Do Neighborhoods Affect Hours Worked: Evidence from Longitudinal Data

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ABSTRACT

Researchers have argued that neighborhoods are an important determinant of labor activity. Using confidential street address data from the NLSY79, respondents were linked to neighborhood social characteristics and measures of job proximity. A one standard deviation increase in the social characteristics of a neighborhood increases annual hours by 6.1%; a similar increase in job proximity raises hours by 4.7%. Labor market activity at the individual level is positively related to labor market activity of neighbors. But employment is not the only neighborhood characteristic that matters. Being in a disadvantaged neighborhood, as measured by a variety of characteristics, reduces market work. Social interactions have non-linear effects with the greatest impact in the worst neighborhoods. Social interactions are also more important for less educated workers. Estimates that do not account for neighborhood selection on the basis of time-invariant and time-varying unobserved individual characteristics substantially overstate the social effects of neighborhoods but understate the effects of job access.

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I. Introduction

Economists and sociologists have argued that neighborhoods affect labor market activity along with a variety of other youth outcomes. Two classes of models have been proposed. Social interaction models posit that low labor attachment in a neighborhood reduces individual labor attachment either because neighbors' behavior affects attitudes toward work or information about job opportunities.[1] Alternatively, spatial mismatch models argue that individuals living in neighborhoods spatially isolated from jobs work less because they have less access to information about job opportunities and face longer commutes.

Estimates of both sets of effects often indicate that neighborhoods are an important determinant of employment but raise concerns with endogenous neighborhood choice. In the case of social interactions models, unobserved individual characteristics that raise participation may lead individuals to choose better neighborhoods.[2] In the case of the spatial mismatch hypothesis, individuals with exogenously low labor force attachment have less incentive to locate in

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[1] See Wilson (1987, 1996), Massey and Denton (1993), Granovetter (1995), Kasarda (1996), Jargowsky (1997).

[2] Early work on social interactions (e.g. Datcher (1982)) find positive effects, although these studies have been criticized for lacking effective controls for neighborhood selection (see Jencks and Mayer 1990a). Many recent studies have employed instrumental variables strategies to control for neighborhood selection. These studies include Case and Katz (1991), who use the behavior of neighbors' neighbors as instruments; Cutler and Glaeser (1997), who use inter-city variations in rivers, highways, and number of jurisdictions as instruments for segregation; Evans, Oates, and Schwab (1992), who use county aggregates as instruments for local behaviors; and Bertrand, Luttmer, and Mullainathan (2000), who use ethnic differences in welfare recipiency. Corcoran, Gordon, Laren, and Solon (1992) and Solon, Page, and Duncan (2000) find weak effects of childhood neighborhood on subsequent outcomes despite weak controls for neighborhood selection. Plotnick and Hoffman (1995), Aaronson (1998), like the present study employ fixed effects strategies. A number of recent studies have turned to experimental designs to obtain exogenous variations in neighborhoods to address these concerns (Ladd and Ludwig (1997), Rosenbaum, DeLuca, and Miller (1999), Katz, Kling, and Liebman (2001), and Ludwig, Duncan, and Hirschfield (2001)). See Haveman and Wolfe (1995), Deitz (Forthcoming) and Haurin, Deitz, and Weinberg (Forthcoming) for a recent reviews.

neighborhoods with good job access. But estimated effects of job access can also be biased downwards since individuals with weak labor force attachment may be attracted to older neighborhoods around central business districts with the best job access (see Glaeser, Kahn, and Rappaport 2000).[3] Thus, endogenous neighborhood choice is likely to bias estimates of social interactions upward, while estimates of the mismatch hypothesis may be biased in either direction.

We address these questions using a unique data set. For administrative purposes, exact street addresses were recorded annually for respondents to the 1979 National Longitudinal Survey of Youth (NLSY79). Using geographic mapping software, respondents were matched to neighborhood social characteristics from the 1990 Census of Population and to job densities constructed the 1987 Economic Census. The NLSY79 data have a number of attractive features for our purposes. First, the longitudinal aspects of the data make it possible to track the same individuals over time as they move across neighborhoods. In addition, the data focus on individuals at the outset of their work careers for whom neighborhood influences are believed to be strongest. Finally, these data oversample blacks and Hispanics making it possible to obtain precise estimates for these groups.[4]

[3] The literature on spatial mismatch dates back to Kain (1968), who finds that as employers' distance from black neighborhoods declines so does the fraction of jobs held by blacks (see further analyses by Offner and Saks (1971) and Leonard (1987)). Much of the work on spatial mismatch exploits inter-neighborhood variation within a metropolitan area. In an influential study using this type of cross-neighborhood variation, Ellwood (1986) found only weak effects of job proximity on employment. More recent intra-city studies by Ihlanfeldt and Sjoquist (1990) and Raphael (1998) have supported the mismatch hypothesis. Another strand of the literature exploits variation across metropolitan areas. Inter-city tests of the mismatch hypothesis generally find stronger effects of job access than do cross-neighborhood studies within a single metropolitan area (examples include Mooney (1969), Farley (1987); Ihlanfeldt and Sjoquist (1989); and Weinberg (1999,2000), although Dworak-Fisher (2002) finds smaller effects; Martin (2001) shows continuing importance of the mismatch). Only recently has work in this vein begun to consider endogenous neighborhood choice (see Raphael (1998), Rogers (1997), Ross (1998) and Weinberg (1999,2000) and Jencks and Mayer (1990b) for a criticism of earlier work).
[4] The data also oversample economically-disadvantaged whites. The present study focuses on urban residents so this group, which is predominantly rural, is largely excluded from the analysis.

Our empirical work addresses four questions. First, we ask whether neighborhood social characteristics and job proximity affect individual employment behavior in an effort to distinguish a contemporaneous, causal effect of neighborhoods from neighborhood selection. Second, we consider whether neighborhoods have non-linear effects and whether the effects vary with individual characteristics. Third, we provide some evidence on the channels through which social effects operate. Finally, we address the various forms of heterogeneity that must be controlled to accurately estimate the effects of neighborhoods.

Our major findings can be summarized as follows. We find that neighborhoods have a significant impact on individual employment outcomes. Interestingly, both social influences and job proximity are found to be important determinants of work.[5] A one standard deviation increase in the social characteristics of a neighborhood increases annual hours by 6.1%; a similar increase in job proximity raises hours by 4.7%. In keeping with existing work, these effects are found to be non-linear: Social influences have the greatest proportional effects in the worst neighborhoods (see for example Crane 1991). Their proportional effects are greatest for less educated individuals. There is some evidence for greater proportional effects among Hispanics. Our results also shed light on the question of how social influences operate. Labor market activity at the individual level is positively related to labor market activity of neighbors, but employment is not the only neighborhood characteristic that matters. We find that educational attainment in neighborhoods has effects that are comparable to those of neighborhood employment. These findings suggest that the

[5] While few studies compare these effects, those that tend to find that social influences have stronger effects than job access (Cutler and Glaeser (1997); O'Regan and Quigley (1992); and Conley and Topa (1999)). Only Weinberg (2000) finds stronger effects of job access.

labor activity of neighbors *per se* may not be the crucial factor.

We examine a variety of statistical models to determine the robustness of the finding that neighborhoods influence labor market choices, starting with weak controls for individual differences and introducing more thorough heterogeneity controls. We exploit the panel aspects of our data to control for both time-invariant individual differences (fixed effects) and individual differences in life-cycle profiles (individual-specific experience profiles). Our results suggest that estimates of neighborhood effects that do not control for heterogeneity properly, even those with a rich set of controls, overstate the social effect of neighborhoods by an order of magnitude.

In contrast, job access generally has a negative impact on hours worked with incomplete controls for unobserved heterogeneity but a significant, positive effect with appropriate controls. This finding indicates that job access is an important determinant of labor attachment but that estimates of job access relying on within-city variation without controls for selection are biased downwards because the neighborhoods with the best job access (i.e., those that are close to central business districts) generally attract individuals with low labor attachment. This finding can explain the relatively weak effects of job access found in intra-city studies of the spatial mismatch hypothesis (e.g. Elwood 1986).

