Who Is Affected by Neighbourhood Income Mix?

Gender, Age, Family, Employment and Income Differences

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ABSTRACT

Currently throughout Western Europe and North America there are a variety of public policy initiatives to achieve neighborhood income diversity, despite widespread scholarly controversy about the nature and importance of neighborhood effects. This paper provides new empirical evidence on the degree to which the mixture of low-, middle-, and high-income males in the neighborhood affects the subsequent earnings of individuals, and to test explicitly the degree to which these impacts vary across gender, age, presence of children, employment status, or income at the start of the analysis period. We employ an inter-temporal differences specification of econometric model to eliminate the potential selection bias arising from unmeasured individual characteristics, and investigate data on 1.67 million adults living in Swedish metropolitan areas 1991-1999. We find that there are important differences in the nature and magnitude of neighborhood income mix effects in several dimensions, but many are statistically and economically significant.

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I. Introduction

Belief in the desirability of mixing residents within neighborhoods on the basis of socioeconomic status undergirds a rich palette of official pronouncements and planning initiatives on both sides of the Atlantic. Recent reviews of policy documents indicate that the growing segregation of different socioeconomic groups is a central concern of governments across the European Union, both original and new members alike (Andersen, 2002, 2003, 2006; Musterd, 2003; Musterd, Ostendorf and de Vos, 2003; Kleinhans, 2004; Norris and Shiels, 2004; Andersson and Musterd, 2005; Berube, 2005; Meen et al. 2005, Pennix, 2006; Tunstall and Fenton, 2006; VROM, 2006). The issues of race and class segregation are typically treated with more official circumspection in the U.S., though President Barack Obama's campaign platform included an explicit set of proposals to deal with spatially concentrated urban poverty (Obama, 2008).

A wide range of programmatic mechanisms have been employed to combat socio-spatial segregation and prevent the formation of new clusters of deprived households. These programs fall under the rubrics of "social mix" in Europe and "mixed income communities" or "poverty deconcentration" in the U.S. Programmatic examples include: urban regeneration measures that replace concentrations of social housing with more diverse housing stocks (UK, NL, US); social housing management and tenant allocation reform (FR, IR, NL); tenant-based housing allowances (FR, US); and land-use planning rules requiring mixed developments (UK, some US locales); see: Murie and Musterd (2004), Berube (2005), Briggs (2005), Musterd and Andersson (2005), Norris (2006).

Despite its current exalted place in the pantheon of policy nostrums, the rationale for pursuing socio-economically mixed neighborhoods has been questioned on conceptual and empirical grounds by a wide range of European and American scholars; see: Atkinson and Kintrea (2000, 2001), Ostendorf, Musterd and de Vos (2001), Kearns (2002), Musterd (2002, 2003), Musterd, Ostendorf and de Vos (2003), Meen et al. (2005), Galster (2005, 2007), Delorenzi (2006), Joseph (2006), Joseph, Chaskin, and Webber (2006), Cheshire (2007), and the set of responses to McCulloch (2001) in the same issue of *Environment and Planning A*., pp. 1335-1369.

Unfortunately, scholarship on the effects of neighborhood socioeconomic mix on individuals' labor market outcomes has been remarkably myopic in probing potential compositional variations in impacts. Some multivariate statistical works have analyzed only a narrow range of individuals, such as those from a particular income or ethnic group; see Oberwittler (2008), Andersson, Musterd, Galster and Kauppinen (2008), Cutler, Glaeser and Vigdor (2008), and Pinkster (2008). Others have analyzed a wider range of individuals, but not explored potential variations in the magnitudes of neighborhood impacts across different subgroups, typically because of inadequate sub-sample sizes; see O'Regan and Quigley (1996), Vartanian (1999a, b), Ostendorf, Musterd, and de Vos (2001), Buck (2001), McCulloch (2001), Van der Klaauw and van Ours (2003), Bolster et al. (2004), Weinberg, Reagan and Yankow (2004), Dawkins, Shen and Sanchez (2005), Musterd and Andersson (2006), Gordon and Monastiriotis (2006), and Galster, Andersson, Musterd and Kauppinen (2008). The one exception has been Musterd, Ostendorf and de Vos (2003), who found that the neighborhood percentage of residents on social benefits had a different correlation with an individual's subsequent probability of staying on benefits depending on the individual's labor market position. No prior studies, however, have conducted a comprehensive statistical investigation into the degree to which the size of neighborhood effect may vary across a multiplicity of individuals, while employing an econometric technique that plausibly reveals unbiased causal relationships. Thus, despite its crucial importance for many current public policy debates, scholars are still in the unfortunate position of being unsure about whether neighborhood income mix independently and substantially affects labor market outcomes for residents and, if so, for what type(s) of residents.

