Wage Premia in Employment Clusters: How Important Is Worker Heterogeneity?

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This article tests whether the correlation between wages and concentration of employment can be explained by unobserved worker productivity. Residential location is used as a proxy for unobserved productivity, and average commute time to workplace is used to test whether location-based productivity differences are compensated away by longer commutes. Analyses using confidential data from the 2000 Decennial Census find that estimates of agglomeration wage premia within metropolitan areas are robust to comparisons within residential location and that estimates do not persist after controlling for commuting costs, suggesting that the productivity differences across locations are due to location, not individual unobservables.

I. Introduction

The strong correlation between wages and the concentration of economic activity has often been cited as evidence of agglomeration economies, but this correlation may also arise because highly productive workers

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prefer locations with high levels of economic activity. In this article, a standard wage model is used to test for wage premia in agglomerated locations, except that a worker's residential location is used as a proxy for his or her unobservable productivity, under the premise that workers sort across residential locations based in part on their permanent incomes or innate labor market productivity. Further, in a locational equilibrium, identical workers should receive equal compensation, and therefore similar workers facing the same housing prices should receive the same wage net of commuting costs. The conceptual experiment is to compare two observationally equivalent individuals who reside in the same location and work in locations with different levels of agglomeration. Does the individual who works in the high-agglomeration location earn a higher wage, suggesting higher productivity at that work location, and if so, does he or she also have a sufficiently longer commute so that the two workers receive the same real wage, suggesting that the workers indeed have similar innate productivity?

A central feature of most agglomeration models is that agglomeration raises productivity. Since firms pay workers the value of their marginal product in competitive labor markets, a natural test for agglomeration economies is whether firms pay a wage premium in areas with concentrated economic activity.[1] Glaeser and Maré (2001), Wheeler (2001), Rosenthal

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[1] Studies of agglomeration use a wide variety of approaches, including examining productivity (Ciccone and Hall 1996; Henderson 2003), employment (Glaeser et al. 1992; Henderson, Kuncoro, and Turner 1995), establishment births and relocations (Carlton 1983; Duranton and Puga 2001; Rosenthal and Strange 2003), coagglomeration of industries (Dumais, Ellison, and Glaeser 2002; Ellison, Glaeser, and Kerr 2010), product innovation (Audretsch and Feldman 1996; Feldman and Audretsch 1999), and land rents (Rauch 1993; Dekle and Eaton 1999). Also see Audretsch and Feldman (2004), Duranton and Puga (2004), Moretti (2004), and Rosenthal and Strange (2004) for detailed surveys of the literature on agglomeration economies and production externalities within cities.

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and Strange (2006), Yankow (2006), Fu (2007), Combes, Duranton, and Gobillon (2008), and Di Addario and Patacchini (2008) all find that wages are higher in large labor markets with high concentrations of employment. Many of these studies also find a positive link between wages and the human capital level associated with an employment concentration.[2]

A classic question in this literature is whether productivity is intrinsically higher in locations with high employment concentrations or whether high-quality workers have simply sorted into those areas.[3] Glaeser and Maré (2001), Wheeler (2001), Yankow (2006), and Combes et al. (2008) find evidence of an urban wage premium using longitudinal data, but worker fixed effects do explain a substantial portion of the raw correlation between agglomeration and wages. These studies often find that wages grow faster in larger urban areas, potentially due to faster accumulation of human capital.[4] The obvious limitation of this approach is that the relationship between agglomeration and wages is identified by the small fraction of people who move from one metropolitan area to another and those moves likely occur in response to attractive opportunities.

Our article proposes a new strategy that avoids relying on movers by drawing explicitly on a well-established feature of urban economies. Specifically, a worker's residential location is used as a proxy for his or her unobservable productivity attributes. This article estimates wage premia across work locations in the same metropolitan area and examines whether these within-area-work-location wage premia are robust to the inclusion of residential location fixed effects.[5] This research design draws on the commonly accepted premise that individuals sort over residential locations based on tastes, which are partially unobservable and correlated with worker productivity.[6] For example, workers with higher productivity know

[2] Other studies, Wheaton and Lewis (2002), Fu (2007), and Combes et al. (2008), find evidence that wages increase with concentrations of employment in an individual's own occupation or industry.

[3] Another major concern is that individual places with unobservables that contribute to higher productivity attract economic activity so that high place-specific productivity contributes to agglomeration rather than the other way around (Ciccone and Hall 1996; Henderson 2003). Regardless, wage-based studies tend to focus on bias from sorting of workers across workplaces. In this context, our analysis might be considered a test of worker sorting versus place-specific productivity differences defined more broadly.

[4] The most compelling evidence behind the human capital accumulation story is provided by Glaeser and Maré (2001), who find that workers who migrate away from large metropolitan areas retain their earnings gains.

[5] Rosenthal and Strange (2006) also examine agglomeration effects on wages within metropolitan areas, but their primary focus is on the attenuation of these economies over space.

[6] A huge literature documents the fact that households are stratified across neighborhoods in part based on income. Gabriel and Rosenthal (1999) directly examine the effect of household sorting on wage models, Bayer, McMillan, and

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that they can expect a higher lifetime income, and therefore they are likely to have a greater willingness to pay for neighborhood amenities. Workers residing in similar-quality locations should have similar levels of productivity, and after controlling for residential location, those workers should earn similar wages unless their employment locations create productivity differences.[7]

As an additional test, we recognize that, in equilibrium, equivalent workers should have the same utility level even if they work in different locations. After controlling for commuting time, workers residing in the same neighborhood should be indifferent between jobs in different locations, even if location contributes to higher productivity and therefore higher nominal wages. Rational workers sort into locations with higher wages until congestion increases commuting time, eroding the real value of the high nominal wage.[8] Specifically, the coefficient on agglomeration in a model of wages net of commuting costs captures differences in returns to unobservable worker attributes. Therefore, an estimate of zero for the agglomeration coefficient in a wage net of commuting cost model implies that there are no productivity differences between observationally similar workers who work in locations with different agglomeration.[9]

We use a sample of individuals residing in mid-sized to large metropolitan areas from the confidential data of the 2000 US Decennial Census long form and estimate the relationship between the concentration of employment in their workplace (employment location) and their wages, controlling for individual attributes plus metropolitan area fixed effects. We find agglomeration effects that are comparable in size to earlier estimates. The agglomeration estimates are unchanged by the use of residential location fixed effects to control for unobserved worker productivity, and our estimates suggest that a one standard deviation increase in agglomeration raises log wages by 0.033. The robustness of our agglomeration estimates to the inclusion of residential fixed effects is consistent

Rueben (2004) estimate models of household sorting over neighborhoods based on race and income, and Epple and Sieg (1999) estimate models of household sorting over communities based on income.

[7] This strategy is also similar to Dale and Krueger (2002), which conditions on the set of colleges to which students applied and whether they were accepted or rejected and then compares outcomes of students with similar choices and acceptances.

[8] Timothy and Wheaton (2001) examine the capitalization of commutes into wages. Earlier wage gradient studies include Madden (1985), Ihlanfeldt (1992), McMillen and Singell (1992), and Ihlanfeldt and Young (1994).

[9] While this compensation logic has been applied to study quality of life (Roback 1982; Gyouko, Kahn, and Tracy 1999; Albouy 2008, 2009) and across metropolitan wage differences (Davis, Fisher, and Whited 2009; Glaeser and Gottlieb 2009), this logic has not been applied to agglomeration economies within metropolitan areas, even though within-metropolitan mobility is substantially higher than mobility across metropolitan areas (Ross 1998).

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with the small estimated within metropolitan area correlation between agglomeration and our observable measure of productivity, education.[10] Further, we find small or zero effects of agglomeration on wages net of commute time when assuming reasonable commuting costs of two times the wage rate, suggesting no differences in real wages. Similar findings arise for the effect of the average education level in a work location on wages.

A key concern with our fixed effect approach is that residential location may provide an imperfect control for unobserved worker quality because sorting is imperfect. We allow for sorting on factors other than permanent income and directly calculate the bias using an errors-in-variables framework. We demonstrate that the inclusion of residential fixed effects significantly reduces bias in our agglomeration estimates and leads to attenuation of the estimated coefficients on observed human capital variables. Empirically, we examine the estimated coefficients on the education variables and find that the estimates are attenuated by the inclusion of the residential controls. Further, attenuation increases substantially as residential controls are refined to smaller geographic units capturing more unobservables, and yet our agglomeration estimates are very stable, suggesting little bias from worker heterogeneity in the original ordinary least squares (OLS) estimates. In addition, our results are robust in models that drop all individual covariates, which should exacerbate bias if imperfect sorting is a serious concern. Finally, our wage net of commute time model explicitly tests whether workers earn a greater real wage, indicating differences in ability, and we find only small wage differences for reasonable commuting costs.