Even our most conservative estimates may overstate the effect of neighborhoods if exogenous innovations in employment status affect the choice of neighborhoods. After presenting our estimates, we assess this possibility by studying the timing of changes in work behavior around moves. There is little evidence for reverse causality.

Two issues that have received considerable attention in the current literature. The first is structural estimation, which attempts to distinguish the channels through which neighborhoods affect outcomes (see Manski (1993), Duncan, Connell, and Klebanov (1997), Brock and Durlauf (1999), and Moffitt (2001)). We focus on controlling for neighborhood selection because we regard it as a pre-condition to structural estimation. This study differs from a number of other recent studies that have addressed neighborhood selection using experimental data. While these studies are valuable, they provide estimates of the effect of the treatment on the treated and are ill suited to assessing the impact of neighborhoods on a randomly selected person.[6] Insofar as one quarter of our sample moves in any given year and 90% of our respondents move at least once, our estimates will better reflect the outcomes of moving a randomly chosen person in the population. Our naturally-occurring data are useful for addressing a different set of questions than experimental data, which frequently involve large interventions (in terms of neighborhood social characteristics). Movers in our study are likely to be better integrated into their new neighborhoods, making our estimates more appropriate for assessing the effect of typical moves or the effects of interventions that alter a neighborhood but leave the underlying social connections intact (see Sobel 2001).

The next section describes the data. The estimation strategy is outlined in section III. Section IV presents the empirical analysis. Section V considers reverse causality. Section VI concludes.

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[6] Consistent with this, these studies often indicate that people who moved under the programs studied were those whose outcomes in their original neighborhoods would have been the worst if they had not moved (see Katz, Kling, and Liebman (2001)). Another issue that arises in experiments is the implications of expanding them (see Sobel (2001)).

II. Data Description

Sample Construction

The primary data source for this study is the National Longitudinal Survey of Youth 1979 (NLSY79). Our data cover the 1979-1994 and 1996 waves. The survey began in 1979 with a sample of 12,686 men and women born between 1957 and 1964. Annual interviews were conducted from 1979 to 1994, and biennially thereafter. The work history files contain detailed longitudinal records of the employment history of each respondent.

In order to construct a sample suitable for empirical analysis we introduce several selection criteria. We limit the sample to a subset of the 6,403 young men in the survey. Because our interest lies in post-schooling labor market activity we follow individuals from the first time they leave school. To avoid counting summer breaks or other inter-term vacations as leaving school, we define a schooling exit as the beginning of the first non-enrollment spell lasting at least 12 consecutive months. Accordingly, respondents are excluded from the sample if the date of schooling exit cannot be clearly ascertained from the data.

Table 1 provides a detailed summary of sample deletions. In order to enter our sample, a respondent must have at least 8 valid observations as a civilian, residing in an MSA, with an exact address match, so we can identify the Census block group in which they reside.[7] In addition, we impose two additional restrictions: (1) Address data that could be matched exactly to a location in the TIGER files for at least 8 years; (2) Valid data for all other variables used in the main analysis, except AFQT score and mother's highest grade completed.

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[7] For an observation to be included we require two consecutive interviews, because we use location information from interview year t and information on the work history and income during year t from interview year t+1.

Table 1 demonstrates how racial composition, age in 1979, rural residence in 1979, educational attainment and performance on the Armed Forces Qualifying Test (AFQT) change as we delete respondents who fail to satisfy various sample selection criteria. The two restrictions that have the largest effects on the sample composition are the requirement of at least 8 observations since leaving school and the requirement that the respondent be living in an MSA during those observations. The first causes a large drop in percent white and a small decrease in education and AFQT. The MSA restriction causes a further decline in the fraction of the sample that is white primarily due to the loss of observations from the predominantly rural poor white oversample.[8]

On the whole, although we implement stringent sample selection criteria, we experience only modest changes in the sample composition. Moreover the geocoding restriction itself has little effect. Thus, we do not believe that we have introduced serious sample selection bias as a result of our sample selection criteria.

Summary Statistics

Table 2 presents sample statistics, based on the person/years, for the variables used in this study. The main dependent variable in our analysis is the natural logarithm of annual hours plus one. We also consider annual hours measured in levels, rather than logs, and the log of annual earnings. To create a measure of annual hours, we utilize the work history hours array, which contains the usual hours worked per week at all jobs from Jan. 1, 1978 through the final interview week. We then sum over all weeks in each calendar year to obtain a measure of annual hours. Data on

[8] The poor white oversample was interviewed at most 12 times, while the cross section and black and Hispanic oversamples were interviewed up to 17 times by 1996. Moreover the poor white oversample is heavily rural. For these reasons, its members are less likely to satisfy the requirement of at least 8 observations in an MSA after leaving formal schooling for at least 12 months.

calendar year earnings were drawn from questions about earnings over the calendar year prior to each interview. Hours and earnings for each calendar year were then linked to data on the respondent's residence at the previous interview so that the residence is contemporaneous with both calendar year hours and earnings.[9] This structure was chosen because most surveys occur during the summer, so that if moves occur midway between interviews on average, the calendar year will best reflect the place of residence.

To capture individual differences in ability to acquire skills, we include the score from the Armed Forces Qualifying Test (AFQT). AFQT scores are constructed from a subset of scores from the Armed Services Vocational Aptitude Battery (ASVAB), which was administered to 94% of the original NLSY79 respondents in 1980. AFQT is used by the armed services to assign workers to various jobs within the military and is considered to be a useful measure of worker trainability (Neal and Johnson, 1996). The AFQT score that we use in our regression analyses is adjusted for the age at which the respondent took the test.[10] The respondents in our sample include the oversamples of blacks and Hispanics, along with a handful of poor whites, who perform less well on the AFQT than a representative sample, like the cross section. Therefore, it is not

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[9] To be included in the sample in any given year a respondent also had to complete the following wave of the survey. This requirement was necessary in order to ensure that the residential location and earnings data are contemporaneous. The earnings questions pertain to the previous calendar year (except in the 1996 wave when the survey was biennial and two years of earnings data were collected), but the residential information pertains to the current place of residence. Requiring two consecutive interviews to be completed ensures that earnings data are matched to data on the residence at the time the earnings accrued. We note that the two-consecutive interview requirement was not necessary for constructing annual hours in that respondents who missed interviews were asked about their hours worked since their last interview. Our procedure of requiring two consecutive interviews does increase the accuracy of the hours data because accuracy drops as time elapses.

[10] The test is designed and normed for individuals age 17 and above, but was administered to NLSY79 respondents in 1980 when some were 15 and 16. On average they scored 10 percentage points below the older respondents. To control for this, we use the cross section sample within the NLSY79 to construct birth year mean scores. We then take each respondent's individual score and subtract from the birth year mean.

surprising that the mean deviation in AFQT from birth year means is –8.7.

The average number of respondent's own children residing in the household is 0.6. This number is low due to the age range of the respondents, lack of marital stability, and tendency for children of divorce to reside with the mother.

We geocoded the respondents addresses and merged in neighborhood social characteristics at the Census tract and block group level from the 1990 Census of Population. Information on job access came from the 1987 Economic Census that reports employment at the zip code level. Jobs were matched to the latitude and longitude of the zipcode centroid. In geocoding the respondents addresses we identified the latitude and longitude of current residence. Combining this information we were able to calculate the number of jobs within a 5-mile radius of the respondent weighted by the inverse of the distance from the respondent's residence. We also construct population densities in an analogous procedure using zipcode data from the 1990 Census of Population. To control for differences across metropolitan areas in transportation costs and density, the job and population densities are taken as log deviations from MSA means.[11]

Since we rely heavily on fixed effects estimation, identification of social characteristics and job access come from individuals who move across neighborhoods. There is a lot of movement across tracts and block groups in our data. Twenty-five percent (23 %) of the person/year observations involve a move across Census block groups (tracts). At the individual level, 92 percent (89 %) of the individuals move across block groups (tracts) at least once.