This paper aims to contribute to these scholarly and public policy deliberations by providing new empirical evidence from Sweden quantifying the degree to which the mixture of low-, middle-, and high-income males in the neighborhood affects the subsequent labor earnings of individuals, and to test explicitly the degree to which these effects vary across individuals according to their gender, age, presence of children, employment status, and income at the start of the analysis period. We employ an inter-temporal, first-differences specification of econometric model to eliminate the potential bias arising from unmeasured individual characteristics leading to neighborhood selection. We find that there are important differences in the effect of neighborhood income mix in many dimensions, with some apparent impacts being substantial indeed.

II. Why Might Neighbourhood Economic Mix Affect Different Individuals Differently? Theoretical Considerations

Neighborhood effects may transpire through a variety of causal mechanisms that can occur either through social interactions within the neighborhood and/or by actions of others located outside of the neighborhood; for extended discussion, see especially Jencks and Mayer (1990), Duncan, Connell and Klebanov (1997), Gephart (1997), Friedrichs, (1998), Dietz (2002), Sampson, Morenoff, and Gannon-Rowley (2002), and Ioannides and Loury (2004). The potential intra-neighborhood mechanisms include socialization (norms, collective control, peers, role models), networks, and relative deprivation. All these mechanisms may come into play with variations in the economic mix of neighborhoods, as both the nature and content of the social interactions will change. The potential extra-neighborhood mechanisms are stigmatization, institutional resources, and accessibility. All these mechanisms may also come into relevance with changes in the economic mix of neighborhoods to the extent that powerful external actors (employers, bureaucrats, public and private sector leaders) change their opinions about places, the resources they invest in these places, and the location of employment relative to these places as a consequence. While current scholarship is not decisive, it suggests that several intra- and extra-neighborhood mechanisms above may be relevant; see especially Van Kempen (1997); Dietz (2002); Sampson, Morenoff and Gannon-Rowley (2002); Ellen and Turner (2003); and Galster (2005).[1] Our purpose here is to speculate for each mechanism why one might expect variations in its power to influence residents' labor earnings depending on their individual characteristics.

A. Socialization

There have been several studies examining neighborhood social relationships that suggest that the influence of peers and role models may be strongest for younger males; see Sullivan (1989), Anderson (1990, 1991); Case and Katz (1991),Diehr et al (1993), South and Baumer (2000) and Ginther, Haveman and Wolfe (2000).[2] These peers and role models may affect these younger males' incomes by shaping attitudes

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[1] I recognize that practitioners who deal directly with deprived neighborhoods hold divergent and conflicting opinions about which neighborhood effect mechanisms are most important (Atkinson and Kintrea, 2004).

[2] Also see the reviews in Leventhal and Brooks-Gunn (2000) and Friedrichs, Galster and Musterd (2003).

and behaviors towards education, labor force participation, and criminal activities. Local social control in extreme circumstances may limit residents' ability and willingness to look for employment opportunities outside of the neighborhood (Pinkster, 2008). This may be especially true for areas where more traditional, patriarchal norms affect the ability of women to work, especially if they have children. We would also expect that adults (especially women) with children and those who worked fewer hours would spend more time in the neighborhood, all else equal, and thus be more subject to the aforementioned potential forces of socialization operating there. It is unclear, therefore, whether men or women would be expected to be influenced more by neighborhood socialization forces.