This article is organized as follows. Section II presents our residential location fixed effects empirical methodology and presents our errors-in-variables analysis. Sections III and IV describe the data and present the fixed effect estimates, including estimates using alternative geography and alternative fixed effect definitions. Section V presents our wage net of commute time model, and Section VI extends the empirical model to include controls for the education level of workers. Section VII concludes.

II. Residential Fixed Effects Methodology

The model imposes the standard assumption that firms pay workers their marginal revenue product and differences in nominal wages capture the returns to higher productivity in agglomerated work locations. The

[10] The across–metropolitan area correlation between education and agglomeration is substantially larger than the within-metropolitan correlation, suggesting a substantial across-metropolitan correlation between ability and agglomeration, which is consistent with the large declines in the agglomeration estimates found by Glaeser and Maré (2001) and Combes et al. (2008) from the inclusion of individual fixed effects.

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logarithm of individual i's wage (y_{ij}) in work location j is

$$y_{ij} = \beta X_i + \gamma Z_j + \alpha_i + \varepsilon_{ij}, \tag{1}$$

where X_i is a vector of individual observable attributes, Z_j is employment concentration in the employment location j, α_i is an individual-specific random effect that captures heterogeneity in labor market productivity but that is uncorrelated with X_i,[11] and ε_{ij} is a random error that allows current wage to differ from permanent income or earnings capacity. If individuals sort over employment locations based on their permanent income ($\beta X_i + \alpha_i$), or tastes that are correlated with productivity, the unobserved component of productivity α_i will be correlated with Z_j or

$$E[Z_j \alpha_i] \neq 0,$$

biasing estimates of γ. Typically, the concern is that high-ability individuals sort into high-agglomeration locations, biasing the estimates of agglomeration effects on wages upward.

A. Residential Location as a Proxy for Worker Unobservables

Our proposed solution is based on the simple idea that individuals sort into residential locations based on their unobservables, and therefore one can minimize unobservable differences between workers by comparing individuals who reside in the same location. The properties of residential sorting models with taste unobservables have been well established by Epple and Platt (1998), Epple and Sieg (1999), and Bayer and Ross (2006). Specifically, these models imply perfect stratification, so that if individuals sort across residential locations based solely on a common measure of location quality (W_k) and their demand for location quality, then each residential location k will contain workers in a continuous interval of location quality demand.

If we assume demand depends on permanent income, which depends on worker's innate productivity ($\beta X_i + \alpha_i$),[12] worker productivity will be

[11] The assumption that X_i and α_i are uncorrelated can be made without loss of generality by considering β as representing the reduced form relationship between observables and wages. Specifically, let κ_i be the true unobserved productivity that correlates with X_i, and assume that the conditional expectation of κ_i can be written as a linear function λX_i. Under those conditions, the expectation of eq. (1) may be written as follows: $E[y_{ij} - \gamma Z_j | X_i] = \beta X_i + \kappa_i = \beta X_i + E[\kappa_i | X_i] + (\kappa_i - E[\kappa_i | X_i]) = (\beta + \lambda) X_i + \alpha_i$, yielding a reduced-form model specification, where $E[\kappa_i | X_i]$ represents that bias λ and α_i is orthogonal to X_i by construction because κ_i in the last term has been differenced by its conditional expectation.

[12] For example, see Gabriel and Rosenthal (1999), Bayer et al. (2004), and Epple and Sieg (1999) for evidence of individuals and households systematically sorting across neighborhoods and communities based on wages or income.

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monotonic in location quality, or in other words, locations can be ordered so that if

$$W_k < W_{k+1}$$

for location k, then in equilibrium

$$\delta_k < \beta X_i + \alpha_i < \delta_{k+1}$$

for all individuals i residing in location k, where δ_k is assumed to be less than δ_{k+1} for any k. If there are a large number of residential choices, then

$$\delta_k \approx \beta X_i + \alpha_i. \tag{2}$$

Figure 1 illustrates this partial equilibrium sorting pattern where a band of individuals with similar permanent income $\beta X_i + \alpha_i$ reside in the same community and these groups are monotonically ordered by permanent income over K communities of increasing attractiveness. The slanted lines represent loci of boundary individuals who all have the same permanent income and are indifferent between the neighborhoods on either side of a locus.

Under these assumptions, consistent estimates of γ can be obtained by substituting equation (2) into equation (1) and estimating the following equation:

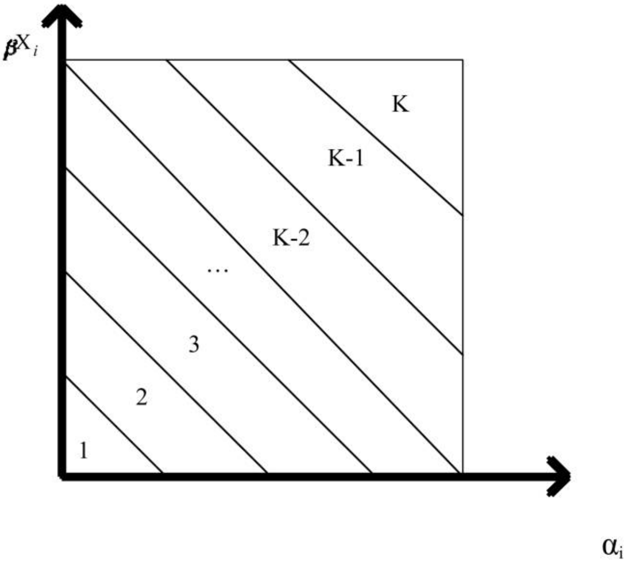

FIG. 1.—Sorting equilibrium

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$$y_{ijk} = \delta_k + \gamma Z_j + \varepsilon_{ijk}, \tag{3}$$

where δ_k might be captured by residential location fixed effects. Workers in the same residential location are assumed to have identical productivity, and so unexplained wage differences across workers in the same residential location must reflect aspects of productivity associated with work location, such as agglomeration, rather than worker unobservables.[13]

B. Imperfect Neighborhood Sorting and Errors-in-Variables

The assumption of complete sorting based on permanent income or innate ability implies that the residential location fixed effects fully capture individual productivity. Such a strong assumption seems unrealistic since residential location choice is influenced by tastes that are unlikely to be perfectly correlated with permanent income, and in practice observed human capital variables, like education, have strong predictive power in our wage equations even after controlling for residential location fixed effects. The predictive power of human capital variables rejects the implications of equation (3).

Therefore, the empirical model is extended to allow the residential location fixed effect δ_k to differ from the productivity of an individual residing in k by an unobservable (μ_i):

$$\delta_k = \beta X_i + \alpha_i + \mu_i. \tag{4}$$

For example, μ_i may represent individual tastes for neighborhood quality that are independent of productivity or permanent income. Initially, we will assume that μ_i is uncorrelated with $\beta X_i + \alpha_i$, but we will examine the effect of relaxing this assumption in our numeric calculations.

This heterogeneity leads to a classic errors-in-variables problem. This result is easily observed by substituting equation (4) into equation (1), yielding

$$y_{ijk} = \delta_k + \gamma Z_j + (\varepsilon_{ij} - \mu_i), \tag{5}$$

where δ_k is positively correlated with μ_i by construction. The reader should note that μ_i represents tastes and only enters equation (5) as measurement error because the fixed effect contains μ_i.

The negative correlation between the fixed effect δ_k and the error $(\varepsilon_{ij} - \mu_i)$ will attenuate the estimates of δ_k toward zero. Given the assumption that Z_j is positively correlated with worker ability ($\beta X_i + \alpha_i$), the estimate of γ

[13] Our residential fixed effects model is related to traditional control function estimators. Blundell and Dias (2009) define a control function estimator based on the conditional orthogonality of model observables and unobservables. Our inclusion of residential fixed effects absorbs α_i so that Z_j is orthogonal to the remaining unobservable.