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[11] Non-differenced estimates are similar. The choice of a five-mile radius was made to reduce the computational burden. Experimentation showed that the results are not sensitive to this choice.

III. Estimation Strategy

This section describes our estimation strategy. As emphasized, a concern with existing studies of neighborhood effects is that the choice of neighborhood is affected by unobserved characteristics. There are two fundamental approaches to this problem – to start with contaminated data and introduce controls for heterogeneity; or to identify and exploit an exogenous source of variation in neighborhood choice. We have chosen the first approach in the absence of attractive instruments for neighborhood choice. To gauge the impact of individual heterogeneity on estimates of neighborhood effects and to enable the reader to assess the appropriate controls, we start with specifications with weak controls for individual heterogeneity and introduce more thorough controls. We then investigate the effectiveness of our controls by investigating changes in labor market activity around moves.

We exploit the longitudinal aspects of these data by estimating panel regressions. Consider a general model,

$$y_{it} = \boldsymbol{a}_{it} + X_{it}\boldsymbol{b} + N_{it}\boldsymbol{g} + U_{it}\boldsymbol{q} + \boldsymbol{e}_{it}.$$

Here, y_{it} is a measure of labor market outcomes for individual i in year t. We consider a range of specifications. Our main results take y_{it} to be the natural logarithm of annual hours (plus one). This places greater weight on changes in hours around zero, because the same absolute change represents a greater percentage change. We also consider annual hours in levels as well as the log of annual income.

The individual's observable characteristics at time t are given by X_{it}. The social characteristics and job accessibility of the neighborhood in which he resides at time t are given by

N_{it}. The unemployment rate at time t in i's county of residence is given by U_{it}.[12]

We consider three main specifications: OLS estimates, in which the intercept is constrained to be equal for all individuals ($\boldsymbol{a}_{it} = \boldsymbol{a}\ \forall i,t$); fixed effects models, in which the intercept is allowed to vary across individuals but not over time ($\boldsymbol{a}_{it} = \boldsymbol{a}_i\ \forall t$); and fixed effects models that allow the effects of experience to vary across individuals.[13] The latter estimates are based on individual deviations from the typical experience profile where the strength of the common experience profile is allowed to vary across individuals. These estimates were generated in a three-stage procedure. First, we construct a typical experience profile for each variable (dependent and independent) by regressing it on a quadratic in experience using all person/year observations in the sample. Let z_{it} denote an arbitrary variable used in the analysis and e_{it} denote years of potential experience, we estimate

$$z_{it} = \boldsymbol{f}_{1i}^{z} + e_{it}\boldsymbol{f}_{2}^{z} + e_{it}^{\ 2}\boldsymbol{f}_{3}^{z} + \boldsymbol{u}_{it}^{z}.$$

Predicted values of each variable are then obtained

$$\hat{z}_{it} = e_{it}\hat{\boldsymbol{f}}_{2}^{z} + e_{it}^{\ 2}\hat{\boldsymbol{f}}_{3}^{z}.$$

To allow the effect of experience to vary across individuals, second stage individual-specific regressions were run in which each variable was regressed on an intercept and the typical experience profile for that variable as follows:

$$z_{it} = \boldsymbol{m}_{i}^{z} + \hat{z}_{it}\boldsymbol{y}_{i}^{z} + \boldsymbol{z}_{it}^{z}.$$

[12] We have also considered models that include year effects, but do not report them here because the year effects are frequently insignificant and have little impact on the parameters of interest.

[13] Unmeasured neighborhood characteristics generate a correlation in the residuals from observations drawn from the same neighborhood. Our standard errors correct for this correlation. Our OLS standard errors also

The residuals from this regression were obtained. In the third stage regression, each variable (dependent and independent) was replaced by its $\boldsymbol{z}_{it}^{z}$, the deviation of that variable from its typical experience profile[14]

$$\boldsymbol{z}_{it}^{y} = \boldsymbol{z}_{it}^{X}\boldsymbol{b} + \boldsymbol{z}_{it}^{N}\boldsymbol{g} + \boldsymbol{z}_{it}^{U}\boldsymbol{q} + \boldsymbol{n}_{it}\,.$$

These estimates are reported below.

IV. Empirical Results

Baseline Estimates

Our initial measures of neighborhood characteristics are the employment rate of adult men in the respondent's block group and the job density measure. Adult employment is intended to capture social influences. To the extent that neighbors are an important source of information about job opportunities, neighborhood employment rates will also affect available information.

Table 3 reports our main estimation results, starting with OLS estimates, which suppress the individual fixed effects ($\boldsymbol{a}_{it} = \boldsymbol{a}\ \forall it$). Our first model includes only a limited set of controls (education, a quadratic in potential experience, and dummy variables for race and Hispanic background). The implied effects of a one standard deviation change in the neighborhood variables are reported in brackets beneath the estimates and standard errors. A one percentage point increase in adult male employment raises annual hours by 2.2%. Contradicting the spatial mismatch

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correct for the correlation in errors within individuals, which are estimated explicitly in the other specifications.

[14] An alternative procedure would have been to allow for individual-specific linear or quadratic experience profiles in addition to the individual fixed effects. Since hours are concave over the lifecycle, the present procedure allows for a more plausible functional form than a linear experience profile. This procedure, like the individual-specific linear experience trends, uses approximately 2 degrees of freedom for each respondent, whereas individual-specific quadratic experience profiles use 3 degrees of freedom for each respondent. Under the hypothesis that the effect of experience varies across individuals, but that the shape of the experience profile is similar across people, estimates from the current procedure should be similar to those that allow for

hypothesis, individuals living in areas that are closer to jobs do not work more than those that are far from jobs. Education and experience both raise work. Blacks work substantially less than observationally equivalent whites.

The second specification includes a richer set of time-invariant control variables (AFQT, a dummy variable equal to one if AFQT was missing, mother's education, a dummy variable equal to one if mother's education was missing, and foreign born) and time-varying controls (marital status and the natural logarithm of (one plus) the number of own children present). Including these controls reduces the effect of adult male employment. Job proximity takes on the expected positive sign, although the effect is small and estimated imprecisely. Higher AFQT scores are associated with more work. Surprisingly, an increase in mother's education is associated with lower hours. This result is due in part to colinearity with AFQT. Finally, people born outside the US work more than non-immigrants. As expected, married men and men with more own children present work more.

The standard deviation across neighborhoods of the employment rate and the job proximity measure are .148 and 1.065 respectively. Given these estimates, our estimates reported in column 2 suggest that a one standard deviation increase in neighborhood employment raises annual hours by 28% while a one standard deviation increase in job proximity leads to an increase of .4%. Although our dependent variable, annual hours, is not directly comparable to the neighborhood employment variable, the coefficient above 1 may indicate instability of the model. Overall, OLS estimates that control for individual heterogeneity using a rich set of control variables imply

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individual quadratic effects. Consistent with this hypothesis, the present procedure generates point estimates implausibly large social effects of neighborhoods and a weak or even negative effect of job proximity.

The between estimator (reported in column 3) is of interest in that it corresponds most closely to the estimator that would be obtained from pure cross-sectional data. The estimates for neighborhood employment exceeds the other estimates by a considerable margin, again indicating that naï ve estimates of social interactions substantially overstate the true effects. Similarly, the between regression shows small effects for job access.[15]

If neighborhood amenities are normal goods, one would expect individuals with higher labor attachment and hence higher incomes to choose to live in better neighborhoods. Similarly, individuals with strong labor attachment have the most to gain from living in neighborhoods with good job access. On the other hand, individuals with weak labor attachment may be attracted to the older neighborhoods near central business districts. To control for time-invariant individual differences in attachment that affect neighborhood choices, specifications 4 and 5 include individual fixed effects (these regressions are "within" regressions). After including individual fixed effects, the variance in the neighborhood variables is one quarter of the raw variance.

