Spatial Effects upon Employment Outcomes: The Case of New Jersey Teenagers

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Two related bodies of research link the intra-metropolitan distribution of households to labor market outcomes. These distinct perspectives extend the standard human capital model of labor markets to consider the effect of space on labor market operations, each presuming a somewhat different mechanism of causation. Research addressing the well-known "spatial mismatch hypothesis" focuses on the impact of job decentralization on the employment prospects of minority households who, through constraints on housing choices, are left behind. In this work, space affects the level and distribution of minority employment through proximity to jobs. As jobs increasingly decentralize and minorities remain concentrated in central cities, minority access to jobs declines, lowering their employment rates and earnings. While the evidence on the importance of the mismatch in jobs is not definitive, it continues to be a focus of scientific and policy interest. (See Kain 1992 and Holzer 1991 for recent reviews.)

Another and distinct hypothesis, associated with William Julius Wilson's (1987) work on the so-called "urban underclass," suggests that the social isolation resulting from the concentration of minorities has a negative effect on individuals more generally, and on their labor market performance specifically. While the empirical evidence on this mechanism is ambiguous (see Jencks and Mayers 1990 for a review and Manski 1993 for a critique), several recent empirical studies support some version of this hypothesis. Using different data but similar approaches, Brooks-Gunn et al. (1993), Clark (1992), and Crane (1991) each found evidence of effects of neighborhood composition on youth high school dropout rates.[1] More directly related to labor market concerns, Case and Katz (1991) analyzed data on poor neighborhoods within Boston, concluding that neighborhood peers substantially influence a variety of youth behaviors, including propensity to work. A neighborhood might affect labor markets through several mechanisms—for example, the absence of positive role models, the lack of informal job contacts, the presence of disruptive influences. These differ from the presumed mechanism underlying the spatial mismatch hypothesis. According to this latter research, it is the *internal* composition of a neighborhood that matters, rather than the relationship of that neighborhood to *external* employment opportunities.

A unifying theme in all this research is that urban labor market outcomes are influenced by more than the individual characteristics recognized in the standard human capital model. Even beyond characteristics of the local labor market, this work suggests that information about the local *residential* environment may improve our models of urban labor market outcomes.

This paper provides tests of the relative importance of spatial factors. We develop and apply a standardized approach to measuring job access, one that can be duplicated for a large number of metropolitan areas. Using a unique data set created and analyzed within the U.S. Bureau of the Census, we estimate a series of employment probability models based on a standard human capital model. We then expand this model to include information on proximity to jobs and various neighborhood characteristics. This permits us to examine the importance of these spatial attributes, frequently omitted from other models. It also permits us to examine the *relative* importance of these spatial variables.

Throughout our analysis, we find strong evidence of the importance of spatial factors in determining youth employment outcomes. As for which factors matter most, our results suggest that they differ both by the outcome examined and by the city.

I. Methodology

The Data

Through arrangements with the U.S. Bureau of the Census, we have created a data set containing all records of non-Hispanic white (white), non-Hispanic black (black), and Hispanic youth aged 16 to 19, residing with at least one parent and located in one of the 73 largest U.S. metropolitan areas. In this paper, we report on an analysis of the urban labor markets in the state of New Jersey. We have all records, rather than just the 1/10 or 1/100 publicly available samples. Thus, even limiting the analysis to one state, the sample contains more than 28,000 youth who reside in one of New Jersey's four largest metropolitan areas (Newark, Bergen-Passaic, Middlesex, and Monmouth). The most important aspect of the data set is that each record in our 1990 extract is coded by census tract. We have matched this data set with aggregate census tract characteristics, such as the percentage of the census tract population that is poor, lives in a female-headed household, is employed, is black, and so on. This generates a large sample of observations on youth and their labor market outcomes matched to a distinctly rich neighborhood context. (Results are shown below in text Tables 1A and 1B and in Appendix Table A1.)

Throughout our analysis, we find strong evidence of the importance of spatial factors in determining youth employment outcomes.

The second portion of the data is compiled from the transportation subsample of the 1990 Census, available at the tract level through the Census Transportation Planning Package (CTPP) for large Metropolitan Statistical Areas (MSAs). The CTPP provides direct information about commuting patterns and proximity to jobs at the census tract level. The raw data provided by the CTPP, matrices of zone-to-zone commuting patterns and peak commute times, are sufficient to create a variety of well-defined tract level measures of employment access. (The derivation of these measures is discussed in Appendix B.) These job proximity measures are linked to the individual record through tract identifiers, providing us with both neighborhood and job access information for all youth in the sample. As described in Appendix B, we have created several measures of employment access for each census tract in the four metropolitan areas. It is worth noting that these access measures are based on travel time, so they incorporate information on both spatial distance and transportation ease.

The Statistical Model

The first step of the analysis is based on a logit model relating youth employment probabilities to individual and family characteristics:

[1] Crane's results have been questioned by Clark's failure at replication using similar data (Clark 1992) and by the methodological criticism of Manski (1993).

$$\log [p_i/(1 - p_i)] = \alpha X_i, \quad (1)$$

where X_i is a vector of those individual and family characteristics found by previous research to be relevant for youth employment outcomes.[2] We then contrast results from this model with an expanded statistical model that includes both job proximity and neighborhood characteristics:

$$\log [p_i/(1 - p_i)] = \alpha X_i + \beta A_i + \gamma N_i, \quad (2)$$

where A_i is a measure of employment access, and N_i is a vector of neighborhood (census tract) characteristics found to be important through previous empirical work.[3]

II. Results

We estimate equations (1) and (2) for the Newark MSA, examining probabilities of both employment and "idleness" (that is, not-in-school-and-not-employed). First we analyze all youth, then white, black, and Hispanic youth separately. We then present the results of these models for all four metropolitan areas, investigating consistency in the effects of neighborhood and accessibility upon labor market outcomes.

Newark

Table 1A presents estimates of the youth employment model, equation (1), for all Newark youth, and for white, black, and Hispanic youth separately. Most results confirm previous findings. Females and older youth are more likely to be working. School enrollment decreases the likelihood of working, as does the birth of a child for teenaged girls. Youth in female-headed households are somewhat less likely to be working, while those in a family with at least one parent working are also more likely to be working. Differences in the intercepts by race reveal lower employment probabilities for minority youths, particularly for black youth.

Some variation in results is present across demographic groups. Racial groups differ somewhat in the specific measure of education that is most important in affecting employment outcomes.[4] While the coefficient of the head of the household's education is always negative, it is not significant for blacks. The effect of household income (excluding the youth's

Table 1A
Logit Models of Household-Level Determinants of Employment: Newark Teenagers
t-ratios in parentheses

Coefficient	All Youth	White	Black	Hispanic
Sex	.353	.351	.273	.399
(1 = Female)	(8.08)	(6.85)	(2.75)	(2.47)
Age	.305	.315	.279	.415
(years)	(10.82)	(8.77)	(5.04)	(4.47)
Education	.123	.182	.030	.075
(years)	(5.73)	(6.16)	(.84)	(1.24)
HS graduate	−.107	−.398	.408	.175
(1 = yes)	(1.55)	(4.50)	(3.13)	(.76)
Female-headed household	−.134	−.014	−.138	−.493
(1 = yes)	(2.18)	(.17)	(1.26)	(2.15)
Head of household's	−.030	−.031	−.008	−.039
education (years)	(4.29)	(3.89)	(.40)	(1.91)
Parent working	.818	.616	.836	.863
(1 = yes)	(8.63)	(4.34)	(5.51)	(3.04)
Youth in school	−.845	−.945	−.762	−.505
(1 = yes)	(13.19)	(11.27)	(6.54)	(2.36)
Family size	−.011	.012	−.003	−.173
(persons)	(.72)	(.53)	(.11)	(2.97)
Children ever born	−1.010	−.679	−1.048	−1.076
(1 = yes)	(5.59)	(1.89)	(4.46)	(1.69)
Other household income	−.002	−.002	.001	.003
(000 dollars)	(5.02)	(5.49)	(.73)	(1.65)
White	−6.548	−7.140		
(1 = yes)	(13.04)	(11.37)		
Black	−7.420		−6.515	
(1 = yes)	(14.64)		(6.25)	
Hispanic	−7.015			−8.091
(1 = yes)	(13.90)			(4.81)
Number of observations	10245	6900	2529	816
Chi-squared	1728	759	846	201
−2logL	12475	8807	2660	931

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[2] See O'Regan and Quigley (1996) for a full description of such a model, and Freeman (1982) for a full description of relevant characteristics.

[3] For examples of such characteristics, see Plotnick and Hoffman (1995) and Duncan (1994). For examples of work similar to this study that have incorporated either job proximity or neighborhood characteristics in this fashion—but not both—see Ihlanfeldt and Sjoquist (1990), Case and Katz (1991), and Duncan (1994).