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continues to be biased upward since worker ability is imbedded in the fixed effect and the associated correlation between Z_j and δ_k biases the coefficient on Z_j upward. Intuitively, the attenuated fixed effect estimates provide only a partial control for $\beta X_i + \alpha_i$, and potentially the estimates might be improved by directly including X_i in the location fixed effect model specification:

$$y_{ijk} = \beta X_i + \delta_k + \gamma Z_j + (\varepsilon_{ij} - \mu_i). \tag{6}$$

Further, given that α_i is unobserved, the estimate of β, the coefficient vector for observable human capital variables, conditional on residential fixed effects, will be attenuated relative to the OLS estimates from equation (1). As illustrated in figure 1, two individuals with different X_i's residing in the same neighborhood or community are likely to have different α's; otherwise, they would have had different permanent incomes and would have chosen different neighborhoods. This selection process into neighborhoods creates a negative correlation between X_i and α_i within any residential location (Gabriel and Rosenthal 1999; Bayer and Ross 2006), attenuating the estimated coefficients on the human capital variables. This bias, however, provides a metric for assessing whether the residential location fixed effects successfully capture individual unobserved productivity. Specifically, the estimated coefficients on human capital variables in the OLS and residential fixed effects models can be compared, and if the inclusion of fixed effects reduces the estimated coefficients, then the residential fixed effects have captured variation associated with unobserved productivity attributes.[14]

The problem described above involves bias arising from errors-in-variables with multiple correlated regressors. Given the complexity of this problem, we turn to numeric calculations of the bias in estimated parameters in order to confirm the intuition discussed in the preceding paragraphs. Specifically, we manually calculate the formulas for the omitted bias in each parameter, use our data to estimate the variances and covariances for key

[14] One might ask whether such attenuation could be explained by measurement error in our education variables given the common perception that measurement error is exacerbated by the inclusion of fixed effects. The answer is yes and no. The attenuation bias from measurement error is exacerbated by the inclusion of fixed effects only when the fixed effects can systematically explain variation in the control variable, in this case our observable measure of productivity—education. Therefore, one must ask why the residential fixed effects are correlated with observable productivity, presumably sorting, and then ask whether the fixed effects should not be also correlated with unobservable aspects of productivity. While some of the attenuation in parameter estimates may be due to increased attenuation from measurement error in education, this attenuation likely can only arise due to a correlation between residential location and productivity variables, and so supports our claim that the increased attenuation is evidence that our fixed effects provide a proxy for productivity in wage regressions.

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observables, and then calculate the bias in our key parameters. Our calculations are conducted for three specifications:

$$y_{ij} = X_i + Z_j + \alpha_i + \varepsilon_{ij}, \tag{7a}$$

$$y_{ijk} = \delta_k + Z_j + (\varepsilon_{ij} - \mu_i), \tag{7b}$$

$$y_{ijk} = 0X_i + \delta_k + Z_j + (\varepsilon_{ij} - \mu_i). \tag{7c}$$

Equation (7a) is a traditional estimation that is biased by the omission of unobserved productivity variables. Equations (7b) and (7c) incorporate individual productivity unobservables by including residential location fixed effects, but they suffer from bias due to errors-in-variables that arise because residential sorting is driven in part by factors unrelated to total productivity. The "true" coefficient on X_i in (7c) is zero because total productivity is captured by δ_k. The resulting estimates, however, will be nonzero because X_i is correlated with the location fixed effect, which in turn is biased due to the errors-in-variables term μ_i arising from imperfect sorting.

Without loss of generality, all coefficients are initialized to one, and the impact of a variable on wages is captured by the standard deviation of the variable. Again, without loss of generality, we assume that the correlation between X_i and α_i is zero,[15] and for our baseline case we also assume that the correlation between $(X_i + \alpha_i)$ and μ_i is zero. For the baseline model, the variances of X_i and α_i are initialized to one. The variance of Z_j is set to 0.051, based on comparing the standardized estimates of employment density from the fixed effects wage equation (shown later in the article in table 4) to the standardized influence of the worker education variables on wages. The standardized influence of education is calculated using a constructed education index, which is created by multiplying the value of each education dummy variable by its estimated coefficient in the same residential fixed effect model. The correlation between Z_j and $(X_i + \alpha_i)$ is set to 0.040, which is the conditional correlation between employment density and the education index variable.[16] Finally, the variance of the residential location taste unobservable is set to three in order to match the observed

[15] See note 11 for a precise discussion of the assumption that X_i and α_i are uncorrelated.

[16] The standardized coefficients on employment density in our fixed effects model (see table 4) is approximately 0.225 times the standard deviation of the education index. The standardized effect and correlations are based on conditioning out other individual controls except education and metropolitan area fixed effects.

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attenuation of the estimates on the human capital variables of approximately 25% when residential fixed effects are included in the model.[17]

Table 1 presents the expectation for parameter estimates, or the sum of the true value plus the bias, using standard omitted variable calculations.[18] Panel A presents the expectations of estimates given the baseline variances and correlations described above, and the following panels present expectations after changing one of the variance-covariance terms. The baseline results show that the OLS estimate of 1.177 in column 1 is biased above the true value of one. The bias on the agglomeration variable is actually increased by replacing observable human capital measures with residential location fixed effects (col. 2). This increase arises from the high variance assigned the taste unobservable, and bias is decreased between equations (7a) and (7b) in models where that variance of μ_i is less than two. Nonetheless, column 3 illustrates that the bias is reduced by approximately 25% relative to OLS by inclusion of residential fixed effects into a model that controls for observable productivity or human capital (7c). Finally, looking at the second row of panel A, the attenuation in the coefficient estimate on human capital is about 0.25 as calibrated to be consistent with attenuation in our empirical models.

[17] The attenuation of the coefficients for educational attainment dummy variables is between 22% and 23% in the initial model that controls for census tract fixed effects, and attenuation increases to 24%–26% with block group fixed effects and to 26%–29% with housing submarket by census tract fixed effects.

[18] The expected value of parameter estimates can be calculated using the underlying model rather than the more typical least squares calculations, which require a specification for the fixed effects model such as the inclusion of residential location dummy variables. Rather, the expected value of wages conditional on the fixed effect model is

$$E[y_{ijk}|\delta_k,\ Z_j] = \delta_k + Z_j - E[\mu_{ik}|\delta_k,\ Z_j],$$

and the expectation of the unobservable can be expressed as a linear function of the fixed effect and an orthogonal regressor if expectations are assumed to be a linear function of conditioning variables:

$$\begin{aligned} E[\mu_{ik}|\delta_k,\ Z_j] &= \alpha_0 + \alpha_1\delta_k + \alpha_2(Z_j - E[Z_j|\delta_k]) + \alpha_2 E[Z_j|\delta_k] \\ &= (\alpha_0 + \alpha_2\gamma_0) + (\alpha_1 + \alpha_2\gamma_1)\delta_k + \alpha_2(Z_j - E[Z_j|\delta_k]), \end{aligned}$$

where α_2 captures the bias in the coefficient on Z_j, but this bias involves a conditional expectation, $E[Z_j|\delta_k] = \gamma_0 + \gamma_1\delta k$. In order to calculate the bias in terms of unconditional moments, we recognize that $\gamma_1 = \mathrm{Cov}\,[Z_j,\ \delta_k]/\mathrm{Var}[\delta_k]$ and $(\alpha_1 + \alpha_2\gamma_1) = \mathrm{Cov}\,[\mu_{ik}, \delta_k]/\mathrm{Var}[\delta_k]$, and then reversing the process yields an equivalent coefficient on Z_j in a model where the other regressor is orthogonal. The resulting two equations can be solved for the bias, and the results are identical to the results of the least-squares omitted variable calculation in the case where one actually observes the true fixed effect and can include it as a regressor.