B. Networks

Limited social ties with employed and better-educated people is an often-observed characteristic of non-employed and lower-income people (Tiggs, Brown, and Green, 1998; Fernandez and Harris, 1992; Pinkster, 2008). Several studies from both the U.S. and Europe support the idea that these limited "bridging" social networks reduce economic opportunities; see Bertrand, Luttmer and Mullainathan (2000); Buck (2001); Farwick (2004); and Pinkster (2008). In addition, younger and lower-income individuals may be more apt to have more geographically localized networks (Fischer, 1982), inasmuch as they have had less ease of geographic mobility and less cumulative time to develop a wide range of employment-related and institutionally related contacts. Adults with children are also likely to develop a denser network of relationships that is more focused on the neighborhood.[3]

C. Competition

We would expect that parents would be more acutely attuned to intra-neighborhood competitive pressures than childless individuals, and would be prone to strive for greater hours worked and better-paying employment in an effort to buy their children better consumer goods. As with all intra-neighborhood mechanisms, we would also expect those who worked fewer hours to have greater exposure to whatever mechanism(s) were operative. Our expectations regarding gender and income are less clear. We would expect males in general to be more vulnerable to competitive pressures, given traditional gender differences in socialization. Yet, McCulloch (2001)

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[3] See the reviews in Kleinhans (2004) and Kleit (2008).

found that disadvantaged British women (though not men) were more likely to experience a variety of more negative outcomes if they lived in affluent areas, indicative of a stronger inter-group competition mechanism for them. We also think it likely that higher-income individuals would be more sensitive to status competition. However, two other British studies that found that health issues for lower-income individuals were more problematic when they lived in more affluent areas (Duncan and Jones, 1995; Shouls et al., 1996).

D. Stigmatization

Case study evidence suggests that place-based stigmatization is an oft-occurring process in Western Europe; see especially Wacquant (1993), Power (1997), Taylor (1998), Atkinson and Kintrea (1998), Forrest and Kearns (1999), Dean and Hastings (2000), Hastings and Dean (2003), Martin and Watkinson (2003) and Hastings (2004). Permentier, Bolt and van Ham (2007) found that Utrecht neighborhood reputations were significantly correlated with their socio-economic characteristics, while their physical and functional features were of less importance. Thus, it is certainly plausible that this mechanism might work to limit income-earning possibilities for residents of such areas. However, there is little to indicate which residents would be most vulnerable. We speculate that those who are older, who work more, and who earn more would be less vulnerable to stigmatization by prospective employers, inasmuch as they will have been more likely to develop stronger resumes and thus be less likely to be stereotyped according to their place of residence.

E. Institutional Resources

In the U.S. there is considerable evidence of the varied quality of public institutional resources (such a schools) across neighborhoods, with a strong positive correlation between institutional capacity and area socioeconomic status (Kozol, 1991). However, in our study site the correlation is likely the opposite due to conscious efforts of the Swedish welfare state to provide compensatory services (temporary employment bureaus, adult education and retraining centers, health care facilities) in more disadvantaged neighborhoods. We are unsure of the effect such a spatial bias in institutions has upon the incomes of residents of the neighborhoods where they are concentrated (e.g., these institutions may unintentionally create a dependency on episodic, low-wage jobs). Regardless, those who are younger, work less, and have lower incomes should disproportionately be affected.

F. Accessibility

Numerous U.S. studies have investigated the issue of differential accessibility to work: the "spatial mismatch" hypothesis; for reviews, see Kain (1992) and Ihlanfeldt (1999). Though the Swedish public transportation is considerably more developed than in most U.S. cities where the spatial mismatch hypothesis arose, we note that a concern over accessibility has been sufficient to generate some study in Sweden (ROGER to insert SWE reference). Should accessibility to good-paying jobs be a non-trivial neighborhood effect mechanism in Sweden, we would expect that those who face the greatest transportation challenges—the young and lower-income (due to lower auto ownership rates) and those with children (who require transportation to day care)—would feel the greatest impact.

G. The Magnitude of Neighborhood Income Mix Effects on Different Individuals: Summary Speculations

What does the foregoing discussion suggest about the effects of neighborhood income mix on different types of residents? Our provisional speculations are summarized in Table 1. Although there certainly remain ambiguous expectations, there are some consistencies regardless of which neighborhood effect mechanism is dominant. The strongest effects likely should be evinced for younger, lower-income residents who work fewer hours and who have children. No clear expectations regarding gender differences emerge.