[4] In models in which years of education is the only measure of a youth's education, this variable is significantly positive for all four models. However, when high school completion is also included, this latter measure significantly (and positively) affects black youth employment rates, while neither is significant for Hispanic youth.

earnings) on employment follows a similar pattern. Increased family resources reduce youth employment.

Measuring the effect of family socioeconomic characteristics is complicated by the relationship between youth work and school decisions. While some interdependence clearly is present in these outcomes, we have simplified our estimation by treating school status as an exogenous control. In terms of family socioeconomic status, higher status decreases the likelihood of in-school youth working, while increasing the likelihood of working for out-of-school youth.

Table 1B

Logit Models of Household-Level Determinants of Idleness: Newark Teenagers

t-ratios in parentheses

Coefficient	All Youth	White	Black	Hispanic
Sex	−.322	−.262	−.308	−.604
(1 = Female)	(3.68)	(2.04)	(2.30)	(2.19)
Age	.636	.618	.626	.702
(years)	(13.45)	(7.95)	(9.29)	(5.07)
Education	−.315	−.406	−.259	−.273
(years)	(11.48)	(8.70)	(6.75)	(3.71)
HS graduate	.362	.632	.225	.381
(1 = yes)	(3.15)	(3.29)	(1.38)	(1.08)
Female-headed household	.364	.382	.265	.611
(1 = yes)	(3.54)	(2.24)	(1.83)	(1.83)
Head of household's	−.062	−.065	−.098	−.017
education (years)	(4.77)	(3.66)	(3.79)	(.52)
Parent working	−.416	−.484	−.513	.532
(1 = yes)	(3.54)	(2.09)	3.37	(1.34)
Family size	.037	−.038	.039	.158
(persons)	(1.48)	(.70)	(1.25)	(2.24)
Children ever born	1.666	1.702	1.618	1.831
(1 = yes)	(9.81)	(4.12)	(7.95)	(3.20)
Other household income	−.004	−.003	−.005	−.008
(000 dollars)	(2.97)	2.06	(1.79)	(1.28)
White	−9.246	−7.607		
(1 = yes)	(10.70)	(5.29)		
Black	−8.463		−8.276	
(1 = yes)	(9.75)		(6.73)	
Hispanic	−8.943			−12.274
(1 = yes)	(10.34)			(4.81)
Number of observations	10245	6900	2529	816
Chi-squared	9749	7399	1684	694
−2logL	4454	2166	1822	438

To eliminate this problem, we have also estimated this model using "idleness" (not-working-and-not-in-school) as the dependent variable. Table 1B reports the results of identical models (except that the school-status variable is omitted). We expect that all variables indicating higher family socioeconomic status will decrease youth idleness. This expectation is borne out. The two sets of results are quite comparable. We include both outcome measures in our analysis, as spatial factors are likely to affect school and work decisions differently.

In the next step of the analysis, the logit model is expanded to include neighborhood information. We examine two categories: employment access and measures of "social access." Employment access is measured by an index of employment "potential" derived from the assumption that work-trip destinations are generated by a Poisson process.[5] A lack of social access is indicated by various measures of neighborhood composition.

Preliminary analysis with a larger set of neighborhood variables[6] established that one measure of racial composition (percent white) and four measures of tract poverty or employment levels (percent poor, on public assistance, unemployed, and adults working) are consistently important in affecting outcomes. Table 2 presents the correlation coefficients of the relevant variables for Newark. Neighborhood demographic measures are highly correlated in Newark; with only one exception the correlation coefficients among these measures exceed 0.76. The job access measure is only weakly correlated with the demographic characteristics of neighborhoods, however.

The appropriate functional form for these variables is not known a priori. Indeed, it is possible that neighborhood effects matter only after some threshold is reached, affecting the logit of employment in a nonlinear fashion. We estimated a series of models to test for nonlinearities, and while there is some evidence that the relationship may be complicated, no nonlinear representation seemed superior to simple

[5] As explained in Appendix B, the relative accessibility of census tracts within each metropolitan area is quite insensitive to assumptions about the trip generation process. Results using the assumption of a Poisson process are similar to those based upon a more general assumption of a negative binomial process. In fact, for these metropolitan areas, the standard gravity model provides job access measures that are correlated with these more sophisticated measures at greater than 0.98.

[6] These included, for example, percent black, Hispanic, in owner-occupied home, and in female-headed household, and tract median income.

Table 2
Correlation Matrix of Neighborhood Measures for Newark MSA

	Percent:					
	White	Public Assistance	Poor	Unemployed	Adults Not at Work	Job Access
Percent White	1.000					
Percent Public Assistance	−.798	1.000				
Percent Poor	−.783	.927	1.000			
Percent Unemployment	−.818	.896	.877	1.000		
Percent Adults Not at Work	−.572	.776	.764	.766	1.000	
Job Access	.318	−.433	−.450	−.436	−.310	1.000

continuous measures of neighborhood attributes.[7] We report results using continuous measures.

We estimated a variety of models of youth employment probabilities with these neighborhood variables. The results for the individual and family-level variables were essentially unchanged, with the exception that family background variables generally decrease slightly in magnitude and statistical significance. This suggests that, while neighborhood characteristics may spuriously capture omitted family influences (Corcoran et al. 1992), the reverse is also the case. Empirical work that does not include information about neighborhoods likely overstates the (direct) influence of family characteristics on employment outcomes.

Results for the neighborhood variables are presented in Tables 3A and 3B. Panel A presents results for all youth, and Panels B through D present results separately for white, black, and Hispanic youth. In Model I of each panel and table, employment access is the sole neighborhood variable included. In the case of youth employment, improved job access has a significant and positive effect for all youth and for black youth. For youth idleness, job access is highly significant for all youth and for black youth.

The independent effect of access does not persist when other neighborhood characteristics are added, singly (Models II to VI) and in pairs (Models VII to X). In almost every case, the measure of access to jobs is insignificant when measures of neighborhood racial composition or neighborhood poverty/employment are included. In the sample of all Newark youth, each neighborhood variable, when entered individually, is significant and is of the expected sign. This is also true for the separate samples of white and black youth.[8]

The high correlation among many of the neighborhood variables means that the relative importance of neighborhood measures cannot be determined with precision. While employment access is not particularly highly correlated with the other tract variables, the correlations among the other variables are quite high. The effect of this is illustrated in the results of models VII to X for white youth employment (Table 3A, Panel B). Each neighborhood composition measure is significant when included separately. However, when pairs of variables are included, generally neither neighborhood variable is significant. Note, however, that according to a standard likelihood ratio test, the set of measures is significantly different from zero. In the aggregate for youth employment and for black youth separately (both employment and idleness), it does appear that neighborhood poverty/employment characteristics have a stronger effect than does the racial composition of the neighborhood. However, idleness of Hispanic youth appears more strongly influenced by neighborhood racial composition.

Some caution is in order in evaluating these results. Several recent papers have highlighted the difficulty of controlling adequately for family characteristics and choice when identifying neighborhood and other potential influences on social outcomes (Corcoran et al. 1992, Evans, Oates, and Schwab 1992, and Plotnick and Hoffman 1995). Other work has emphasized the circumstances in which the logic of the identification of peer influences is problematic (Manski 1993). The potential endogeneity of neighborhoods is also a source of concern in this empirical work. Endogeneity may be manifest in several ways. Our empirical analysis is more successful in dealing with some of the sources of this simultaneity than others.

The most obvious source of statistical problems in the interpretation of findings about youth employ-

[7] We were especially concerned with measuring threshold effects for racial composition and the fraction of the population in poverty.

[8] For Hispanic youth, several neighborhood variables are significant, but not all. In part, this reflects the smaller sample sizes of Hispanic youth.

Table 3A

Neighborhood Determinants of Employment for Newark Youth[a]

t-ratios in parentheses

	I	II	III	IV	V	VI	VII	VIII	IX	X
A. All Teenagers (10245 observations)										
Chi-squared	1732	1757	1772	1772	1772	1835	1775	1775	1774	1839
−2logL	12471	12445	12431	12430	12431	12367	12428	12457	12429	12364
Access	.006	.004	.000	.000	.000	.002	.000	.000	.000	.001
	(2.10)	(1.36)	(.06)	(.10)	(.04)	(.66)	(.05)	(.18)	(.14)	(.47)
Percent White		.608					.266	.252	.237	.249
		(5.05)					(1.83)	(1.71)	(1.57)	(1.96)
Percent Poor			−2.687				−2.153			
			(6.20)				(4.14)			
Percent on Public Assistance				−2.567				−2.074		
				(6.24)				(4.15)		
Percent Unemployment					−4.130				−3.343	
					(6.21)				(4.03)	
Percent Adults Not at Work						−3.400				−3.184
						(9.96)				(8.88)
B. White Teenagers (6900 observations)										
Chi-squared	759	763	766	765	767	822	766	765	767	822
−2logL	8807	8802	8799	8801	8798	8744	8799	8800	8798	8744
Access	.000	.000	−.001	−.001	−.002	.002	−.001	−.001	−.002	.002
	(.02)	(.02)	(.46)	(.33)	(.58)	(.62)	(.40)	(.24)	(.53)	(.62)
Percent White		.393					.087	.179	.060	.005
		(2.14)					(.35)	(.74)	(.24)	(.03)
Percent Poor			−2.495				−2.210			
			(2.78)				(1.82)			
Percent on Public Assistance				−2.093				−1.548		
				(2.42)				(1.37)		
Percent Unemployment					−3.469				−3.209	
					(2.91)				(2.00)	
Percent Adults Not at Work						−3.808				−3.804
						(7.87)				(7.60)

[a]Logit models include household level variables reported in Table 1A.

ment is the omission of individual or family characteristics. In particular, family variables have been shown to be very important determinants of youth outcomes (Corcoran et al. 1992), yet are frequently omitted from empirical work. Since family characteristics are likely to be correlated with neighborhood characteristics, it is possible that measures of neighborhood characteristics are merely proxies for family effects. By using only at-home youth, we have access to the range of census information on the youth's family. These attributes really "matter" in the empirical results.

