is the case for the price information used by Roback. As a result, regressing log wages on a log price variable constructed as such, the coefficient picks up a within-city effect in addition to the cross-city effect. There may very well be differences in the relative cost of acquiring different standards of living within a city, but accounting for this by introducing intra-city variation in the price index that is explicitly tied to the observed wage is inappropriate. The principal focus should be on cross-city and not within-city effects.

A further problem with Roback's (1988) price variable is that she uses the actual budget dollar amounts instead of price index values. The budgets formerly produced by the BLS are based on what it would cost a family of four in a given city to obtain a given standard of living. The BLS computes the budgets (B_{ij}) for each standard of living (i) in each city (j) by multiplying local prices (P_{ij}) by a basket of goods (X_{ij}) for each standard of living. The basket is also allowed to vary across cities within a standard of living, but is intended to maintain a given standard of living across cities. Ignoring temporarily that the basket varies across cities, recognize that $B_{ij} = P_{ij}X_i$. Regressing $\ln W_{ij}$ on $\ln(P_{ij}X_i)$ is clearly not the same as regressing $\ln W_{ij}$ on $\ln P_{ij}$ because X_i is increasing with income. If one were to use the same budget, $B_j = P_jX$, (e.g., the intermediate standard of living budget) for all workers within a given city and hence have no intra-city variation in budgets, then there would be no problem because taking logs causes ln$X$ to drop into the constant term. Using budgets instead of price index values and allowing the budgets to vary across types of workers within cities means that the "price" variable is severely confounded by intra-city variations in consumption. In other words, the estimates are biased by the fact that workers within a city who have higher wages also have higher standards of living and are assigned a higher consumption basket.

In work not shown, we attempt to replicate Roback's (1988) empirical work and test the sensitivity of the results to alternative measurement of prices. The results suggest that Roback's estimates are biased by using budgets rather than index values and allowing the budgets to vary across workers within a given city. We first estimate θ by assigning all workers in a given city the same index (or, equivalently, a common budget). We find wage-price elasticity estimates without controls for amenities of 0.70, 0.56, and 0.45 using the low, intermediate, and high standard of living price indices. We next assign budgets to workers based on standard of living similar to Roback (1988). To do this, we assume that workers in the upper third of the within-city income distribution have a high standard of living, workers in the middle third have an intermediate standard of living, and workers in the lower third have a low standard of living. Measuring prices in this manner, we find a wage-price elasticity of 1.00, again absent amenities. This coefficient is very close to Roback's estimate of 0.97, especially considering the replication of how she assigns budgets is not exact. This estimate is likely biased, though, because budgets are allowed to vary within cities, and hence the estimate largely reflects intra-city differences in consumption. Alternatively, one can allow the price level to vary across standards of living, but include standard of living dummies, so that identification comes only from inter-city variation in prices. Such estimation yields a wage-price elasticity of 0.46.[10] These results are quite interesting. When identification comes only from variation in prices across cities, the estimated wage-price elasticity ranges from 0.45 to 0.70, depending on how prices are assigned. When we allow identification from intra-city variation in assigned budgets, however, we get a coefficient equal to one. Thus, it appears that Roback's (1988) estimates are in error. However, the replication of Roback (1988) here does not include amenities and does not account for measurement error in the price index. In subsequent sections of this paper, we estimate the wage-price elasticity using more recent data controlling for amenities and using instrumental variables to account for measurement error.

Henderson (1982) also estimates a variant of equation (6) where he includes the log of housing prices instead of a composite price index. The housing price measure used is the estimated ownership cost for housing for an intermediate budget in the BLS Urban Family Budget Data for Autumn 1977. Other studies have included housing prices in wage equations as well, often as a control variable when the main investigation is something else, but Henderson is one of the few to include housing prices along with amenities in an analysis of interarea wage differentials. Henderson is also one of the few studies in this area to look at after-tax earnings instead of pre-tax earnings. He finds point estimates of 0.17 and 0.21 for the coefficient on log housing prices in alternate specifications that vary in the amenities included. Henderson does not incorporate non-housing prices in his regressions, however, and he measures housing prices by ownership costs, though rents are likely preferable.

Two recent working papers by Albouy (2008b) and Davis and Ortalo-Magné (2008) are also interested in the relationship between wages and prices. Albouy (2008b) attempts to

[10]Similarly, including standard of living dummies and allowing the wage-price elasticity to vary by standard of living yields price coefficients of 0.59, 0.52, and 0.40 for the low, intermediate, and high standard of living groups, though the coefficients are not statistically different from each other at conventional levels.

construct improved quality of life rankings for cities by among other things incorporating non-housing prices and federal income taxes into the rankings. His main finding is that improved quality of life estimates rank large cities more favorably than has been the case using previous methods. He also computes city fixed effects for log housing prices and log wages and regresses the log housing prices on log wages and amenities. The regression yields a coefficient of 1.41. Based on his chosen parameters (for the budget shares of housing and non-housing, etc.), this suggests that his model quite accurately predicts the relationship between housing prices and wages across cities. The empirical work in the current paper differs from that in Albouy (2008b) in at least two important ways. First, Albouy uses combined data on housing values and rents to measure housing prices. The preferred estimates, however, in this paper measure housing prices solely by rents. As shown later, the results in this paper are significantly affected by measuring housing prices by values instead of rents. A second difference between the current paper and Albouy (2008b) is that we estimate a wage-price elasticity, while he estimates a price-wage elasticity. In theory, the two should be multiplicative inverses, *ceteris paribus*, but in practice the two estimates differ in the treatment of non-housing prices. Albouy does not explicitly control for non-housing prices, but instead infers non-housing prices from housing prices.

Davis and Ortalo-Magné (2008) develop a model of the equilibrium relationship between wages and prices across cities that assumes a Cobb-Douglas utility function and therefore that the expenditure share for housing is constant across cities. They test their model by predicting city-specific rental values as a function of wages and comparing predicted rents to observed rents, where quality is held constant for both housing and labor. Davis and Ortalo-Magné predict rents for city j as a function of wages in the city according to the formula, $\widehat{r_j} = \bar{r}\ (\widetilde{w}_j/\overline{w})^{1/\alpha}$, where $\bar{r}$ and $\bar{w}$ are the mean values of rents and household wage income across cities, $\tilde{w}_j$ is household wage income in city j fully adjusted for the price of non-housing goods, and α is the constant expenditure share of housing, which Davis and Ortalo-Magné set equal to 0.24. They find that observed rents are under-dispersed compared to what is predicted by their model, i.e., rents are too low in many high wage areas and too high in many low wage areas. Davis and Ortalo-Magné concede that the omission of amenities from their analysis may adversely affect their results. Measurement error in $\tilde{w}_j$ may also partially explain their findings.[11]

4. Data and Methods

In the empirical section of this paper, we begin by estimating a variant of equation (6) that includes amenities (Z):

$$\ln W_{ij} = X_{ij}\beta + \theta \ln P_j + \gamma Z_j + \varepsilon_{ij}. \tag{7}$$

We use earnings and individual characteristics data from the 2006 Current Population Survey Outgoing Rotation Group (CPS-ORG) files merged with data on prices and amenities from several sources.[12] The sample used consists of all employed wage and salary workers ages 18-61 (inclusive), who are not full-time students. We also exclude all persons with imputed earnings to avoid imputation bias, which would bias θ toward zero (Hirsch and Schumacher 2004; Bollinger and Hirsch 2006).[13] The dependent variable is the log of the hourly wage ($\ln W_{ij}$). We use the reported hourly wage for workers who are paid by the hour and do not receive tips, commissions, or overtime. For workers who are not paid by the hour or who receive tips, commissions or

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[11] This is conceptually similar to measurement error biasing the coefficient on wages in a rent regression toward zero.

[12] Prices and amenities are measured at the city level, where a city is defined as a Core Based Statistical Area or a Combined Statistical Area.

[13] Imputation bias would likely result because imputed earners are often assigned wages of workers in different metropolitan areas or even different regions.

overtime, the hourly wage is computed by dividing usual weekly earnings by the usual number of hours worked per week.

The preferred estimates adjust wages for federal income taxes, but we also estimate equation (7) using pre-tax wages for the sake of comparison. As discussed by Henderson (1982) and Albouy (2008a,b), the progressivity of the federal income tax system causes workers in high wage areas to pay a higher percentage of their income in federal income taxes than workers in relatively low wages areas. The marginal benefit, however, to an individual worker of her federal income tax contributions is zero because workers consume the same level of federal public services regardless of their federal tax payments. In other words, while workers pay higher federal income taxes in high wage areas, they do not receive higher federal benefits. Consequently, when choosing among cities, workers are concerned with the wages they would earn net of federal taxes in each city instead of gross wages.

The present study does not adjust wages for social security contributions or state income tax payments. It would be relatively straightforward to estimate social security contributions for individual workers, but estimating the benefits to workers of their contributions would be more difficult. We could also estimate state income tax payments for workers, but adjusting wages for state income taxes is inappropriate unless we also adjust wages for other state and local taxes because states differ in their reliance on income taxes. Even if we could compute the total burden of all state and local taxes to each worker, we would still need to account for the benefits from state and local expenditures that each worker receives. Given the complexities involved with estimating the net fiscal incidence of social security payments and state taxes, we make no adjustment for them in the dependent variable.[14] Because the dependent variable in this study is the log of the hourly wage, the analysis is only affected by social security payments and state

[14] Hence, use of the term after-tax wages implies wages net of federal income taxes only.

taxes to the extent that their net fiscal incidence is not proportional to wages for homogenous workers in different areas.[15] However, to the extent that the total net burden of social security and state and local taxes and expenditures for homogenous workers is higher (lower) in high wage areas, regression estimates of θ that only account for federal income taxes may overstate (understate) the true value of θ.

Federal income tax liabilities are not reported in the CPS-ORG files, but are instead estimated using the federal tax schedule and based on several assumptions. We assume that all married couples file jointly and receive two personal exemptions and non-married persons have a filing status of single and receive one personal exemption. Itemized deductions are assumed to equal 20 percent of annual earnings, where annual earnings are equal to usual weekly earnings times 48.3 (the average number of weeks worked for workers in the March CPS). Taxpayers take the standard deduction if it is more than their itemized deductions. Deductions and exemptions are subtracted from annual earnings to estimate taxable income. Tax schedules are then used to compute federal tax liabilities. We next compute the average tax rate for each taxpayer (τ_{ij}), and then multiply the hourly wage by one minus the average tax rate to compute after-tax hourly wages ($W_{ij}(1-\tau_{ij})$).

All regressions include a number of individual characteristic variables intended to make workers roughly similar across cities. The individual characteristics included are eleven dummy variables for highest level of education received, a quartic specification for experience, and dummy variables for mutually exclusive race/ethnicity categories (Black, Asian, Hispanic, and other), female, married, employed part-time, enrolled part-time in school (measured for workers

[15] To illustrate, suppose we have an equal rate tax (τ) on wages (W) in all areas. Wages net of the tax are W(1-τ). Because the dependent variable is in logs, note that ln$W$(1-τ)) = ln$W$ + ln(1-τ)). Because τ is a constant, regression results will be equivalent (except for the constant term in the regression) regardless of whether the dependent variable is the log of pre-tax wages (ln$W$) or the log of after-tax wages (ln$W$(1-τ))).

under 25), union member, naturalized citizen, and non-citizen. Additionally, we include nine occupation dummies, eleven industry dummies, and three dummies for whether the worker is a federal, state, or local government employee. We also include 11 month-in-sample dummies.

The baseline price index is constructed using the ACCRA *Cost of Living Index* for 2006. The ACCRA index is produced quarterly based on prices collected by local chambers of commerce for a basket of 57 goods and services meant to be representative of actual consumer expenditures.[16] The prices of the 57 goods and services are then weighted (based somewhat on CES expenditure data) to form a composite price index and six sub-indices for housing, groceries, utilities, transportation, healthcare, and miscellaneous goods and services. However, the baseline price index based solely on ACCRA data may not accurately measure intercity variation in prices. One prominent reason is that ACCRA measures housing prices as a weighted average of the price of two goods: apartment rent and homeowner principal and interest, with homeowner costs being given a much greater weight (.82) than apartment rent (.18). Housing rents measure the price paid per unit of time for the use of housing, and are therefore the ideal measure of housing prices.[17] Homeowner costs may be an inappropriate measure of the user cost of housing because they are based on housing values. Homeownership involves both a consumption decision and an investment decision, and the value of a house is equal to the expected net present value of the income stream it generates. If expected future growth in rents differs across cities and over time, then so will the ratios of rents to housing values. Empirical evidence suggests that this is indeed the case (Clark 1995; Davis, Lehnert and Martin 2008).

[16] While many of the goods in the index might be thought of as traded goods, the law of one price does not strictly hold because most goods are sold at retail. Retailing in San Francisco is more expensive than retailing in Topeka, KS because of higher commercial land rents and higher wages needed to compensate for higher housing rents (and subsequently higher non-housing costs). The spread of online shopping is likely to have important effects in pushing homogenous goods towards a single price, but this is not accounted for under current ACCRA methods.

[17] For this reason, the Consumer Price Index produced by the BLS measures housing prices solely by rents.

Housing values may even be subject to bubbles based on irrational speculation about the growth in future benefits (Case and Schiller 2003). Therefore, measuring housing prices using house values is likely to be inappropriate because house values are not based solely on the present user cost of housing. This may be especially true for recent years given the relatively large increase in housing values, especially in several metropolitan areas with a relatively inelastic supply of housing (Glaeser, Gyourko and Saiz 2008).

Additional difficulties arise with the ACCRA index because prices are not reported for all areas in each year. This has two drawbacks. First, ACCRA often contains no information on prices for a given city, and hence we must exclude the city from the analysis. This limits the analysis to 167 cities, though the cities that remain account for 68 percent of workers in the CPS. A second problem is that prices are reported at the sub-metropolitan level and must be aggregated to produce city-level averages using population weights, yet not all areas within a metropolitan area are necessarily included. To the extent that sub-metropolitan areas for which prices are reported are not representative of areas in the same city for which prices are not reported, the average price level in the city will be measured with error. For a further discussion of issues associated with using the ACCRA index to measure interarea price differences, see Koo, Phillips, and Sigalla (2000).

To address the potential problems that result from using ACCRA data to measure housing prices, we also compute a modified price index that measures housing prices solely by rental costs from the 2006 American Community Survey (ACS).[18] To do this, we use microdata available from the Integrated Public Use Microdata Series (IPUMS) produced and distributed by Ruggles et al. (2008) to estimate quality-adjusted average gross rents for each city in the

[18] The ACCRA *Cost of Living Index* also reports average rents for an area, but for a number of reasons quality-adjusted rents from the ACS are likely preferable to rents from the ACCRA index.

sample.[19] The first step is to regress log gross rents, R, for each housing unit on a vector of housing characteristics, F, and a vector of city-specific fixed effects, α:

$$\ln R_{ij} = F_{ij}\Gamma + \alpha_j + u_{ij}. \tag{8}$$

The housing characteristics included are dummy variables for the number of bedrooms, the total number of rooms, the age of the structure, the number of units in the building, modern plumbing, modern kitchen facilities, and lot size for single-family homes. The results for housing characteristics from this estimation are generally as expected and are reported in Appendix Table A. We then use the estimated parameters to predict average gross rents for each city holding the housing characteristics constant at their mean level for the entire sample.[20] We then divide the quality-adjusted average gross rents for each city by the mean across cities and multiply by 100 to create a housing price index based on quality-adjusted gross rents. We then compute a modified composite price index by taking a weighted average of the rent-based housing price index and non-housing prices from ACCRA, where housing prices are given a weight of 0.29 and non-housing prices are a given a weight of 0.71.[21] Weights are chosen based on calculations from the 2005 Consumer Expenditure Survey suggesting that housing (based on gross rents) represents 29 percent of average consumption expenditures.[22]

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[19] Gross rents include rents as well as basic utilities (water, electricity, and gas) and home heating fuels (wood, kerosene, oil, coal, etc.). These utilities are often included in rental payments for some renters, but not for others. Therefore, gross rents are more comparable across households because they include utilities and fuels for all renter households.

[20] If, however, there are unobserved aspects of housing quality that are correlated with wages in a city, the estimated wage-price elasticity may be upwardly biased.

[21] For these purposes, non-housing prices are computed as a weighted average of ACCRA sub-indices for groceries (0.13), transportation (0.25), healthcare (0.06), and miscellaneous goods and services (0.56). Note, that this excludes utilities in addition to housing because utilities are largely already included in gross rents.

[22] Note that this expenditure share for housing differs from official reports of the CES expenditure share for both "Housing" and "Shelter." The housing share based on gross rents used herein includes certain utilities but excludes others and also excludes expenditures for household operations, housekeeping, and household furnishings. The housing share of 0.29 also differs from the official CES tabulations in that homeowner housing expenditures are measured by implicit rents and not by out-of-pocket expenses such as mortgage interest.

For the sake of comparison, we also compute a modified price index that measures housing prices by quality-adjusted housing values from the 2006 ACS computed in a manner similar to quality-adjusted gross rents. For this second modified price index, housing prices are given a weight of 0.23 because values do not include utilities and non-housing prices (now including utilities) are given a weight of 0.77.

Summary statistics for several price variables are reported in Table 1. As seen, the modified price index using gross rents is considerably less dispersed than both the baseline price index and the modified price index using housing values. Equivalently, housing values are more dispersed across cities than are gross rents. Non-housing prices are much less dispersed across cities than both rents and values, but there is still considerable variation in non-housing prices. Appendix Table B lists the 167 cities included in the sample and their value for each of the three composite price indices.

Table 1: Summary Statistics for Price Indices, 2006

	Min.	Max.	St. Dev.
Baseline Price Index	84.0	157.9	12.0
Rent-based Modified Price Index	84.1	141.8	9.2
Housing Value-based Modified Price Index	80.3	184.8	15.7
Quality-Adjusted Gross Rents	66.4	184.4	20.0
Quality-Adjusted Housing Values	46.9	395.0	52.9
Non-housing Prices	86.7	124.4	5.7

Notes: Un-weighted mean is normalized to 100. Standard Deviation is un-weighted. Includes 167 cities.