[Table 1 about here]

III. Selection / Omitted Variables Bias as a Challenge for Measuring the Magnitude Of Neighborhood Effects

Regardless of the potential casual mechanism(s) at work, there has been a sizable literature on both sides of the Atlantic devoted to measuring the independent magnitude of the effect of a neighborhood's household composition on economic outcomes, employing multivariate statistical analyses on both cross-sectional and longitudinal databases of individuals. Most of this work has focused on the experience of neighborhood context as a child or adolescent providing a lagged labor market consequence; see: Payne (1987); Moffitt (1992); Corcoran et al. (1992); Haveman and Wolfe (1994); Gottschalk, McLanahan, and Sandefur (1994); Gottschalk (1996); Mayer (1997); Vartanian (1999a, b); Pepper (2000); Ginther, Haveman and Wolfe (2000); Holloway and Mulherin (2004). Others, as in the current paper, have examined the relationship between neighborhood population characteristics and contemporaneous economic outcomes for adults; see O'Regan and Quigley (1996); Buck (2001); Weinberg, Reagan and Yankow (2004); Musterd and Andersson (2005, 2006); Andersson et al. (2005); Dawkins, Shen and Sanchez (2005), Galster, Andersson, Musterd and Kauppinen (2008). These studies typically have observed nontrivial partial correlations between various measures of the economic composition of neighborhood residents and several measures of lagged or contemporaneous adult labor market performance, though there have been some exceptions; see: Haveman and Wolfe (1994); McCulloch (2001); Musterd, Ostendorf and de Vos (2003); and Drever (2004).

The accuracy of the neighborhood-outcome relationships measured by many of these studies is subject to challenge, however, due to potential geographic selection/omitted variable bias (Ginther, Haveman and Wolfe 2000)[4]. The basic issue is that adults have certain (unmeasured) motivations and skills related to their own economic prospects and move to certain types of neighborhoods as a consequence of these attributes. Any observed relationship between their neighborhood conditions and economic outcomes may therefore be biased because of this systematic spatial selection process, *even if all their observable characteristics are controlled* (Manski 1995, 2000; Duncan et al. 1997; Duncan and Raudenbush 1999, Dietz 2002). Flipped

[4] There are other daunting methodological challenges as well, of course; see Galster (2008).

on its head, the problem can be formulated as omitted variables bias. Is the observed statistical relationship between individual outcomes and neighborhood composition indicative of the neighborhood's independent effect, or merely unmeasured (unobserved, uncontrolled) characteristics of adults that truly affected outcomes but also (spuriously, in the extreme) led to neighborhood choices as well?[5]

There have been several types of methodological responses to this challenge:

- *Random Assignment Experiments:* Data are produced by an experimental design whereby households are randomly assigned to different neighborhoods, such as the Moving To Opportunity (MTO) demonstration (Goering and Feins, 2003; Orr et al., 2003; Kling, Liebman and Katz 2007; Ludwig et al. 2008).
- *Natural Quasi-Experiments:* Data are produced from idiosyncratic public policy initiatives (typically involving subsidized housing) that create exogenous variation in neighborhood environments for tenants (Rosenbaum, Reynolds and DeLuca, 2002; Briggs, 1997, 1998; Oreopolis, 2003; Edin, Fredricksson and Aslund, 2003 and Aslund and Fredricksson, 2005).
- *Fixed Effect Models Based on Longitudinal Data:* Unobserved, time-invariant characteristics of individuals that may lead to both neighborhood selection and labor force outcomes are measured by individual dummy variables (Weinberg, Reagan and Yankow 2004).
- *Instrumental Variables for Neighborhood Characteristics:* Proxy variables for neighborhood characteristics are devised that only vary according to attributes exogenous to the individual (Foster and McLanahan, 1996; Galster, Marcotte et

[5] The direction of the bias has been the subject of debate, with Jencks and Mayer (1990) and Tienda (1991) arguing that neighborhood impacts are biased upwards, and Brooks-Gunn, Duncan, and Aber (1997) arguing the opposite.

al. 2007; Kling, Liebman and Katz 2007; Ludwig et al. 2008; Cutler, Glaeser and Vigdor 2008).