Controlling for fixed individual differences reduces the relationship between neighborhood employment rates and work by two-thirds compared to the corresponding OLS estimates. Thus, a correlation between unobserved fixed individual characteristics and neighborhood characteristics accounts for the majority of the social "effects" of neighborhoods estimated using OLS. Whereas

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that are quite close to those that allow for individual quadratics but standard errors that are substantially lower.

[15] We also estimated the model using GLS. Not surprisingly given the importance of selection, the model failed a Hausman test, yielding a p-value beneath .0001.

the OLS model implied an instability in the social effects, these estimates do not. Based on these estimates, annual hours worked are increased by 9.5% by a one standard deviation increase in neighborhood employment. Including fixed effects also makes the effect of job proximity positive and significant, both economically and statistically. A one standard deviation increase in job access leads to a 3.6% increase in annual hours. This finding indicates that OLS estimates of the effect of job locations are biased down because of a tendency for low attachment workers to cluster in neighborhoods that are close to jobs.[16] Thus, it appears that estimates of the mismatch hypothesis that use cross-neighborhood variation in job access risk understating the true effects of neighborhoods. Given the interest in manufacturing as a source of jobs for less skilled men, we tested the robustness of these results by focusing only on manufacturing job densities and obtained similar results.

Individuals may exhibit different growth rates in labor attachment as well as time-invariant differences. Individuals experiencing the greatest increases in labor force attachment might be expected to upgrade their neighborhoods more rapidly than others. To control for this possibility, the estimates in columns 6 and 7 control for individual-specific experience effects as well as individual fixed effects. The estimates of neighborhood employment are somewhat beneath the fixed effects estimates, however the effects of job locations are greater than those that only include individual fixed effects. This reduction in social interactions is consistent with dynamic selection on the basis of unobserved time-varying characteristics. On the other hand, it is well known that, in the

[16] Regressions of job density on distance from the central business district (measured using the location of the county courthouse of the central city or the central city of the main PMSA in a CMSA) show that job densities decline with distance from the business district. The same result holds true for manufacturing job densities.

presence of measurement error, the reduction in variance in the independent variables from fixed effects estimates biases such estimates downward (see, for example, Griliches and Hausman 1986). These estimates, which exclude 85% of the raw variance and 40% of the variance available in the fixed effects, may suffer from attenuation bias. Most likely, the true effects lie between these estimates and the fixed effects estimates. These estimates imply that a one standard deviation increases in neighborhood employment and job proximity raise annual hours by 6.1% and 4.7% respectively.

Alternative Specifications

This section considers a range of alternative specifications to investigate the robustness of the preceding results. The measure of work effort in the previous section was the natural logarithm of one plus annual hours. This specification places considerable weight on changes in hours among people with low hours. While much of the interest in neighborhoods arises from concern about people on the margins of working, it is worth probing the sensitivity to the non-linearity imposed by this model. Table 4, panel 1 takes annual hours, measured in levels, as our dependent variable (the models are the same as those reported in columns (2), (5), and (7) of table 3). The estimates of the effect of neighborhood employment in this model show sign patterns that are similar to those from the logarithmic model. OLS estimates of social effects are quite large and decline as fixed effects and then individual-specific experience profiles are included. Even in the most conservative estimates, a one percent increase in neighborhood employment raises annual hours by 1.48 per year or by .08% of its mean of 1886. The estimates for the job density variable also show a sign pattern that is similar to the logarithmic estimates, but the point estimates for the linear model are less precise. Thus, when annual hours are taken as the dependent variable the estimated effects of neighborhood employment and job access are substantially smaller than in the logarithmic model, suggesting that the effects of the neighborhood variables are higher for people who are at the margins to work and who receive more weight in the logarithmic specification.[17]

In panel 2 of table 4 we estimate a model with the natural logarithm of annual earnings as the dependent variable. There are two reasons why one might expect neighborhoods to affect annual earnings as well as hours worked. If a high level of labor force attachment in a neighborhood affects preferences toward work, one might see an increase in work effort conditional on hours worked as well as an increase in hours worked. Likewise, if better job access gives people better information about work opportunities, it may increase the quality of job matches as well hours worked. As indicated, neighborhood selection is likely to bias naive estimates of neighborhood effects upward if neighborhood amenities are normal goods. Given that hours affect the choice of neighborhood through income, this bias may be particularly acute with earnings as the dependent variable.

The estimates, presented in panel 2 of table 4, show similar sign patterns to the main estimates in Table 3, with the neighborhood employment variable declining consistently as more controls are included and the job density variable starting negative and becoming positive with the addition of controls. Given the reduction in sample, it is not surprising that the earnings estimates are less precise.[18] In all cases, the estimated effects are smaller (closer to zero) when earnings are

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[17] We also considered censoring in the data. Formally, the data have a tobit structure, with a modest portion of the sample (7.4% of respondent-years) reporting zero hours worked. Not surprisingly, tobit estimates, both those without fixed effect and those with fixed effects (estimated using a procedure developed by Honore (1992)) yield somewhat greater effects than those from OLS, but the two estimates are qualitatively similar.

[18] In order to focus on the effects of neighborhoods on job quality, we exclude years in which the respondent reported zero earnings. Because we require at least eight interviews with positive earnings, the switch of the

taken as the dependent variable than when annual hours are the dependent variable. Thus, there does not appear to be an effect of neighborhoods on hourly wages in addition to the effect on hours worked.

Panel 3 of table 4 returns to a model in which the dependent variable is log annual hours, but includes population density in addition to the other neighborhood variables. Population density can either capture heterogeneity as people with lower labor attachment choose to live in higher density inner-city neighborhoods or labor supply. These specifications show that population density is associated with lower annual hours in OLS, but it is insignificant in specifications that include either fixed effects or fixed effects with individual-specific time trends. If population density captures the effects of labor supply, one would expect a negative coefficient even in the models with fixed effects. The lack of significance with either fixed effects specification indicates that the negative effect of population density in OLS reflects the concentration of people with low labor attachment in inner city neighborhoods where densities are highest. For this reason, our remaining models exclude the population density.

In the fourth panel of table 4, we investigate the appropriate level of aggregation at which neighborhoods should be measured. Data are available at the Census tract and block group levels, with tracts being collections of a small number of typically homogenous block groups.[19] Because

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dependent variable reduces the sample 39% (from 27,313 to 16,667). If response rates decline with actual earnings, the estimated effect of the variables of interest earnings on estimates may be biased downward. To include people with zero earnings, we estimated models with log(1+earnings) as the dependent variable and obtained similar results.

[19] Block groups are clusters of blocks typically containing between 250 and 550 housing units, with the ideal size being 400 housing units. Census tracts are clusters of block groups, typically containing between 2,500 and 8,000 residents, that are homogeneous with respect to socio-economic characteristics (U.S. Department of Commerce 1992).

neighborhoods vary most at the most disaggregate level, there has often been the presumption that the finest measures of neighborhood are the most appropriate. On the other hand, interactions are likely to occur beyond one's most immediate neighbors, making somewhat broader measures of neighborhoods attractive. At this level of aggregation, the block group and tract-level variables are highly correlated, with the correlations typically lying between .8 and .9.

To explore these possibilities, we have re-estimated our basic log annual hours models using tract-level measures in place of the block group measures. The estimated coefficient on the tract level employment rate of adult men implies that a one standard deviation improvement in the employment rate of neighbors leads to a 4.6 percent increase in annual hours. This implied effect is only 75 percent of the 6.1 percent effect obtained using block group characteristics. Thus we conclude that nearer neighbors have a larger effect on labor market choices than do more distant neighbors.[20]

A Variety of Neighborhood Characteristics

The preceding estimates indicate that neighborhood employment rates and job access exert a strong influence on individual work decisions. This section reports estimates based on other measures of neighborhood characteristics for the specifications in columns (2), (5), and (7) of Table 3. The purpose of the analysis is to probe the sensitivity of the preceding results and provide some evidence on the channels through which social influences operate.