In addition to estimating equation (7), we are also interested in the relationship between wages and the prices of housing and non-housing goods and services. Therefore, we also divide the price index into housing prices, P_1, and non-housing prices, P_2, and include them in logarithmic form in the log wage equation separately:

$$\ln W_{ij} = X_{ij}\beta + \theta_1 \ln P_{1j} + \theta_2 \ln P_{2j} + \gamma Z_j + \varepsilon_{ij}. \tag{9}$$

Examining housing prices separately from non-housing prices is interesting for several reasons. For one, it allows us to test if the prediction of equation (4) holds for housing and non-housing prices separately. Additionally, a large literature in urban and regional economics following Roback (1982) ranks the quality of life across cities using implicit prices of amenities computed as the sum of compensating differentials in housing and labor markets, $dP_1/dZ - dW/Z$. Few of these studies incorporate non-housing prices (Gabriel et al. 2003; Shapiro 2006; Albouy 2008b are recent exceptions). The justification for this exclusion is often that non-housing prices are relatively unimportant (Beeson and Eberts 1989). The non-trivial variation in non-housing prices illustrated in Table 1 combined with the large budget share for non-housing consumption, however, suggests that non-housing prices may be quite important. The few papers that do incorporate non-housing prices often do so in a less than ideal way.[23] Separating housing and non-housing prices allows us to examine the importance of each in explaining interarea wage differentials.

Theory and previous empirical evidence predict that amenities also affect both wages and local prices. Therefore, the regressions also include a number of different amenities from several sources found to be important in previous literature.[24] A list of variables and data sources is included in Appendix Table C. Without including amenities, the estimated relationship between

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[23] For example, both Shapiro (2006) and Albouy (2008b) infer non-housing prices from housing prices by regressing non-housing prices on housing prices using the ACCRA *Cost of Living Index*. However, their approach ignores differences in non-housing prices across cities that are not correlated with housing prices. The analysis in this dissertation suggests that regressing non-housing prices on division dummies, city size dummies, and amenities in addition to housing prices does a much better job of predicting non-housing prices than housing prices alone.
[24] Many of these are reported at the sub-metropolitan level and had to be aggregated to the CBSA/CSA level using populations as weights.

wages and prices could be biased.[25] Data for several natural amenities are obtained from the USDA Economic Research Service. These include the mean January temperature in degrees Fahrenheit, mean July temperature, mean hours of January sunlight, mean July relative humidity, the percent of land area covered by water, and five indicator variables for topography that range from very flat to mountainous. The flattest land surface is the omitted reference group. Mean annual inches of precipitation and snow are obtained from *Cities Ranked and Rated, 2nd Edition*. Maps were consulted to create indicator variables for whether a city is located on the coast of the Atlantic Ocean, Pacific Ocean or Gulf of Mexico. Data on violent crime and property crime per capita were obtained from the Census Bureau's USA Counties website.[26] The mean commuting time in minutes for workers in a city was computed using the 2006 ACS microdata. Two measures of air pollution, ozone and particulate matter 2.5, were computed using the EPA AirData database.[27] The regressions also include eight census division dummies and six city population size dummies to account for residual differences in amenities.[28] The city size dummies should also help control for differences in unobserved worker ability across cities.[29] No specification of amenities is likely to fully capture differences in the quality of life across cities, but the hope is that the variables used in this paper do a reasonably good job of controlling for differences in the quality of life across cities.

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[25] For example, a pure consumption amenity is likely to drive up housing prices and drive down wages, which would bias the wage-price elasticity toward zero.

[26] http://censtats.census.gov/usa/usa.shtml.

[27] Pollution values were unavailable for several small cities and were imputed based on average values by Census division and city size. Particulate matter was imputed in this manner for 16 cities, and ozone was imputed for 23 cities. We tested the potential effect of this imputation by estimating the regressions without pollution variables and estimating the regressions with pollution variables but only for cities that had unimputed pollution levels. The main results of this paper do not appear to be affected by the imputation of pollution values for these small cities.

[28] The seven city size categories are: 0-199,999; 200,000-299,999; 300,000-499,999; 500,000-999,999; 1,000,000-1,999,999; 2,000,000-4,999,999; and 5,000,000+.

[29] Glaeser and Maré (2001), Yankow (2006), and Krupka (2008) all find that the nominal city size wage premium falls after controlling for individual fixed effects using panel data on workers, suggesting that large cities attract more able workers.

5. Empirical Results: The Elasticity between Wages and the General Price Level

This section presents results of the elasticity between wages and the general price level using the baseline price index, the price index modified using quality-adjusted gross rents, and the price index modified using quality-adjusted house values. All regressions include the full list of amenities, division dummies, city size dummies, and individual characteristics as explanatory variables. The results for these variables were generally as expected. Full results for the preferred specification are provided in Appendix Table D.[30] We begin by estimating the regressions using Ordinary Least Squares and then proceed to instrument for prices to account for measurement error, which would bias the estimated coefficients toward zero. All of the price index coefficients in this section are statistically different from zero at the 1% level using cluster robust standard errors, but the more appropriate null hypothesis is whether or not they are different from unity.[31]

Ordinary Least Squares

We first estimate the wage-price elasticity, θ, using the baseline price index via OLS. This specification is comparable to that of DuMond et al. (1999), but the equation herein contains many more amenities, more recent data, and uses after-tax wages as the dependent variable.[32] As seen in the first column of Table 2, this specification yields an estimate of θ of 0.314, and the coefficient is statistically different from one at the 1% level. According to this estimate, a one percent increase in the general price level in a city is associated with a 0.31 percent increase in after-tax wages. This is also considerably lower than the previous estimate of 0.46 by DuMond et al. (1999). This may suggest that the sharp increase in housing values in

[30] As discussed in more detail later, the preferred specification is to measure prices by the log of the rent-based price index and instrument using log gross rents from the previous year.
[31] Unless otherwise noted, all standard errors in this paper are robust clustered by city.
[32] A more subtle difference is that DuMond et al. (1999) include workers with imputed earnings, which likely biases their estimates toward zero.

recent years causes the ACCRA index to be a worse measure of the cost of living in 2006 than it was between 1985 and 1995, the time period considered by DuMond et al. (1999).

Table 2: OLS Results for Three Price Indices

	1	2	3
Log Baseline Index	0.314[c]		
	(0.048)		
Log Rent-Based Modified Index		0.760[c]	
		(0.078)	
Log Value-Based Modified Index			0.416[c]
			(0.049)
R^2	0.494	0.495	0.494

Notes: Dependent variable is the log of hourly wages net of federal income taxes computed from the 2006 CPS-ORG files. Standard errors in parentheses are robust, clustered by CSA/CBSA. Regressions contain observations on 71,705 workers in 167 cities. Regressions also include 8 Census division dummies, 6 city size dummies, January temperature, July temperature, January sun, July humidity, the % of land area covered by water, 4 indicators for topography, 3 indicators for coastal location, precipitation, snow, violent crime, property crime, ozone, particulate matter (2.5), mean commute time, 11 education dummies, a quartic specification for experience, dummy variables for whether a worker is female, Black, Asian, Hispanic, Other, married, employed part-time, enrolled part-time in school, a member of a union, a naturalized citizen, or a non-citizen, 9 occupation dummies, 11 industry dummies, 3 dummies for government employment, and 11 month in sample dummies. The Baseline Index refers to the price index constructed solely using ACCRA data. The two modified indices combine housing prices from the Census with non-housing prices from ACCRA. See text for further details.
[c] Significantly different from unity at the 1% level.

The baseline index likely does a poor job of measuring differences in prices across cities in part because it measures housing prices primarily by house values instead of rents. Therefore, the rent-based modified price index, which measures housing prices solely by gross rents from the ACS is likely more appropriate. Using the rent-based price index, OLS yields an estimated wage-price elasticity of 0.760, much higher than for the baseline price index. This is an important result. It appears that the wage-price elasticity using the baseline price index is biased toward zero in part because of how housing prices are measured. However, this estimate for the rent-based index is still significantly less than one.

We also estimate θ using the housing value-based modified price index. Using OLS, the estimated coefficient is 0.416 and is significantly less than one. Interestingly, the coefficient for the value-based modified index is greater than that for the baseline index. This suggests that measuring housing prices by values may not be the only source of measurement error in the baseline index.[33]

Instrumental Variables

Even after measuring housing prices by quality-adjusted gross rents from the ACS, the price index may still be measured with considerable error. Housing prices as measured are likely subject to some degree of sampling error and non-housing prices measured in the ACCRA *Cost of Living* Index may be subject to a number of sources of measurement error. Random measurement error will bias the coefficient on the log price index toward zero, and including variables that are highly correlated with the price index such as amenities, division dummies, and city size dummies, may exacerbate measurement error bias. We next use instrumental variables to account for measurement error in the price indices. We use as instruments the lagged housing and non-housing components of the individual price indices. If measurement error is random, then instrumenting for the price index using the previous year's components should produce consistent estimates of θ. If measurement error in the price index is serially correlated, however, instrumenting using lagged prices will not produce consistent coefficient estimates. Tables 3, 4,

[33] It may also be the case that housing values are measured with greater error than rents and this leads to greater measurement error bias for the value-based modified index than the rent-based index. Bucks and Pence (2006), however, report that homeowner reported housing values are fairly accurate.

and 5 present 2SLS results for the baseline price index, the rent-based modified index, and the value-based modified index, respectively. All instruments used are highly significant in the first stage regressions, which are reported in the lower half of each table.

Table 3: 2SLS Results for the Baseline Index

	1	2	3
<u>Second-Stage Results</u>			
Log Baseline Index, 2006	0.317[c]	0.437[c]	0.333[c]
	(0.051)	(0.091)	(0.050)
R^2	0.495	0.495	0.495
<u>First-Stage Results</u>			
Log Housing Price Index, 2005	0.395***		0.353***
	(0.015)		(0.021)
Log Non-Housing Index, 2005		1.211***	0.368***
		(0.159)	(0.097)
Partial R^2 of Excluded Instruments	0.837	0.370	0.862

Notes: Regressions contain observations on 69,743 workers in 157 cities. The dependent variable and additional regressors are the same as in Table 2. Standard errors in parentheses are robust, clustered by CSA/CBSA.
[c] Significantly different from unity at the 1% level. *** Significantly different from zero at the 1% level in the first-stage regressions.

We first instrument for the baseline price index using the housing price index constructed from the ACCRA *Cost of Living Index* for 2005.[34] The results are reported in the first column of Table 3. First, note that the housing price index from the previous year can explain a substantial portion of the variation in the baseline index as illustrated by the very high partial R^2 of 0.837 for

[34] Of the 167 cities in the sample, only 157 are included in the ACCRA *Cost of Living Index* for 2005. The other 10 cities are not included in 2SLS estimates when prices from ACCRA are used as an instrument. Appendix Table A1 indicates the 10 cities without ACCRA information for 2005.

the first-stage regression. The coefficient of the log of the baseline price index in the log wage equation, however, is virtually identical to the OLS result. We next instrument for the baseline index using non-housing prices for the previous year. As seen, lagged non-housing prices explain less of the variation in the baseline index (though still a considerable amount), but the second-stage coefficient does increase somewhat to 0.437. In the third column of Table 3, we include lagged housing and non-housing prices together as instruments yielding a coefficient of 0.333 in the second-stage regression. Thus regardless of the instrument(s) used, the estimated elasticity between wages and prices is still considerably less than one using the baseline price index.

Table 4: 2SLS Results for the Rent-Based Modified Index

	1	2	3
Second-Stage Results			
Log Rent-Based Modified Index, 2006	**0.994**	0.603[c]	0.830[a]
	(0.106)	(0.108)	(0.091)
R^2	**0.494**	0.495	0.495
First-Stage Results			
Log Gross Rents, 2005	0.377***		0.290***
	(0.024)		(0.017)
Log Non-Housing Index, 2005		0.878***	0.594***
		(0.077)	(0.047)
Partial R^2 of Excluded Instruments	0.657	0.526	0.859

Notes: Regression in column 1 contains observations on 71,705 workers in 167 cities, while regressions in columns 2 and 3 contain observations on 69,743 workers in 157 cities. The dependent variable and additional regressors are the same as in Table 2. Standard errors in parentheses are robust, clustered by CSA/CBSA.
[a] Significantly different from unity at the 10% level. [c] Significantly different from unity at the 1% level. *** Significantly different from zero at the 1% level in the first-stage regressions.

We next use 2SLS for the preferred measure of prices, the rent-based modified price index. While the OLS coefficient estimate for the rent-based price index is much closer to unity than the baseline index, it is still significantly less than one. If random measurement error is driving the coefficient away from one, then instrumenting may produce consistent estimates. We first instrument for the log of the rent-based modified price index using quality-adjusted log gross rents from the previous year. As reported in the first column of Table 4, instrumenting in this manner yields a coefficient estimate of 0.994 that is nearly identical to one. Therefore, instrumenting for the rent-based price index using rents for the previous year provides empirical support for the full compensation hypothesis. We next instrument for the rent-based modified index using non-housing prices for the previous year. The 2SLS coefficient estimate in this case, 0.603, is considerably lower than that found using OLS. Finally, when we use both gross rents and non-housing prices as instruments for the rent-based price index, we get a coefficient estimate of 0.830 that is statistically different from unity at the 10% level.

Non-housing prices are constructed from the ACCRA *Cost of Living Index* and are likely subject to considerable measurement error, some of which is likely persistent within cities over time. If measurement error in non-housing prices is serially correlated, then instrumenting for the general price level using non-housing prices will not yield consistent estimates of θ. The divergence between the estimates in the first and second columns of Table 4 suggests that this is indeed the case. Quality-adjusted gross rents are estimated from the ACS PUMS and may also be subject to some measurement error such as due to sampling. However, the measurement error in log gross rents is much more likely to be classical in nature. If the measurement error in the lag of log gross rents is purely random and uncorrelated with measurement error in the rent-based price index, then the 2SLS estimates in the first column of Table 4 are consistent. This seems quite plausible. If log gross rents are a valid instrument, over-identification in the specification of the third column allows us to examine the validity of non-housing prices as an instrument. Doing so, we get a Hansen J Statistic of 11.297, which allows us to reject non-housing prices as a valid instrument at the 1% level. Thus the coefficient in the first column of Table 4 is the preferred estimate of the elasticity between wages and the general price level.

Table 5: 2SLS Results for the Value-Based Modified Index

	1	2	3
<u>Second-Stage Results</u>			
Log Value-Based Modified Index, 2006	0.478[c]	0.395[c]	0.447[c]
	(0.059)	(0.071)	(0.052)
R^2	0.494	0.495	0.495
<u>First-Stage Results</u>			
Log Housing Values, 2005	0.347***		0.294***
	(0.015)		(0.017)
Log Non-Housing Index, 2005		1.340***	0.552***
		(0.138)	(0.077)
Partial R^2 of Excluded Instruments	0.835	0.481	0.899

Notes: Regression in column 1 contains observations on 71,705 workers in 167 cities, while regressions in columns 2 and 3 contain observations on 69,743 workers in 157 cities. The dependent variable and additional regressors are the same as in Table 2. Standard errors in parentheses are robust, clustered by CSA/CBSA.
[c] Significantly different from unity at the 1% level. *** Significantly different from zero at the 1% level in the first-stage regressions.

For the sake of comparison, we also estimate the wage-price elasticity for the value-based modified price index using 2SLS. The results are presented in Table 5. In all three columns, the coefficient on the value-based price index is considerably less than one, again suggesting that measuring housing prices by housing values is inappropriate. Interestingly, though, the estimates in the first and third columns are a little higher than the corresponding estimates for the baseline index in Table 3, while the estimate in the second column that instruments for the general price level using non-housing prices is lower than in Table 3.

A recap of the results in this section is warranted. Theory and intuition predict that the elasticity between wages and the general price level should be close to one. When housing prices are measured by homeowner values, the estimated elasticity between wages and the general price level is never more than 0.5, even when we use instrumental variables to account for measurement error. When housing prices are measured by rents, though, the estimated elasticity between wages and the general price level increases considerably. Using OLS the estimated wage-price elasticity is still less than one, but instrumenting for the log of the rent-based modified price index using the log of quality-adjusted gross rents for the previous year, the wage-price elasticity is equal to one for all practical purposes. This result supports the full compensation hypothesis and has important implications for researchers estimating the implicit prices of amenities. In the next section, we examine the sensitivity of θ to alternative specifications.

6. The Elasticity between Wages and the General Price Level for Alternative Specifications

In this section, we briefly examine the sensitivity of the 2SLS wage-price elasticity estimates using the rent-based price index to alternative specifications and samples. The results prove to be quite robust. The first row of Table 6 reproduces estimates for the preferred specification from the first column of Table 4 in which we instrument for the log of the rent-based price index using log gross rents from the previous year. The remainder of Tables 6, 7, and 8 present results for alternative specifications and samples instrumenting for the log of the rent-based priced index using log gross rents.

Table 6: 2SLS Results for the Rent-Based Index under Alternative Specifications

	Coefficient	Standard Error
(1) Preferred Specification	**0.994**	**0.106**
(2) Including Imputed Earners	0.697[c]	0.072
(3) Pre-Tax Wages	1.062	0.114
(4) Including State Fixed Effects	0.949	0.110
(5) Renters Only	1.037	0.128
(6) Homeowners Only	1.011	0.122
(7) 1990 Data	0.967	0.111

Notes: Results in rows 1-6 are from 2SLS regressions for the log of the rent-based price index using log gross rents for 2005 as an instrument. Results in row 7 are from 2SLS regressions using 1990 data with log gross rents in 1990 as an instrument. Standard errors in parentheses are robust, clustered by CSA/CBSA. See text for further details.
[c] Significantly different from unity at the 1% level.