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Table 1
Calculation of the Expectation of Parameter Estimates

Parameters	Ordinary Least Squares	Residential Fixed Effect	Residential Fixed Effect + Observables
A. Baseline:			
Agglomeration	1.177	1.213	1.133
Human capital	.998		.749
B. Larger correlation between agglomeration and ability:			
Agglomeration	1.356	1.427	1.268
Human capital	.993		.746
C. Lower variation associated with agglomeration:			
Agglomeration	1.251	1.301	1.188
Human capital	.998		.749
D. Higher variation associated with ability:			
Agglomeration	1.251	1.285	1.188
Human capital	.998		.749
E. Lower variation in preferences:			
Agglomeration	1.177	1.194	1.125
Human capital	.998		.705
F. Correlation between preferences and ability:			
Agglomeration	1.177	1.252	1.157
Human capital	.998		.753

NOTE.—The cells contain the true value of the parameter plus the calculated bias based on the models specified in eqq. (7a)–(7c). The baseline calculations are based on a variance of X_i and α_i of one, a variance of Z_j of 0.051, a variance of t_{jk} of 0.084, a correlation between Z_j and $(X_i + \alpha_i)$ of 0.034, and a variance of μ_i of three. All baseline values are preserved in the following panels except for the specific variance or correlation being modified in the panel. Changes in panels B through D are always by a factor of two, in panel E the variation is reduced from 3.0 to 2.4 in order to increase the attenuation in the human capital estimate to 30%, and in panel F the correlation between μ_i and $(X_i + \alpha_i)$ is 15%. In panels C and F, the variance of the residential preference parameter increases from 3.0 to 6.0 in order to keep the attenuation of human capital variables in model 3 approximately constant.

While the magnitude of the bias changes with the variance and covariance terms, the basic pattern of results remains the same. Increasing the correlation between individual productivity (both observed and unobserved) and agglomeration (panel B), decreasing the relative contribution of agglomeration to wages (panel C), or increasing the contribution of unobserved ability (panel D) all increase the bias in agglomeration estimates.[19] All changes are by a factor of two, with the change in the correlation having the largest effect on bias. Nonetheless, in all cases, the inclusion of fixed effects in a model with human capital controls (col. 3) continues to reduce the bias in the agglomeration estimate by approxi-

[19] Note that we also increase the variance of the taste unobservable when we increase the variance of unobserved ability in panel D by a factor of two in order to recalibrate the attenuation on the human capital estimate.

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mately 25%. Panel D reduces the variance associated with unobserved preferences for residential location to 2.4 in order to match the attenuation of the education variable estimates of 30% arising with more restrictive fixed effects. This change improves the fixed effect estimates as expected, reducing the bias in column 3 by approximately 30% relative to OLS.

Finally, in panel F, we allow for a positive correlation between residential location tastes μ_i and permanent income $(X_i + \alpha_i)$. The reader should note that this positive correlation further attenuates the estimates on observable human capital X_i. Given that we calibrate our calculations to the attenuation observed in our empirical analysis, this correlation is linked to the variance of μ_i. We set the correlation to 0.15, which obtains the proper attenuation of about 25% when the variance of μ_i is doubled to six.[20] The resulting calculations yield a very similar pattern to the previous panels: with residential fixed effect estimates in column 2 lying above the OLS estimates and the estimates for column 3 with observables included lying below. The reduction in bias in column 3 is only 11%, substantially below the reduction in the earlier panels, but this result should not be surprising because the correlation implies much larger variation associated with tastes so that the residential fixed effects are less informative.

These calculations confirm the key assertions above. The inclusion of residential fixed effects into a model that controls for observable productivity leads to a substantial reduction in the bias in agglomeration estimates. We also confirm that the coefficient estimate on human capital attenuates with the inclusion of residential fixed effects to control for unobserved worker productivity. Further, sensitivity analyses confirm that the reduction in bias is quite stable over parameter values except for the variance of unobserved location preferences, where a decrease (increase) in variance results in a larger (smaller) reduction in the bias associated with the agglomeration variable.

III. Sample and Data

The models in this article are estimated using the confidential data from the long form of the 2000 US Decennial Census. The sample provides detailed geographic information on individual residential and work location. A subsample of prime-age (30–59 years of age), full-time (usual hours worked per week 35 or greater), male workers is drawn for the 49 Consolidated Metropolitan and Metropolitan Statistical Areas (MSAs) that have one

[20] This relationship effectively places an upper bound on the correlation between μ_i and $(X_i + \alpha_i)$ in order to maintain credible values for the variance of residential tastes that are unrelated to permanent income. For example, if the correlation is 0.30, the variance of μ_i must be set to 21 in order to match the observed attenuation on education.

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million or more residents.[21] These restrictions lead to a sample of 2,234,092 workers.

The dependent variable, logarithm of wage, uses a wage that is calculated by dividing an individual's 1999 labor market earnings by the product of number of weeks worked in 1999 and usual number of hours worked per week in 1999. The wage rate model includes a standard set of controls, including age, race/ethnicity, educational attainment, marital status, presence of children in household, immigration status, industry, occupation,[22] and metropolitan area fixed effects. The model includes controls for share of college-educated employees in a worker's industry or occupation at the metropolitan level.[23] The mean and standard deviations for these variables are shown in table 2 separately for the college-educated and non-college-educated subsamples.

We consider two alternative variables to capture employment concentration: the number of workers employed in an employment location and the employment density.[24] Models are estimated controlling for residential location and employment concentration at a variety of levels of geographical aggregation. Our preferred specification defines residential locations at the census tract level; employment location at the residential Public Use Microdata Area (PUMA) level, where residential PUMAs are defined based on having a minimum of 100,000 residents; and measures agglomeration using employment density. For clarity, it is important to realize that the census also defines a Workplace Public Use Microdata Area (WP-PUMA). These areas are often larger than PUMAs and are less attractive for our purposes because they are defined inconsistently across metropolitan areas, with WP-PUMAs being identical to most PUMAs in some areas but being much larger (sometimes encompassing the entire central county of an area) in others. Finally, additional specifications are

[21] This sample is comparable to the sample drawn from the Public Use Microdata Sample (PUMS) of the 2000 Census by Rosenthal and Strange (2006), except that we explicitly restrict ourselves to considering residents of mid-sized and large metropolitan areas.

[22] Workers are classified into 20 major occupation codes and 15 major industry codes.

[23] These controls are similar in spirit to controls used by Glaeser and Maré (2001) for occupation education levels nationally. The industry, occupation, and Metropolitan Statistical Area (MSA) fixed effects do not absorb as much variation as the MSA-occupation cell fixed effects used by Rosenthal and Strange (2006). Given our use of residential tract fixed effects, it is not feasible to simultaneously include MSA-occupation fixed effects. However, the models without residential fixed effects have been reestimated with MSA-occupation fixed effects, and results were similar. Further, models including MSA-occupation fixed effects were estimated for some subsamples based on a small number of very large MSAs, and all findings are robust.

[24] The agglomeration variables are constructed using all full-time workers, not just the prime-age male workers present in the regression sample.

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Table 2
Variable Names, Means, and Standard Deviations

Variable Name:	Non–College Graduates	College Graduates
Dependent variable:		
Average hourly wage	20.103	35.987
	(30.828)	(55.428)
Workplace controls:		
Total residential PUMA employment (in 1,000,000s)	.0488	.0641
	(.0575)	(.0759)
PUMA employment density (in 10,000s/square kilometer)	.2646	.4772
	(1.1004)	(1.5306)
Share of college-educated workers in PUMA	.358	.405
	(.094)	(.101)
Average commute time to PUMA in minutes	26.573	28.195
	(6.629)	(7.787)
Metropolitan area controls:		
Percent college-educated in MSA and occupation	.026	.056
	(.035)	(.045)
Percent college-educated in MSA and industry	.033	.051
	(.028)	(.035)
Individual worker controls:		
Age of worker	42.580	43.024
	(7.980)	(8.076)
Non-Hispanic white worker	.672	.813
	(.470)	(.390)
African American worker	.126	.058
	(.332)	(.233)
Hispanic worker	.159	.043
	(.365)	(.204)
Asian and Pacific Islander worker	.042	.084
	(.200)	(.278)
High school degree	.346	
	(.476)	
Associate degree	.488	
	(.500)	
Four-year college degree		.599
		(.490)
Master degree		.255
		(.436)
Degree beyond master		.146
		(.353)
Worker single	.285	.230
	(.452)	(.421)
Presence of own children in household	.474	.502
	(.499)	(.500)
Born in the United States	.800	.826
	(.400)	(.379)
Years in residence if not born in the United States	18.574	17.432
	(10.809)	(11.669)
Quality of spoken English	.164	.168
	(.370)	(.374)
Sample size	1,415,176	927,916

NOTE.—Means and standard deviations are for a sample of 2,343,092 observations containing all male full-time workers aged 30–59 in the metropolitan areas with populations of over one million residents, where full-time worker is defined as having worked an average of at least 35 hours per week. PUMA= Public Use Microdata Area; MSA = Metropolitan Statistical Area. Standard deviations are shown in parentheses.

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estimated that control for average commute time for the PUMA in which employment is located or the fraction of workers employed in the PUMA who have a college degree or above. All standard errors are clustered by workplace.