A second source of concern is the youth's choice of neighborhood. Here again, by limiting attention to at-home youth, we can presume that this choice is made by the parent(s), using the standard transportation-housing cost calculus. Household choice is exogenous to the transport demands of youth. Of course, to the extent that household choices about residential location are influenced by the impact of neighborhood characteristics on youth employment, a focus on at-home youth will not eliminate this source of simultaneity.

A third source of concern is the definition and computation of the accessibility measure itself. We should emphasize that this measure is not computed from the observed commuting patterns of teenagers. Nor is it computed with reference to the location of jobs that might be "suitable" for teenagers (Ihlanfeldt and Sjoquist 1990). It is merely the "standard" acces-

Table 3A continued

Neighborhood Determinants of Employment for Newark Youth[a]

t-ratios in parentheses

	I	II	III	IV	V	VI	VII	VIII	IX	X
C. Black Teenagers (2529 observations)										
Chi-squared	854	860	866	869	867	875	867	869	868	877
−2logL	2652	2646	2640	2637	2639	2631	2639	2637	2638	2629
Access	.018	.013	.003	.002	.006	.001	.003	.002	.005	−.001
	(2.92)	(2.02)	(.44)	(.22)	(.82)	(.07)	(.38)	(.20)	(.76)	(.19)
Percent White		.468					.236	.150	.154	.299
		(2.28)					(1.06)	(.66)	(.66)	(1.43)
Percent Poor			−2.186				−1.890			
			(3.31)				(2.64)			
Percent on Public Assistance				−2.402				−2.194		
				(3.77)				(3.09)		
Percent Unemployment					−3.518				−3.166	
					(3.55)				(2.82)	
Percent Adults Not at Work						−2.908				−2.720
						(4.47)				(4.11)
D. Hispanic Teenagers (816 observations)										
Chi-squared	206	209	209	208	208	210	210	210	210	211
−2logL	925	922	922	923	923	921	921	922	922	920
Access	−.010	.037	.027	.022	.017	−.005	.043	.041	.034	.032
	(.61)	(1.82)	(1.30)	(1.13)	(.81)	(.26)	(2.01)	(1.98)	(1.59)	(1.51)
Percent White		−2.821					−2.548	−2.448	−2.955	−2.923
		(4.77)					(3.82)	(3.52)	(4.36)	(4.84)
Percent Poor			5.692				1.791			
			(3.39)				(.92)			
Percent on Public Assistance				5.474				1.818		
				(3.75)				(1.04)		
Percent Unemployment					6.860				−1.371	
					(2.41)				(.40)	
Percent Adults Not at Work						1.033				−1.272
						(.07)				(.77)

[a]Logit models include household level variables reported in Table 1A.

sibility measure calculated from observations on the work-trip patterns of all workers—adults and teenagers of all races—within the urban area.

This attention to specification does not, of course, eliminate all sources of simultaneity. To the extent that omitted family or individual characteristics exist that are more strongly correlated with neighborhood variables than with other included controls, the results may be spurious. It is also possible that the residence choices of others in a neighborhood are influenced by youth employment outcomes, affecting the characteristics of the neighborhood indirectly. In Appendix C, we present direct tests for the existence of this indirect relationship for Newark youth. We find little evidence of such a spurious relationship.

The high correlation among the various neighborhood characteristics raises a second issue in interpreting these results. Given the high correlation among neighborhood characteristics, it is difficult to separate the effects of various dimensions of related neighborhood characteristics with any precision. For models in which we include one neighborhood characteristic, this measure acts as a proxy for a collection of characteristics, and the results should be interpreted in that light.

New Jersey Cities

In this section, we expand the sample to include all four metropolitan areas in New Jersey. We estimate

Table 3B
Neighborhood Determinants of Idleness for Newark Youth[a]
t-ratios in parentheses

	I	II	III	IV	V	VI	VII	VIII	IX	X
A. All Teenagers (10245 observations)										
Chi-squared	9756	9784	9781	9788	9784	9777	9793	9797	9793	9794
−2logL	4447	4418	4421	4414	4418	4425	4410	4406	4409	4408
Access	−.013	−.007	.000	.001	−.001	−.004	.000	.001	−.001	−.001
	(2.66)	(1.43)	(.04)	(.22)	(.12)	(.69)	(.04)	(.15)	(.20)	(.25)
Percent White		−1.102					−.792	−.695	−.734	−.901
		(5.33)					(3.37)	(2.91)	(3.03)	(4.14)
Percent Poor			2.892				1.862			
			(5.11)				(2.90)			
Percent on Public Assistance				3.116				2.214		
				(5.77)				(3.55)		
Percent Unemployment					4.880				3.192	
					(5.36)				(3.00)	
Percent Adults Not at Work						2.421				1.732
						(4.66)				(3.17)
B. White Teenagers (6900 observations)										
Chi-squared	7399	7405	7411	7408	7406	7406	7411	7408	7407	7408
−2logL	2166	2161	2155	2157	2159	2160	2155	2157	2159	2157
Access	−.004	−.004	.000	−.001	.000	−.004	.000	−.001	−.001	−.004
	(.54)	(.50)	(.06)	(.11)	(.01)	(.58)	(.05)	(.16)	(.11)	(.52)
Percent White		−.008					.−.031	−.222	−.322	.562
		(2.38)					(.06)	(.48)	(.66)	(1.54)
Percent Poor			4.854				4.767			
			(3.55)				(2.47)			
Percent on Public Assistance				4.137				3.547		
				(3.14)				(1.95)		
Percent Unemployment					5.839				4.388	
					(2.73)				(1.42)	
Percent Adults Not at Work						2.621				2.020
						(2.59)				(1.87)

[a]Logit models include household level variables reported in Table 1B.

similar statistical models, but with larger samples and somewhat lower levels of intercorrelation of neighborhood demographic measures. Table 4 presents a subset of the results for all metropolitan New Jersey youth, which conveys the main findings. Panel A includes results for the estimation of employment probabilities, Panel B summarizes results for the estimation of idleness probabilities.

Model I reports estimates of youth employment probabilities as a function of neighborhood access measures and of individual and household characteristics. The cardinal values of the access measure are hardly comparable across MSAs (see Appendix B and Table 5), so we permit the coefficient on access to vary by MSA. Employment access has a highly significant, positive effect on youth employment in each of the four MSAs.

The other five models include access, but introduce other neighborhood characteristics. Models II to IV include the percent white, the percent on public assistance, and the percent of adults not at work, respectively, in the census tract of residence. Each of these neighborhood composition variables is significant and is of the expected sign. Including these characteristics has little impact on the access coefficients. In Models V and VI, which include the access measures, percent white, and one of the two poverty/employment measures, the results are comparable. Both neighbor-

Table 3B continued

Neighborhood Determinants of Idleness for Newark Youth[a]

t-ratios in parentheses

	I	II	III	IV	V	VI	VII	VIII	IX	X
C. Black Teenagers (2529 observations)										
Chi-squared	1696	1703	1703	1708	1710	1709	1706	1710	1711	1713
−2logL	1810	1803	1803	1798	1796	1797	1800	1796	1795	1793
Access	−.027	−.020	−.011	−.006	−.009	−.006	−.009	−.005	−.008	−.001
	(3.50)	(2.38)	(1.11)	(.57)	(.98)	(.56)	(.94)	(.47)	(.85)	(.13)
Percent White		−.846					−.637	−.508	−.447	−.673
		(2.54)					(1.82)	(1.44)	(1.25)	(2.01)
Percent Poor			1.994				1.492			
			(2.62)				(1.84)			
Percent on Public Assistance				2.562				2.138		
				(3.43)				(2.65)		
Percent Unemployment					4.304				3.664	
					(3.67)				(2.87)	
Percent Adults Not at Work						2.767				2.517
						(3.54)				(3.15)
D. Hispanic Teenagers (816 observations)										
Chi-squared	694	720	706	708	700	695	721	721	720	720
−2logL	437	411	426	423	431	437	411	410	411	411
Access	−.010	.037	.027	.022	.017	−.005	.043	.041	.034	.032
	(.61)	(1.82)	(1.30)	(1.13)	(.81)	(.26)	(2.01)	(1.98)	(1.59)	(1.51)
Percent White		−2.821					−2.548	−2.448	−2.955	−2.923
		(4.77)					(3.82)	(3.52)	(4.36)	(4.84)
Percent Poor			5.692				1.791			
			(3.39)				(.92)			
Percent on Public Assistance				5.474				1.818		
				(3.75)				(1.04)		
Percent Unemployment					6.860				−1.371	
					(2.41)				(.40)	
Percent Adults Not at Work						1.033				−1.272
						(.67)				(.77)

[a]Logit models include household level variables reported in Table 1B.

hood composition variables are significant, and the access measure is important in each of the four cities.