Including Individuals with Imputed Earnings

The analysis thus far has excluded workers with imputed earnings to avoid imputation bias. Including imputed earners would likely result in imputation bias because imputed earners in the CPS are often assigned wages of workers in different metropolitan areas or even different regions (Hirsch and Schumacher 2004). The second row of Table 6 reports results of the wage-price elasticity estimated with workers with imputed earnings included in the sample. Including imputed earners increases the sample size to 108,597 meaning that 34 percent of the workers in this specification have imputed earnings. The effect of imputation bias on the wage-price elasticity is quite severe. With imputed earners included, the wage-price elasticity falls to 0.697 and is statistically different from unity at the one percent level. The attenuation in the coefficient (30 percent) is nearly one to one with the percent of workers with imputed earnings. This reaffirms the initial decision to exclude workers with imputed earnings as suggested by recent literature.

Pre-tax Wages

In the third row of Table 6, we estimate θ via 2SLS using pre-tax wages as the dependent variable. As pointed out by Henderson (1982) and Albouy (2008a,b), the progressivity of the federal income tax causes workers in cities with high nominal wages to pay a higher percentage of their income in federal income taxes than workers in cities with lower nominal wages. For the utility of homogenous workers to be constant across areas, pre-tax wages should be more dispersed across areas than after-tax wages. In other words, workers in high wage areas must be compensated for the higher federal income taxes they pay in addition to the compensation they require for the higher cost of living or worse bundle of amenities. As such, the estimated wage-price elasticity should be higher using pre-tax wages than using after-tax wages. The results in row 3 suggest that this is likely the case. The estimate of θ increases to 1.062, but is not statistically different from unity. We maintain, however, that it is after-tax wages that should equalize across areas controlling for prices, amenities, and individual characteristics, so measuring wages net of federal income tax provides a better test of the theory than measuring wages before federal income tax.

State Fixed Effects

If wages should be measured net of federal income tax, we might also consider adjusting wages for state and local income taxes. Income taxes, however, are only part of the story at the states and local level. To adjust wages for state and local income taxes, we would also need to incorporate information on other state and local taxes and state and local public spending. To avoid the many complexities involved with adjusting wages for state and local taxes and expenditures, we adopt a different approach by examining the robustness of the results to including state fixed effects. If ignoring state taxes and expenditures is biasing the previous results, then we would expect that including state fixed effects would produce a very different estimate of θ than the case in which we include census division fixed effects. As seen in row 4, including state fixed effects reduces the coefficient estimate to 0.949, but it is not statistically different from one. Therefore, the basic findings of this paper are robust to including state fixed effects. The preferred specification, however, is to use census division dummies and not state fixed effects because several states contain only one city in the sample. Including state fixed effects means that θ is only estimated based on states that have more than one city in the sample.

Renters vs. Homeowners

One might also wonder if using a rent-based price index yields different estimates of θ for renters and homeowners. In particular, one might be concerned that the rent-based price index does not accurately measure the prices faced by homeowners and could cause θ estimates for the two groups to diverge. Rows 5 and 6 of Table 6 estimate θ separately for renters and homeowners. The coefficient estimate for renters is 1.037, and the estimate for homeowners is 1.011. Therefore, the coefficient estimates for renters and homeowners separately are slightly higher than the pooled estimate, but neither estimate is statistically different from unity. The coefficient estimates for renters and homeowners are also not statistically different from each other. It appears that differences in prices across cities affect the wages of renters and homeowners roughly the same.

An Earlier Period: 1990

To ensure that the results are not being driven by the particular year chosen, we next reestimate the wage-price elasticity using 1990 data from the CPS, the ACCRA *Cost of Living*

Index, Census microdata, and the various sources of information on amenities, constructing prices and amenities as before.[35] The 1990 sample contains 84,117 observations in 155 cities. The preferred instrument would be to use log gross rents from the previous year, but unfortunately the most recent prior year would be 1980. Instead of using housing prices from such a distant earlier period, we use log gross rents from 1990. If measurement error in the log of the rent-based price index is correlated with measurement error in log gross rents, coefficient estimates from this procedure will be downwardly biased. However, because gross rents from the Census are expected to be measured with relatively little error, downward bias due to measurement error is likely to be minimal. Row 7 of Table 6 presents the wage-price elasticity estimate using data for 1990. The estimate is 0.967 and not statistically different from unity. Thus it appears that the finding of a wage-price elasticity of unity for the rent-based price index is robust to this earlier period chosen.[36]

By Educational Attainment

We may also be concerned that using a single price index ignores differences in prices within cities for different standards of living. Workers do not care about the average price level in a city, but instead care about the prices of goods that they consume. If the relative cost of living in a city varies significantly across different types of workers, estimating θ using a single price index for all workers may be inappropriate. This concern motivated Roback (1988) to use separate price indices for workers with different standards of living.

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[35] One subtle difference in the price index is that the shares of housing and non-housing goods in the rent-based price index are different than in 2006. While gross rents represented 29 percent of total household expenditures in the 2005 CES, the share was only 24 percent for the 1990 CES. Therefore, the 1990 rent-based price index gives housing prices based on Census microdata a weight of 0.24 and non-housing prices from ACCRA a weight of 0.76.

[36] We also estimate the wage-price elasticity for 1990 using a value-based modified index similar to that used above. Using 2SLS with log housing values as an instrument, we find a wage-price elasticity for this price index of 0.579 that is statistically different from one. While this estimate is higher than the corresponding estimate of 0.478 in the first column of Table 5 and would likely be even higher in the absence of measurement error, it is still quite a bit less than unity. This again reaffirms the earlier finding that housing values are an inappropriate measure of the price of housing consumption.

To explore the importance of price differences across standards of living, we form separate rent-based price indices for four educational groups: high school dropouts, high school graduates, persons with some college, and persons with a bachelor's degree or higher. Non-housing prices are taken to be the same for all four groups, but housing prices are allowed to vary by income group in a similar method to that used by Moretti (2008). First, the rent regression of equation (8) is estimated separately for each education group. The estimated parameters are then used to predict quality-adjusted average gross rents for each city and education group holding the housing characteristics constant at their mean level for the education group. Quality-adjusted gross rents for each education level are then combined with non-housing prices to form separate rent-based modified price indices for the four education groups with non-housing prices given a weight of 0.71 and housing prices given a weight of 0.29 as before.

Because there are now four different price indices, we estimate the wage-price elasticity via equation (7) separately for each education group using their education-specific price index. Before doing so, however, it is useful to first estimate the wage-price elasticity separately for each education group using the rent-based index used above that does not vary by education. In other words, we wish to see if the wage-price elasticity varies by education group using the same rent-based price index for all education groups. To do so, we instrument for the log of the general price level using log gross rents from the previous year. The results in the upper panel of Table 7 suggest that the wage-price elasticity does indeed vary by education group and is decreasing with education. DuMond et al. (1999) also find the wage-price elasticity to be generally decreasing with education, though their estimates were less than unity for all four

Table 7: 2SLS Results for the Rent-Based Index by Education Group

	1	2	3	4
	High School Dropout	High School Graduate	Some College	College Graduate
Log Rent-Based Modified Index, 2006	1.439[c]	1.157	0.946	0.673[b]
	(0.156)	(0.129)	(0.141)	(0.150)
Education Group-Specific Log Rent-Based Modified Index, 2006	1.426[b]	1.192	1.038	0.742[a]
	(0.184)	(0.142)	(0.158)	(0.140)
Log Rent-Based Modified Index, 2006 Omitting Mean Commute Time	1.320[b]	1.017	1.005	0.939
	(0.136)	(0.122)	(0.117)	(0.123)
Observations	6,595	19,018	20,236	25,856

Notes: Standard errors in parentheses are robust, clustered by CSA/CBSA.
[a] Significantly different from unity at the 10% level. [b] Significantly different from unity at the 5% level. [c] Significantly different from unity at the 1% level.

education groups.[37] Instrumenting for the log of the general price level using log gross rents from the previous year, we estimate θ for high school dropouts, high school graduates, those with some college, and college graduates to equal 1.44, 1.16, 0.95, and 0.67, respectively. The estimates for high school graduates and those with some college are not statistically different from one, but the estimates for high school dropouts and college graduates are statistically different from one. These estimates suggest that workers with low education are overcompensated for difference in prices across cities, but workers with college degrees are undercompensated for differences in prices. DuMond et al. suggest that θ may decrease with

[37] DuMond et al. (1999) estimate θ for the four education groups to equal 0.49, 0.45, 0.48, and 0.39.

education because educated workers are better able to shift away from housing in areas where housing is relatively expensive. Similarly, we might also expect θ to decrease with education if housing is a more important share of consumption for less educated workers.[38]

It could also be the case that using average prices for all workers adversely affects the estimates. Thus, using the education-specific price indices could yield estimates close to unity for all groups. When we estimate equation (7) separately for each education group using the education-specific price indices, however, the results are not significantly affected as seen in the middle portion of Table 7. The estimates of θ for the four education groups are 1.43, 1.19, 1.04, and 0.74. In other words, while there may be differences in the wage-price elasticity by education group, differences in the price of housing by education group do not appear to be an important part of the explanation.

The estimated wage-price elasticity may also differ across education groups because coefficient estimates for other variables vary by education group. Examining the coefficient estimates for the other variables, one sticks out as particularly troubling. According to the estimates, mean commute time has a very different effect on the wages of workers in different education groups. For the regressions in the upper portion of Table 7, the coefficients for mean commute time are -0.52, -0.64, 0.27, and 1.12, though only the estimates for high school graduates and college graduates are statistically different from zero.[39] Our expectation is that longer commutes in a city should be a disamenity and we expect commute time to have a positive sign. However, collinearity between the price index and commute time may be

[38] This would result because understating housing's share of consumption would cause the composite price index to be under-dispersed. Under-dispersion in the price index would cause the estimated wage-price elasticity to be too large.
[39] The coefficient for mean commute time in the preferred specification in the first column of Table 4 is 0.12 and not statistically different from zero.

influencing the estimates of both. As the coefficient for commute time increases, the coefficient for the price index decreases and vice versa.

Therefore, the coefficient on the rent-based price index may differ across education groups in part because of the differing coefficient on commute time. To explore this further, we estimate the wage-price elasticity using the single rent-based price index separately for each education group excluding commute time from the regression equation.[40] The results are presented in the lower portion of Table 7. Excluding commute time, the education-group specific estimates of θ become 1.32, 1.02, 1.00, and 0.94 with only the estimates for high school dropouts being statistically different from one. Thus while high school dropouts may be more than fully compensated for prices using the price index as constructed, those with a high school degree or higher appear to be fully compensated for differences in prices across areas.

Spatial Autocorrelation

Another potential concern with the preferred specification in the first column of Table 4 is that it does not account for potential spatial autocorrelation. The spatial equilibrium hypothesis, however, predicts that the utility of homogeneous workers will be equal across all areas. In other words, the spatial equilibrium hypothesis suggests that if we properly control for prices and amenities, wages should not be spatially dependent because workers will relocate to arbitrage utility differences across space. If, however, we have not properly controlled for prices and amenities, wages may be spatially auto-correlated because of similarities in these variables in nearby areas. Similarly, if prices and amenities are misspecified, the wage equation error term in a city may be spatially correlated with the error terms of nearby cities. Failing to account for spatial correlation in the dependent variable could cause regression coefficients to be

[40] Excluding mean commute time from the preferred specification in the first column of Table 4 results in a wage-price elasticity of 1.02 that is not statistically different from zero.

inconsistently estimated and failing to account for spatial error correlation could lead to inconsistently estimated standard errors (Anselin, 1988).

In this subsection we consider the sensitivity of the wage-price elasticity to allowing for spatial correlation in the dependent variable and in the error term. To do so, we first estimate quality-adjusted average log wages for each city in the sample by regressing log wages on worker characteristics (X) and city fixed effects (δ):

$$\ln W_{ij} = X_{ij}\beta + \delta_j + \varepsilon_{ij}. \tag{10}$$

The city fixed effects are then used as the dependent variable in the spatial equations. In matrix notation, the general spatial model can be represented by:

$$\delta = \rho M\delta + \theta \ln P + \gamma Z + \omega \tag{11}$$

$$\omega = \lambda M\omega + \zeta,$$

where M is an $n \times n$ weighting matrix that specifies the structure of the spatial correlation, ρ and λ are spatial autocorrelation coefficients for the dependent variable and the error term, respectively, and ζ is a mean zero error term that is i.i.d. across observations. The model in equation (11) allows for spatial correlation in both the dependent variable and in the error term, but we could restrict the spatial structure to allow for spatial correlation in only one of the two. We can set $\lambda = 0$ and estimate what is commonly referred to as the spatial lag model. Alternatively, we can set $\rho = 0$ and estimate what is commonly referred to as the spatial error model. For the sake of completeness, we estimate the spatial lag model, the spatial error model, and the general spatial model that allows for spatial correlation in both the dependent variable and the error term.

For the results reported in this paper, we specify M as a row-standardized “contiguity” matrix by defining all metropolitan areas within 400 miles of each other as neighbors, though the results are qualitatively robust to several alternative specifications.[41] In other words, M places equal weight on all metropolitan areas within 400 miles of metropolitan area j with the diagonal elements of M all equal to zero to prevent j from being its own neighbor. Because the rows of M are standardized to sum to one, $M\delta$ is simply a vector of the average of wage fixed effects in nearby cities. Equation (11) hypothesizes that wages are simultaneously determined (wages in city *j* affect wages in city i, and wages in i also affect wages in *j*), so using OLS to estimate the spatial model is inappropriate. The present paper estimates the spatial models by the Generalized Method of Moments (GMM) estimator developed by Kelejian and Prucha (1998) using the Spatial Econometrics Toolbox for MATLAB developed by James LeSage (1999). The GMM estimator instruments for wages in nearby cities using the averages of the other explanatory variables (prices and amenities) in nearby cities.

Table 8: 2SLS Results for the Rent-Based Index with Spatial Lag and Spatial Error

	1	2	3	4
Log Rent-Based Modified Index, 2006	1.062	1.038	1.042	1.039
	(0.138)	(0.139)	(0.124)	(0.140)
Spatial Lag (ρ)		0.346		0.209
		(0.233)		(0.274)
Spatial Error (λ)			0.462	0.471
			(0.514)	(0.763)

Notes: The dependent variable is the quality-adjusted average log wages for each city for 2006. See the text for further details.

[41] The current paper chooses a cutoff of 400 miles in part to minimize the average number of neighbors while ensuring that all cities have at least one neighbor. The distance cutoff of 400 miles was also recently used by McMillen, Singell, and Waddell (2007) in examining the spatial dependence in college tuition setting on the grounds that it approximates the distance of a 1-day drive. Again, the results are qualitatively robust to increasing the cutoff beyond 400 miles.

The results are presented in Table 8. The first column reports 2SLS results of (11) assuming that both $\lambda = 0$ and $\rho = 0$.[42] This specification essentially differs from the preferred specification in the first column of Table 4 only in that the city is the level of observation and each city is given equal weight in Table 8, while in Table 4 the worker was the level of observation and larger cities were given more weight because they had more workers. The coefficient on the log of the rent-based price index of 1.062 in the first column is slightly larger than one, but the difference is not statistically significant. In the second column, we estimate the spatial lag model that allows for spatial correlation in the dependent variable, but not in the error term. The wage-price elasticity is 1.038 and not statistically different from unity, and the spatial lag term is not statistically different from zero. In the third column, we estimate the spatial error model, which allows for spatial correlation in the errors but not in the dependent variable. The wage-price elasticity is 1.042 and not statistically different from one and the spatial error coefficient is not statistically different from zero. In the fourth column, we estimate the general spatial model that allows for spatial error correlation in both the dependent variable and the error term. The wage-price elasticity is 1.039 and not statistically different from one, and the coefficients for the spatial lag and spatial error are again not statistically different from zero. The results in Table 8, therefore, suggest that wages are not correlated across cities after controlling for prices and amenities. Consequently, the wage-price elasticity estimate of about unity is robust to controlling for the possibility of spatial autocorrelation.

7. Empirical Results: The Elasticity between Wages and Housing and Non-Housing Prices

We next separate the price index into housing and non-housing prices and include them in the wage equation separately. Equation (4) predicts that the wage-price elasticity for a good

[42] Throughout this subsection the log of the rent-based price index is instrumented using log gross rents from 2005.

should be approximately equal to the share of wage income spent on the good. We wish to explore the validity of this hypothesis for both housing and non-housing prices. Assuming after tax wage income is roughly equal to consumption, the expenditure share for a good should be roughly equal to the share of wage income spent on it. Therefore, using expenditure shares computed from the 2005 CES, the expected coefficient for housing is about 0.29 and the expected coefficient for non-housing is roughly 0.71.

Table 9: Separating Housing Prices (Rents) and Non-housing Prices

	1	2	3
	OLS	2SLS	2SLS
Full Adjustment for:	N/A	Non-housing Prices	Housing Prices
Log Gross Rents	0.337	0.297	
	(0.038)	(0.042)	
Log Non-housing Price Index	0.231[c]		0.754
	(0.106)		(0.289)
R^2	0.495	0.483	0.482
First-Stage Results			
Log Gross Rents, 2005		0.934***	0.138***
		(0.032)	(0.029)
Partial R^2 of Excluded Instruments		0.863	0.141

Notes: The dependent variable in column 1 is the log of after-tax hourly wages. In column 2 wages are fully adjusted for non-housing prices, i.e. the coefficient on log non-housing prices is constrained to equal 0.71. In column 3 wages are fully adjusted for housing prices measured by gross rents, i.e. the coefficient on log gross rents is constrained to equal 0.29. All regressions contain observations on 71,705 workers in 167 cities. The additional regressors are the same as in Table 2. Standard errors in parentheses are robust, clustered by CSA/CBSA.