We examine five additional variables. We use the employment rate of adult females as a

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[20] Another dimension along which we can compare the performance of tract and block group characteristics is on their explanatory power. Given the extensive controls already included, models based on tract characteristics explain about the same amount of variance, measured by the R^2 of the regression, as do block group characteristics.

second employment variable to test the robustness of our finding for the adult male employment rate. If there are informational externalities to job holding, living in a neighborhood with higher employment rates of females, as well as males, should raise labor force attachment. We use two variables to measure aggregate levels of human capital in the neighborhood: the fraction of neighborhood residents, over the age of 25, that have not completed high school and the fraction that have completed college. We contrast the effects of employment and human capital with measures of general social disadvantage. For this purpose, we use the fraction of households with public assistance income and the poverty rate among 25 to 34 year olds in the neighborhood, who are closest in age our sample.

Table 5 reports regression results including one neighborhood variable in each regression. For comparison purposes we also report results in which male labor force participation and proximity to jobs are separately included. The patterns of the coefficients across specifications are similar to those reported in tables 3 and 4. Introducing fixed effects greatly reduces the implied social effect of neighborhoods. Based on these estimates, there is little question that estimates of the social effects of neighborhoods that fail to control for fixed individual differences risk overstating these effects. In our fixed effects estimates, we find that a broad range of neighborhood characteristics have a statistically significant effect on individual hours worked.[21] However, when individual-specific experience profiles are included only the estimate for adult male employment and job density are statistically significant at a conventional level. The strongest effects are consistently for neighborhood employment, which may indicate that the primary effect of neighborhoods on

[21] In column 2, only the poverty rate among 25 to 34 year olds is not statistically significant.

work operates through information about jobs and attitudes toward work, however the consistency of the sign patterns of the other variables make it hard to rule out other sources of causality.

Non-Linearities and Interactions with Individual Characteristics

The effects of neighborhoods are, most likely, not uniform. They may be greatest in the neighborhoods at the extreme low end of the distribution (Wilson 1987, 1991, Crane 1991, Galster, Quercia, and Cortes 2000, Krauth 2000). The effects of neighborhoods are also likely to be greatest for individuals who are closest to the margin to work. We examine these hypotheses by including higher order terms in the neighborhood characteristics and interacting the neighborhood characteristics with individual characteristics. The estimates reported are based on models that control for individual fixed effects.[22]

Table 6 reports the effect of including a quadratic term in the neighborhood measures. For the social measures, the estimated sign on the squared term indicates that neighborhoods have the greatest effects at the low end of the distribution. Thus, the employment of men and women in the neighborhood and the fraction of neighborhood residents who have completed college have positive linear terms and negative quadratic terms. Small or negative linear terms and negative quadratic terms indicate that the adverse effects of neighborhood high school dropouts, public assistance recipiency, and poverty among 25-34 year olds are greatest at high levels. Tests for the joint significance of the main effect and squared term indicate that the linear and quadratic terms in the neighborhood variables are jointly significant for both employment rates and the fraction of residents with college degrees. There is no evidence that job access has a non-linear effect,

[22] We use models that included fixed effects without individual-specific experience profiles, because once the

although the log specification already implies that the marginal benefit of additional jobs is greatest in the worst neighborhoods.

Table 6 also reports the effect of a one standard deviation change in each neighborhood variable. To highlight the non-linearities, these effects are reported at three points, the mean neighborhood and in neighborhoods that are one standard deviation better and worse than the mean, with the direction depending on the characteristic. For all social variables, the effects decline considerably as one moves from neighborhoods that are worse than the mean to those that are at the mean or better. The effects of a one standard deviation change in social conditions in neighborhoods that are one standard deviation better than the mean are generally close to zero.

These results also shed some light on which neighborhood characteristics are most important. The largest effects are for the employment rates of adult men in the neighborhood and the fraction of neighborhood residents who have completed college. The employment of women in the neighborhood and the fraction of residents who have not completed high school are both only slightly lower. The finding that female employment increases the labor force attachment of the men in our sample reinforces our conclusion that employment in general is one channel through which neighborhoods affect labor activity, as would be the case if job information from working neighbors was responsible for the effects. Given the high correlations between these variables and the possibility that the education of neighbors affects outcomes through its effect on employment, some caution is in order, but the strong effects of neighborhood education suggest that neighborhood employment *per se* may not be the only factor that affects labor force attachment.

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data are detrended so much of the variation in neighborhood characteristics is lost making it impossible to pick

Table 7 reports interactions with individual characteristics. Model 1 includes interactions with years of schooling minus 12 (the mean in the sample). For the positive neighborhood characteristics the education-interactions are negative, while they are positive for the detrimental characteristics. Thus, the social effects are lower for people with more than 12 years of schooling (education-12>0) and are greater for people with less than 12 years of schooling (education-12<0). Four additional years of school (beyond 12) eliminates all effects. Tests for the joint significance of the neighborhood variables and their interactions with education indicate that they are significant for all of the social variables other than college graduates and the poverty rate of 25-34 year olds.[23]

Model 2 in Table 7 explores interactions between the neighborhood variables and black and Hispanic background. The interactions with black consistently show that the social effects of neighborhoods are greater for blacks, but the differences are not statistically significant (see Duncan, Connell, and Klebanov 1997 on racial differences in neighborhood effects).[24] Given the standard errors, it is not clear whether the effects of neighborhoods are greater for blacks, however social influences do contribute to black-white employment differentials because blacks tend to live in neighborhoods with worse characteristics than non-Hispanic whites.[25]

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up interactions or non-linearities in the effects of neighborhood characteristics.

[23] One explanation for the greater response at low levels of education is that people with less education have lower hours and our log specification places greater emphasis on people with low hours. This does not account for our results in that the education reduces the effect of neighborhoods when the dependent variable is hours in levels (rather than logs).

[24] The interaction with poverty is the sole exception.

[25] The neighborhood male employment rate is lower for blacks than for non-Hispanic whites (59% versus 73%); larger (percentage) differences are seen in the fraction of adults in the neighborhood who did not complete high school (38% versus 22%), the fraction of adults in the neighborhood who completed college (12% versus 21%), the fraction of neighborhood households with public assistance income (19% versus 7%) and the fraction of 25-34 year olds in the neighborhood living in poverty (25% versus 9%).

The estimates for the social variables generally indicate that social influences have a larger effect on Hispanics, who may live in more homogeneous neighborhoods, than on non-Hispanics (see related work by Borjas 1995). The interactions with Hispanic show that they are more sensitive to the neighborhood male employment and high school non-completion and less sensitive to job access. As with blacks, Hispanics tend to live in neighborhoods with worse social characteristics than non-Hispanic whites, which can help explain differences in work.[26]

V. Reverse Causality

A concern with our estimates is that exogenous changes in employment status may lead to changes in neighborhoods rather than the reverse. This possibility is hard to rule out. Nevertheless, it is possible to assess the importance of reverse causality by examining the timing of changes in employment status relative to changes in neighborhood characteristics. A finding that employment rates increase in the years leading up to moves into better neighborhoods would suggest that exogenous changes in employment status are an important determinant of neighborhood choice and that the preceding estimates may be biased upward. While not dispositive, a finding that employment rates are constant or declining prior to moves into better neighborhoods would undermine the reverse causality argument.