- *Confining Analysis to Non-Movers:* Because changes observed in non-movers' neighborhoods will be uncorrelated with their unobserved individual characteristics (both time-varying and time-invariant) the estimated relationship should be unbiased (Galster, Andersson, Musterd and Kauppinen 2008).
- *Difference models based on longitudinal data:* Unobserved, time-invariant characteristics are eliminated by measuring differences between two periods (Bolster et al. 2004; Galster, Andersson, Musterd and Kauppinen 2008).

We employ the difference model approach here (as described in the next section) because it directly eliminates time-invariant characteristics of adults that typically are unobservable and uncontrolled, and thus potentially bias the findings.

IV. Data and Empirical Model

A. The Swedish Data Files

The variables we employ are constructed from data contained in the Statistics Sweden *Louise* files, which are produced annually. These files contain a large amount of information on all individuals age 15 and above and represent compilations of data assembled from a range of statistical registers (income, education, labor market, and population). We have merged selected information about individuals from annual *Louise* files to create a longitudinal database 1991-1999 for all individuals present in Sweden in 1991. Here we analyze only those residing in one of Sweden's three large metropolitan areas: Stockholm, Gothenburg, and Malmo, so we can ensure a meaningfully consistent concept of neighborhood (explained below). We confine our sample to prime working-age individuals (ages 24-60 in 1995) who were residents of Sweden in each year from 1991 to 1999,[6] producing an analysis sample size of 1,667,641. Characteristics of our sample are provided in Table 2.

[Table 2 about here]

We emphasize that our dataset includes observations of virtually the *entire metropolitan population* within the desired adult age and residency range, not a sample. Thus, the t-statistics we present below should not be interpreted as guides for prospective errors involving inferences from a sample to the larger population. Rather, they provide a means of assessing the reliability of estimated coefficients as parameters for the underlying labor earnings function for individuals in Swedish metropolitan areas, given potential functional misspecifications and measurement errors in variables.

B. Our Model for Explaining Individual Incomes

Our outcome of interest is the average annual income from work (measured in Swedish *kronor,* SEK; $1=8 SEK) during a four year period.[7] Since this indicator encapsulates educational credentials, labor force participation, employment regularity, and hourly compensation, we believe it to be the most comprehensive single measure of an individual's economic worth. Average labor incomes for both genders grew substantially between our 1991-1999 analysis period in metropolitan Sweden: 24.7% for males and 22.3% for females.

[6] Our analysis intentionally excludes recent (after 1990) immigrants to Sweden because we believe their labor market experience neither to be indicative of their longer-term economic value nor to be reflective of their initial neighborhood environments when they enter Sweden. We are conducting a companion analysis that focuses on neighborhood effects for immigrants.

[7] Formally, income from work is computed here as the sum of: cash salary payments, income from active businesses, and tax-based benefits that employees accrue as terms of their employment (sick or parental leave, work-related injury or illness compensation, daily payments for temporary military service, or giving assistance to a handicapped relative).

We model the average annual income from work during period t+1 to t+4 ($I_{t1\text{-}4}$) for individual i residing in neighborhood j in local labor market area k as:[8]

$$\ln(I_{ijkt1\text{-}4}) = \alpha + \beta[P_{it}] + \gamma[P_i] + \partial[UP_i] + \theta[N_{ijt}] + \mu[L_{ikt}] + \varepsilon \qquad [1]$$

where:

$[P_t]$ = observed personal characteristics that can vary over time (e.g., marital or fertility status, educational attainment)

[P] = observed personal characteristics that do not vary over time (e.g., year and country of birth)

[UP] = unobserved personal characteristics that do not vary over time (e.g., IQ, prior experiences)

$[N_{jt}]$ = observed characteristics of neighborhood j where individual resides at time t

$[L_{kt}]$ = observed characteristics of local labor market area in which individual resides at time t (e.g., mean annual income from labor)

ε = a random error term with assumed standard statistical properties

We model ($I_{t1\text{-}4}$) as a function of residence during t, realizing that people can and do move during the following period. This lagged specification is intentional so we can keep causation clear, since we know that changes in income can lead to changes in residence. Our point is to test whether initial residential context has any relationship to subsequent income flows, regardless of whether those flows lead to residential mobility or not. Details of variable specifications follow.