IV. Results

Table 3 presents the results for a baseline wage model of agglomeration economies using controls for total employment or employment density at the PUMA level. The estimates on the control variables are quite standard and are stable across the two specifications. Based on these estimates, adding 10,000 workers to an employment location is associated with a 0.54% increase in wages, while an increase in employment density of 1,000 workers per square kilometer is associated with a 0.24% increase in wages.[25]

A. Fixed Effect Estimates

Panel A of table 4 contains the OLS and census tract fixed effect estimates for the baseline model. In the fixed effect model, the positive relationship between agglomeration and wages is robust to the inclusion of location controls, which increases the similarity of individuals over which the effect of agglomeration is identified. In fact, including residential fixed effects has little impact on the estimated coefficients on agglomeration, with the estimate falling by 6.6% in the total employment model and actually increasing slightly in the employment density model. This small bias from sorting on unobservables is consistent with the evidence of sorting on observable human capital variables. The within-metropolitan-area correlation between worker education level and employment density after controlling for other observables is quite small: 0.034 for our education index,[26] 0.029 for whether a worker has at least a 4-year college degree, and 0.019 for whether a worker has a high school diploma or above.

Of course, one explanation for finding little or no bias from sorting is that our residential location fixed effects do not successfully capture worker unobserved productivity. As discussed earlier, if the residential location fixed effects provide effective controls for individual productivity unobservables, the inclusion of location controls should bias the coefficient estimates on human capital toward zero. We find evidence of attenuation

[25] Rosenthal and Strange (2006) estimate models using the Public Use Microdata sample and controlling for total employment within spatial rings of employment estimated from workplace PUMAs. Our estimated magnitudes using total employment in workplace PUMAs are comparable to theirs.

[26] This index was created for the correlation estimates used for our errors-in-variables bias calculations.

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Table 3
Baseline Model of Agglomeration Economies for Logarithm of the Wage Rate

Independent Variable	Total Employment	Density
Total employment (in 1,000,000s)	.544	
	(.070)	
Employment density (in 10,000s per square kilometer)		.024
		(.003)
Percent college-educated in MSA and occupation	.876	.924
	(.138)	(.141)
Percent college-educated in MSA and industry	1.713	1.752
	(.167)	(.164)
Age of worker	.033	.033
	(.001)	(.001)
(Age of worker)^2/100	−.037	−.037
	(.001)	(.001)
Non-Hispanic white worker	.138	.137
	(.005)	(.005)
African American worker	−.006	−.006
	(.005)	(.005)
Hispanic worker	−.015	−.016
	(.005)	(.005)
Asian and Pacific Islander worker	.036	.036
	(.006)	(.006)
High school degree	.138	.138
	(.002)	(.002)
Associate degree	.224	.225
	(.003)	(.003)
Four-year college degree	.422	.424
	(.004)	(.004)
Master degree	.543	.546
	(.005)	(.005)
Degree beyond master	.661	.665
	(.005)	(.005)
Worker single	−.135	−.135
	(.001)	(.001)
Presence of own children in household	.072	.072
	(.002)	(.002)
Born in the United States	−.056	−.056
	(.003)	(.003)
Years in residence if not born in the United States	.009	.009
	(.0001)	(.0001)
Quality of spoken English	.014	.014
	(.003)	(.003)
R^2	.291	.290

Note.—The dependent variable for all regressions is the logarithm of the estimated hourly wages, which is calculated as annual labor market earnings divided by the product of number of weeks worked and average hours worked per week. The key variable of interest is either the total number of full-time workers in an individual's workplace based on residential PUMA (Public Use Microdata Area) or the density of full-time workers in the workplace where full-time worker is defined as worked an average of at least 35 hours per week. The sample of 2,343,092 observations contains male full-time workers aged 30–59 in the selected MSA (Metropolitan Statistical Areas). The models include metropolitan area, 15 industry, and 20 occupation fixed effects, but those estimates are suppressed. Standard errors clustered at the employment location are shown in parentheses.

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Table 4
Agglomeration Wage Models without and with Location Controls

Variable	Total Employment: Ordinary Least Squares	Total Employment: Fixed Effects	Density: Ordinary Least Squares	Density: Fixed Effects
A. Baseline model specification:				
Employment	.544	.508		
	(.070)	(.052)		
Density			.024	.026
			(.003)	(.003)
B. Logarithm of employment and density:				
Employment	.516	.432		
	(.023)	(.024)		
Density			.181	.202
			(.015)	(.015)
C. No individual-level covariates:				
Employment	.544	.575		
	(.078)	(.062)		
Density			.022	.029
			(.003)	(.004)
D. Sample of single men:				
Employment	.409	.368		
	(.055)	(.045)		
Density			.018	.018
			(.002)	(.003)
E. Observationally equivalent cells:				
Employment	.569	.533		
	(.068)	(.059)		
Density			.025	.028
			(.003)	(.004)

NOTE.—The OLS columns in panel A contain the results from table 3, the fixed effect columns contain the results for the same model where metropolitan fixed effects are replaced by census tract of residence fixed effects. Panel B presents estimates controlling for the logarithm of total employment or employment density. Panel C presents estimates for a specification where all individual-worker covariates (as listed in table 2) are excluded, panel D presents estimates for a sample of single men, and panel E presents estimates based on a model that controls for worker cell by census tract fixed effects. Panels A through C and panel E are based on the same sample of 2,343,092 observations, while panel D is based on the subsample of single men with 617,144 observations. Standard errors clustered at the employment location (Public Use Microdata Areas [PUMAs]) are shown in parentheses.

bias for both models. In the density model, the inclusion of residential fixed effects reduces the estimates on above master degree, master degree, 4-year college degree, associate degree, and high school diploma from 0.665, 0.546, 0.424, 0.225, and 0.138 to 0.511, 0.424, 0.330, 0.175, and 0.108, respectively, a reduction of between 22% and 23% in all coefficients.[27]

Our agglomeration economies are quite reasonable and are comparable in magnitude to simple OLS estimates arising from comparisons across

[27] Attenuation of estimates in the total employment and density models is virtually identical.

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metropolitan areas.[28] Specifically, we find that a one standard deviation increase in metropolitan-wide employment or employment density increases the logarithm of wages by 0.062 and 0.044, respectively. Meanwhile, in the census tract fixed effect model, a one standard deviation in workplace total employment or density leads to an increase in log wages of 0.033 and 0.034, which is between half and three-quarters of the traditional across-metropolitan wage premium.

In addition, in panel B of table 4, we examine a wage model that controls for the logarithm of the agglomeration variables, converting the estimated effects into elasticities. The pattern of estimates in panel B is nearly identical to the pattern for the baseline estimates shown in panel A of table 4, and the estimates imply that a doubling of agglomeration economies based on total employment or density is associated with a 4.3% and 2.0% increase in wages in the fixed effects models, respectively, which bracket Combes, Duranton, and Gobillon's (2001) elasticity estimate of 3% after controlling for individual fixed effects.

Further, in panel C, we examine the effect of increasing the bias from unobserved ability by restricting the number of individual controls. Specifically, we reestimate the models in panel A dropping all individual covariates, including the education, age, and family structure variables. The R-squareds fall substantially from 0.29 to 0.20 in the OLS model with the omission of these human capital variables. However, the within-metropolitan-area OLS estimates of agglomeration economies are essentially unchanged, at 0.544 for total employment and 0.022 for employment density. The location fixed effects estimates increase somewhat from 0.508 to 0.575 for total employment and from 0.026 to 0.029 for employment density; these are relatively small increases from the omission of so much information on worker productivity.[29]

In addition, in panels D and E, we examine the effect of basing our estimates on more homogenous comparisons. First, the sample is restricted to single male workers. This population of workers is less likely to have their residential location decision influenced by marital and family obligations. The pattern of estimates is very similar. For example, both the OLS and residential fixed effects employment density estimates are 0.018.[30] In panel E, we organize the sample into cells of observationally equivalent

[28] We estimate the same wage model controlling for metropolitan total employment or the metropolitan-wide employment density, as well as regional fixed effects.

[29] We also experimented with models that do not contain the industry and occupation fixed effects, and the pattern and magnitude of estimates was again very similar.