For this analysis, we regress work behavior on leads and lags of changes in neighborhood characteristics. We explore specifications that include fixed effects and individual-specific

[26] The neighborhood male employment rate is for Hispanics is 67% versus 73% for non-Hispanic whites; high school non-completion among adults neighbors is higher for Hispanics than for both whites and blacks (42% versus 38% and 22%), the fraction of neighborhood residents who have completed college is equal to that for blacks (12% for both versus 21% for whites). The fraction of households in the neighborhood with public assistance income is higher for Hispanics than non-Hispanic whites (15% versus 7%) as is the fraction of 25-34 year olds in the neighborhood in poverty (21% versus 9%), but these figures for Hispanics are beneath those

experience profiles in the manner described above. Formally,

$$y_{it} = \alpha_{it} + X_{it}\beta + \sum_{s=-7}^{7}\left[\phi_s Moved_{i(t+s)} + \psi_s \Delta_{i(t+s)}\right] + \varepsilon_{it} \quad \Delta_{i(t+s)} \equiv N_{i(t+s)} - N_{i(t+s-1)}$$

Here $Moved_{it}$ denotes a set of dummy variables for whether the respondent moved between years *t-1* and *t*, which control for the direct effect of moving, and Δ_{it} denotes the change in the respondent's neighborhood characteristics between periods *t* and *t-1* (this variable is, by construction, equal to zero if the respondent did not move).[27] Only variables corresponding to the next and previous move are included.[28]

The first panel of figure 1 plots the effect of moving to a neighborhood with 1 standard deviation higher adult male employment ($\psi_s \sigma_{Neigh.Emp.}$) along with 2-standard error confidence bands[29]. The results show a decline in hours in years 7 through 5 before the move, a rebound in the next two years, and a slight decline until the move. After the move, hours increase for the first 5 years before declining. Thus, hours trend upward after a move into a high employment neighborhood. Taking time in the neighborhood as an indicator of integration into networks, the effect of being in a high employment neighborhood is greatest after people have become more integrated into their neighborhoods.

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for blacks.

[27] The timing of moves is not available from the survey only information on the place of residence at the time of the interview. In the present analysis, we estimate employment over the survey year to bracket moves.

[28] If, for example, a person moves twice the variables corresponding to the neighborhood of residence after the second move are set to zero until after the first move occurs. Once the second move occurs the variables corresponding to the neighborhood of residence prior to the first move are set to zero. This procedure ensures that the effects of changes in characteristics are not estimated from people who are no longer living in a neighborhood. An alternative would be to include characteristics for all neighborhoods in which the respondents lived. We have chosen this procedure because we believe that the first-order effect of a neighborhood comes from people who are currently living in it, although estimates that do not zero out in the case of multiple moves also show little evidence of reverse causality.

[29] These estimates net out the direct effect of moving. More precisely, they give the effect of moving from one

The second panel of figure 1 shows the effect of moving to a neighborhood with one standard deviation better job access. Hours decline in years 7 and 5 before the move and display small movements up and down until the move. Hours increase in the two years after the move and, with some fluctuations remain at their new higher level.

Both models show that hours are flat in the years immediately before a move and that they increase in the years after the move. Although the confidence intervals preclude precisely timing changes in hours around a move, overall our tests of reverse causation are reassuring in that we do not find evidence of an increase in hours in the years preceding a move, but do find increases in hours after moves into better neighborhoods.

VI. Conclusion

Researchers have argued that neighborhoods affect labor force attachment as well as other youth outcomes. Such effects may stem from social interactions or job proximity. Estimates generally suggest that neighborhood effects are present, but have raised concerns with endogenous neighborhood selection. Using confidential, administrative data, respondents to the NLSY79 were linked to measures of neighborhood social characteristics at the Census tract and block group levels from the 1990 Census and measures of job proximity estimated from the 1987 Economic Census. Social influences and job proximity are both important determinants of employment status. A one standard deviation increase in the social characteristics of a neighborhood increases annual hours by 6.1%; a similar increase in job proximity raises hours by 4.7%.

Social interactions have non-linear effects with the greatest proportional impact in the worst

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neighborhood to another with one standard deviation higher adult male employment relative to moving between neighborhoods. Neighborhoods also exert a greater influence on less educated workers and Hispanics. We find relationships between a variety of neighborhood social characteristics and work indicating that being in a disadvantaged neighborhood is important but that the labor activity of neighbors *per se* may not be the crucial factor. The analysis indicates that estimates that do not account for neighborhood selection on the basis of time-invariant unobserved individual characteristics substantially overstate the social effects of neighborhoods but understate the effects of job access. To address the possibility of reverse causality, we study the timing of employment changes around moves and find little evidence that increases in employment lead people to move to better neighborhoods.

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neighborhoods with the same employment.

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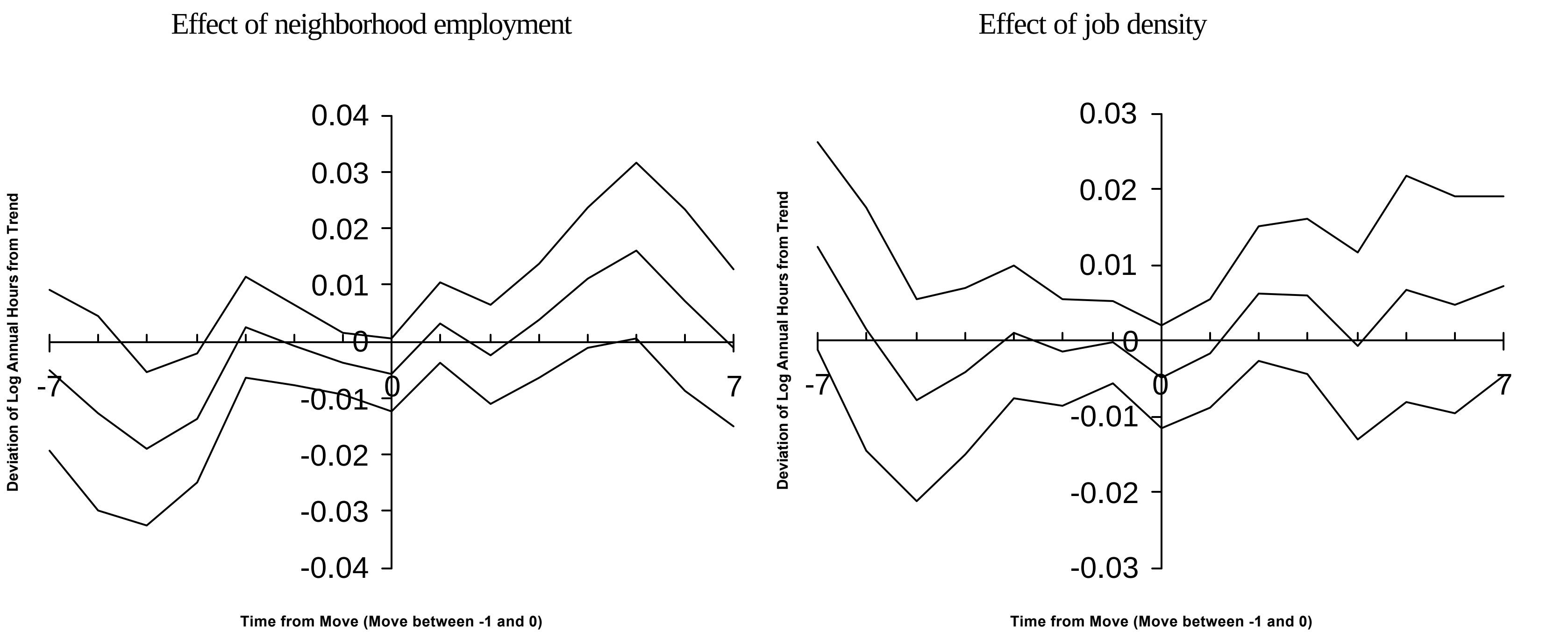
Figure 1. Annual hours around a move to a better neighborhood, fixed effects with individual-specific experience profiles.



Note. Two standard error bounds shown. Figures respectively show the effect on log annual hours of moving to a neighborhood with one standard deviation higher neighborhood employment and one standard deviation greater job density within a five mile radius from one with the mean levels between years –1 and 0. Estimates controls for marital status, the log of one plus the number of own children present, the unemployment rate in the county, years prior to / since move, individual fixed effects, and allow for individual differences in the strength of experience profiles.