In this study we operationalize "neighborhood" as the area delineated by a "SAMS" defined by Statistics Sweden. The SAMS classification scheme is designed to identify relatively homogeneous areas by taking into account housing type, tenure and construction period; as such they are roughly comparable to a U.S. census tract. We

[8] The log-linear transformation not only is appropriate given the positive skew of the income distribution, but also has sound grounding in economic theory, implicitly suggesting that income is a multiplicative (not additive) function of personal, neighborhood, and labor market characteristics.

confine our analyses to the three Swedish metropolitan areas so that geographic scale of neighborhood can be made more comparable across individuals being analyzed.[9] We emphasize that using such a relatively large geographic scale of neighborhood likely has the effect of reducing the measured neighborhood effects, inasmuch as other studies using European data have consistently found stronger effects at smaller spatial scales (Buck, 2001; Bolster et al., 2004; Andersson and Musterd, "What Scale Matters?" forthcoming ROGER to provide citation here & references). We have also eliminated from our study 21,000 individuals residing in 36 SAMS areas because they represented heavy concentrations of (low-income) students and as such were unrepresentative of Swedish low-income neighborhoods.[10]

We focus on the income mix of neighborhood as the [N] variable of importance for three reasons. First, this is the aspect of neighborhood that has been the focus of the scholarly literature beginning with the "concentrated poverty" thesis of Wilson (1987). Second, this dimension has been the focal point of several public policy initiatives in both the U.S. and Western Europe, as explained in the introduction. Third, an earlier study using similar Swedish data found that initial neighborhood income mix was more strongly correlated with subsequent levels of individual incomes than neighborhood mix defined by education, ethnicity, family status, or housing tenure (Andersson et al. 2008). As our measure of neighborhood income mix we specify the proportion of working age (20-64 years) males in the lowest 30% of the nationwide male income distribution and that proportion in the highest 30% of the distribution; the middle 40% becomes the excluded

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[9] There remains some unavoidable inter-urban variation in SAMS scale nevertheless. At the extremes, the smallest SAMS in Gothenburg have a population of about 500 but in Stockholm the largest contain over ten times as many people.

[10] The algorithm we used to identify and eliminate such areas was that the SAMS had a proportion of low income males greater than two standard deviations above the mean and a percentage of students greater than two standard deviations above the mean of the aforementioned subsample. These areas were located in the cities of Lund, Uppsala, and Gothenburg.

reference category. For brevity we will subsequently refer to these groups as "lower-income," "middle-income," and "higher-income" neighbors. We adopt this convention because of the longstanding precedent in the literature investigating the external consequences of both the most- and least-advantaged segments of the neighborhood's population (Jencks and Mayer, 1990; Brooks-Gunn, Duncan and Aber, 1997). In our database we only observe these neighborhood conditions at two points in time.

We operationalize the observed personal characteristics of individuals [P_t] and [P] with a set of variables describing their demographic and household characteristics, educational attainments, nativity and immigrant status, and features of their employment during the period that will affect their income but are likely not related to neighborhood context (such as parental leave, illness, or attending school). We operationalize [L_t] with the mean labor income for the local labor market (an area somewhat smaller than a metropolitan area) in which the individual resided during the period in question; see Table 2.

C. Strategy for Estimating the Causal Effect of Neighborhood Income Mix

As noted above, the principal challenge in quantitative neighborhood effects research is avoiding biased estimates of θ arising from failure to control for [UP] in [1]. We address this challenge by exploiting the fact that we have neighborhood conditions, personal characteristics, labor market characteristics, and multi-year average personal incomes measured at two points in time. By differencing [1] between these two points (let this time difference be denoted Δ^f) we obtain:

$$\Delta^f \ln(I_{ijk}) = \Delta^f\alpha + \beta\Delta^f P_{it} + \varphi\Delta^f UP_{it} + \theta\Delta^f N_{ijt} + \mu\Delta^f L_{ikt} + \Delta^f\varepsilon \qquad [2]$$

Note that differencing removes the troubling unobserved, time- invariant individual characteristics [UP].[11]