[c] Significantly different from the budget share (0.29 for housing and 0.71 for non-housing) at the 1% level. *** Significantly different from zero at the 1% level in the first-stage regressions.

We first estimate the log wage equation with log gross rents and log non-housing prices included simultaneously via OLS. As discussed above, measurement error in prices may bias coefficients toward zero. Alternatively, if non-housing prices are measured with considerable error, while housing prices are measured with relatively little error, the coefficient on housing prices could be biased upward. Measurement error would bias the coefficient on non-housing prices toward zero, and the downward bias in the coefficient for non-housing prices would cause the coefficient on housing prices to be positively biased because of a partially omitted variable. In other words, housing prices could pick up some of the effect of non-housing prices. This is especially problematic given the very high correlation between log gross rents and log non-housing prices; the raw correlation coefficient between the two is 0.718. The results in column 1 of Table 9 suggest that log gross rents may indeed be picking up some of the effect of log non-housing prices. The coefficient on log gross rents is 0.337, and is statistically different from zero at the 1% level but not statistically different from the budget share of 0.29. The coefficient on log non-housing prices is 0.231, and is statistically different from zero at the 5% level and statistically different from the budget share of 0.71 at the 1% level.

Ideally, we would like to simultaneously instrument for housing and non-housing prices to account for measurement error in both. One possibility would be to use lagged values of both as instruments. However, because measurement error in non-housing prices is likely to be serially correlated, instrumenting for non-housing prices using its lagged value will not yield consistent estimates. Instead, we explore estimating the log housing price and log non-housing price coefficients separately while constraining the other to equal its budget share and instrumenting using log gross rents for the previous year. This is a hybrid between full adjustment and partial adjustment for prices used by previous researchers. Constraining one of the coefficients to be different from its true value, however, will likely bias the other in the opposite direction. First stage results at the bottom of Table 5 confirm that the log of gross rents from the previous year is a significant predictor of both log gross rents and log non-housing prices.

In column 2 of Table 9, wages are fully adjusted for non-housing prices by constraining the coefficient on log non-housing prices to equal 0.71, i.e., we estimate:

$$\ln W_{ij} - .71 * \ln P_{2j} = X_{ij}\beta + \theta_1 \ln P_{1j} + \gamma Z_j + \varepsilon_{ij}. \tag{12}$$

The coefficient on log gross rents is estimated by 2SLS using log gross rents for the previous year as an instrument. As seen, the coefficient on log gross rents falls to 0.297 and is not statistically different from 0.29. In other words, when we fully adjust wages for non-housing prices, the elasticity between wages and housing prices (measured by gross rents) is nearly identical to housing's budget share as predicted by theory.

In column 3 of Table 9, wages are fully adjusted for housing prices by constraining the coefficient on log gross rents to equal 0.29:

$$\ln W_{ij} - .29 * \ln P_{1j} = X_{ij}\beta + \theta_2 \ln P_{2j} + \gamma Z_j + \varepsilon_{ij}. \tag{13}$$

The coefficient for log non-housing prices is estimated by 2SLS using log gross rents for the previous year as an instrument. Obviously, if the true value of θ_1 is greater than 0.29, the estimate for θ_2 will be upwardly biased. That said, the coefficient on log non-housing prices is 0.754 and is not statistically different from 0.71. The 2SLS results in Table 9, therefore, suggest that the prediction of equation (4), that the wage-price elasticity for a good is equal to its budget share, holds for housing and non-housing prices separately.

Table 10: Separating Housing Prices (Values) and Non-housing Prices

	1	2	3
	OLS	2SLS	2SLS
Full Adjustment for:	NA	Non-housing Prices	Housing Prices
Log Housing Values	0.143[c]	0.091 [c]	
	(0.024)	(0.021)	
Log Non-housing Price Index	0.165 [c]		-0.641 [c]
	(0.132)		(0.226)
R^2	0.494	0.482	0.484
First-Stage Results			
Log Housing Values, 2005		0.992***	0.097***
		(0.024)	(0.015)
Partial R^2 of Excluded Instruments		0.949	0.281

Notes: The dependent variable in column 1 is the log of after-tax hourly wages. In column 2 wages are fully adjusted for non-housing prices, i.e. the coefficient on log non-housing prices is constrained to equal 0.77. In column 3 wages are fully adjusted for housing prices measured by housing values, i.e. the coefficient on log housing values is constrained to equal 0.23. All regressions contain observations on 71,705 workers in 167 cities. The additional regressors are the same as in Table 2. Standard errors in parentheses are robust, clustered by CSA/CBSA.
[c] Significantly different from the budget share (0.23 for housing and 0.77 for non-housing) at the 1% level. *** Significantly different from zero at the 1% level in the first-stage regressions.

In Table 10, we reestimate the regressions in Table 9 using quality-adjusted housing values rather than rents to measure housing prices.[43] When we do so, the coefficients on log housing and log non-housing prices are always significantly less than their budget shares. In fact, when we fully adjust wages for housing prices measured by housing values in column 3 of Table 10, the log of the non-housing price index has a significantly negative coefficient. This reinforces results in the previous section suggesting that housing values are an inappropriate measure of housing prices.

[43] The expected shares for housing and non-housing now change to 0.23 and 0.77 because housing values do not include utilities and non-housing prices now do.

8. Implications for Estimating Implicit Prices of Amenities

The empirical results in this paper have important implications for researchers interested in estimating the implicit prices of amenities or ranking the quality of life across cities. The relationship between wages and prices is consistent with the full compensation hypothesis when we measure housing prices by rents and use lagged rents as an instrument for the general price level. When we measure housing prices by values, however, the relationship between wages and prices is highly inconsistent with the full compensation hypothesis, even when using instrumental variables. This suggests that using housing values along with wages to infer implicit prices of amenities is likely to produce biased estimates. To illustrate, we estimate Census division amenity values by regressing log after-tax wages fully adjusted by both the rent-based modified price index and the baseline price index on eight Census division dummy variables. These regressions contain individual worker characteristics but no city level controls other than Census division indicators. The results are presented in columns 1 and 2 of Table 11.[44] The implicit price of a division's amenities is measured as the negative of its division dummy coefficient for fully adjusted wages. In other words, a low division coefficient indicates a high value of amenities.

If the true wage-price elasticity is equal to one and the rent-based modified index measures the general price level across cities without systematic error (but potentially random error), then the estimates of amenity values by division in column 1 are consistently estimated.[45] The estimates in column 1 suggest that the Middle Atlantic, Pacific, and New England (the

[44] We also regressed the log of the rent-based modified index on log gross rents, amenities, region dummies, and city size dummies to obtain predicted values that "net out" potential measurement error. Division dummies estimated for wages fully adjusted using the predicted values of the rent-based modified priced index were nearly identical to those in column 1 using the actual values.

[45] The previous two sections argue that there is systematic measurement error within cities over time in non-housing prices. This measurement error, however, can still be unsystematic across cities for a given time period.

omitted category) divisions have the most highly valued bundles of amenities. A coefficient of 0.065 for the West South Central division suggests that a marginal worker will require a 6.5 percent higher "real wage" to live in the West South Central division than in New England to compensate for the worse bundle of amenities.

Table 11: Amenity Values by Census Division

Wages Fully Adjusted Using:	Rent-based Modified Price Index	Baseline Price Index	Gross Rents Only
Middle Atlantic	-0.017	-0.044	-0.020
	(0.019)	(0.042)	(0.020)
East North Central	0.075***	0.128***	-0.019
	(0.018)	(0.019)	(0.012)
West North Central	0.070***	0.160***	-0.038**
	(0.017)	(0.022)	(0.018)
South Atlantic	0.041**	0.112***	-0.033
	(0.020)	(0.030)	(0.020)
East South Central	0.059***	0.132***	-0.045***
	(0.017)	(0.022)	(0.014)
West South Central	0.065***	0.203***	-0.054***
	(0.020)	(0.032)	(0.016)
Mountain	0.072***	0.159***	-0.018
	(0.020)	(0.021)	(0.015)
Pacific	-0.004	-0.036	-0.003
	(0.022)	(0.041)	(0.020)

Notes: Regressions contain detailed individual characteristics as in Table 2, but no city-level variables other than Census division dummies. New England is the reference group. Standard errors in parentheses are robust, clustered by CSA/CBSA.
** Significantly different from zero at the 5% level. *** Significantly different from zero at the 1% level.

Fully adjusting wages using the baseline index, however, may upwardly bias estimates of amenity values in areas with high values of the index and downwardly bias estimates of amenity values in areas with low values of the index. This result follows because housing values are more dispersed than rents across cities, but rents measure the true user cost of housing. The rank ordering of division dummies in column 2 is similar to that in column 1, but the estimated coefficients are much larger. According to wages fully adjusted using the baseline index, a marginal worker will require a more than 20 percent higher "real wage" to live in the West South Central than in New England. However, because the baseline index measures housing prices primarily by housing values, the estimated amenity prices in column 2 are biased.

This paper also has implications for researchers who neglect to include non-housing prices in measuring the implicit price of amenities.[46] Column 3 of Table 11 reports the results of division dummies for log wages fully adjusted for gross rents (assuming a budget share of 0.29) but not non-housing prices. The ranking of the coefficients is nearly the opposite of that in column 1. The West South Central is now the most amenable and the New England and Pacific divisions are now the worst. These results confirm that ignoring non-housing prices downwardly biases amenity values for areas with high non-housing prices and upwardly biases amenity prices for areas with low non-housing prices.[47]

9. Conclusion

Differences in wages across areas can be partially explained by differences in prices and amenities. For a given price level, workers are willing to accept lower wages to live and work in more amenable locations. Controlling for amenities, wages must be higher in high price areas in order for workers to achieve equal utility across locations. This paper presents a simple model

[46] Researchers including Gabriel et al. (2003), Shapiro (2006) and Albouy (2008b) have begun to incorporate non-housing prices in amenity prices, but this was not generally the case until recently.
[47] Interestingly, though, the biases from measuring housing prices by housing values and ignoring non-housing prices are in opposite direction. As a result, measuring housing prices by values and ignoring non-housing prices produces amenity estimates generally between those in column 2 and column 3.

that predicts that the elasticity of the wage with respect to the price of a good is proportional to the share of wage income spent on the good. The model also suggests that if workers' consumption equals their wage income, then the elasticity between wages and the general price level should equal one. However, to the extent that the assumptions of the model do not hold, the actual relationship between wages and prices may differ from that predicted by the model.

Measuring housing prices by rents, we find that the elasticity between wages and the general price level is nearly identical to one after instrumenting for the general price level using rents for the previous year. We also present evidence that the wage-price elasticities for housing and non-housing prices are equal to their budget shares when housing prices are measured by rents. These results provide empirical evidence in support of the full compensation hypothesis.

Importantly, though, when housing prices are measured by housing values, the elasticity between wages and the general price level is less than 0.5. The findings in this paper have important implications for estimating the implicit prices of amenities. Measuring housing prices by values instead of rents will bias estimates and cause cities with high housing values to have the relative value of their amenities overstated.

ESSAY II: TEACHER SALARIES AND TEACHER UNIONS: A SPATIAL ECONOMETRIC APPROACH

1. Introduction

Teacher pay is an issue that has received much attention from researchers, politicians, and the general public. Teacher pay is important for several reasons. For one, state and local governments spend a large portion of their budgets on education. For the 2005-06 school year, public school districts in the U.S. had current expenditures per pupil of $9,138, with more than 60 percent of current expenditures going toward teacher salaries and benefits.[48] Teacher pay is also important because of the sheer number of public school teachers in the U.S. In 2006, full-time equivalent employment of elementary and secondary teachers by state and local governments was more than 4.6 million, making teachers by far the largest group of state and local government employees.[49] Teacher pay is also likely to affect the ability of school districts to recruit and retain quality teachers as suggested by a sizable literature in education finance (e.g., Murnane and Olsen 1989, 1990; Figlio 1997, 2002; Clotfelter et al. 2008)

In this paper, we use a spatial econometric framework to examine the effects of teacher unions and other school district characteristics on teacher salaries. While a large literature has investigated the determinants of teacher salaries, only a handful of these studies have used spatial econometric methods to account for spatial dependence in teacher salaries. The few studies that do so provide analysis of individual states and give little attention to the effects of unions. To the researcher's knowledge, this paper is the first to examine teacher salaries in a spatial econometric framework for the 48 contiguous U.S. states.

[48] Source: U.S. Census Bureau, Governments Division
[49] Source: U.S. Census Bureau, Governments Division

Unions are also likely to affect school districts in ways other than increasing teacher salaries. For one, unions are likely to affect the level of fringe benefits that teachers receive as part of their compensation package. Unions are likely to work toward better health insurance benefits, better pension benefits, and greater job security for the teachers they represent. Unions may also affect other school district characteristics such as the student-teacher ratio and the level and composition of non-instructional expenditures. Perhaps most importantly, the overall influence of unions may affect the amount that students learn through altering inputs into the production of education and by empowering teachers.[50] The current paper, however, focuses on the effect of unions on the salaries that teachers are paid. Additional effects that unions might have on school districts are beyond the scope of this paper.

The results in this paper confirm that teacher salaries are positively affected by salaries in nearby districts even after controlling for several other variables that explain teacher salaries. We find that a one percent increase in the average salary of experienced teachers in nearby districts increases salaries for experienced teachers in a given district by between 0.51 and 0.68 percent. For beginning teachers the effect of salaries in nearby districts is even stronger with estimates between 0.85 and 0.92 percent. Furthermore, accounting for spatial dependence produces estimates for several variables that differ considerably from models that ignore the spatial relationship. Incorporating union spillovers at the state level, we find that collective bargaining and union membership density increase the salaries of experienced teachers by as much as 16 and 21 percent, respectively, but the estimated effects on the salaries of beginning teachers are much smaller. This result is consistent with predictions from a median voter model.

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[50] The overall effect of unions on learning is unclear *a priori*. For example, unions may increase the level of inputs, but decrease the efficiency with which those inputs are used. Hoxby (1996) finds that unions have a negative overall effect on student performance.

The remainder of the paper is organized as follows. The next section reviews the literature on teacher salaries, paying specific attention to studies that focus on the effects of unions and studies that examine teacher salaries using a spatial econometric framework. Section 3 presents the empirical model, and section 4 discusses data. Empirical results are presented in section 5 and a brief summary is provided in a concluding section.

2. Theory and Previous Literature

Numerous research studies investigate the determinants of teacher salaries. Many of these focus specifically on how teacher unions affect teacher salaries.[51] Theory suggests that the effect of unions on the level and structure of teacher salaries should be determined by union goals and bargaining power. Union goals are most readily understood by reference to some form of a median voter model, discussed below. Bargaining power derives from numerous factors, including state collective bargaining laws, the extent of organizing, political support, the structure of districts (which affects employer concentration), and financial ability to pay.

Previous empirical studies typically regress the log of average salaries on union activity measures and other characteristics of the school district and local labor market. Results vary considerably across studies in part due to the measures of teacher salaries and union activity that are used. A few studies find little to no effect, but most recent studies find at least a modest positive effect.[52] Hoxby's (1996) finding of a roughly five percent effect of collective bargaining representation on average teacher salaries is fairly representative of most studies.

[51] Early reviews of the literature are provided by Lipsky (1982), Ehrenberg and Schwarz (1986), and Freeman (1986).

[52] In a recent paper Lovenheim (2008) finds collective bargaining coverage to have virtually no effect on average teacher salaries in three Midwestern states. His results, however, also suggest that collective bargaining is associated with increased enrollment and teacher employment in these states. If the increased employment decreased the average level of experience and education in these districts, then the effect of unionization on average salaries may be downwardly biased.

Still, at least a few studies find union effects as large as 20 percent of wages (e.g., Baugh and Stone 1982; Zwerling and Thomason 1995).

A few studies also recognize that teacher unions might differentially affect the salaries of teachers within a given district. In other words, unions not only affect the average level of salaries but also the distribution of salaries within a district. Holmes (1976) finds that unions increase both the return to experience and the return to education within a district. Similarly, Delaney (1985) finds that collective bargaining increases the salary differential between experienced teachers and inexperienced teachers. Zwerling and Thomason (1995) and Lentz (1998) also find that while unions have a positive and significant effect on the salaries of teachers earning the highest salary in a district, unions have a small (though still positive) and insignificant effect on the lowest salary in a district. Babcock and Engberg (1999) and Ballou and Podgursky (2002) also suggest that the average levels of teaching experience and education in a district affect the returns to experience and education as well.

The effect of teacher unions on intra-district salary differentials is often explained by appealing to the median voter model. Virtually all public school districts, including those without collective bargaining, pay teachers according to a salary schedule that maps salary to teaching experience and education. In the absence of union pressures, district administrators may dictate a salary schedule that is more appealing to marginal teachers than median teachers, with the marginal teachers being those at the tails of the distributions of experience and education. However, the union's preferred salary structure may be heavily influenced by the preferences and hence characteristics (i.e. experience and education) of the median teacher in the district. According to data tabulated from the 1999-2000 Schools and Staffing Survey (SASS)

Teacher Survey, about half of public school teachers in the U.S. had advanced degrees and the average experience was about fifteen years.

Because there are multiple dimensions to union contracts (returns to experience, returns to education, the level of fringe benefits, etc.), the median voter model may not adequately explain the salary determination process within districts. Having multiple choice variables means that there is likely no single median voter whose preferences are decisive. Instead, it may be useful to more generally view union preferences as resulting from a majority coalition of teachers. Union cohesion may even require that there be a super-majority coalition. Even with multiple choices to be made, though, it still seems likely that teachers with median levels of experience and education will be important members of the majority coalition and will push for a salary structure that benefits them. Teachers with little or no experience may be the ones most likely to be left out of the majority coalition for several reasons. First, inexperienced teachers may be less likely to be members of the union and less likely to be active in the union when they are members. Additionally, union contracts are often negotiated months or even years in advance of the school year for which they apply. As a result, the very newest teachers never had a vote on how the salary schedule would be structured. School district administrators, however, are likely to be sensitive to market conditions for new school teachers since this is when teachers are most mobile. Thus, we might expect unionized school districts with strong union bargaining power to respond to the preferences of experienced (median voter) teachers, whereas nonunion districts or union districts where bargaining power is weak may be more responsive to market conditions for new teachers.