TABLE 1
Sample Selection Criterion

Reason for deletion from sample	N	Rural Residence in 1979	Poor White Over – Sample	White	Black	Hispanic	Age in 1979	Highest Grade Completed At time of first leaving school for at least 12 months	Age-Adjusted AFQT Score
Male respondents in NLSY79	6,304	0.219[a]	0.116	0.627	0.241	0.132	18.6	11.8[b]	-5.861[c]
After deletion because less than 8 interviews since leaving school for at least 12 months with participation in the next interview are not available	4,936	0.222[d]	0.101	0.588	0.265	0.146	18.4	11.6[e]	-7.703[f]
After deletion because less than 8 interviews as a civilian	4,575	0.221[g]	0104	0.592	0.259	0.149	18.5	11.6[h]	-8.328[i]
After deletion because less than 8 years in an MSA	3,459	0.126[j]	0.064	0.551	0.287	0.169	18.5	11.7[k]	-7.686[l]
After deletion because less than 8 interviews with valid latitude and longitude	2,658	0.111[m]	0.052	0.539	0.284	0.177	18.6	11.6[n]	-8.072[o]
After deletion because less than 8 interviews with valid data for all variables used in the main analysis except AFQT and mother's education	2,352	0.086[p]	0.051	0.531	0.284	0.185	18.6	11.6	-8.055[q]

a N = 5,522 b N = 6,237 c N = 5,912 d N = 4,757 e N = 4,897 f N = 4,698 g N = 4,459 h N = 4,542 i N = 4,363 j N = 3,364 k N =3,432 l N = 3,305 m N = 2,584 n N = 2,637 o N = 2,538 p N = 2,344 q N = 2,251

Table 2. Summary of Final Sample of Person/Years

	Mean	Std. Dev.
Individual Characteristics (NLSY79)		
Annual Hours	1885.63	915.249
Log Annual Hours	7.6943	2.045
Calendar Year Income $1990	21,457.41	14,848.07
Log Calendar Year Income $1990	9.677	0.931
Highest Grade Completed	11.987	2.284
Experience	9.750	4.907
Black	0.290	0.485
Hispanic	0.193	0.394
Immigrant	0.089	0.285
AFQT (deviation form cross section birth year mean)	-8.785	23.149
AFQT Score Missing	0.040	0.196
Mother's Education	10.710	3.323
Mother's Education Missing	0.076	0.265
Married Spouse Present	0.379	0.485
Log(1+Own Children)	0.335	0.502
Year	88.074	4.606
Number of years in sample	12.357	2.658
Neighborhood Characteristics (1990 Census)		
Employment Rate of Adult Men (at block level)	0.676	0.148
Employment Rate of Adult Men (at tract level)	0.676	0.128
Employment Rate of Adult Women (at block Level)	0.526	0.140
Employment Rate of Adult Women (at tract Level)	0.523	0.117
Fraction of Neighborhood Residents who have not completed High School	0.307	0.189
Fraction of Neighborhood residents who have completed College	0.166	0.146
Fraction of Households with Public Assistance Income	0.118	0.126
Poverty Rate among 25-34 Year Olds in Neighborhood	0.159	0.172
County Unemployment Rate	4.517	3.902
Proximity to Jobs (1987 Economic Census)		
Number of Jobs Weighted by Inverse of Distance	26958.39	44401.91
Log Number of Jobs Weighted by Inverse of Distance Differenced From MSA Distance Weighted Mean Number of Jobs	-	1.033
Population Density (1990 Census)		
Total Population Weighted by Inverse of Distance	178,396.62	205,722.59
Log Population Weighted by Inverse of Distance Differenced From MSA Distance Weighted Mean Population	-	0.697

Note: All variables have 27,218 observations except AFQT, which has 26,129 observations, Mother's education, which has 25,139 observations, and Log Calendar Year Income, which has 16,557 observations.

Table 3. Effects of neighborhood employment and job proximity on log annual hours.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	Between	Within	Within	Deviat.	Deviat.
Employment of adult men in neighborhood	2.166	1.886	2.684	0.658	0.641	0.417	0.410
	(0.206)	(0.199)	(0.276)	(0.139)	(0.138)	(0.154)	(0.154)
	[0.321]	[0.280]	[0.398]	[0.098]	[0.095]	[0.062]	[0.061]
Log number of jobs weighted by inverse of distance	-0.020	0.004	0.013	0.031	0.034	0.043	0.044
	(0.026)	(0.025)	(0.032)	(0.014)	(0.014)	(0.016)	(.016)
	[-0.028]	[0.004]	[0.014]	[0.033]	[0.036]	[0.046]	[0.047]
Education	0.148	0.069	0.090				
	(0.011)	(0.013)	(0.017)				
Experience	0.068	0.032	0.043	0.071	0.055		
	(0.014)	(0.014)	(0.058)	(0.011)	(0.012)		
Experience^2	-0.003	-0.002	-0.001	-0.005	-0.004		
	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)		
Black	-0.555	-0.241	-0.121				
	(0.067)	(0.069)	(0.077)				
Hispanic	-0.048	-0.123	-0.093				
	(0.063)	(0.075)	(0.088)				
AFQT		0.014	0.013				
		(0.002)	(0.002)				
AFQT score missing		-0.230	-0.241				
		(0.119)	(0.131)				
Mother’s education		-0.022	-0.021				
		(0.009)	(0.010)				
Mother’s education missing		-0.521	-0.516				
		(0.143)	(0.142)				
Foreign		0.205	0.105				
		(0.085)	(0.108)				
Married, with spouse present		0.409	0.589		0.148		0.080
		(0.043)	(0.114)		(0.034)		(.033)
Log(1+own children present)		0.215	0.271		0.079		0.058
		(0.047)	(0.110)		(0.038)		(.041)
County unemployment rate	-0.005	-0.004	0.01	-0.03	-0.03	-0.02	-0.02
	(0.008)	(0.008)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)
Individual Fixed Effect	No	No	-	Yes	Yes	Yes	Yes
Dev. from Individual Specific Time Trend	No	No	No	No	No	Yes	Yes
R^2	0.102	0.132	0.267	0.523	0.523	0.617	0.617

Note: Standard errors, which correct for within-.neighborhood correlation in residuals, in parentheses. OLS standard errors also correct for within-person correlation in residuals. Implied effect of a one standard deviation change in brackets. Sample contains 27,313 observations. Independent and dependent variables in deviations from individual-specific time trend regressions use residuals from separate regressions on a quadratic in experience for each respondent.

Table 4. Effects of neighborhood employment and job proximity on annual hours (in levels).			
	(1)	(2)	(3)
	OLS	Within	Deviat.
Annual Hours in levels as dependent variable. (N=27,313)			
Employment of adult men in neighborhood	835.497	243.049	148.058
	(76.244)	(57.356)	(62.402)
	[123.830]	[36.023]	[21.944]
Log number of jobs weighted by inverse of distance	-13.308	8.943	10.095
	(9.652)	(6.780)	(7.439)
	[-14.189]	[9.535]	[10.763]
R^2	0.193	0.557	0.666
Log Earnings as dependent variable (N=16,667)			
Employment of adult men in neighborhood	0.897	0.291	0.164
	(0.099)	(0.074)	(0.080)
	[0.133]	[0.043]	[0.024]
Log number of jobs weighted by inverse of distance	-0.003	0.009	0.015
	(0.010)	(0.009)	(0.009)
	[-0.003]	[0.010]	[0.016]
R^2	0.292	0.608	0.717
Log annual hours as dependent variable including population density as a control (N=24,529)			
Employment of adult men in neighborhood	1.697	0.603	0.378
	(0.196)	(0.140)	(0.156)
	[0.252]	[0.089]	[0.056]
Log number of jobs weighted by inverse of distance	0.109	0.048	0.044
	(0.039)	(0.021)	(0.022)
	[0.116]	[0.051]	[0.047]
Log population weighted by inverse of distance	-0.216	-0.051	-0.004
	(0.052)	(0.039)	(0.043)
	[-0.151]	[-0.036]	[-0.003]
R^2	0.130	0.521	0.616
Log annual hours as dependent variable, with neighborhood employment at tract level (N=27,313)			
Employment of adult men in neighborhood (tract-level)	2.483	0.758	0.369
	(0.231)	(0.176)	(0.204)
	[0.317]	[0.097]	[0.047]
Log number of jobs weighted by inverse of distance	0.016	0.036	0.043
	(0.025)	(0.013)	(0.016)
	[0.017]	[0.038]	[0.046]
R^2	0.134	0.523	0.617
Includes a Quadratic in Experience	Yes	Yes	1^{st} Stage
Includes Education, Race, Hispanic Background, AFQT, Mother's Education, and Immigrant Status	Yes	-	-
Includes Marital Status and Log(1+Own Children)	Yes	Yes	Yes

Note: Standard errors, which correct for within-neighborhood correlation in residuals, in parentheses. OLS standard errors also correct for within-person correlation in residuals. Implied effect of a one standard deviation change in brackets. Independent and dependent variables in deviations from individual-specific time trend regressions use residuals from separate regressions on a quadratic in experience for each respondent.