[30] The decline in estimated agglomeration effects for the sample of single male workers is not driven by marital status. Rather, single male workers are younger and have less education on average than married males, and our estimated

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individuals based on discrete variables for age, race/ ethnicity, education, family structure, and immigration status,[31] and we control for cell by census tract fixed effects so that our estimates are truly based on comparing very similar individuals who reside in the same location. As in panel D, agglomeration estimates are not affected by the inclusion of residential location controls.[32]

B. Alternative Workplace and Residential Location Definitions

Table 5 presents estimates using two additional employment location definitions. As discussed above, the PUMA is defined by the census to contain approximately 100,000 residents. The largest alternative workplace definition is the Workplace Public Use Microdata Area (WP-PUMA). There are approximately 25% more PUMAs than WP-PUMAs in our sample. We also examine models where agglomeration is measured at the zip code, and our sample contains about six times as many zip codes as residential PUMAs. The average areas of these geographies are 846, 183, and 7 square miles for WP-PUMAs, PUMAs, and zip codes, respectively.[33] Residential fixed effects are included at the census tract level.

The standardized estimates are the largest for PUMA, suggesting that measurement error might be worse for smaller workplace definitions and for the more idiosyncratically defined WP-PUMAs, but the estimates for the other geographies are sizable and statistically significant. The pattern of results across columns is remarkably similar except for two minor differences. When workplace PUMA is used to measure agglomeration, the inclusion of fixed effects leads to an increase in the agglomeration estimate of 17%. For the PUMA and zip code models, estimates are fairly stable, changing by only about 8% with the inclusion of fixed effects.

Table 6 presents estimates based on alternative geographic definitions of residential location. The largest neighborhood definition is the residential PUMA, with estimates shown in panel A, followed by estimates based

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agglomeration effect increases moderately with an individual's level of human capital. In addition, we estimated models for single and married workers separately by education level and found similar results.

[31] Households are divided by three age, five race, six education, four family structure (based on presence of children by marital status), and three immigration categories (based on whether born in the United States and time in the United States if not), allowing for a total of 1,080 possible cells.

[32] From this point forward, we only present estimates using employment density, but estimates using total employment are very similar.

[33] The areas of PUMAs and zip codes appear reasonable. Rosenthal and Strange (2006) examine wage effects within rings of the number of full-time workers between 0 and 5 miles of a location (78 square miles) and find effects that are four to 10 times larger than the effects found between 5 and 25 miles. Fu (2007) finds that attenuation is fairly flat within 6 miles for human capital externalities and flat within 3 miles for employment density.

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Table 5
Employment Density Models with Alternative Workplace Definitions

Variable	Ordinary Least Squares	Fixed Effects
A. Workplace PUMA:		
Density	.063	.074
	(.015)	(.016)
Standardized density	.025	.029
B. PUMA:		
Density	.024	.026
	(.003)	(.003)
Standardized density	.031	.034
C. Zip code tabulation area:		
Density	.013	.012
	(.003)	(.003)
Standardized density	.030	.027

Note.—The workplace geography for each panel is used to calculate employment density for the models presented in that panel. The estimates in panel B contain the results from table 4, where workplace is defined based on residential Public Use Microdata Areas (PUMAs). Panel A defines workplace using the larger workplace PUMAs, and panel C using the five-digit census-defined zip code tabulation areas. Estimates on employment density are scaled or standardized using the within-metropolitan-area standard deviation of that variable for the specific geography. The standard deviations for employment density are 0.391, 1.292, and 2.284 for the workplace PUMA, residential PUMA, and zip code tabulation area, respectively. All fixed effect models include census tract of residence fixed effects. The models include the standard covariates shown in table 2, and estimates are based on the full sample of 2,343,092 observations for panels A and B and on 2,132,986 observations with a successful zip code place of employment match for panel C. Standard errors clustered at the employment location are shown in parentheses.

Table 6
Employment Density Models with Alternative Residential Neighborhood Definitions

Variable	Ordinary Least Squares	Fixed Effects
A. PUMA density	.024	.028
	(.003)	(.003)
B. Zip code tabulation area density		.027
		(.003)
C. Census tract density		.026
		(.003)
D. Census block density		.026
		(.003)

Note.—The residential neighborhood geography for each panel is used to define the residential location fixed effects. The estimates in panel C contain the results from table 4, where fixed effects are defined using census tracts. Panel A defines the fixed effects using residential PUMAs, panel B using the five-digit census-defined zip code tabulation areas, and panel D using census block groups. All models define employment density based on workplace at the PUMA level. The models include the standard covariates shown in table 2, and they use the full sample of 2,343,092 observations. Standard errors clustered at the employment location are shown in parentheses.

on the smaller zip codes in panel B. Census tracts are even smaller, with populations between 1,500 and 8,000 (panel C), and block groups are smaller still, with populations between 600 and 3,000 (panel D). The fixed effect estimates of agglomeration are nearly identical across the four panels. However, the attenuation in the estimates on education variables, which in-

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dicates the degree to which the fixed effects can capture unobserved ability, varies dramatically. The inclusion of residential PUMA fixed effects leads to an attenuation of 8%–12%, while zip codes lead to 15%–17%, census tracts to 22%–23%, and block groups to 24%–26% declines in estimated education coefficients. The fixed effects that capture more detailed spatial resolution lead to greater attenuation, presumably capturing a more homogeneous population on productivity in these smaller neighborhoods, and yet these different fixed effects produce very similar agglomeration estimates.[34]

C. Improving the Residential Location Controls

In this section, we consider expanded fixed effect models that better control for unobserved heterogeneity. Ortalo-Magné and Rady (2006) find substantial heterogeneity among homeowners within neighborhoods but considerable homogeneity among renters and among homeowners who moved into the neighborhood at similar times. Presumably, renters and recent homeowners chose this neighborhood based on current prices and neighborhood amenities and therefore are similar, while homeowners who moved to the neighborhood in earlier years chose this neighborhood based on different prices and amenities. Alternatively, one physical residential location might be divided into different submarkets based on the type of housing stock. For example, an individual who resides in a small loft in an apartment building may be very different from someone who selects a large single-family dwelling in the same residential location.

In order to address these concerns, we develop residential location fixed effects by tenure in residence and by housing stock categories. For the tenure of residence fixed effect model, a full set of tract fixed effects are created for each of the following categories: renters; owners who have been residing in the neighborhood for less than 1 year, between 1 and 5 years, and for more than 5 years. For the housing stock model, tract fixed effects are created for each of seven housing stock categories: mobile home, multifamily with one bedroom or less, multifamily with two bedrooms, multifamily with three bedrooms or more, single family with two or fewer bedrooms, single family with three bedrooms, and single family with four or more bedrooms. The results are shown in table 7. The expansion of the residential fixed effects has little impact on the estimated agglomeration effects. Further, both fixed effects significantly increase the attenuation of the coefficient estimates on the human capital variables, from between 22% and 23% with census tract fixed effects to 26%–29%, while leaving the agglomeration estimates unchanged.

[34] We return to census tract fixed effects for the rest of this article to facilitate comparison to the earlier results. Regardless, the substantive results of this article are robust to any of the geographies considered in this section.

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Table 7
Employment Density Models with Alternative Neighborhood Fixed Effects

Variable	Ordinary Least Squares	Fixed Effects
A. Census tract fixed effects density	.024	.026
	(.003)	(.003)
B. Census tract by tenure in residence fixed effects density		.025
		(.003)
C. Census tract by housing stock fixed effects		.025
		(.003)

NOTE.—All models use workplace agglomeration at the PUMA level. The models in panel A control for census tract fixed effects. The models in panel B control for tenure-based fixed effects that include a unique fixed effect for each of four tenure categories in each census tract. The models in panel C control for housing stock fixed effects that include a unique fixed effect for each housing stock category in each census tract. The four tenure categories are renter, owner in residence less than 1 year, owner in residence between 1 and 5 years, and owner in residence more than 5 years. The seven housing stock categories are mobile home, multifamily with one bedroom or less, multifamily with two bedrooms, multifamily with three bedrooms or more, single family with two or fewer bedrooms, single family with three bedrooms, and single family with four or more bedrooms. The models include the standard covariates shown in table 2, and standard errors clustered at the employment location are shown in parentheses. Sample size = 2,343,092.