It is reasonable to expect that unions might increase the salary differential between beginning and experienced teachers even absent the median voter mechanism. Entry-level teachers searching for jobs are substantially more mobile than experienced teachers. Hence, starting salaries among nearly all districts must be reasonably competitive in order to attract teachers of any given quality. As teacher mobility decreases with experience and district-specific tenure employer monopsony power may increase, leading to salaries below competitive levels for experienced (but not beginning) teachers in districts with low mobility. Union bargaining power can offset this monopsony power, thus creating a union wage gap for experienced teachers (i.e., higher salaries for experienced teachers in union than in nonunion districts), while at the same time having little effect on starting salaries for union relative to nonunion teachers.

Unions might also increase the salary differential between teachers with and without advanced degrees. This shifting in the salary structure may even result in less experienced teachers having lower salaries than would be the case in the absence of union negotiations. Such a "deferred payment" scheme should attract teachers willing to accept low initial salaries based on the expectation that they will receive subsequent rewards for tenure and an advanced degree.

Chambers (1977), Delaney (1985) and Zwerling and Thomason (1995) suggest that union activity in a district produces positive wage spillovers in nearby districts. In fact, all three studies suggest that the union spillover effect on wages is larger than the direct effect. A few more recent studies suggest that teacher wage spillovers may be more direct. Wagner and Porter (2000), Greenbaum (2002), Babcock, Engberg and Greenbaum (2005), and Millimet and Rangaprasad (2007) find using spatial econometric methods that teacher salaries in a district are positively influenced by teacher salaries in nearby districts.[53] In other words, teacher salaries

[53] Ready and Sandver (1993) also find that salaries are correlated with salaries in nearby districts. However, their analysis is based on OLS and does not account for the simultaneity of salaries for districts in close proximity. As will be discussed in more detail in the next section, appropriate spatial methods account for the simultaneity in teacher salaries using instrumental variables.

appear to be spatially dependent, at least in the states considered in these studies.[54] However, each of these studies examines a single state and with the exception of Babcock et al. (2005) do not generally focus on the effects of unions. The current paper makes an important contribution to the literature by using a national level dataset to examine the effect of unions on teacher salaries in a spatial econometric framework.

3. Empirical Model

Most previous studies of the determinants of teacher salaries do not account for spatial dependence. The usual estimation equation in these studies is given by:

$$Y = X\beta + u, \tag{1}$$

where Y is an $n \times 1$ vector of teacher salaries (usually measured in logs), X is an $n \times k$ matrix of explanatory variables, β is a $k \times 1$ vector of parameters, and u is a mean zero error term assumed to be i.i.d. across observations.

In this paper we consider the possibility that teacher salaries are spatially correlated after controlling for other determinants of teacher salaries. The primary concern is that teacher salaries in a district may be affected by teacher salaries in neighboring districts. This type of spatial dependence is likely to occur for at least two reasons. First, school districts likely compete with nearby districts for quality teachers. If one district offers a salary substantially below that of nearby districts, they will have difficulty hiring and retaining quality teachers. Thus school district administrators have incentives to keep teacher salaries, especially starting salaries, competitive with salaries in nearby districts. Similarly, comparisons of salaries in nearby districts are almost always used in contract negotiations between administrators and

[54] Wagner and Porter (2000) examine school districts in Ohio; Greenbaum (2002) and Babcock et al. (2005) examine districts in Pennsylvania; and Millimet and Prangasad (2007) examine districts in Illinois.

teacher unions. Thus spatial dependence in teacher salaries seems quite plausible. If there is spatial dependence in the dependent variable then methods that do not account for this are likely to produce inconsistent coefficient estimates (Anselin, 1988). A second concern is that there may be spatial correlation in the error term, say from spatially correlated measurement error in an explanatory variable (e.g., the county unemployment rate) for nearby districts (Kalenkoski and Lacombe 2008). Failing to account for spatial correlation in the error term may result in standard errors that are inconsistently estimated.

The spatial model can be represented by:

$$Y = \rho WY + X\beta + u \tag{2}$$

$$u = \lambda Wu + \varepsilon,$$

where W is an $n \times n$ weighting matrix that specifies the structure of the spatial correlation, ρ and λ are spatial autocorrelation coefficients for the dependent variable and the error term, respectively, and ε is a mean zero error term that is i.i.d. across observations. In the current paper, W is specified based on the distance between school districts. For row i of matrix W, districts that are more than 50 miles away from i are given zero weight. In other words districts are only considered neighbors if they are within 50 miles of each other. Districts within 50 miles of i are weighted based on their inverse distance to i, so that nearer districts are given more weight than districts further away.[55] The choice of this weighting matrix reflects the assumption that salaries in a district are most strongly affected by salaries in other districts that are closest to it and that the effect is attenuated with distance. The matrix is also structured so that the elements in each row sum to unity and all diagonal elements are equal to zero.[56] In other words, WY is a distance-weighted average of teacher salaries in nearby districts, and Wu is a distance-

[55] Alternatively, we could equally weight all districts within 50 miles. The results are not considerably affected by this modification to the weighting scheme.
[56] The zero diagonals reflect the assumption that a district cannot be its own neighbor.

weighted average of the error terms in nearby districts. We can also estimate a spatial model that allows for a more robust specification of the spatial dependence in the errors. Specifically, instead of assuming that $u = \lambda Wu + \varepsilon$ as in equation (2), we can estimate an equation with a spatially lagged dependent variable while allowing for error correlation or clustering within groups.[57] In the empirical analysis to follow we estimate both equations that model the spatial error correlation as in equation (2) and equations that include a spatially lagged dependent variable, then account for cross-sectional dependence in the error terms.[58]

It should be clear that teacher salaries in neighboring districts may be simultaneously determined. Salaries in district *j* affect salaries in district *i*, but salaries in district *i* also affect salaries in district *j*. Because of the simultaneity involved, using Ordinary Least Squares (OLS) to estimate the spatial model is inappropriate. Instead, instrumental variable methods should be used. More specifically, the present paper estimates the spatial models by the Generalized Method of Moments (GMM) estimator developed by Kelejian and Prucha (1998) using the Spatial Econometrics Toolbox for MATLAB developed by James LeSage and described in LeSage (1999). The GMM estimator instruments for WY using WX and W^2X as instruments. In other words, the estimator instruments for salaries in nearby districts using the distance-weighted averages of the other explanatory variables in nearby districts along with the distance-weighted averages of their neighbors' neighbors' characteristics.

Kelejian and Prucha (1998) outline the conditions under which the GMM estimator provides consistent estimates. While the reader is referred to Kelejian and Prucha (1998) for a more formal discussion, a brief discussion of some of these assumptions is useful. First, the

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[57] Some spatial econometric studies also estimate a model that assumes that there is spatial correlation in the errors but not in the dependent variable, i.e. $\rho = 0$. Because we have reason to believe that teacher salaries are directly affected by teacher salaries in neighboring districts, the spatial error model that assumes no spatial correlation in the dependent variable seems inappropriate.

[58] Wooldridge (2003, 2006) provides a discussion of the use of cluster methods in applied econometrics.

diagonal elements of W are zero. Additionally, $[I-\rho W]$ is nonsingular, where I is an $n \times n$ identity matrix, and the absolute values of ρ and λ are each less than one. The row and column sums of W and $[I-\rho W]^{-1}$ are bounded in absolute value, where $[I-\rho W]^{-1}$ is an $n \times n$ inverse matrix. Additionally, the matrix of exogenous regressors and the matrix of instruments have full column rank. It must also be the case that at least one of the nonconstant exogenous regressors in the model has a nonzero effect on Y. In other words, we need the instruments to predict WY in order for the model to be identified. Again, for a more complete discussion and a proof of the consistency of the estimator, the reader is referred to Kelejian and Prucha (1998).

The marginal effects of the exogenous variables in equation (2) are not equal to the parameters in vector β. Rewriting equation (2) as:

$$Y = [I-\rho W]^{-1}X\beta + [I-\rho W]^{-1}u, \tag{3}$$

is an identity matrixit is easily seen that the partial derivative of Y with respect to a single exogenous variable X_k $(n \times 1)$ is given by:

$$\partial Y/\partial X_k' = \begin{bmatrix} \partial Y_1/\partial X_{1k} & \partial Y_1/\partial X_{2k} \dots & \partial Y_1/\partial X_{nk} \\ \partial Y_2/\partial X_{1k} & \partial Y_2/\partial X_{2k} \dots & \partial Y_2/\partial X_{nk} \\ \vdots & \vdots \quad \ddots & \vdots \\ \partial Y_n/\partial X_{1k} & \partial Y_n/\partial X_{2k} \dots & \partial Y_n/\partial X_{nk} \end{bmatrix} = \beta_k[I-\rho W]^{-1}. \tag{4}$$

Therefore, the marginal effect on teacher salaries of an explanatory variable such as union activity is $\beta_k[I-\rho W]^{-1}$. If X_k is measured at the district level, then the *average* marginal effect of an increase in X_k in a district on teacher salaries in that district is equal to β_k times the average of the diagonal elements of the $[I-\rho W]^{-1}$ matrix. More formally, setting

$$A = [I-\rho W]^{-1} = \begin{bmatrix} a_{11} & a_{12} \dots & a_{1n} \\ a_{21} & a_{22} \dots & a_{2n} \\ \vdots & \vdots \quad \ddots & \vdots \\ a_{n1} & a_{n2} \dots & a_{nn} \end{bmatrix}, \tag{5}$$

the average marginal effect of an increase in X_k in a district on teacher salaries in that district is $\beta_k \frac{1}{n}\sum_{j=1}^{n} a_{jj}$. If X_k is measured at a level of concentration larger than the district, such as the state, then the average marginal effect of an increase in X_k at the state level on teacher salaries in a district is equal to $\beta_k \frac{1}{n}\sum_{j=1}^{n}\sum_{i=1}^{n} d_{ij}a_{ij}$, where d_{ij} is equal to one if $i = j$ or if i and j are in the same state and are defined as neighbors according to the spatial weight matrix (i.e. within 50 miles of each other in this paper). Because X_k is measured at the state level, the marginal effect of X_k on Y for district *i* includes not only the direct effect of district *i* but also the indirect effects of "neighboring" districts in the same state. Kim, Phipps, and Anselin (2003) show that if X_k does not vary among neighboring districts (e.g., the variable is measured at the state level and all neighbors are in the same state), then the average marginal effect of a unit increase in X_k is equal to $\beta_k/(1-\rho)$. We can think of $\frac{1}{n}\sum_{j=1}^{n}\sum_{i=1}^{n} d_{ij}a_{ij}$ as a *spatial multiplier* with both $\frac{1}{n}\sum_{j=1}^{n} a_{jj}$ and $1/(1-\rho)$ as special cases. In the results section, we report both the coefficient estimates and the average marginal effects for the exogenous variables in the spatial models.

4. Data

The primary data used in this analysis come from the school district survey of the 1999-2000 Schools and Staffing Survey (SASS) conducted by the National Center for Education Statistics (NCES) and completed by school district administrators. We examine school districts in the 48 contiguous U.S. states. Additional data are obtained from the NCES Common Core of Data (CCD), the NCES Comparable Wage Index (CWI) which measures the wages in the local labor market of occupations comparable to teaching, the NCES School District Demographics System (SDDS), and the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics

(LAUS). Table 1 provides summary statistics for the variables used in the study and documents the source for each.

Table 1: Summary Statistics and Data Sources

Variable	Mean	Std. Dev.	Min	Max	Source
Salary BA0	25,901	3,802	16,350	43,085	SASS
Salary MA20	48,986	11,349	20,775	98,207	SASS
Comparable Wage Index (CWI)	0.896	0.113	0.703	1.244	CWI
Log (Salary BA0/CWI)	10.269	0.105	9.862	10.673	SASS & CWI
Log (Salary MA20/CWI)	10.779	0.155	10.205	11.365	SASS & CWI
Collective Bargaining	0.612	0.487	0	1	SASS
Meet and Confer	0.078	0.268	0	1	SASS
State Collective Bargaining Share	0.567	0.410	0	1	SASS
State Union Membership	0.765	0.185	0.312	0.992	SASS
Days of School	178.614	4.691	142	288	SASS
Student-Teacher Ratio	15.086	3.904	3.088	107.241	SASS
Share of Secondary Teachers	0.386	0.158	0	1	SASS
Share of White Teachers	0.900	0.188	0	1	SASS
Share of Teachers Dismissed	0.007	0.021	0	0.491	SASS
% Δ Enrollment, 1994-1999 (/100)	0.039	0.174	-0.658	4.452	CCD
Log Enrollment	7.787	1.433	3.367	13.905	SASS
Share of White Students	0.763	0.272	0	1	SASS
Share of Low Income Students	0.397	0.250	0	1	SASS
Share HS Plus	0.796	0.099	0.201	1	SDDS
Share BA Plus	0.194	0.118	0.016	1	SDDS
Share w/ Children<18	0.319	0.054	0.123	1	SDDS
Share of Homeowners	0.734	0.115	0	0.970	SDDS
County Unemployment Rate	0.046	0.026	0.007	0.301	LAUS

Note: The dataset contains observations on 4237 school districts included in the 1999-2000 SASS.

Teacher salaries are investigated for both beginning teachers and experienced teachers and come from the 1999-2000 SASS. Beginning teacher salaries are measured by the base salary according to the district's salary schedule for teachers with no teaching experience and only a bachelor's degree (BA0). Salaries for experienced teachers are measured by the base salary on the district's salary schedule for teachers with 20 years of teaching experience and a master's degree related to the teaching field (MA20).[59] Beginning teachers in the sample have a mean salary of $25,901 while the mean salary for experienced teachers is nearly twice that. There is also considerably more variation in the salaries of experienced teachers. The standard deviation in salaries for experienced teachers is nearly three times that of beginning teachers.

For both beginning and experienced teachers, the dependent variable in the analysis below is measured as the log of the reported salary relative to the level of comparable wages of workers in the same labor market using the CWI (Taylor and Fowler 2006). In other words, if W_{exp} is the nominal salary for experienced teachers, W_{beg} is the nominal salary for beginning teachers, and W_{comp} is the value for the comparable wage index, then the dependent variable for experienced teachers is ln (W_{exp}/W_{comp}) and the dependent variable for beginning teachers is ln (W_{beg}/W_{comp}). Comparable wages provide a good measure of the relative cost of living in a particular labor market and also serve as a proxy for the opportunity cost of teaching in a given market (Stoddard 2005). Without measuring teacher salaries relative to comparable wages, spatial autocorrelation could in part result from salaries being high for all districts in expensive labor markets and low for districts in inexpensive labor markets. Measuring teacher salaries relative to comparable wages, any spatial autocorrelation that we find will be more than just the result of nearby districts responding similarly to similar living costs.

The regression analysis below includes a number of important explanatory variables. The effect of unions is given considerable emphasis in this paper and union activity is measured in three different ways. We first measure union activity by two mutually exclusive indicator

[59] Individual teachers are sometimes paid amounts above that required by the salary schedule for special credentials or extra duties such as coaching a sports team. However, the salary measures in this paper are for the base salary in the district and do not include extra pay for special credentials or extra duties.

variables for collective bargaining and the presence of a meet and confer agreement in the district. Meet and confer agreements are not binding on school districts and are considered to be a much weaker form of union activity than collective bargaining. As seen in Table 1, more than 61 percent of the districts engage in collective bargaining, and another eight percent have meet and confer agreements. Thus roughly 31 percent of districts have neither. Previous literature has suggested that union activity in neighboring districts has important spillover effects, and the next two measures of union activity incorporate union spillovers. The second measure of union activity is the share of districts in a state with a collective bargaining agreement.[60] If collective bargaining has important spillovers effects for teacher salaries in nearby districts, the effect for the state collective bargaining share should be greater than the effect for the collective bargaining indicator variable. The third measure of union activity is the percentage of teachers in a state who are members of a teacher union, which also incorporates union spillovers. Unfortunately, union membership at the district level is not available, so we cannot compare the effects of state membership density to that of district membership density. Though the second and third measures of union activity both incorporate union spillovers, they are different measures and could produce somewhat different results.[61]

The analysis also includes a number of other important variables thought to affect teacher salaries. Teachers are expected to require greater compensation for longer school years, so the number of days in the school year is expected to have a positive sign. Teachers likely prefer

[60] We also experimented with measuring union activity by the share of districts in a state with a meet and confer agreement and the share of districts in a state with any sort of agreement (e.g., collective bargaining or meet and confer). The results for the share of districts with meet and confer was either negative or small and insignificant, consistent with later results suggesting that meet and confer agreements do not increase teacher salaries. The results for the share with any agreement variable are qualitatively similar to the results for the share with collective bargaining. These results are available from the author.

[61] One limitation to the current paper is that all of the union activity measures are treated as exogenous. If union activity is in fact endogenous because of simultaneity, omitted variables, or measurement error, coefficient estimates could be biased and inconsistent.

smaller classes, so the student-teacher ratio is expected to have a positive coefficient. Secondary teaching may be more difficult or require greater skills, so the share of secondary teachers is expected to have a positive coefficient. The share of teachers who are white is included as a control, but the expected effect is somewhat unclear. Districts that have dismissed relatively large numbers of teachers recently may have a low need for teachers and pay less competitive salaries. Districts that have experienced increased enrollments over the previous five years are expected to have a high demand for teachers and be willing to pay higher relative salaries. Similarly, larger districts are expected to pay more competitive salaries, and the log of district enrollment is expected to have a positive coefficient. Teachers may require compensating differentials to teach students from disadvantaged backgrounds, so the share of students who are white is expected to have a negative effect, while the share of students that are low income as measured by free or reduced lunch eligibility is expected to have a positive effect. More educated residents are thought to have greater demand for education, so the share of adults (age 25+) living in the district with at least a high school degree and the share of adults with at least a bachelor's degree are both expected to have positive coefficients. Residents with children are expected to demand greater spending on education, so the share of households with at least one child under age 18 is expected to have a positive effect. Renters may be more likely than homeowners to support spending on education, so the share of households who are homeowners is expected to have a negative effect. Finally, the unemployment rate in the county in which the district is located is included to capture local labor market conditions. Higher unemployment is likely to make it more difficult to find a well-paying career outside of teaching and is expected to have a negative effect on teacher salaries.