Table 5. Estimates of Various Neighborhood Characteristics on Annual Hours.			
	(1)	(2)	(3)
	OLS	Within	Deviat.
Employment rate of adult men in neighborhood	1.880	0.612	0.383
	(0.196)	(0.137)	(0.153)
	[0.279]	[.091]	[0.057]
	{0.132}	{0.523}	{0.617}
Employment rate of adult women in neighborhood	1.634	0.346	0.195
	(0.191)	(0.127)	(0.137)
	[0.229]	[0.049]	[0.027]
	{0.128}	{0.523}	{0.617}
Log number of jobs weighted by inverse of distance	-0.034	0.025	0.037
	(.024)	(0.014)	(0.016)
	[-.036]	[0.027]	[0.039]
	{0.118}	{0.523}	{0.617}
Fraction of neighborhood residents that have not completed high school	-1.075	-0.363	-0.176
	(0.179)	(0.116)	(0.130)
	[-0.203]	[-0.069]	[-0.033]
	{0.124}	{0.523}	{0.617}
Fraction of neighborhood residents that have completed four year college	0.808	0.202	0.204
	(0.172)	(0.114)	(0.131)
	[0.118]	[0.029]	[0.030]
	{0.120}	{0.523}	{0.617}
Fraction of household with public assistance income	-2.615	-0.491	-0.214
	(0.259)	(0.196)	(0.221)
	[-0.329]	[-0.062]	[-0.027]
	{0.137}	{0.523}	{0.617}
Poverty rate among 25-34 year olds in neighborhood	-1.311	-0.189	-0.220
	(0.188)	(0.121)	(0.138)
	[-0.226]	[-0.033]	[-0.037]
	{0.127}	{0.523}	{0.617}
Includes a Quadratic in Experience	Yes	Yes	1st Stage
Includes Education, Race, Hispanic Background, AFQT, Mother's Education, and Immigrant Status	Yes	-	-
Includes Marital Status and Log(1+Own Children)	Yes	Yes	Yes

Note. Standard errors in parentheses. Standard errors correct for within-neighborhood correlation in residuals. OLS standard errors also correct for within-person correlation in residuals. Implied effect of a one standard deviation change in brackets. R^2 reported in braces. Estimates are from separate regressions for each independent variable. Sample contains 27,313 observations

Table 6. Non-linearities in Neighborhood Characteristics

				Implied Effect of a One SD change at:		
	Main Effect	Square Term	$\div^2$ (2) / R^2	One SD worse than the mean	At the mean	One SD better than the mean
Employment rate of adult men in neighborhood	2.326 (0.772)	-1.394 (0.574)	9.087 / 0.524	.126	.065	.004
Employment rate of adult women in neighborhood	2.113 (0.650)	-1.713 (0.565)	10.572 / 0.523	.111	.044	-.024
Log number of jobs weighted by inverse of distance	0.027 (0.017)	0.001 (0.007)	2.620 / 0.523	.026	.028	.030
Fraction of neighborhood residents that have not completed high school	0.096 (0.290)	-0.630 (0.417)	.109 / 0.523	-.100	-.055	-.010
Fraction of neighborhood residents that have completed four year college	0.896 (0.288)	-1.262 (0.421)	9.700 / 0.523	.122	.069	.016
Fraction of household with public assistance income	-0.394 (0.375)	-0.193 (0.786)	1.104 / 0.522	-.062	-.055	-.049
Poverty rate among 25-34 year olds in neighborhood	0.383 (0.276)	-0.913 (0.461)	1.924 / 0.522	-.038	.016	-

Note. Standard errors, which correct for within-neighborhood correlation in residuals, in parentheses. A separate regression was run for each neighborhood characteristic. Regressions also include individual fixed effects, education, a quadratic in potential experience, marital status, and log(1+number of own children present). Sample contains 27,313 observations. Reported $\div^2$ statistics are for the joint significance of the neighborhood variable and its square. The critical values for a $\div^2$(2) at the 10%, 5%, and 1% levels are 4.61, 5.99, and 9.21 respectively. Implied effects are the derivatives of log annual hours with respect to each neighborhood variable (at the given point) times the variables standard deviation.

Table 7. Neighborhood Characteristics Interacted with Individual Characteristics

	Model 1			Model 2			
	Neighborhood Characteristic Interacted with Education minus 12			Neighborhood Characteristic Interacted with Black and Hispanic Background			
	Main Effect	Interaction With Education	÷^{2} (2) / R^2	Main Effect	Interaction with Black	Interaction With Hispanic	÷^{2} (3) / R^2
Employment rate of adult men in neighborhood	0.577	-0.211	19.74 /	0.280	0.385	0.905	10.39 /
	(0.130)	(0.061)	0.523	(0.157)	(0.290)	(0.376)	0.523
	[0.086]	[-.031]		[0.041]	[0.057]	[0.134]	
Employment rate of adult women in neighborhood	0.349	-0.100	7.458 /	0.240	0.073	0.415	4.331 /
	(0.129)	(0.057)	0.523	(0.139)	(0.300)	(0.315)	0.523
	[0.049]	[-0.014]		[0.034]	[0.078]	[0.058]	
Log number of jobs weighted by inverse of distance	0.025	-0.0004	3.087 /	0.040	0.029	-0.127	10.60 /
	(0.014)	(0.006)	0.523	(0.014)	(0.046)	(0.038)	0.523
	[0.027]	[-0.004]		[0.043]	[0.031]	[-0.135]	
Fraction of neighborhood residents that have not completed high school	-0.333	0.074	8.992 /	-0.033	-0.402	-0.585	3.837 /
	(0.111)	(0.045)	0.523	(0.156)	(0.270)	(0.251)	0.523
	[-0.063]	[0.014]		[-0.006]	[-0.076]	[-0.110]	
Fraction of neighborhood residents that have completed four year college	0.234	-0.060	3.069 /	0.125	0.294	-0.006	3.491 /
	(0.134)	(0.056)	0.523	(0.118)	(0.295)	(0.379)	0.523
	[0.034]	[-0.009]		[0.018]	[0.043]	[0.001]	
Fraction of household with public assistance income	-0.467	0.031	6.546 /	-0.248	-0.181	-0.623	2.854 /
	(0.183)	(0.095)	0.523	(0.305)	(0.417)	(0.515)	0.523
	[-0.059]	[0.004]		[-0.031]	[-0.023]	[-0.078]	
Poverty rate among 25-34 year olds in neighborhood	-0.162	0.048	2.061 /	-0.123	0.052	-0.392	0.521 /
	(0.113)	(0.054)	0.523	(0.200)	(0.270)	(0.310)	0.523
	[-0.028]	[0.008]		[-0.021]	[0.009]	[-0.068]	

Note. Standard errors, which correct for within-neighborhood correlation in residuals, in parentheses. Implied effect of a one standard deviation change in brackets. Estimates for interactions with education and with black and Hispanic background are from separate regressions. Separate regression were run for each neighborhood characteristic. Regressions also include individual fixed effects, education, a quadratic in potential experience, marital status, and log(1+number of own children present). Sample contains 27,313 observations. Reported ÷^{2} statistics are for the joint significance of the neighborhood variable and the interaction(s). The critical values for a ÷^{2}(2) at the 10%, 5%, and 1% levels are 4.61, 5.99, and 9.21 respectively. The critical values for a ÷^{2}(3) at the 10%, 5%, and 1% levels are 6.25, 7.81, and 11.34 respectively.