V. Model of Wages Net of Commuting Costs

The second component of our strategy for testing whether the estimated value of γ is biased by unobserved differences in worker productivity draws upon the locational equilibrium requirement that no workers desire to change either their residential or employment locations. Observationally equivalent workers residing in the same location should earn the same wages net of commute or the same real wage unless some workers have higher productivity.[35] Under the assumption that the urban economy is in equilibrium, we attribute systematic differences in wages net of commuting costs across work locations to the sorting of individuals. Accordingly, a finding of no systematic relationship between real wages and agglomeration in a model of wages net of commuting costs is consistent with a zero correlation between agglomeration and unobserved differences in worker productivity.[36]

Formally, locational equilibrium requires that

$$U(y_j,\ P_k,\ V_{jk}) = U(y_{j'},\ P_k,\ V_{j'k}), \tag{8}$$

[35] Gabriel and Rosenthal (1996) and Petitte and Ross (1999) apply a similar logic to study the welfare effects of residential segregation by testing whether African Americans had longer commutes after including residential and/or employment location fixed effects.

[36] The residential fixed effect model assumes sorting based on permanent income, and so temporary work location wage premia do not influence sorting. This assumption is not required when commute times compensate for wage differences, because in that case real wages (net of commute costs) do not depend on work location.

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where U is the indirect utility function of a type of individuals who resides in location k and is observed in both employment locations j and j', P_k is the price per unit of housing services in location k, and V_{jk} is the commuting time or cost between locations k and j. Ogawa and Fujita (1980) and Fujita and Ogawa (1982) consider a simple model of the urban economy with production externalities (agglomeration economies) and commuting, where work hours and land consumption are fixed. In this model, the equilibrium condition in equation (8) requires that wages net of commuting costs must be the same across all employment locations j conditional on a worker's residential location. Specifically,

$$U(y_j - \eta V_{jk},\ P_k) = U(y_{j'} - \eta V_{j'k},\ P_k) \text{ or } y_j - \eta V_{jk} = y_{j'} - \eta V_{j'k} \tag{9}$$

over all work locations j and j', where η is the per mile or minute commuting costs.[37] Note that wages net of commute costs or real wages in this context are constant across work locations even though agglomeration economies exist as reflected by nominal wage differences across locations.

If a locational equilibrium holds on average across residential and employment locations, we can impose the following condition:

$$\eta t_{jk} = \gamma_0 Z_j + \xi_{jk}, \tag{10}$$

where t_{jk} is the commute time between locations j and k as a share of the average workday and η is the cost of commuting as a fraction of the wage rate. As in the previous section, $\gamma_0 Z_j$ captures the true wage premium arising in employment location j, which must in equilibrium be compensated by longer commutes, and ξ_{jk} is a stochastic error term. Note that if commuting increases the workday by 1%, the wages for time spent at work would need to increase by 1% in order to just compensate the worker at his wage for the time spent commuting.

Subtracting equation (10) from (6) yields

$$(y_{ijk} - \eta t_{jk}) = \beta X_i + \delta_k + (\gamma - \gamma_0) Z_j + (\varepsilon_{ij} - \mu_i) - \xi_{jk}. \tag{11}$$

If ξ_{jk} is orthogonal to Z_j, γ represents the reduced form fixed effect estimate from equation (6), and γ_0 captures the agglomeration wage premium for which commutes compensate. Accordingly, an estimate of zero on Z_j in equation (11) implies that the agglomeration wage premium that was estimated in equation (6) is fully compensated by commutes. On the other hand, a positive estimate for $(\gamma - \gamma_0)$ is consistent with agglomeration

[37] See Ross and Yinger (1995) and Ross (1996) for examples of this locational equilibrium condition in a traditional monocentric urban model with endogenous housing demand. In fact, eq. (9) will hold in any model where either leisure does not enter preferences or total work hours including commute time are fixed.

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economy estimates from equation (6) that are biased upward by workers sorting based on their unobserved ability.

While we have no direct information on the correlation between ξ_{jk} and Z_j in equation (10), our key concern, workers sorting based on their productivity, does not naturally give rise to such a correlation. High-ability workers may sort into high-agglomeration locations that require long commutes to reach, but this sorting will not affect the wage premia required to assure that equivalent workers are indifferent between employment locations. We will revisit possible sources of correlation between ξ_{jk} and Z_j later in this section.

Further, we have information on the variance of ξ_{jk} and its covariance with X_i under the assumption that the correlation between ξ_{jk} and Z_j is zero. Specifically, equation (10) implies

$$\eta^2 \mathrm{Var}\,[t_{jk}] = \gamma_0^2 \mathrm{Var}\,[Z_j] + \mathrm{Var}[\xi_{jk}], \tag{12a}$$

$$\eta \mathrm{Cov}[t_{jk},\ X_i] = \gamma_0 \mathrm{Cov}\,[Z_j,\ X_i] + \mathrm{Cov}[\xi_{jk},\ X_i]. \tag{12b}$$

As in our earlier errors-in-variables calculations, the variance of the standardized effect of agglomeration on wages in our fixed effect regression is 0.051. The variance of the standardized effect of commute time from a similar regression is 0.084. The conditional correlation of our education index with employment density is 0.040, and with commute time it is 0.034.[38] The implied variance of ξ_{jk} is 0.033, but the implied correlation between ξ_{jk} and Z_j is only 0.005 due to the very similar covariances of the first two terms in equation (12b).

Using these figures and assuming that the correlation of ξ_{jk} with unobservable human capital is the same as with observable human capital (as was done in all earlier calculations), we repeat our errors-in-variables calculations from above for equation (11) assuming the true estimate of $(\gamma - \gamma_0)$ is zero. However, the implied correlation between ξ_{jk} and human capital is so small that it has no appreciable impact, and our bias calculation for the estimate of $(\gamma - \gamma_0)$ in equation (11) yields the same bias of 13.3% as in our fixed effect specification (eq. [7c]), third column of panel A of table 1. Even if we allow for correlations of the magnitudes found between Z_j and X_i of plus or minus 0.040, our errors-in-variables calculations never change the bias by more than 0.2 percentage points.

In order to estimate equation (11), we need the average commute as a share of the workday for each worker. This variable is calculated as an

[38] The systematic selection of workers across commutes based on income or wage rate is well established in urban economics. See LeRoy and Sonstelie (1983) and Glaeser, Kahn, and Rappaport (2008).

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individual's time spent commuting as a share of average daily work time including commuting time.[39] However, this variable includes information on average hours worked, and if labor supply responds to either the higher wages or the longer commutes associated with agglomeration, the unobservable in the average commute as share of work day will be correlated with agglomeration. Further, self-reported commute times or even average commute times between specific residential and employment locations that rely on small numbers of commuters for many paths allow for substantial measurement error. We address these concerns by measuring average commute time for each employment location and instrumenting for commute time as a share of the workday with this average.[40] The predicted value of commute time as a share of workday based on average commute time for each employment location is then used to calculate the wage net of commuting costs.

The key unknown parameter for calculating the net wage is the value placed upon commute time by workers. Timothy and Wheaton (2001) find compensation rates of between 1.6 and 3.0 times the wage. In our data, we obtain estimates on commute time of 1.8 times the wage in the instrumental variables model of log wage on commute time as a share of the workday. Further, Small (1992) estimates that on average the monetary cost of commuting is proportional to and similar in magnitude to the wage, suggesting a compensation rate of two if people also value their time spent commuting at the wage rate and suggesting an even larger compensation rate if we recognize that monetary commuting costs are paid with after-tax income.[41]

Accordingly, we estimate models using the wage net of commute costs based on commuting time valued at 1.5, 2.0, and 2.5 times the wage. Table 8, panel A, presents these results for our full sample. After conditioning on commuting costs, the residual wage premium associated with higher real earnings is 0.008, 0.002, and −0.005 for commuting costs at 1.5, 2.0, and 2.5 times the wage, respectively, which is noticeably smaller than our estimates of the agglomeration wage premium of 0.026.

[39] The two-way commute time divided by the sum of commute time and one-fifth of average hours worked per week assuming a 5-day work week.

[40] The first stage includes all control variables in the log wage equation except for the agglomeration variable, so that the entire effect of agglomeration is captured directly by the estimated coefficient on the agglomeration variable. Note that models in which the agglomeration variable is included in the first stage yield nearly identical results.

[41] The literature on commute time historically finds that time costs of commutes are valued at approximately half the wage (Small 1992). However, more recent estimates from Brownstone and Small (2005) and Small, Winston, and Yan (2005) find evidence that commuting time is valued at about 90% of the wage. Also, these models ignore uncertainty in daily commute times and even in the required number of trips, which might raise the overall costs.