5. Results

We begin by estimating equation (1) using OLS. However, because social comparisons are likely to be important in the determination of teacher salaries and because previous research has found evidence of spatial dependence in teacher salaries, the preferred methods account for spatial correlation in the dependent variable. We next estimate the spatial model of equation (2) using GMM, which also models spatial dependence in the error term. We then estimate the equation that includes a spatially lagged dependent variable with standard errors clustered by state. Results that measure union activity by collective bargaining and meet and confer indicator variables are discussed first and reported in Tables 2 and 3 for experienced and beginning teachers, respectively. Tables 4 and 5 reestimate the equations in Tables 2 and 3 measuring union activity by the share of districts in the state with a collective bargaining agreement. Tables 6 and 7 reestimate the equations in Tables 2 and 3 measuring union activity by the share of teachers in the state who are members of a teacher union. For exogenous variables in the spatial models, we report coefficient estimates, standard errors in parentheses, and average marginal effects in brackets computed as described above in Section 3.[62] When discussing magnitudes for these variables, we will focus on the marginal effects.[63]

Spatial Correlation Coefficients

The results confirm that salaries are spatially dependent for both experienced teachers and beginning teachers even after controlling for many other variables that explain teacher salaries. The spatial models that also model the spatial error correlation in column 2 of Tables 2 and 3 report statistically significant spatial lag coefficients (ρ) of 0.68 and 0.92 for experienced and beginning teachers, respectively. The larger coefficient for beginning teachers is likely due

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[62] For ease of discussion, we often refer to the average marginal effect simply as the marginal effect.
[63] Note that coefficient estimates in the OLS equations can be directly interpreted as marginal effects because of the linearity assumption.

to the greater mobility of new than experienced teachers. District administrators may be especially concerned with keeping beginning salaries competitive in order to be able to hire and retain beginning teachers. Because experienced teachers are usually less mobile, spatial dependence in salaries for experienced teachers may result more from union bargaining efforts to keep salaries comparable to those in nearby districts. According to these estimates, a one percent increase in the distance-weighted average of experienced teacher salaries in nearby districts increases salaries for experienced teachers in a given district by 0.68 percent. For beginning teachers, the effect of salaries in nearby districts is even stronger; a one percent increase in the distance-weighted average of salaries for beginning teachers in nearby districts increases salaries for beginning teachers by 0.92 percent. For experienced teachers, the spatial error coefficient (λ) in the second column of Table 2 is small and not statistically different from zero. For beginning teachers, however, the spatial error coefficient in column 2 of Table 3 is -0.29 and is statistically different from zero. A negative coefficient for the spatial error term is counterintuitive but actually quite common when estimating a general spatial model. For experienced teachers the spatial lag coefficient is virtually unchanged in column 3 of Table 2 when we do not specifically model the spatial error structure. For beginning teachers, however, the spatial lag coefficient decreases slightly to 0.85, as seen in column 3 of Table 3, but the difference is not significant. More generally, marginal effects and statistical significance for most variables are virtually unchanged by moving from column 2 to column 3.

Table 2: Log Salary Regressions for MA20 with Union Indicator Variables

	1	2	3
Spatial Lag (ρ)		0.6811***	0.6766***
		(0.0294)	(0.0791)
Spatial Error (λ)		-0.0275	
		(0.0583)	

Collective Bargaining	0.1095***	0.0374***	0.0378***
	(0.0052)	(0.0051)	(0.0132)
		[0.0417]	[0.0421]
Meet and Confer	0.0110	0.0037	0.0044
	(0.0084)	(0.0064)	(0.0087)
		[0.0041]	[0.0049]
Days of School (/100)	0.3011***	0.1273***	0.1274**
	(0.0453)	(0.0353)	(0.0568)
		[0.1421]	[0.1419]
Student-Teacher Ratio (/100)	-0.0006	0.0380	0.0418
	(0.0619)	(0.0470)	(0.0474)
		[0.0424]	[0.0466]
Share of Secondary Teachers	0.0588***	0.0579***	0.0576**
	(0.0135)	(0.0103)	(0.0237)
		[0.0646]	[0.0642]
Share of White Teachers	0.0411***	0.0210**	0.0212**
	(0.0134)	(0.0103)	(0.0088)
		[0.0234]	[0.0236]
Share of Teachers Dismissed	-0.0794	-0.0609	-0.0577
	(0.0997)	(0.0762)	(0.0818)
		[-0.068]	[-0.0643]
% Δ Enrollment, 1994-1999 (/10,000)	2.2773*	0.5479	0.5467
	(1.2666)	(0.9687)	(0.9146)
		[0.6116]	[0.6089]
Log Enrollment	0.0168***	0.0118***	0.0121***
	(0.0019)	(0.0014)	(0.0038)
		[0.0132]	[0.0135]
Share of White Students	-0.0159	-0.0058	-0.0058
	(0.0119)	(0.0090)	(0.0147)
		[-0.0065]	[-0.0065]
Share of Low Income Students	-0.0568***	-0.0215**	-0.0216**
	(0.0114)	(0.0088)	(0.0081)
		[-0.0240]	[-0.0242]
Share HS Plus	0.0112	0.0073	0.0078
	(0.0382)	(0.0289)	(0.0631)
		[0.0081]	[0.0087]
Share BA Plus	0.1704***	0.1217***	0.1230***
	(0.0267)	(0.0203)	(0.0426)
		[0.1359]	[0.1370]
Share w/ Children<18	-0.3345***	-0.2022***	-0.2017***
	(0.0408)	(0.0316)	(0.0522)

		[-0.2257]	[-0.2247]
Share of Homeowners	0.0142	-0.0172	-0.0177
	(0.0220)	(0.0168)	(0.0301)
		[-0.0192]	[-0.0197]
County Unemployment Rate	0.8452***	0.5115***	0.5148***
	(0.0929)	(0.0714)	(0.1076)
		[0.7539]	[0.7545]
R^2	0.2548	0.5636	0.5649

Notes: Column 1 is estimated by OLS and columns 2 and 3 are estimated by GMM. The dependent variable is the log of the salary for teachers with 20 years of experience and a master's degree relative to the Comparable Wages Index. Standard errors are in parentheses and average marginal effects are in brackets for the spatial models. Standard errors in column 3 are clustered by state.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 3: Log Salary Regressions for BA0 with Union Indicator Variables

	1	2	3
Spatial Lag (ρ)		0.9155***	0.8477***
		(0.0405)	(0.0654)
Spatial Error (λ)		-0.2910***	
		(0.0128)	
Collective Bargaining	0.0071*	0.0023	0.0023
	(0.0040)	(0.0025)	(0.0048)
		[0.0034]	[0.0030]
Meet and Confer	-0.0110*	0.0001	0.0007
	(0.0064)	(0.0044)	(0.0051)
		[0.0001]	[0.0009]
Days of School (/100)	0.0887***	0.0369	0.0540*
	(0.0344)	(0.0233)	(0.0305)
		[0.0548]	[0.0695]
Student-Teacher Ratio (/100)	-0.0934**	-0.0084	-0.0018
	(0.0470)	(0.0322)	(0.0341)
		[-0.0125]	[-0.0023]
Share of Secondary Teachers	0.0456***	0.0363***	0.0363**
	(0.0103)	(0.0071)	(0.0164)
		[0.0540]	[0.0467]
Share of White Teachers	-0.0013	-0.0011	-0.0000

	(0.0102)	(0.0072)	(0.0070)
		[-0.0016]	[0.0000]
Share of Teachers Dismissed	-0.0094	-0.0563	-0.0587
	(0.0756)	(0.0544)	(0.0413)
		[-0.0837]	[-0.0755]
% Δ Enrollment, 1994-1999 (/10,000)	0.8973	-0.0599	0.1801
	(0.9610)	(0.6715)	(0.8163)
		[-0.0890]	[0.2317]
Log Enrollment	0.0080***	0.0022**	0.0034*
	(0.0014)	(0.0010)	(0.0019)
		[0.0033]	[0.0044]
Share of White Students	-0.0064	-0.0008	-0.0028
	(0.0090)	(0.0060)	(0.0073)
		[-0.0012]	[-0.0036]
Share of Low Income Students	-0.0107	-0.0029	-0.0058
	(0.0086)	(0.0060)	(0.0085)
		[-0.0043]	[-0.0075]
Share HS Plus	-0.1466***	-0.0326	-0.0388
	(0.0290)	(0.0200)	(0.0279)
		[-0.0485]	[-0.0499]
Share BA Plus	0.0597***	0.0418***	0.0536**
	(0.0202)	(0.0135)	(0.0220)
		[0.0621]	[0.0690]
Share w/ Children<18	-0.2495***	-0.0964***	-0.1033**
	(0.0310)	(0.0229)	(0.0403)
		[-0.1433]	[-0.1329]
Share of Homeowners	0.0325*	-0.0230*	-0.0261
	(0.0167)	(0.0120)	(0.0187)
		[-0.0342]	[-0.0336]
County Unemployment Rate	0.5251***	0.1871***	0.2527***
	(0.0705)	(0.0458)	(0.0830)
		[0.5029]	[0.5114]
R^2	0.0598	0.4774	0.5065

Notes: Column 1 is estimated by OLS and columns 2 and 3 are estimated by GMM. The dependent variable is the log of the salary for teachers with no experience and a bachelor's degree relative to the Comparable Wages Index. Standard errors are in parentheses and average marginal effects are in brackets for the spatial models. Standard errors in column 3 are clustered by state.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Collective Bargaining and Meet and Confer Indicators

Estimating the effect of teacher unions on teacher salaries is a primary concern of this paper. Previous studies have usually found that unions increase teacher salaries, at least for experienced teachers, but these studies do not generally account for spatial dependence in teacher salaries. For experienced teachers, OLS suggests that the presence of collective bargaining increases teacher salaries by roughly 12 percent. Accounting for a spatially lagged dependent variable, however, the average marginal effect of collective bargaining for experienced teachers is only 0.042, suggesting that collective bargaining increases salaries for experienced teachers by a little over four percent.[64] Thus, it appears that failing to account for spatial dependence causes one to overstate the effects of collective bargaining in a district on the salaries of experienced teachers. However, because collective bargaining is measured at the district level but may have spillover effects across districts, some of the observed wage spillover in Table 2 may be a union spillover. Later on, we will measure union activity by two measures that incorporate union spillover effects, the share of districts in a state with collective bargaining and the share of workers in a state who are members of a teacher union.

For beginning teachers accounting for spatial dependence has a similar effect on coefficient estimates for collective bargaining but on a much smaller scale. OLS suggests a small but statistically significant effect of collective bargaining, just less than one percent. The spatial models, however, suggest an even smaller effect that is not statistically different from zero. Thus, consistent with previous literature, the results in Tables 2 and 3 suggest that collective bargaining increases teacher salaries for experienced teachers but not for beginning teachers.

[64] Recall that the average marginal effect is the arithmetic mean of the marginal effects for individual districts. Differences in the estimated marginal effects across districts are generally quite small and result from the structure of the weighting matrix.

Tables 2 and 3 also include an indicator variable for the presence of a meet and confer agreement in the district. A meet and confer agreement is a much weaker form of union activity than collective bargaining, so we expect the effect of a meet and confer agreement on teacher salaries to be smaller than the effect of collective bargaining. For experienced teachers the meet and confer effect is small, positive, and statistically insignificant in all three specifications in Table 2. For beginning teachers the meet and confer effect in the OLS specification is negative and statistically significant though small. Accounting for spatial dependence, though, the meet and confer effect for beginning teachers is very small, positive, and highly insignificant. The results, therefore, suggest that while collective bargaining increases the salaries of experienced teachers, the presence of a meet and confer agreement has virtually no effect on the salaries of experienced or beginning teachers. Although we cannot test it, meet and confer agreements may provide other benefits to workers as compared to nonunion districts such as providing a modest level of collective voice.

Additional Explanatory Variables

The results in Tables 2 and 3 suggest that additional variables affect teacher salaries as well. These include characteristics of the teachers, the school district, the students, the local residents, and the local labor market. Importantly, the results for the spatial models are often quite different from the OLS results for equation (1) in the first column of the tables. Here we discuss the results in the third columns of Table 2 and 3 for the spatial model that clusters standard errors by state instead of modeling the spatial error correlation. Because the dependent variables are measured in logs, the marginal effects can be loosely interpreted as percentage changes. The length of the school year has a positive and statistically significant effect on the salaries of experienced teachers with a marginal effect of 0.142. For beginning teachers the effect is smaller, though still statistically significant at the 10 percent level, with a marginal effect of 0.070.[65] The student-teacher ratio has a positive but insignificant effect for salaries of experienced teachers, and a small negative and highly insignificant effect for beginning teachers. For both experienced and beginning teachers, salaries increase with the percentage of teachers who teach secondary grades, with significant marginal effects of 0.064 and 0.047, respectively. This suggests that secondary teaching is either less pleasant or requires greater skills or greater effort than teaching primary grades (Walden and Sogutlu 2001). The results also suggest that the percentage of teachers who are white has a positive and statistically significant effect on the salaries of experienced teachers, with a marginal effect of 0.024 but a small, negative, and highly insignificant effect on the salaries of beginning teachers. For experienced teachers, this may result from white teachers having better outside labor market options than non-white teachers, perhaps in part due to discrimination, though the effect is not especially large. The percentage of teachers dismissed in the previous year and the growth in enrollment over the last five years both have a statistically insignificant effect on the salaries of both beginning and experienced teachers. Larger school districts pay higher salaries to both experienced and beginning teachers, with enrollment elasticities of 0.014 and 0.004, respectively. This may suggest that larger school districts are worse places to work and require compensating differentials (Walden and Newmark 1995). The share of students who are white has a small and highly insignificant effect for both experienced and beginning teachers. This is in contrast to Martin (forthcoming) who finds that teachers require positive compensating wage differentials to work in districts with a higher percentage of minority students. The share of low-income students has a significant coefficient for experienced teachers, with a marginal effect of -0.024, but a small and insignificant effect for beginning teachers.

[65] The effect for beginning teachers, however, is marginally insignificant in column 2.

The results also suggest that teacher salaries are affected by the local demand for education. Increases in the share of the adult population with a college degree increases salaries for both experienced and beginning teachers, with marginal effects of 0.137 and 0.069. The share of the population with a high school degree or higher, however, has a positive and insignificant effect on the salaries of experienced teachers and a negative and insignificant effect on the salaries of beginning teachers. The share of households in a district with children under age 18 results in significantly lower salaries for both experienced and beginning teachers, with marginal effects of -0.225 and -0.133. This is in contrast to expectations that households with children would demand greater education services and be willing to support higher teacher salaries. The share of households in a district who are homeowners has a negative but insignificant effect on the salaries of both experienced and beginning teachers.[66] The results also suggest that local labor market conditions measured by the county unemployment rate have a statistically significant effect on the salaries of experienced and beginning teachers, with marginal effects of 0.755 and 0.511, respectively. However, the positive effect of unemployment on teacher salaries is somewhat unexpected.

Measuring Union Activity by the State Share of Districts with Collective Bargaining

Tables 4 and 5 present the results of re-estimating the equations in Tables 2 and 3 measuring union activity by the share of districts in a state with a collective bargaining agreement. The first thing to note is that the spatial lag coefficient decreases to 0.51 and 0.56 for experienced teachers in columns 2 and 3 of Table 4. This seems to confirm our earlier hypothesis that the spatial lag coefficients in Table 2 were partially capturing union spillovers. Also unlike

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[66] In column 2 of Table 3, however, the share of homeowners has a marginal effect of -0.034 that is significant at the 10 percent level. This result may weakly suggest that renters are more supportive of spending on education than are homeowners, perhaps in part because renters do not believe that they bear the burden of local property taxes to finance education (Martinez-Vasquez and Sjoquist, 1988).

the results in Table 2, the spatial error coefficient for experienced teachers in Table 4 is positive and statistically significant. For beginning teachers coefficients for the spatial lag and the spatial error in Table 5 are nearly identical to the estimates reported in Table 3.