In Panel B, the results for predicting teenage idleness differ slightly. The access measure is significant in the simplest model (Model I), but in more complex specifications, access appears to be less important. Individually, and in pairs, other neighborhood measures have important effects upon the probability of idleness of urban youth.

It is certainly possible that the effect of neighborhood composition differs across metropolitan areas. We have investigated models of this general specification (see Appendix Table A1). On purely statistical grounds, the complete disaggregation of neighborhood measures across MSAs does improve the employment probability model, but does not improve the idleness results.[9] The magnitudes, however, are essentially the same.[10]

[9] The χ^2s for the fully interacted models, compared to those without MSA-specific coefficients, are as follows:

Model	Employment χ^2	Idleness χ^2	Degrees of Freedom
II	24	2	3
III	16	2	3
IV	31	4	3
V	31	3	6
VI	39	3	6

[10] In addition, we have estimated these models separately for

Table 4

Neighborhood Determinants of Employment Outcomes for New Jersey Youth[a]

28191 Observations, t-ratios in parentheses

	I	II	III	IV	V	VI
A. Employment						
Chi-squared	3838	3874	3891	3963	3894	3975
-2logL	35243	35207	35190	35118	35187	35106
Access:						
Bergen-Passaic	.030	.024	.017	.025	.017	.022
	(3.47)	(2.78)	(1.96)	(2.92)	(2.00)	(2.56)
Middlesex	.041	.036	.031	.026	.031	.024
	(6.56)	(5.72)	(4.84)	(4.01)	(4.86)	(3.73)
Monmouth	.010	.008	.007	.010	.007	.009
	(5.15)	(4.08)	(3.80)	(5.35)	(3.66)	(4.67)
Newark	.006	.006	.004	.004	.004	.004
	(3.57)	(3.26)	(2.23)	(2.37)	(2.36)	(2.29)
Percent White		.491			.188	.295
		(5.99)			(1.77)	(3.50)
Percent on Public Assistance			−2.208		−1.760	
			(7.14)		(4.42)	
Percent Adults Not at Work				−2.242		−2.074
				(11.02)		(9.94)
B. Idleness						
Chi-squared	27909	27952	27958	27938	27967	27966
-2logL	11172	11129	11123	11143	11114	11115
Access:						
Bergen-Passaic	−.034	−.013	.007	−.015	.006	−.002
	(1.96)	(.74)	(.40)	(.84)	(.33)	(.13)
Middlesex	−.038	−.018	−.005	−.015	−.004	−.005
	(2.82)	(1.33)	(.37)	(1.08)	(.32)	(.35)
Monmouth	−.005	.002	.004	−.002	.005	.003
	(1.17)	(.57)	(1.06)	(.50)	(1.32)	(.79)
Newark	−.008	−.006	.000	−.004	−.001	−.003
	(2.29)	(1.58)	(.12)	(.98)	(.39)	(.75)
Percent White		−.916			−.524	−.768
		(6.58)			(3.00)	(5.29)
Percent on Public Assistance			2.951		2.006	
			(7.12)		(3.84)	
Percent Adults Not at Work				1.884		1.353
				(5.51)		(3.75)

[a]Logit models include household level variables reported in Tables 1A and 1B. Each model also includes separate intercepts for the different metropolitan areas.

white, black, and Hispanic youth. For white youth, results reported in Table 4 and Appendix Table 1 are confirmed. The results are more fragile when the sample is confined to minority youth. Many of the variables that are significant for all specifications with the larger samples are insignificant for the minority samples. The pattern of results suggests that the samples of minority youth are too small to permit estimation of MSA-specific and race-specific coefficients. For that reason, we focus on the all-youth estimates.

III. Implications

The statistical results for this sample of New Jersey youth suggest that neighborhood composition and employment access affect labor market outcomes, although the quantitative estimates differ by area and by outcome. The character of urban neighborhoods and the effect of neighborhood composition on out-

Table 5
Average Characteristics of Neighborhoods in New Jersey MSAs

MSA Residences of	Sample Size	Job Access	Fraction: White	Fraction: Public Assistance	Fraction: Adults Not at Work
Newark					
All Youth	10245	27.037	.704	.357	.071
White Youth	6900	28.444	.910	.331	.032
Black Youth	2529	23.491	.194	.416	.164
Hispanic Youth	816	26.129	.536	.395	.116
Bergen-Passaic					
All Youth	6227	5.971	.852	.355	.043
White Youth	5164	6.060	.934	.350	.030
Black Youth	528	5.463	.295	.385	.130
Hispanic Youth	535	5.609	.608	.379	.084
Middlesex					
All Youth	5713	8.136	.899	.309	.033
White Youth	5064	8.105	.929	.307	.029
Black Youth	367	8.836	.661	.319	.060
Hispanic Youth	282	7.799	.688	.342	.068
Monmouth					
All Youth	6006	26.191	.925	.370	.040
White Youth	5446	26.494	.948	.368	.036
Black Youth	352	22.540	.608	.390	.087
Hispanic Youth	208	24.431	.866	.375	.056

comes vary across metropolitan areas. This accounts for some of the observed differences in youth employment outcomes. Moreover, within metropolitan areas, large differences are found in the average characteristics of neighborhoods in which youth of different races and ethnicities reside. For example, in Newark, 81.5 percent of white youth live in census tracts in which 90 percent or more of the population is white. In contrast, slightly less than 20 percent of Hispanic youth, and only 4 percent of black youth, live in such tracts. Table 5 summarizes the average characteristics of neighborhoods in which youth of different races reside. These differences may lead to large differences in employment outcomes for youth.

Table 6 indicates the importance of these differences in employment access and neighborhood demographics in affecting employment outcomes by race and ethnicity.[11] The first column in the table presents the employment probability estimated for the "average" youth in each of these four metropolitan areas. The second column presents the employment probability of the same "average" youth living in the neighborhood in which the average white youth resides, in each metropolitan area. The third and fourth columns present the employment probabilities estimated for the same youth living in the neighborhood inhabited by the average black and Hispanic youths, respectively. Panel B presents the same simulation using idleness instead of employment. Many of these differences are quite large.

In Bergen-Passaic, residence in the neighborhood in which the average white youth lives (compared to that in which the average black lives) increases youth employment rates by 2.3 percentage points, from 39.9 to 42.2 percent. A similar comparison of employment rates for those living in the average white and average Hispanic neighborhoods shows a smaller difference. In Middlesex, the differences are approximately of the same magnitude

Table 6
Employment Outcomes for Youth with Average Capital Characteristics in Different Neighborhoods
Percent

	All Youth	White Youth	Black Youth	Hispanic Youth
A. Employment				
Newark	37.45	43.46	32.76	36.84
Bergen-Passaic	41.77	42.15	39.85	40.02
Middlesex	46.99	47.37	44.61	43.46
Monmouth	44.97	45.00	44.87	44.50
	All Youth	White Youth	Black Youth	Hispanic Youth
B. Idleness				
Newark	4.66	3.83	7.44	5.63
Bergen-Passaic	4.19	3.98	5.92	4.92
Middlesex	3.50	3.41	4.27	4.33
Monmouth	4.29	4.22	5.39	4.56

[11] These probabilities are computed relying upon the coefficients from Model VI in Appendix Table A1. The coefficients of the individual and household demographic variables (not presented) and the average characteristics of the sample of youth are used, together with the coefficients reported in Appendix Table A1 and the average neighborhood characteristics in each MSA.

(a 2.8 percentage point increase for white-black comparisons, and a 3.9 percentage point increase for the white-Hispanic comparison). In Monmouth, located on the New Jersey shore, differences in average neighborhood characteristics have much smaller effects on youth employment rates, while in Newark, the effect is strikingly large. In Newark, predicted employment rates for the average white neighborhood are almost one-third higher than for the average black neighborhood.

Results for youth idleness are comparable. In general, the largest disparities are between probabilities for the average white and the average black neighborhoods. Across these MSAs, the effect varies, and the difference is greatest for the largest and most urban metropolitan area in our sample, Newark.