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Table 8
Model of Wage Net of Commuting Costs

Parameter	Costs 1.5 × Wage	Costs 2.0 × Wage	Costs 2.5 × Wage
A. Baseline sample employment density	.008	.002	−.005
	(.001)	(.001)	(.001)
B. Automobile commuters employment density	.013	.006	−.001
	(.003)	(.002)	(.004)
C. Northeast employment density	.007	.001	−.004
	(.001)	(.001)	(.002)
D. Midwest employment density	.011	.002	−.007
	(.003)	(.002)	(.003)
E. South employment density	.012	.001	−.010
	(.004)	(.004)	(.005)
F. West employment density	.006	−.007	−.021
	(.003)	(.003)	(.005)

NOTE.—The three columns present employment density estimates from the census tract of residence fixed effect agglomeration models after netting predicted commuting costs as share of day out of worker's wage for different cost factors. Individual's total commute time (both ways) as a share of their entire workday (average hours worked per week divided by five plus the total commute time) is predicted using the average commute time for the PUMA employment location. Panel A is for the baseline sample with 2,343,092 observations. Panel B presents the same estimates for a subsample of automobile commuters with 2,073,487 observations. Panels C through F present estimates for subsamples in Northeast, Midwest, South, and West census regions with 569,806, 527,781, 637,023 and 608,482, respectively. Standard errors clustered at the employment location are shown in parentheses.

At this point, we return to equation (11) and concerns that ξ_{jk} and Z_j may be correlated. While individual selection may not create a correlation between ξ_{jk} and Z_j, aggregate location and commuting patterns may create a correlation between these two variables. For example, mass transit may be more feasible and available and automobile travel may be impeded by congestion when commuting to agglomerated locations. To the extent that variation in commute time resulting from mode choice enters the unobservable ξ_{jk}, the higher rates of mass transit use in agglomerated locations will cause a correlation between $\xi_{jk}$and Z_j. If this were the case, we would not expect our wage net of commute time results to be robust when we examine a sample where all individuals use the same mode of commuting. Similarly, one might imagine that commuting patterns differ systematically across regions of the country, and so any bias from the correlation between ξ_{jk} and Z_j would differ across those regions as well. The additional panels of table 8 show the correlations for a sample of automobile users and for each region separately,[42] and all results are robust across the subsamples.[43]

[42] We focus on the automobile because the bulk of commuters use automobiles, and the mean PUMA commute time is a weak instrument for commute time in the mass transit sample. For reasonable scalings between commute time as a share of workday and PUMA average commute, the patterns in table 8 are replicated for the mass transit sample.

[43] The comparable residential fixed effect estimates are 0.032, 0.023, 0.038, 0.046, and 0.047 for the automobile user Northeast, Midwest, South, and West sub-

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VI. Human Capital Externalities

Table 9 presents estimates for models that also include a control for the workplace share of workers with a 4-year college degree or higher. The extended model is still consistent with agglomeration economies, with a coefficient estimate of 0.022 for the fixed effects model with the full sample (panel A), very similar to the estimate in table 4, and very small estimates of bias from the wage net of commuting costs model. The education level of workers in a workplace is also positively associated with wages, which is consistent with standard human capital externality explanations (Rauch 1993; Moretti 2004; Rosenthal and Strange 2006). As before, the estimated effect of agglomeration on wages is robust to the inclusion of residential fixed effects, but the estimated effects of share college-educated decline from 0.359 to 0.151, suggesting that workers sort over workplace human capital levels.[44] As with agglomeration, the estimate of bias (share college coefficient in the net wage model) is substantially smaller than the fixed effect estimate of the effect of share college-educated on wages.

Panels B, C, and D of table 9 present estimates for a model with no covariates for the full sample; for the baseline model using the subsample of single, male workers; and for a model controlling for observationally equivalent individual cells by census tract fixed effects. As in table 4, all results are robust, and the general pattern of findings persists. Notably, the omission of all covariates leads to an increase of 0.05 in the fixed effect estimate for workplace share college, presumably due to the correlation between own education and share college, and as predicted by our errors-in-variables calculations, the estimate of bias from the wage net of commute time model increases by 0.05 as well.

VII. Summary and Conclusions

We find that within-metropolitan wage premia cannot be explained by high-productivity workers sorting into agglomerated locations, and so these wage premia must arise from location-specific differences. Specifically, the estimates for both total employment and employment density

samples, respectively. Again, the pattern of results for the net wage models is nearly identical in models using total employment to measure agglomeration.

[44] Rosenthal and Strange (2006) control separately for the number of college-educated and non-college-educated workers. In our sample, however, the number of college-educated and non-college-educated workers has a correlation above 0.97 even after conditioning on metropolitan area or residential PUMA. Further, we have identified at least one specification where we observe a sign reversal so that wages fall with the number of college-educated workers. When we estimate models that are directly comparable to Rosenthal and Strange (2006), our estimated effect sizes on college-educated and non-college-educated employment are similar in magnitude to their estimates for a 5-mile radius circle.

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Table 9
Employment Density and Workplace Human Capital Models

Variable	Ordinary Least Squares (1)	Fixed Effects (2)	Wage Net of Commute Costs (3)
A. Baseline model specification:			
Density	.015	.022	.001
	(.001)	(.003)	(.001)
Share workers with college	.359	.151	.032
	(.020)	(.014)	(.014)
B. No individual-level covariates:			
Density	.010	.024	.003
	(.001)	(.003)	(.001)
Share workers with college	.479	.199	.079
	(.024)	(.015)	(.014)
C. Sample of single men:			
Density	.012	.015	−.001
	(.001)	(.002)	(.003)
Share workers with college	.260	.136	.045(
	(.021)	(.016)	.015)
D. Observationally equivalent cells:			
Density	.017	.024	−.001
	(.002)	(.003)	(.001)
Share workers with college	.353	.172	.034
	(.019)	(.018)	(.018)

NOTE.—Column 1 presents OLS estimates, col. 2 presents estimates using census tract fixed effects, and col. 3 presents the wage net of commuting cost model with commute time valued at 2.0 times the wage. Panel A presents estimates from the baseline specification presented in table 3 extended to include a control for the share of workers with a college degree at the workplace. Panel B presents estimates for a specification where all individual worker covariates (as listed in table 2) are excluded, panel C presents estimates for a sample of single men, and panel D presents estimates based on a model that controls for cells of observationally equivalent workers by census tract fixed effects. Panels A, B, and D are based on the same sample of 2,343,092 observations, while the panel C is based on the subsample of single men, with 617,144 observations. Standard errors clustered at the workplace are shown in parentheses.

indicate a positive relationship between workplace agglomeration and firm wages, and these estimates are unchanged by the inclusion of residential location controls intended to absorb worker heterogeneity, even when residential fixed effects are included for each group of observationally equivalent individuals. The magnitudes of these estimates are sizable, with standardized effects between one-half and three-quarters of the estimated across-metropolitan wage premium for the same sample. Estimates for the individual education variables attenuate when the residential controls are included, which is consistent with the residential controls capturing unobserved heterogeneity. The attenuation increases substantially as location controls are refined by focusing on smaller geographic measures of residential location or housing submarkets within those locations, and yet these changes have no impact on the agglomeration estimates, which is consistent with our main finding of little bias from worker sorting. This

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finding is also consistent with the small within-metropolitan correlation between agglomeration and education.

The wage net of commute time model yields quite modest estimates of bias in the estimated effect of agglomeration on wages for reasonable values of commuting costs. These findings suggest that the observed nominal wage differences do not represent differences in ability across workers, because the commute time variable captures commuting costs accurately and wages net of commuting costs do not vary systematically across employment locations, presumably leaving similar workers with similar levels of well-being.

Finally, an extended specification is estimated that includes a variable intended to capture human capital externalities, the share of workers with a 4-year college degree or above. We find that wages increase with the concentration of college-educated workers. However, the effect of human capital externalities on wages falls by over half with the inclusion of residential location fixed effects, likely because high-productivity individuals are sorting across work locations based on education levels. However, the resulting fixed effect estimates are still sizable, and the bias indicated by our wage net of commuting cost model is substantially smaller than the fixed effect estimates of the relationship between wages and share college-educated workers.

This article also has more general implications concerning the nature of urban labor markets. Only limited empirical evidence exists to support the idea that urban labor markets are in a locational equilibrium. This article provides direct evidence that wage differences within metropolitan areas are on average offset by longer commutes, and so household mobility within metropolitan areas appears to eliminate real wages differences.

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