Table 4: Log Salary Regressions for MA20 with State Collective Bargaining Share

	1	2	3
Spatial Lag (ρ)		0.5120***	0.5639***
		(0.0430)	(0.1021)
Spatial Error (λ)		0.1455**	
		(0.0649)	
State Collective Bargaining Share	0.1736***	0.0800***	0.0719***
	(0.0057)	(0.0092)	(0.0218)
		[0.1505]	[0.1495]
Days of School (/100)	0.1955***	0.1175***	0.1184*
	(0.0436)	(0.0368)	(0.0641)
		[0.1235]	[0.1262]
Student-Teacher Ratio (/100)	-0.0999*	0.0270	0.0027
	(0.0594)	(0.0504)	(0.0636)
		[0.0284]	[0.0029]
Share of Secondary Teachers	0.0857***	0.0657***	0.0681***
	(0.0130)	(0.0109)	(0.0241)
		[0.0690]	[0.0726]
Share of White Teachers	0.0374***	0.0238**	0.0233**
	(0.0128)	(0.0106)	(0.0094)
		[0.0250]	[0.0248]
Share of Teachers Dismissed	-0.0892	-0.0487	-0.0648
	(0.0955)	(0.0778)	(0.0804)
		[-0.0512]	[-0.0691]
% Δ Enrollment, 1994-1999 (/10,000)	2.8204**	0.9897	1.0232
	(1.2134)	(1.0026)	(0.9781)
		[1.0401]	[1.0908]
Log Enrollment	0.0220***	0.0162***	0.0149***
	(0.0018)	(0.0016)	(0.0047)
		[0.0170]	[0.0159]
Share of White Students	-0.0256*	-0.0112	-0.0110
	(0.0114)	(0.0097)	(0.0165)

		[-0.0118]	[-0.0117]
Share of Low Income Students	-0.0373***	-0.0212**	-0.0204**
	(0.0109)	(0.0091)	(0.0090)
		[-0.0223]	[-0.0217]
Share HS Plus	-0.0653*	-0.0137	-0.0149
	(0.0364)	(0.0313)	(0.0699)
		[-0.0144]	[-0.0159]
Share BA Plus	0.1676***	0.1372***	0.1281***
	(0.0255)	(0.0217)	(0.0457)
		[0.1442]	[0.1366]
Share w/ Children<18	-0.2981***	-0.2057***	-0.2098***
	(0.0391)	(0.0333)	(0.0537)
		[-0.2162]	[-0.2236]
Share of Homeowners	0.0430**	-0.0056	-0.0037
	(0.0211)	(0.0179)	(0.0308)
		[-0.0059]	[-0.0039]
County Unemployment Rate	0.7432***	0.5516***	0.5306***
	(0.0891)	(0.0803)	(0.1097)
		[0.6918]	[0.6933]
R^2	.3162	.5487	.5458

Notes: Column 1 is estimated by OLS and columns 2 and 3 are estimated by GMM. The dependent variable is the log of the salary for teachers with 20 years of experience and a master's degree relative to the Comparable Wages Index. Standard errors are in parentheses and average marginal effects are in brackets for the spatial models. Standard errors in column 3 are clustered by state.

* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 5: Log Salary Regressions for BA0 with State Collective Bargaining Share

	1	2	3
Spatial Lag (ρ)		0.9168***	0.8552***
		(0.0364)	(0.0553)
Spatial Error (λ)		-0.3117***	
		(0.0166)	
State Collective Bargaining Share	0.0281***	0.0051*	0.0059
	(0.0045)	(0.0029)	(0.0052)
		[0.0457]	[0.0326]

Days of School (/100)	0.0643*	0.0303	0.0476
	(0.0344)	(0.0232)	(0.0307)
		[0.0452]	[0.0619]
Student-Teacher Ratio (/100)	-0.1303***	-0.0155	-0.0075
	(0.0469)	(0.0323)	(0.0317)
		[-0.0231]	[-0.0098]
Share of Secondary Teachers	0.0511***	0.0375***	0.0376**
	(0.0103)	(0.0071)	(0.0164)
		[0.0560]	[0.0489]
Share of White Teachers	-0.0028	-0.0014	-0.0003
	(0.0101)	(0.0071)	(0.0071)
		[-0.0021]	[-0.0004]
Share of Teachers Dismissed	-0.0129	-0.0562	-0.0596
	(0.0754)	(0.0542)	(0.0410)
		[-0.0839]	[-0.0775]
% Δ Enrollment, 1994-1999 (/10,000)	1.0546	-0.0315	0.2061
	(0.9580)	(0.6682)	(0.8212)
		[-0.0470]	[0.2682]
Log Enrollment	0.0092***	0.0024**	0.0036*
	(0.0014)	(0.0010)	(0.0019)
		[0.0036]	[0.0047]
Share of White Students	-0.0089	-0.0013	-0.0034
	(0.0090)	(0.0060)	(0.0074)
		[-0.0019]	[-0.0044]
Share of Low Income Students	-0.0056	-0.0017	-0.0047
	(0.0086)	(0.0060)	(0.0084)
		[-0.0025]	[-0.0061]
Share HS Plus	-0.1814***	-0.0370*	-0.0431
	(0.0287)	(0.0200)	(0.0265)
		[-0.0552]	[-0.0561]
Share BA Plus	0.0641***	0.0410***	0.0540**
	(0.0201)	(0.0134)	(0.0223)
		[0.0612]	[0.0703]
Share w/ Children<18	-0.2418***	-0.0944***	-0.1004**
	(0.0309)	(0.0223)	(0.0398)
		[-0.1409]	[-0.1306]
Share of Homeowners	0.0453***	-0.0209*	-0.0246
	(0.0166)	(0.0119)	(0.0186)
		[-0.0312]	[-0.0320]
County Unemployment Rate	0.4997***	0.1785***	0.2459***
	(0.0703)	(0.0445)	(0.0799)

		[0.4836]	[0.5093]
R^2	.0661	.4783	.5084

Notes: Column 1 is estimated by OLS and columns 2 and 3 are estimated by GMM. The dependent variable is the log of the salary for teachers with no experience and a bachelor's degree relative to the Comparable Wages Index. Standard errors are in parentheses and average marginal effects are in brackets for the spatial models. Standard errors in column 3 are clustered by state.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

The results in Table 4 also suggest that the share of districts with collective bargaining has a statistically significant effect on salaries of experienced teachers, with a marginal effect of 0.150 in column 3. This is more than three times the effect of collective bargaining in the third column of Table 2 suggesting that the spillover effects from collective bargaining are considerably larger than the direct effects. In results not shown, we also estimated regressions that simultaneously included both an indicator variable for collective bargaining in a district and the state share of districts with collective bargaining. In these regressions the effects on the indicator variable were virtually zero and not statistically significant, while the effects for the state collective bargaining share were virtually identical to the results in Tables 4 and 5. This suggests that being in a heavily unionized state has a much more important effect on teacher salaries than being in a district with collective bargaining.

For beginning teachers, the share of districts with collective bargaining is significant at the 10 percent level in column 2 of Table 5, with a marginal effect of 0.046. Clustering standard errors by state, however, the effect is no longer statistically significant. This suggests that the state collective bargaining share likely has at most a weak effect on the salaries of beginning teachers. The results for the additional explanatory variables in column 3 of Tables 4 and 5 are qualitatively similar to the corresponding results in Tables 2 and 3.

Measuring Union Activity by State Union Membership

Following Zwerling and Thomason (1995) we also explore measuring union activity by the percentage of teachers in a state who are members of a teacher union. Tables 6 and 7 present the results of re-estimating the equations in Tables 2 and 3 measuring union activity by state union membership. Like the share of districts in a state with collective bargaining, state union membership density incorporates union spillovers. These two measures, however, could produce different results. For example, the state membership density could have a stronger effect if it is a better measure of union strength. A union bargaining in a district in which a large percentage of the teachers are union members is likely to have more power in contract negotiations. Furthermore, union members may be more active politically, even in districts without a collective bargaining agreement. The votes of teachers can be quite important in state and local elections, especially in school board elections, where a relatively low percentage of the general population turns out to vote, but a larger percentage of teachers do (Moe 2006). When teachers are highly organized, school boards may feel significant pressure to concede higher salaries and other union demands.

Table 6: Log Salary Regressions for MA20 with State Union Membership Density

	1	2	3
Spatial Lag (ρ)		0.5375***	0.5807***
		(0.0469)	(0.1020)
Spatial Error (λ)		0.1356**	
		(0.0646)	
State Union Membership	0.3421***	0.1378***	0.1246***
	(0.0123)	(0.0207)	(0.0428)
		[0.2719]	[0.2682]

Days of School (/100)	0.3078***	0.1620***	0.1655**
	(0.0437)	(0.0374)	(0.0653)
		[0.1714]	[0.1773]
Student-Teacher Ratio (/100)	-0.0128	0.0656	0.0482
	(0.0599)	(0.0500)	(0.0718)
		[0.0694]	[0.0517]
Share of Secondary Teachers	0.0647***	0.0570***	0.0583**
	(0.0132)	(0.0108)	(0.0249)
		[0.0603]	[0.0625]
Share of White Teachers	0.0323**	0.0226**	0.0215**
	(0.0130)	(0.0106)	(0.0091)
		[0.0239]	[0.0230]
Share of Teachers Dismissed	-0.1216	-0.0581	-0.0751
	(0.0969)	(0.0782)	(0.0857)
		[-0.0615]	[-0.0805]
% Δ Enrollment, 1994-1999 (/10,000)	1.4630	0.4501	0.4269
	(1.2298)	(1.0014)	(0.9918)
		[0.4762]	[0.4576]
Log Enrollment	0.0174***	0.0141***	0.0127***
	(0.0018)	(0.0016)	(0.0041)
		[0.0149]	[0.0136]
Share of White Students	-0.0315***	-0.0121	-0.0119
	(0.0116)	(0.0098)	(0.0170)
		[-0.0128]	[-0.0128]
Share of Low Income Students	-0.0337***	-0.0207**	-0.0201**
	(0.0111)	(0.0091)	(0.0095)
		[-0.0219]	[-0.0215]
Share HS Plus	0.0517	0.0375	0.0385
	(0.0361)	(0.0306)	(0.0652)
		[0.0397]	[0.0413]
Share BA Plus	0.1259***	0.1184***	0.1105**
	(0.0258)	(0.0216)	(0.0442)
		[0.1253]	[0.1184]
Share w/ Children<18	-0.3114***	-0.2087***	-0.2141***
	(0.0397)	(0.0334)	(0.0546)
		[-0.2208]	[-0.2294]
Share of Homeowners	0.0313	-0.0152	-0.0129
	(0.0214)	(0.0178)	(0.0319)
		[-0.0161]	[-0.0138]
County Unemployment Rate	0.5914***	0.4903***	0.4747***
	(0.0909)	(0.0796)	(0.1207)

		[0.6270]	[0.6294]
R^2	0.2965	0.5428	0.5456

Notes: Column 1 is estimated by OLS and columns 2 and 3 are estimated by GMM. The dependent variable is the log of the salary for teachers with 20 years of experience and a master's degree relative to the Comparable Wages Index. Standard errors are in parentheses and average marginal effects are in brackets for the spatial models. Standard errors in column 3 are clustered by state.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

Table 7: Log Salary Regressions for BA0 with State Union Membership Density

	1	2	3
Spatial Lag (ρ)		0.9070***	0.8508***
		(0.0380)	(0.0562)
Spatial Error (λ)		-0.2996***	
		(0.0144)	
State Union Membership	0.0654***	0.0062	0.0070
	(0.0096)	(0.0065)	(0.0129)
		[0.0505]	[0.0377]
Days of School (/100)	0.0781**	0.0380*	0.0537
	(0.0340)	(0.0230)	(0.0342)
		[0.0551]	[0.0694]
Student-Teacher Ratio (/100)	-0.1233***	-0.0091	-0.0016
	(0.0466)	(0.0321)	(0.0328)
		[-0.0132]	[-0.0021]
Share of Secondary Teachers	0.0486***	0.0363***	0.0365**
	(0.0102)	(0.0071)	(0.0164)
		[0.0526]	[0.0472]
Share of White Teachers	-0.0042	-0.0014	-0.0002
	(0.0101)	(0.0072)	(0.0070)
		[-0.0020]	[-0.0003]
Share of Teachers Dismissed	-0.0198	-0.0562	-0.0597
	(0.0753)	(0.0544)	(0.0409)
		[-0.0814]	[-0.0772]
% Δ Enrollment, 1994-1999 (/10,000)	0.8386	-0.0720	0.1613
	(0.9563)	(0.6696)	(0.8241)
		[-0.1043]	[0.2085]
Log Enrollment	0.0085***	0.0022**	0.0034*
	(0.0014)	(0.0010)	(0.0019)
		[0.0032]	[0.0044]

Share of White Students	-0.0108	-0.0012	-0.0032
	(0.0090)	(0.0061)	(0.0075)
		[-0.0017]	[-0.0041]
Share of Low Income Students	-0.0036	-0.0025	-0.0053
	(0.0086)	(0.0061)	(0.0081)
		[-0.0036]	[-0.0068]
Share HS Plus	-0.1673***	-0.0320	-0.0374
	(0.0281)	(0.0195)	(0.0278)
		[-0.0464]	[-0.0483]
Share BA Plus	0.0573***	0.0403***	0.0526**
	(0.0201)	(0.0135)	(0.0228)
		[0.0584]	[0.0680]
Share w/ Children<18	-0.2424***	-0.0981***	-0.1023**
	(0.0308)	(0.0225)	(0.0397)
		[-0.1422]	[-0.1322]
Share of Homeowners	0.0462***	-0.0220*	-0.0260
	(0.0166)	(0.0121)	(0.0193)
		[-0.0319]	[-0.0336]
County Unemployment Rate	0.4655***	0.1837***	0.2466***
	(0.0707)	(0.0449)	(0.0813)
		[0.4706]	[0.5038]
R^2	0.0679	0.4777	0.5060

Notes: Column 1 is estimated by OLS and columns 2 and 3 are estimated by GMM. The dependent variable is the log of the salary for teachers with no experience and a bachelor's degree relative to the Comparable Wages Index. Standard errors are in parentheses and average marginal effects are in brackets for the spatial models. Standard errors in column 3 are clustered by state.
* Significant at 10%; ** Significant at 5%; *** Significant at 1%.

The spatial lag and spatial error results in Tables 6 and 7 measuring union activity by the state membership share are very similar to the corresponding estimates in Tables 4 and 5. The results for the non-union variables in column 3 are quite similar to the corresponding estimates in Tables 4 and 5 as well. For experienced teachers the state membership density has a significant marginal effect of 0.268 in column 3 of Table 6, but the effect for beginning teachers is insignificant. While the marginal effect for experienced teachers is larger than that in Table 4, we must also account for the fact that the state membership density is less dispersed than the state collective bargaining share to assess their relative impacts. The state collective bargaining share has a minimum value of zero, a maximum value of one, and a standard deviation of 0.410, while the state membership density ranges between 0.312 and 0.992 and has a standard deviation of 0.185. Therefore, according to the column 3 estimates in Tables 4 and 6 moving from a state with no collective bargaining to a state with complete collective bargaining coverage would increase salaries for experienced teachers by 16.1 percent, while moving from the state with the lowest membership density to the state with the greatest membership density would increase salaries for experienced teachers by 20.9 percent. Alternatively, moving from one standard deviation below the mean to one standard deviation above the mean of union activity increases salaries for experienced teachers by 13.2 percent for the collective bargaining share but only by 11.4 percent for the state membership density. Moving from the 25^{th} percentile to the 75^{th} percentile of union activity increases salaries for experienced teachers by 14.1 percent for the collective bargaining share and by 9.3 percent for the state membership density. Thus, although the two measures differ, their estimated impacts on the salaries of experienced teachers are both fairly large.

6. Conclusion

Using a national level dataset this paper has shown that salaries for both experienced and beginning teachers are considerably affected by teacher salaries in nearby districts, though the effect is larger for beginning teachers. Investigations of the determinants of teacher salaries that ignore spatial dependence are likely to be misspecified. The results of the spatial regressions suggest that a number of other important factors affect teacher salaries. The effect of unions on teacher salaries is given considerable attention in this paper. Accounting for union spillovers, we find that collective bargaining and union membership density in a state increase salaries for experienced teachers by as much as 16 and 21 percent, respectively, but the estimated effects on the salaries of beginning teachers are much smaller. Given the relatively weak bargaining position of beginning relative to experienced teachers within unions (i.e. potential members versus voting members), this result is not surprising. Although the median voter model explanation for this pattern of union wage effects appears most persuasive, we cannot rule out the possibility that wage-depressing monopsony effects among experienced teachers are being offset in union but not nonunion districts.

Appendix

Appendix Table A: Housing Characteristic Results for Log Rent Equation, 2006

Variable	Coefficient	Std. Error
Two Rooms	0.030*	0.016
Three Rooms	0.032*	0.017
Four Rooms	0.073***	0.017
Five Rooms	0.138***	0.017
Six Rooms	0.226***	0.018
Seven Rooms	0.309***	0.018
Eight Rooms	0.383***	0.019
Nine Rooms or More	0.348***	0.021
One Bedroom	0.103***	0.014
Two Bedrooms	0.308***	0.014
Three Bedrooms	0.358***	0.015
Four Bedrooms	0.380***	0.016
Five Bedrooms or More	0.284***	0.021
Built 1990-1999	-0.073***	0.005
Built 1980-1989	-0.160***	0.004
Built 1970-1979	-0.220***	0.004
Built 1960-1969	-0.252***	0.005
Built 1950-1959	-0.274***	0.005
Built 1940-1949	-0.288***	0.006
Built before 1940	-0.251***	0.005
Lives in Mobile Home or Trailer	-0.227***	0.016
Single-Family Home Detached	0.079***	0.015
Single-Family Home Attached	-0.007	0.016
Two-Unit Building	0.370***	0.047
3-4 Unit Building	0.333***	0.047
5-9 Unit Building	0.326***	0.047
10-19 Unit Building	0.363***	0.047
20-49 Unit Building	0.326***	0.047
50 Plus Unit Building	0.298***	0.047
House on Less than 10 Acres	0.409***	0.05
House on 10 Acres or More	0.254***	0.051
Kitchen Facilities	-0.115***	0.02
Plumbing Facilities	0.248***	0.022

Note: Standard errors are robust.
* Significantly different from zero at the 10% level. ** Significantly different from zero at the 5% level. *** Significantly different from zero at the 1% level.