IV. Conclusion

This paper analyzes employment and "idleness" outcomes for a large sample of urban youth. The analysis is based upon observations on at-home youth and their families, the employment access of the neighborhood in which they reside, and the socioeconomic character of those neighborhoods.

The analysis documents the importance of human capital and family attributes in conditioning the labor market outcomes for youth living at home. In addition to individual-level determinants, we find evidence of substantial spatial linkages to employment outcomes. While not consistently significant across metropolitan areas, measures of access to jobs are important in affecting employment in some areas, especially for minority youth. Access appears to play essentially no role in determining youth idleness, an outcome dominated by youth school-enrollment status. Furthermore, whether as a measure of social access, role models, or peer influence, neighborhood composition matters consistently. Measures of the presence of employed and non-poor individuals (presumably those with knowledge of and contact with jobs) affect youth employment. Even with large samples of data, however, we are less successful in distinguishing among these distinct, but closely related, potential causes.

Simulations using these results demonstrate quite clearly that the constellation of factors that distinguish "good" from "bad" neighborhoods affects teenage employment in profound ways.

Appendix Table A1

Neighborhood Determinants of Employment Outcomes for New Jersey Youth[a]

28191 Observations, t-ratios in parentheses

	I	II	III	IV	V	VI
A. Employment						
Chi-squared	3848	3904	3913	4002	3931	4021
-2logL	35233	35177	35168	35079	35150	35060
Access						
Bergen-Passaic	.066	.068	.069	.070	.069	.071
	(3.45)	(3.49)	(3.52)	(3.63)	(3.51)	(3.65)
Middlesex	.026	.276	.023	.017	.028	.021
	(2.17)	(2.34)	(1.99)	(1.39)	(2.38)	(1.74)
Monmouth	.006	.007	.006	.007	.008	.008
	(1.86)	(2.25)	(1.96)	(2.07)	(2.38)	(2.35)
Newark	.004	.002	.001	.001	.001	.001
	(3.37)	(1.88)	(.45)	(.99)	(.51)	(.71)
Percent White						
Bergen-Passaic		.156			.229	.027
		(1.17)			(1.06)	(.19)
Middlesex		.819			.893	.731
		(3.86)			(2.96)	(3.38)
Monmouth		−.210			−.691	−.268
		(.94)			(2.30)	(1.19)
Newark		.592			.203	.225
		(6.43)			(1.63)	(2.26)
Percent Public Assistance						
Bergen-Passaic			−.269		.443	
			(.42)		(.42)	
Middlesex			−2.798		.521	
			(2.48)		(.32)	
Monmouth			−.760		−2.785	
			(.87)		(2.38)	
Newark			−.753		−2.248	
			(7.62)		(4.58)	
Percent Adults Not at Work						
Bergen-Passaic				−2.049		−2.140
				(3.58)		(3.60)
Middlesex				−1.536		−1.261
				(3.25)		(2.62)
Monmouth				−1.059		−1.115
				(2.99)		(3.14)
Newark				−3.579		−3.285
				(11.03)		(9.24)

[a]Logit models include household-level variables reported in Tables 1A and 1B. Each model also includes separate intercepts for the different metropolitan areas.

Appendix Table A1 continued

Neighborhood Determinants of Employment Outcomes for New Jersey Youth[a]

28191 Observations, t-ratios in parentheses

	I	II	III	IV	V	VI
B. Idleness						
Chi-squared	27913	27955	27960	27944	27970	27969
-2logL	11167	11126	11121	11137	11110	11111
Access						
Bergen-Passaic	−.026	−.011	−.004	−.026	−.005	−.010
	(3.58)	(.27)	(.10)	(.66)	(.11)	(.25)
Middlesex	−.003	−.001	.003	.010	.004	.011
	(.11)	(.04)	(.12)	(.35)	(.16)	(.39)
Monmouth	.001	.002	.002	.000	.001	.001
	(.14)	(.25)	(.26)	(.03)	(.21)	(.21)
Newark	−.007	−.003	.000	−.002	.000	−.001
	(3.16)	(1.37)	(.13)	(.78)	(.08)	(.23)
Percent White						
Bergen-Passaic		−.690			−.543	−.676
		(3.25)			(1.61)	(2.98)
Middlesex		−.855			−.255	−.651
		(2.42)			(.41)	(1.77)
Monmouth		−.811			−.198	−.752
		(2.31)			(.38)	(2.14)
Newark		−.986			−.614	−.808
		(6.23)			(3.13)	(4.71)
Percent Public Assistance						
Bergen-Passaic			2.179		.882	
			(2.34)		(.58)	
Middlesex			4.114		4.033	
			(2.22)		(1.24)	
Monmouth			3.192		3.297	
			(2.37)		(1.65)	
Newark			3.077		2.007	
			(6.35)		(3.28)	
Percent Adults Not at Work						
Bergen-Passaic				.955		.329
				(.96)		(.30)
Middlesex				2.265		2.108
				(2.25)		(2.00)
Monmouth				.909		.908
				(1.36)		(1.33)
Newark				2.400		1.590
				(4.88)		(2.94)

[a]Logit models include household-level variables reported in Tables 1A and 1B. Each model also includes separate intercepts for the different metropolitan areas.

Appendix B: The Computation of Spatial Access

In the text, we employ a measure of the accessibility of each census tract to employment locations. This measure is derived from the "potential access" measures widely used by transport planners. (See Isard (1960) for an early review or Smith (1984) for a more recent treatment.) These measures are derived from observations on the work-trip patterns of commuters and the transport linkages in an urban area.

The accessibility measures are based upon the data available through the Census Transportation Planning Package (CTPP) for large metropolitan areas. The CTPP data are obtained from the Transportation Supplement of the 1990 Census. Each metropolitan area is divided into Traffic Analysis Zones (TAZs). Zone-to-zone peak commute flows (T_{ij}) as well as peak travel times (d_{ij}) are reported. From the elements of the matrix, the number of workers resident in each TAZ (R_i) can be estimated ($R_i = \Sigma_j T_{ij}$). Similarly, the number of individuals working in each zone (W_j) can be estimated ($W_j = \Sigma_j T_{ij}$).

The most widely used empirical model of the accessibility of particular residential locations is based upon the gravity concept:

$$T_{ij} = \alpha R_i^{\beta} W_j^{\gamma} / d_{ij}^{\delta}, \tag{B1}$$

where Greek letters denote parameters. Isard (1960) provides a number of physical and social scientific justifications for the formulation. Flows between i and j are positively related to the "masses" of residences and workplaces and inversely related to the "distance" (travel time) between i and j.

Estimates of the parameters yield a measure of the accessibility of each residence zone to the workplaces, which are distributed throughout the region (Isard 1960, p. 510),

$$(A_i = \sum_j \hat{T}_{ij} / R_i^{\hat{\beta}}), \tag{B2}$$

where $\hat{T}$ is computed from the parameters estimated by statistical means.

More sophisticated measures of access recognize that the transport flows to each destination are count variables. The Poisson distribution is often a reasonable description for counts of events that occur randomly.

Assuming the count follows a Poisson distribution, the probability of obtaining a commuting flow T_{ij} is

$$\mathrm{pr}(T_{ij}) = e^{-\lambda ij} \lambda_{ij}^{Tij} / Tij! \tag{B3}$$

where λ_{ij} is the Poisson parameter. Assuming further that

$$\exp[\lambda_{ij}] = \alpha R_i^{\beta} W_i^{\gamma} / d_{ij}^{\delta} \tag{B4}$$

yields an estimable form of the count model (since $E(T_{ij}) = \lambda_{ij}$). See Smith (1987) for a discussion. Estimates of the parameters similarly yield a measure of the accessibility of each residence zone to workplaces in the region,

$$A_i = \sum_j \hat{\lambda}_{ij} / R_j^{\hat{\beta}}. \tag{B5}$$

A more general model of the flow count between i and j relaxes the Poisson assumption that the mean and variance are identical. For example, following Greenwood and Yule (1920), Hausman, Hall, and Griliches (1984, p. 922) assume that the parameter λ_{ij} follows a gamma distribution $G(\omega_{ij})$ with parameters ω_{ij}. They show that, under these circumstances, the probability distribution of the count is negative binomial with parameters ω_{ij} and η,

$$\mathrm{pr}(T_{ij}) = \frac{G(\omega_{ij} + T_{ij})}{G(\omega_{ij})G(T_{ij} + 1)} \left(\frac{\eta}{1 + \eta}\right)^{\omega_{ij}} (1 + \eta)^{-T_{ij}}. \tag{B6}$$

Again, assuming that

$$\exp[\omega_{ij}] = \alpha R_i^{\beta} W_j^{\gamma} / d_{ij}^{\delta} \tag{B7}$$

yields an estimable form of the count model and the resulting accessibility index for each residence zone.