Appendix Table B: Price Indices by City, 2006

	Baseline Price Index	Rent-based Index	Value-based Index
Albany-Schenectady-Amsterdam, NY CSA	109.5	108.3	105.4
Albuquerque, NM CBSA	102.8	101.1	101.0
Amarillo, TX CBSA	88.7	91.9	85.9
Anniston-Oxford, AL CBSA	92.8	92.7	89.7
Appleton-Oshkosh-Neenah, WI CSA	97.2	99.6	97.9
Asheville-Brevard, NC CSA	101.1	97.1	100.7
Atlanta-Sandy Springs-Gainesville, GA-AL CSA	98.3	105.8	99.7
Augusta-Richmond County, GA-SC CBSA	93.7	94.5	91.3
Austin-Round Rock, TX CBSA	96.0	107.0	98.4
Bakersfield, CA CBSA	111.4	105.2	119.6
Bangor, ME CBSA*	104.4	100.0	98.9
Baton Rouge-Pierre Part, LA CSA	96.7	96.8	93.2
Beaumont-Port Arthur, TX CBSA	93.1	94.2	86.3
Bellingham, WA CBSA	108.4	104.7	118.6
Bend-Prineville, OR CSA*	111.4	110.2	130.2
Birmingham-Hoover-Cullman, AL CSA	96.8	98.7	95.8
Bloomington, IN CBSA	97.6	98.9	92.8
Bloomington-Normal, IL CBSA	99.7	99.1	93.7
Boise City-Nampa, ID CBSA	98.1	100.6	102.4
Boston-Worcester-Manchester, MA-RI-NH CSA	131.1	123.2	139.3
Bowling Green, KY CBSA	93.7	92.2	88.8
Brownsville-Harlingen-Raymondville, TX CSA	88.0	87.8	83.3
Buffalo-Niagara-Cattaraugus, NY CSA	105.0	100.3	96.3
Burlington-South Burlington, VT CBSA	119.8	115.9	114.8
Cape Coral-Fort Myers, FL CBSA	108.9	112.7	121.2
Cedar Rapids, IA CBSA	93.5	94.4	92.4
Champaign-Urbana, IL CBSA	96.5	99.4	93.6
Charleston, WV CBSA	93.5	88.5	87.9
Charleston-North Charleston, SC CBSA	99.6	103.4	102.4
Charlotte-Gastonia-Salisbury, NC-SC CSA	92.0	98.2	95.5
Chattanooga-Cleveland-Athens, TN-GA CSA	93.9	90.6	91.4
Chicago-Naperville-Michigan City, IL-IN-WI CSA	113.1	112.5	115.5
Cincinnati-Middletown-Wilmington, OH-KY-IN CSA	94.2	95.6	93.9
Cleveland-Akron-Elyria, OH CSA	99.4	98.9	97.7
Colorado Springs, CO CBSA	95.3	102.7	100.1
Columbia, MO CBSA	92.2	95.4	91.3
Columbia-Newberry, SC CSA	94.3	97.4	92.9
Columbus-Auburn-Opelika, GA-AL CSA	95.7	98.1	94.3
Columbus-Marion-Chillicothe, OH CSA	103.3	101.1	98.5

Corpus Christi-Kingsville, TX CSA	88.8	97.2	85.9
Dallas-Fort Worth, TX CSA	94.0	104.4	94.1
Davenport-Moline-Rock Island, IA-IL CBSA	96.6	94.7	92.0
Dayton-Springfield-Greenville, OH CSA	94.5	96.2	93.4
Decatur, IL CBSA	90.8	90.8	85.5
Denver-Aurora-Boulder, CO1 CSA	102.1	105.4	107.6
Des Moines-Newton-Pella, IA CSA	93.3	95.0	89.7
Detroit-Warren-Flint, MI CSA	105.1	101.6	100.1
Dover, DE CBSA	100.3	103.7	100.9
Eau Claire-Menomonie, WI CSA	94.6	92.7	92.3
El Paso, TX CBSA	92.5	91.9	88.5
Erie, PA CBSA	97.8	95.4	91.9
Eugene-Springfield, OR CBSA	110.0	105.2	112.9
Evansville, IN-KY CBSA*	96.3	92.0	91.0
Fargo-Wahpeton, ND-MN CSA	95.3	91.8	89.4
Farmington, NM CBSA	97.6	94.0	100.7
Fayetteville, NC CBSA	99.8	100.8	95.5
Fayetteville-Springdale-Rogers, AR-MO CBSA	91.6	93.0	92.7
Florence-Muscle Shoals, AL CBSA	88.2	87.3	86.2
Fort Collins-Loveland, CO CBSA	103.5	105.6	110.0
Fort Smith, AR-OK CBSA	87.9	87.9	84.2
Fort Walton Beach-Crestview-Destin, FL CBSA	98.1	104.6	106.7
Fort Wayne-Huntington-Auburn, IN CSA	92.0	93.3	86.7
Fresno-Madera, CA CSA	122.0	107.9	124.4
Gainesville, FL CBSA	96.0	101.4	102.7
Grand Rapids-Muskegon-Holland, MI CSA	103.0	99.1	98.4
Green Bay, WI CBSA	95.3	97.4	95.9
Greensboro--Winston-Salem--High Point, NC CSA	92.4	94.5	93.0
Greenville-Spartanburg-Anderson, SC CSA	92.7	92.8	92.1
Gulfport-Biloxi-Pascagoula, MS CSA	95.9	99.1	94.3
Hagerstown-Martinsburg, MD-WV CBSA*	94.8	94.0	97.7
Harrisburg-Carlisle-Lebanon, PA CSA	103.3	100.7	98.6
Harrisonburg, VA CBSA	106.6	97.6	104.6
Hartford-West Hartford-Willimantic, CT CSA	118.3	112.8	120.0
Hickory-Lenoir-Morganton, NC CBSA	97.8	92.8	93.6
Houston-Baytown-Huntsville, TX CSA	90.1	102.0	91.3
Huntsville-Decatur, AL CSA	91.6	94.2	91.1
Indianapolis-Anderson-Columbus, IN CSA	97.0	97.9	92.2
Jacksonville, FL CBSA	97.1	92.9	87.1
Jacksonville, NC CBSA*	95.7	103.2	103.2
Jackson-Yazoo City, MS CSA	91.6	96.4	93.4
Janesville, WI CBSA	98.2	97.4	96.0
Johnson City-Kingsport-Bristol (Tri-Cities), TN-VA	89.6	86.0	87.4

CSA			
Johnstown, PA CBSA*	93.2	88.6	85.3
Joplin, MO CBSA	84.2	84.1	80.3
Kalamazoo-Portage, MI CBSA	98.6	95.8	93.8
Kansas City-Overland Park-Kansas City, MO-KS CSA	95.5	99.3	94.4
Killeen-Temple-Fort Hood, TX CBSA	91.1	95.8	88.4
Knoxville-Sevierville-La Follette, TN CSA	88.9	88.7	88.7
Lafayette-Acadiana, LA CSA	97.5	93.2	91.0
Lake Charles-Jennings, LA CSA	95.5	92.2	88.8
Lancaster, PA CBSA	109.4	105.8	103.9
Laredo, TX CBSA	84.0	86.4	80.4
Las Cruces, NM CBSA	100.2	94.6	97.3
Las Vegas-Paradise-Pahrump, NV CSA	110.1	109.2	115.3
Lawrence, KS CBSA	94.9	98.4	94.6
Lawton, OK CBSA*	90.0	88.8	84.6
Lexington-Fayette--Frankfort--Richmond, KY CSA	96.6	92.7	92.2
Little Rock-North Little Rock-Pine Bluff, AR CSA	91.7	93.0	89.1
Longview-Marshall, TX CSA	88.6	91.1	85.4
Los Angeles-Long Beach-Riverside, CA CSA	148.3	128.4	158.5
Louisville-Jefferson--Elizabethtown--Scottsburg, KY-IN CSA	97.6	97.5	97.5
Lubbock-Levelland, TX CSA	86.7	95.1	85.0
Macon-Warner Robins-Fort Valley, GA CSA	94.9	94.3	91.4
McAllen-Edinburg-Mission, TX CBSA	85.5	87.1	82.1
Memphis, TN-MS-AR CBSA	94.1	98.7	91.6
Miami-Fort Lauderdale-Miami Beach, FL CBSA	117.2	118.1	125.0
Midland-Odessa, TX CSA	88.7	94.1	84.0
Milwaukee-Racine-Waukesha, WI CSA	101.2	102.7	104.6
Minneapolis-St. Paul-St. Cloud, MN-WI CSA	101.9	107.4	108.7
Mobile-Daphne-Fairhope, AL CSA	92.1	95.4	92.6
Montgomery-Alexander City, AL CSA	96.8	94.7	89.4
Myrtle Beach-Conway-Georgetown, SC CSA	94.8	97.2	99.9
Nashville-Davidson--Murfreesboro--Columbia, TN CSA	94.8	98.9	97.0
New Orleans-Metairie-Bogalusa, LA CSA	97.2	105.6	99.0
New York-Newark-Bridgeport, NY-NJ-CT-PA CSA	151.8	132.0	149.8
Norwich-New London, CT CBSA	119.0	116.1	121.3
Oklahoma City-Shawnee, OK CSA	92.0	95.4	89.6
Omaha-Council Bluffs-Fremont, NE-IA CSA	89.6	95.5	89.0
Orlando-Deltona-Daytona Beach CSA	104.8	111.0	111.4
Panama City-Lynn Haven, FL CBSA	97.4	106.3	110.2
Pensacola-Ferry Pass-Brent, FL CBSA	96.8	100.0	98.1
Peoria-Canton, IL CSA	97.7	96.1	94.1

Philadelphia-Camden-Vineland, PA-NJ-DE-MD CSA	122.7	117.0	116.0
Phoenix-Mesa-Scottsdale, AZ CBSA	102.2	106.8	112.8
Pittsburgh-New Castle, PA CSA	95.9	94.3	90.2
Port St. Lucie-Sebastian-Vero Beach, FL CSA	104.2	108.5	114.2
Portland-Lewiston-South Portland, ME CSA*	120.2	106.2	115.0
Portland-Vancouver-Beaverton, OR-WA CBSA	113.6	111.4	120.9
Prescott, AZ CBSA	109.8	106.1	119.6
Pueblo, CO CBSA	90.4	90.6	90.0
Raleigh-Durham-Cary, NC CSA	95.7	101.8	98.6
Reno-Sparks-Fernley, NV CSA	110.6	111.0	126.8
Richmond, VA CBSA	109.0	105.1	104.4
Roanoke, VA CBSA	92.0	94.7	93.3
Rochester-Batavia-Seneca Falls, NY CSA	102.3	105.7	97.9
Sacramento--Arden-Arcade--Truckee, CA-NV CSA*	123.8	116.1	135.9
Salt Lake City-Ogden-Clearfield, UT CSA	100.0	103.1	100.9
San Antonio, TX CBSA	92.7	97.5	89.5
San Diego-Carlsbad-San Marcos, CA CBSA	148.3	133.8	164.2
San Jose-San Francisco-Oakland, CA CSA	157.9	141.8	184.8
Sarasota-Bradenton-Punta Gorda, FL CSA	103.4	110.6	118.2
Savannah-Hinesville-Fort Stewart, GA CSA	102.2	103.0	102.2
Seattle-Tacoma-Olympia, WA CSA	114.1	114.9	127.0
Shreveport-Bossier City-Minden, LA CSA	93.3	92.7	89.3
Sioux Falls, SD CBSA	92.5	94.3	90.8
South Bend-Elkhart-Mishawaka, IN-MI CSA	94.2	94.2	89.6
Spokane, WA CBSA	98.5	102.3	100.7
Springfield, IL CBSA	92.8	95.3	89.2
Springfield, MO CBSA	92.9	93.1	90.8
St. Louis-St. Charles-Farmington, MO-IL CSA	97.0	97.9	95.1
Stockton, CA CBSA*	121.9	113.5	134.2
Syracuse-Auburn, NY CSA	100.8	101.8	96.3
Tampa-St. Petersburg-Clearwater, FL CBSA	100.0	106.4	107.8
Toledo-Fremont, OH CSA	98.1	95.3	94.9
Topeka, KS CBSA	91.0	93.6	87.9
Tucson, AZ CBSA	99.6	103.0	108.1
Tulsa-Bartlesville, OK CSA	91.1	95.0	90.5
Tuscaloosa, AL CBSA	95.2	97.8	96.1
Valdosta, GA CBSA	93.2	92.7	91.0
Virginia Beach-Norfolk-Newport News, VA-NC CBSA	105.7	104.4	105.9
Waco, TX CBSA	89.7	92.3	86.2
Washington-Baltimore-Northern VA, DC-MD-VA-WV CSA	133.4	121.5	128.6
Waterloo-Cedar Falls, IA CBSA	92.5	90.9	88.1

Wausau-Merrill, WI CSA	94.0	94.7	91.0
Wichita-Winfield, KS CSA	94.9	94.7	88.4
York-Hanover-Gettysburg, PA CSA	101.5	97.9	98.0
Youngstown-Warren-East Liverpool, OH-PA CSA	94.4	89.9	88.6

*Indicates ACCRA data are not available for 2005.

Appendix Table C: Variables and Data Sources

Variable	Data Source
Log Wage	Current Population Survey
Worker Characteristics	Current Population Survey
Baseline Price Index	ACCRA
Rent-based Modified Price Index	American Community Survey & ACCRA
Housing Value-based Modified Price Index	American Community Survey & ACCRA
Quality-Adjusted Gross Rents	American Community Survey
Quality-Adjusted Housing Values	American Community Survey
Non-housing Prices	ACCRA
Gulf Coast	Consulted Map
Atlantic Coast	Consulted Map
Pacific Coast	Consulted Map
January Temperature	ERS Natural Amenities Scale
July Temperature	ERS Natural Amenities Scale
January Sun	ERS Natural Amenities Scale
July Humidity	ERS Natural Amenities Scale
% Water Area	ERS Natural Amenities Scale
Topography 2	ERS Natural Amenities Scale
Topography 3	ERS Natural Amenities Scale
Topography 4	ERS Natural Amenities Scale
Topography 5	ERS Natural Amenities Scale
Precipitation	Cities Ranked and Rated
Snow	Cities Ranked and Rated
Violent Crime	USA Counties Website
Property Crime	USA Counties Website
Mean Commute Time	American Community Survey
Ozone	EPA AirData Database
Particulate Matter (2.5)	EPA AirData Database
Census Division Indicators	Assigned According to Census Geography
City Size Indicators	Population Estimates according to Census Bureau

Appendix Table D: Additional 2SLS Regression Results for Preferred Specification

Variable	Coefficient	Std. Error
Log Rent-Based Modified Index, 2006	0.994***	0.106
Mean Commute Time	0.122	0.273
Gulf Coast	0.002	0.015
Atlantic Coast	-0.028*	0.016
Pacific Coast	-0.034	0.035
Precipitation	0.102	0.075
Snow	-0.029	0.028
January Temperature	-0.296***	0.104
July Temperature	0.184	0.217
January Sun	-0.008	0.019
July Humidity	-0.001	0.072
% Water Area	-0.140***	0.036
Topography 2	-0.009	0.008
Topography 3	0.006	0.013
Topography 4	-0.009	0.013
Topography 5	-0.024	0.019
Violent Crime	6.227***	2.195
Property Crime	0.230	0.588
Particulate Matter (2.5)	0.733***	0.279
Ozone	-1.428	1.001
Middle Atlantic	-0.047***	0.016
East North Central	0.021	0.021
West North Central	0.000	0.020
South Atlantic	0.007	0.020
East South Central	-0.013	0.025
West South Central	0.028	0.025
Mountain	0.073**	0.032
Pacific	0.037	0.048
Size 2: 200,000-299,999	-0.005	0.019
Size 3: 300,000-499,999	-0.009	0.021
Size 4: 500,000-999,999	0.002	0.017
Size 5: 1,000,000-1,999,999	0.031*	0.018
Size 6: 2,000,000-4,999,999	0.041**	0.019
Size 7: 5,000,000 and over	0.027	0.025
9 Years of Schooling	0.029**	0.012
10 Years of Schooling	0.075***	0.014
11 Years of Schooling	0.094***	0.020
12 Years of Schooling, No Diploma	0.118***	0.013
12 Years of Schooling, HS Diploma or GED	0.203***	0.014

GED	-0.062***	0.011
Some College	0.294***	0.017
Associate's Degree	0.371***	0.016
Bachelor's Degree	0.542***	0.019
Master's Degree	0.674***	0.021
Professional Degree	0.977***	0.024
Doctorate Degree	0.862***	0.035
Experience	0.049***	0.003
Experience2	-0.002***	0.000
Experience3	0.000***	0.000
Experience4	-0.000***	0.000
Female	-0.161***	0.004
Black	-0.129***	0.006
Asian	-0.036***	0.011
Hispanic	-0.126***	0.009
Other	-0.077***	0.012
Married	0.081***	0.004
Employed Part-time	-0.114***	0.006
Naturalized Citizen	-0.072***	0.009
Non-Citizen	-0.182***	0.014
Enrolled Part-time in School	0.025	0.015
Union Member	0.152***	0.008
Federal Government Employee	0.315***	0.044
State Government Employee	0.038	0.044
Local Government Employee	0.067	0.043
Non-profit Sector Employee	-0.065***	0.011
Mining Industry	0.505***	0.075
Construction Industry	0.215***	0.046
Manufacturing Industry	0.256***	0.042
Wholesale or Retail Trade Industry	0.074*	0.040
Transportation or Utilities Industry	0.252***	0.041
Information Industry	0.271***	0.043
Finance, Insurance, and Real Estate Industry	0.260***	0.042
Professional and Business Services Industry	0.216***	0.042
Education and Health Services Industry	0.121***	0.041
Hospitality Industry	0.013	0.042
Other Services Industry	0.044	0.043
Management Occupation	0.349***	0.009
Professional Specialty Occupation	0.246***	0.012
Service Occupation	0.027***	0.009
Sales Occupation	0.163***	0.010

Administrative Occupation	0.070***	0.008
Farming, Fishing, and Forestry Occupation	0.054	0.039
Construction and Extraction Occupation	0.149***	0.011
Installation, Maintenance, and Repair Occupation	0.170***	0.010
Production Occupation	0.026**	0.012

Notes: Standard errors are clustered by CBSA/CSA. Regression results are for the preferred specification in the first column of Table 4 in which the log of the rent-based price index is instrumented using log gross rents from the previous year. See the text and Table 4 for further information.
* Significantly different from zero at the 10% level. ** Significantly different from zero at the 5% level. *** Significantly different from zero at the 1% level.

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