The count models are clearly nested. If η is infinitely large, then equations (B6) and (B7) specialize to (B3) and

Table B1
Parameter Estimates of Negative Binomial, Poisson, and Gravity Models of Transport Access
Asymptotic t ratios in parentheses

	Newark	Bergen-Passaic	Middlesex	Monmouth
A. Negative Binomial				
α	1.249	.529	.073	.793
β	.342	.474	.545	.421
γ	.341	.378	.384	.445
δ	.705	.842	.856	.872
η	.555	.587	.527	.608
log likelihood	−116818	−71835	−63415	−56296
B. Poisson				
α	−.187	−1.557	−1.327	−.991
β	.511	.718	.666	.530
γ	.424	.474	.465	.598
δ	.806	.967	.894	.918
log likelihood	−296466	−209995	−174066	−156235
C. Gravity Model				
α	.601	−.371	−.337	−.796
β	.307	.427	.473	.486
γ	.274	.325	.313	.358
δ	.485	.569	.622	.593
R^2	.225	.245	.280	.293
Number of Observations	32157	18419	16760	15009

Table B2
Simple Correlation Coefficients among Census Tract Access-to-Employment Measures Derived from Negative Binomial, Poisson, and Gravity Models

	Gravity vs. Poisson	Gravity vs. Binomial	Binomial vs. Poisson
Newark	.980	.994	.988
Bergen-Passaic	.982	.993	.995
Middlesex	.973	.989	.976
Monmouth	.909	.989	.954

(B4). If η is finite, then the mean and the variance of the count variables are not identical (as assumed by the Poisson representation).

The accessibility measure derived from the gravity model, equations (B1) and (B2), may be interpreted as a simple linear approximation to either of these theoretical count models. (Smith (1987) provides a thorough discussion of the link between gravity and Poisson models.)

Table B1 presents parameter estimates of the three models for four metropolitan areas in New Jersey. The models are estimated using the CTPP data from the 1990 Census. For each of these metropolitan areas, the TAZs are coterminous with census tracts. The matrices of tract-to-tract commuting flows are sparse, with many zeros. For example, the Newark metropolitan area has 448 census tracts. Of the 200,704 possible commuting patterns (448 times 448), 168,547 of them are zero. (In part, this reflects the fact that the underlying counts and transportation times are gathered from a sample of about 15 percent of the population.) The estimates of the negative binomial and Poisson models are obtained by maximum likelihood methods, adjusting the likelihood function for this truncation.[12] In contrast, the gravity model is estimated in the most straightforward manner—by applying ordinary least squares to equation (B1) in logarithmic form using the non-zero observations.[13]

As the table indicates, the hypothesis of Poisson flows is rejected in favor of the negative binomial.[14] In each case, the estimate of η is rather precise, and it implies that the ratio of the variance to the mean ($[1 + \eta]/\eta$) is on the order of 2.5 or 3.

Table B2 presents the correlations among the census tract accessibility measures derived from the three models. Although the negative binomial model fits the data better than the Poisson model, the differences in the accessibility measures computed from them are very small. Similarly, the table shows that, for each of the four New Jersey metropolitan areas, the gravity model yields an almost identical measure of census tract access to employment.

[12] The coefficients are estimated using the programs STATA and TSP. The refinement to recognize the truncated character of the data is more or less irrelevant, empirically. The coefficients are quite similar when this subtlety is simply ignored.

[13] More elaborate treatments are readily available. See, for example, Weber and Sen (1985).

[14] This finding parallels that obtained by Raphael (1995) for San Francisco Bay Area teenagers.

Appendix C: Explicit Tests for Endogeneity

As noted in the text, a major concern in designing and interpreting the statistical models of labor market outcomes is the exogeneity of the neighborhood variables that have been measured. The statistical models have been designed to guard against the possibility that these geographic indicators are endogenous to labor market choices. We address the simultaneity issue by considering the decisions of "at home" youth, whose residence choices have been made by parents, and by relying upon extensive measures of household demographics. Despite this, the possibility remains that some unobserved characteristics of households affect both neighborhood choices and youth employment choices.

This appendix provides further evidence on the exogeneity of neighborhood characteristics based upon the Hausman specification test.

In the text, four variables are used to measure aspects of urban neighborhoods: percent white (X_1), percent receiving public assistance (X_2), percent of adults not at work (X_3), and the census tract access measure (X_4). These variables are used in a variety of logit specifications. The most general of these are two logit models including three of the measures: (X_1, X_2, and X_4) and (X_1, X_3, and X_4).

We construct instruments for each of these four variables. We then include the instruments, together with the

Table C1
Tests of Exogeneity of Neighborhood Influences upon Employment Outcomes for Newark Teenagers[a]
χ^2 Statistics

Age Group	In-School Youth	Out-of-School Youth	All Youth
A. Neighborhood Influences: Percent White, Access, Percent on Public Assistance			
Ages 16–20	8.045	3.669	7.513
Ages 16–19	8.596	2.347	6.027
Ages 17–20	9.397	4.014	7.343
Ages 17–19	10.146	3.908	5.395
B. Neighborhood Influences: Percent White, Access, Percent Adults Not at Work			
Ages 16–20	4.536	3.895	5.114
Ages 16–19	4.303	2.364	3.294
Ages 17–20	5.846	4.529	5.169
Ages 17–19	5.616	4.439	2.772

[a] The critical values of χ^2 with 3 degrees of freedom are 7.810 and 11.300 respectively at the 0.05 and 0.01 levels of confidence.

original variables in the logit model, and finally test the joint significance of the instruments. The hypothesis that the neighborhood variables are jointly exogenous can be tested using standard likelihood ratios.

As instruments, we use census tract measures correlated with each of these four neighborhood indicators but not themselves determinants of employment choice. For percent white, we use as an instrument the tenure of the household and the percentage of housing of that tenure type in the tract. (There is abundant evidence that, for reasons of permanent income, racial discrimination, and so on, minority households, other things equal, differ systematically in tenure type from white households. But, practically no one would argue that homeownership causes higher levels of employment.)

For the percent receiving public assistance and the percent of adults not at work, we use a measure of the availability of appropriately sized units, conditioning on household size.[15]

For the access measure, we employ the fraction of workers of common industry and occupation in the MSA residing in the tract. This is a measure of the heterogeneity of industry or occupation of any household member.

Table C1 reports the results of the Hausman specification test for Newark youth in differing age groups. The tests are constructed separately for in-school and out-of-school youth and for all youth.

As the table indicates, in no case can we reject the hypothesis of the exogeneity of the neighborhood influences at the 0.01 level. At the 0.05 level, we can reject the hypothesis of exogeneity for in-school youth of one of the models, but not the other.

As shown in the table, when the model includes a variable measuring the percent on public assistance, the χ^2 is significant for one subsample, in-school youth. However, when the model includes a variable measuring the percent of adults not at work—perhaps a superior measure of the availability of informal information about employment opportunities—each of the three measures of neighborhood effects upon teenage employment is shown to be exogenous, according to conventional statistical criteria.

[15] We can use the same instrument for both neighborhood measures because we never use these variables together in any logit estimation. The housing availability measure weights the fraction of the housing stock in the census tract of each size (number of rooms) by the relative frequency in the MSA that a household of that size (number of individuals) lives in that sized unit. This is a probabilistic measure of residence based on the availability of "typical" housing.

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Discussion

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Katherine O'Regan and John Quigley have written an excellent paper, using intra-urban spatial variation to try to isolate the connection between neighborhood and employment and schooling outcomes for teenagers. They find strong effects of neighborhood poverty and unemployment on teenage employment and idleness (not being at school or at work). They also find that actual physical access to jobs is relatively unimportant. It seems like another victory for the loose form of the spatial mismatch hypothesis (segregation by race and income affects employment outcomes), although something of a loss for the strict form of the spatial mismatch hypothesis (location matters because of transportation distance to work).[1] The paper is clear and well done, and it is truly "state of the art" in using cross-neighborhood variation within cities to identify the effects of neighborhood on outcomes.

My thoughts on this topic will be arranged in two categories: (1) discussion of the implications of these results for policy and for future research, and (2) discussion of the basic approach of using intra-urban variation to identify neighborhood effects. The first section accepts O'Regan and Quigley's results and discusses what they mean for policy; the second section discusses the perils of using within-city data for these purposes and how one might eliminate some of those dangers.

The Implications of O'Regan and Quigley's Results

The empirical issues involved in estimating the importance of space are extremely dense and often daunting. Issues of omitted variables, endogeneity, and measurement error plague the research in this area (including, of course, my own work). O'Regan and Quigley's research represents a superb effort, but there can be no doubt that we are still far from being able to establish conclusively (1) a firm connection between neighborhood and outcomes, or (2) the neighborhood mechanisms that really matter, or (3) the way these neighborhood mechanisms influence childhood development and employment outcomes. However, these issues are so important, and they extend to so much of the work in social science and to such a wide range of policy-making, that we must welcome truly significant contributions like that of O'Regan and Quigley quite warmly. We also must hope that this conference represents a renewed commitment to continue the quest for more understanding and better methods for dealing with these problems. This first section of my comments presents a brief description of why these issues are so important and what O'Regan and Quigley's results in particular mean, both for policy and for social science.

[1] Kain (1968) is the father of the spatial mismatch hypothesis. I have taken to splitting the hypothesis into strong and weak forms, where the strong form states that minority problems are related to distance from jobs and the fact that minorities are constrained to live in their neighborhoods, whereas the weak form argues that segregation, which is a result of discrimination, leads to poor minority outcomes.

Implications of a Connection between Neighborhood and Outcomes

The very existence of a strong causal connection between neighborhood and individual outcomes immediately implies the existence of strong, spatially related externalities, especially if that connection does not work through the provision of local public goods. If a person's identity influences, to even a small degree, the outcomes of his entire neighborhood, then private, free market outcomes there may be not only inequitable but also, quite possibly, highly inefficient. A classic externality exists, because an individual's skill level and work habits influence his neighbors' outcomes in a way that is not regulated through the market.

Location-specific spillovers stemming from the effects of concentration of poverty may suggest, among other things, a need for strongly subsidized education for the poor. As the education of one member of the neighborhood will benefit all of his neighbors, that person's education choice will not internalize all of the neighbors' benefits, and that person will underinvest in education relative to the social optimum. Individual migration decisions will also fail to internalize effects on local neighborhoods. In principle, such results could provide a rationale for a federal government role in reducing white flight, for example, or subsidizing other migration decisions. Once we have clearly established the connection between neighborhood attributes and outcomes, the floodgates have been opened for justifying a myriad of governmental policies. Of course, the standard cautions (which this author believes strongly) about the tendencies of governmental policies to exacerbate rather than improve existing market failures also apply in this case.

> *If we believe in neighborhood effects, then by altering where the poor live or who their neighbors are, we can improve their lives.*

A particular example of this last point occurs when local governments take actions that change the neighborhood composition of adjoining areas. One locality may create attractive zoning regulations that draw the wealthy from another area and thus impose significant externalities on the poor remaining in the other area. While I believe strongly in the benevolent effects in many cases of local competition among governments, just as I believe in the benefits of local competition among firms, the presence of substantial externalities may limit the extent to which we want to decentralize certain types of power to local hands. In particular, local control over redistributional activities is known to lead to sorting by income classes. If neighborhood effects are real, then this income sorting may be highly inefficient and socially costly in a way that will not be internalized by local governments.

Of course, these are primarily efficiency issues, and much of the discussion in this area relies on equity concerns. If neighborhood effects are clearly established, then it becomes tempting to ask whether we cannot use these neighborhood effects to achieve equity goals of redistribution between races or between income groups. In other words, if we believe in neighborhood effects, then by altering where the poor live or who their neighbors are, we can improve their lives. Of course, it is still a matter of debate whether space-based redistributional methods (which might include programs helping minorities relocate or community-based redevelopment projects) are particularly efficient means of achieving equity goals. It may well be that cash or simple in-kind transfers are cheaper and more effective means of achieving equity goals than attempts to guide which neighborhood people choose to live in. Naturally, even the most recalcitrant opponent of space-based programs would be forced to accept that it would be of clear social benefit to eliminate spatial distortions created by government policies, such as greater availability of or more access to AFDC payments in high poverty areas, or police discrimination in white neighborhoods.

Finally, it is worthwhile mentioning that documenting the kinds of neighborhood connections that operate is of huge relevance to economics and other social sciences. Much of modern growth theory hinges on externalities in the production of knowledge. Issues in labor economics and macroeconomics are also possibly related to the presence of spillovers across workers in the accumulation and use of human capital. This type of research is invaluable in helping us to document the presence or absence of such forces.

Implications of How Neighborhoods Change Outcomes

One of the strongest implications of O'Regan and Quigley's work is that local poverty matters, while physical distance to jobs does not matter nearly as much. The authors do not really try to distinguish between different forms of poverty or joblessness, but rather restrict themselves to distinguishing between the two fairly different hypotheses. They make a strong case for the importance of local poverty, relative to local job access.

Ideally, we would be able to sort out which types of poverty matter most in creating neighborhood effects. Do neighborhood effects work through the percentage of the population relying on the government? In that case, the admittedly odd implication would be to eliminate all social programs. Is the important attribute the raw income level of the neighborhood? Then, the implication might be to hand out cash. Is the important attribute the concentration of the adult idle? In this case, the goal of policy should be getting people to work. Alternatively, racial composition or some other variable might represent the crucial neighborhood effect.

As pleasant as it might be to believe that we can simply use multiple variable regressions to distinguish among these hypotheses, I am dubious at best about the possibilities for this type of work (O'Regan and Quigley are, too). These poverty-related neighborhood characteristics are tightly correlated in the data. Distinguishing between the effects of unemployment versus poverty versus single-parent families is enormously difficult. The selection and endogeneity problems differ for each one of these variables and further complicate the analysis, and I am not sure that we ever will believe anything we see that differentiates between these forces.

Given these problems, I believe that Quigley and O'Regan adopt the right approach. They basically look at two hypotheses: Is it neighborhood composition that is the major difficulty in poor neighborhoods? Or is it the lack of proximity to employment? These two variables, neighborhood composition and physical location of employment, are not tightly connected, and the authors do seem to be able to effectively distinguish between them. They reject the idea that the lack of proximity to jobs is the major problem. My own work in this area (Cutler and Glaeser 1995) has also found that proximity to jobs is not a particularly large determinant of neighborhood effects. It does seem that the problem of poorer neighborhoods is not the absence of local employment but rather the presence of broader social problems that leave lasting scars on youths growing up in poverty-stricken areas.

The most straightforward interpretation of O'Regan and Quigley's work is that job access, the variable that relates to immediate benefits from legal-sector employment, is relatively unimportant. The variable that appears related to a culture of poverty and its effect on long-term human capital development is important, however. The findings suggest that we should not expect that an individual who is whisked away from a poor neighborhood and dropped into a high-employment neighborhood will immediately see an improvement. More likely, the children of this individual will have better peers and better role models and will eventually learn from the new location. The implication is that neighborhoods are about long-term accumulation of skills or attributes and not about an immediate return to paid work.

Such a policy conclusion casts doubts on the effectiveness of employment zones, enterprise zones, improvements in transportation for inner-city residents, or any policy focused primarily on cutting the costs of moving between ghettos and jobs. While such programs surely will not hurt, and may even be of some benefit, they will not address the primary problems of inner-city neighborhoods. The benefits of these programs will show up only gradually, and through an indirect effect of employment levels on long-run human capital accumulation. Neighborhoods will change, if the O'Regan and Quigley results are right, only if the cycles of poverty are broken and if their residents become employed and acquire human capital. Unfortunately, this type of policy implication goes against any kind of quick fix. Urban policy must be about changing long-run human capital accumulation and altering the patterns of family responsibility.

Implications of How These Mechanisms Work

Quigley and O'Regan do not really begin to tell us how locational unemployment levels actually drive youth idleness. The reader can immediately imagine several mechanisms by which this effect could work through the public sector or the provision of locational services. For example, even when different census tracts are part of the same school district, they may not have access to the same schools, and it may be school quality that is driving this effect.

Are crime levels higher in these poverty areas? The number of poorer youths involved in some form of crime is quite high: About 35 percent of the National Longitudinal Survey of Youth's 20-year-olds have committed crimes recently. Are these young adults just avoiding the legal sector? Is drug use and availability important for this group? Is gang membership important?

Are the poverty effects working through an absence of role models? An easy way to test this is to see whether neighborhood effects become important for children who have both parents present and "successful," where successful may just mean employed. These mechanisms should not be seen as alternatives to a basic poverty effect; rather, analysis should first document the role of poverty and then try to decompose the ways that poverty drives poor outcomes.

It would also be helpful to know if neighborhood effects are seen for younger children, and at what age these forces start to be important. When do we begin to see school dropout rates respond to local area attributes? Naturally, all of these questions form an agenda for many future papers and go far beyond the scope of this work, but they are important if we are to formulate policy on the basis of these types of results. For example, if we found that all the neighborhood effects worked completely through school quality, and school quality was a function of spending, then it would make sense to consider equalizing school spending. If neighborhood effects worked through school quality, but the relevant school quality effect worked through peer interactions at the school, then busing or, alternatively, a measure for paying children with high human capital to go to school with children with low human capital might be preferable. If neighborhood effects worked through high crime rates, and these crime rates discouraged legal activities and encouraged illegal activities, then altering the policing structure might be appropriate.

The point is not that a clear mandate exists on what should be done, but rather that determining *how* neighborhoods affect outcomes, if indeed neighborhoods do affect outcomes, is critically important for determining our overall policy approach. We cannot even begin to think about the right steps to take to eliminate the problems of the inner city without first being convinced that neighborhoods, rather than individuals, are important factors in creating social problems; without knowing which types of neighborhood characteristics drive poor outcomes; or without understanding the mechanisms by which they drive these outcomes.

A Discussion of the Intra-Urban Approach

As I have argued elsewhere (Cutler and Glaeser 1995), using intra-urban variation to identify the effects of neighborhood characteristics on individual outcomes poses two major problems. O'Regan and Quigley are aware of both, but it is worthwhile discussing the assumptions needed to avoid these problems and whether or not we think that these assumptions are palatable.

The first problem is that omitted family and child characteristics surely are highly correlated with neighborhood choice. Neighborhoods are endogenously chosen, and individuals select into different locations based on their characteristics. Some of these characteristics will be the observables that O'Regan and Quigley do use in their work. Other relevant characteristics relating to neighborhood choice might be the willingness to sacrifice for future benefit (patience), unobserved human capital and skills, or connections with and attitudes toward mainstream society. If negative attributes are correlated with choices to live in poorer neighborhoods, then our estimates of neighborhood effects will be biased upward, since neighborhood characteristics will be correlated with omitted variables that work in the same direction (as long as bad neighborhoods attract low-potential individuals).

The price of going to inter-urban variation is a tremendous loss of the variation found in neighborhood differences.

The second problem, which also stems ultimately from the endogeneity of neighborhood choice, is that identical individuals must in equilibrium be indifferent between neighborhoods. Thus, the marginal individual making the decision about neighborhood location must be indifferent between living in a poor neighborhood and living in a rich neighborhood. (Housing prices surely go a major part of the way to induce this indifference.) This effect will mean that we should not see neighborhood differences in utility levels of the decision-makers, if we are able to control for all individual attributes.

My approach to these problems has been sheer cowardice. David Cutler and I avoided using intra-urban variation entirely and identified neighborhood effects from inter-urban variation. We were able to use governmental and topographic features of different urban areas to instrument for the degree of segregation within the area. Unfortunately, we had only weak methods of dealing with inter-urban mobility, which is also endogenous. More important, the price of going to inter-urban variation (also the approach used in O'Regan and Quigley 1995b), is a tremendous loss of variation. In the extreme case, where every urban area was identical but had huge neighborhood differences, inter-urban variation would yield no evidence whatsoever. While the world is less extreme than that, all researchers lose a large amount of information when they give up the information contained in within-city data, and a huge cost is attached to adopting that type of strategy. I think that in the long run we will be better off figuring out ways to use the intra-urban data than we are relying solely on inter-urban variation.

However, using intra-urban variation requires dealing seriously with all the potential biases that such data create. Consider the following earnings equation:

$$E_i = X_i'\beta + Z_j'(\theta(Z) + \theta_i) + \alpha_i + \varepsilon_i \qquad (1)$$

where E reflects some outcome variable (perhaps earnings, or some propensity towards idleness), X represents observed individual characteristics, β the returns to those characteristics, Z observed neighborhood characteristics, $\theta(Z)$ the average returns to those characteristics, θ_i the individual specific returns to those characteristics, α_i omitted ability, and ε_i an independently distributed error term. The potential problems with using ordinary least squares to estimate the equation, and the possible solutions, are discussed below.

Case One—Garden Variety Omitted Variables

In this case, $\theta(Z) = \theta$, $\theta_i = 0$, and the covariance of α_i and Z is not equal to zero. Ordinary least squares will yield biased coefficients, because neighborhoods are correlated with unobserved attributes. O'Regan and Quigley (1995a) are aware of this problem and handle it by implicitly assuming that parental job attributes determine location and that these attributes are orthogonal to teenage attributes. In their words, household choice is "made by the parent(s), using the standard transportation-housing costs calculus. Household choice is exogenous to the transport demands of youth." As the equation illustrates, the necessary condition for unbiased estimates is not the exogeneity of location choice with respect to youth's employment concerns, but rather the orthogonality of location with respect to youth's employment concerns.

The authors assert, perhaps correctly, that households do not choose location based on what will make employment more probable for their children. I am skeptical of this comment in many cases, especially given what we know about how sensitive parents are to school quality in their location choice. Nevertheless, even accepting this assertion, the parental factors that induce parents to locate in high-poverty areas are surely correlated with the characteristics of youth that determine employment probabilities. Indeed, O'Regan and Quigley assert that, in their data, family characteristics "really 'matter' in the empirical results." If the observables matter so much, surely the unobservables matter too, and the results are biased.

How can we work to improve this problem? First and most classical is the instrumental variables approach. The goal is to find a parental characteristic that determines location but is clearly orthogonal to omitted youth characteristics that drive location. One possibility is that the industrial or occupational training of parents might influence locational choice.

Naturally, we would have to control for the overall quality of industry or occupation as well. The method would involve creating a location measure for each industry/occupation pair and also an average wage and average skill measure for each industry/occupation pair. The location measures (where the industry/occupation employment is located in the city) might be clean instruments if the industry/occupation quality measures are also included in the regression. Alternatively, in data samples where we know when the parents came to the city, we could use the areas of the city being built then to get a sense of where the parent would have been attracted to initially, and use that as an instrument. Ideally, we could use randomized data (such as the Gautreaux or Moving to Opportunity experiments) to get better instruments as well.

A second approach is to get a sense of how big the selection problems are. How much is sorting by parental observables? How strong is the correlation between parents and children? How big would the unobservables need to be, relative to observables, to invalidate the results? These kinds of sensitivity analyses are made possible by Quigley and O'Regan's use of Census variables with a battery of parental background data, and I believe that the authors should exploit this information as much as possible.

In a final approach, the authors could separate individuals into long-term and short-term residents of the community. Presumably location choice would be less of an issue for long-term residents. If the data showed that neighborhood was most important for long-term residents, this would lead us to believe that it is neighborhood that drives outcomes. If neighborhood is more important for short-term residents, then we would have to believe that outcomes drive neighborhood choice.

Case Two—Random Coefficients

In this case, $\theta(Z) = \theta$, and $\theta_i \neq 0$, but $\alpha_i = 0$, and the covariance of θ_i and the Z variables is not equal to zero. This is a version of the standard Roy model, where individuals have different returns to different neighborhoods and will select into the neighborhoods that give them higher returns. While the relative returns may be parental returns, so long as they are parental returns, ordinary least squares will yield biased coefficients, because neighborhoods are correlated with unobserved attributes. The problem here is not that omitted variables are present that positively affect employment and are also correlated with neighborhood, but rather that the returns to neighborhood location itself differ across neighborhoods. A particular, real world example of this concern is the fact that the minorities who have selected to live in rich neighborhoods are minorities for whom that neighborhood is particularly valuable, so that it is impossible to translate from information about those people to general results about the importance of location for minorities.

This version of the problem has two approaches. The first tends to be highly parametric and involves assumptions about the distribution of the returns to neighborhood. Luckily a large literature exists on this topic, stemming from Heckman's work in the 1970s, and well-worked-out techniques are available for dealing with this problem parametrically. However, while the robustness of the neighborhood results to Heckman-type corrections would be an extremely pleasant thing to see, I am not sure that skeptical readers would be completely convinced by this type of approach.

A second approach to this topic examines whether the returns to neighborhood location differ much, using observable characteristics. This type of test is readily performable and amounts to looking at the cross-effects between individual and neighborhood characteristics. These cross-effects are in fact intrinsically interesting, as well as useful in providing evidence about the extent to which returns to neighborhood differ over varying types of people. Of course, it is worthwhile remembering that even if little difference is found in the returns to neighborhood variables by observables, significant differences still might exist in the returns to neighborhood by unobservables.

Case Three—Endogenous Average Returns

In this case, $\theta(Z) = \theta(Z)$, $\theta_i = 0$, and $\alpha_i = 0$. Here the returns to different neighborhoods are the function of market forces, and in equilibrium the same people will be indifferent between neighborhoods; that is, the distributions of populations will select to the point where individuals are indifferent between different neighborhoods. In part, this issue is the most easily resolved by O'Regan and Quigley's argument that parents select on the basis of their own needs, not the needs of their children. If they are right, then parents will be indifferent but children need not be, and identification still makes sense. In this case, it is enough that location be exogenous, and we are not concerned about the correlation of location with unobservables.

While the argument that they use is both technically correct and quite possibly true, the authors could take this issue much more seriously. It would help to show the factors that parents select on and try and predict what determines the parent's choice of location, and to show that it has little to do with variables that affect children's outcomes. More generally, to the extent that the authors are able to indicate compensating differentials in other areas—high housing costs in the areas where children benefit most—it will be more plausible to believe that the equilibrium does not rely completely on children being indifferent. Indeed, in some ways this problem is the least troublesome, because it does not involve any estimation bias. Instead, what is involved here is the question of why we would expect to find neighborhood effects, if the ability to migrate between neighborhoods exists. Much of the answer assuredly lies in the nature of the equilibrium and of the forces that equilibrate the system.

These three problems with intra-urban data are potentially quite serious. I have presented them separately, but further problems arise if all three problems occur at once. However, approaches to these problems can be developed and O'Regan and Quigley have made invaluable steps forward, both by formalizing some of their responses to these criticisms and by using such a rich, strong data source.

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