D. Strategy for Estimating the Different Magnitudes of Neighborhood Income Mix Effect on Different Types of Individuals

Our strategy for investigating the degree to which the impacts of neighborhood income mix varies across individuals according to their gender, age, presence of children, employment status, or income at the start of the analysis period involves two prongs. The first involves creating interaction terms with our two (difference in) neighborhood income mix variables (proportion low- and proportion high-income) with the various aspects of individuals we have discussed above: gender (males), age, presence of children, full-time employment, and income. Statistical significance of these interaction terms' coefficients in equation [2] provides evidence of a difference in the magnitude of neighborhood effect across these dimensions of difference in individuals. Anticipating such, our second strategic prong will be to probe more deeply into these differences by stratifying the sample along the aforementioned dimensions, estimate equation [2] for each, and compare coefficients of the neighborhood income mix variables. For this exercise we will compare 48 groups, with stratifications by: gender (2 groups); age in 1995 (3 groups: 24-30, 31-46, 47-60 years of age)[12]; presence of children (2 groups); full-time employment, which we define as more that 152 days per

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[11] In preliminary analyses where we estimated equation [1] for either beginning or ending year (instead of differencing), we find a substantially larger coefficient for θ, suggesting that the differencing approach is an important vehicle for reducing biased estimates. (Details are available from the first author.) We acknowledge that this differencing approach does not eliminate the potential bias that may arise if there are time-varying unobserved personal characteristics [UP_{it}] that significantly shape both neighborhood selection and income. We think that this is unlikely, given the short period over which we calculate the differences.

[12] We selected these age breaks for several reasons. First, ages 30 and 46 are the years where the majority of our sample does not have any children (between these ages the majority does). Second, age 30 is the year when we observe the greatest rate of cohorts having moved in the prior year. Third, age 46 is approximately the year of peak median annual earnings.

year for males or 144 days per year for females (2 groups); and income, which we define by those in lowest or highest 30 percent of the gender-specific metropolitan Swedish income distribution (2 groups).

V. Findings

A. The Difference Model with Interaction Effects for Different Types of Individuals

Table 3 shows the parameter estimates from our difference specification [2] using the interaction terms for the neighborhood income mix variables described above. The control variables of time-varying personal characteristics perform as expected. Income gains are greater for those who enhance their educational credentials, have fewer years currently studying, change their civil union status, and take advantage of the generous Swedish benefits for sick leave or parental leave. Those who are phasing into retirement or who have an increase in the number of children under age 7 see smaller income gains. Local labor markets with greater average income growth subsequently convey analogous gains to individual residents, presumably by is association with expanding local employment opportunities.

[Table 3 about here]

The first major observation regarding the neighborhood income mix variables is that all the main effect and interaction terms are highly statistically significant, with the lone exception of the interaction between gender and change in the proportion of high-income neighbors. Increases in the proportion of low-income neighbors have a more negative impact (compared to such an increase in middle-income neighbors) on the incomes of individuals who: do not work full-time, are males, have children, are younger, and earn higher-incomes. Increases in the proportion of high-income neighbors (compared to such an increase in middle-income neighbors) have a more positive impact on the incomes of individuals who: are not fulltime employees, are males, have no children, are older, and earn higher-incomes.

To aid in the interpretation of the net direction and size of these joint main and interaction effects, we show in Table 4 the estimated relationships between the percentage change in income growth and ten percentage-point changes in neighborhood income mix variables for the different groups of individuals being considered; these changes represent slightly more than one standard deviation of observed changes in these variables. We present estimated changes for hypothetical people aged 27 and 54 who earned at either the 30th or 70th percentiles for their gender-specific income distribution in 1995.[13]

[Table 4 about here]

Examination of Table 4 leads to two main conclusions regarding overall patterns of log-linear interactive relationships. First, for all combinations of parameters used for simulation here, increases in the share of low-income neighbors and corresponding decreases in the share of middle-income neighbors retard subsequent earnings growth of individuals. For all cases this retardation is economically substantial, often with double-digit percentage changes associated with ten percentage-point changes in neighborhood income mix. The negative impact is especially large for males and those with higher incomes, with children, or not working fulltime. Second, the net impact of increases in the share of high-income neighbors and corresponding decreases in the share of middle-income neighbors can be positive or negative, depending on the group. Younger males and females who work fulltime evince nontrivial net negative impacts from such changes in mix, especially if they have children. On the other hand, older

[13] Low income is defined as 1109.25 (male); 876.5 (female) 100 SEK and high income as 2144.25 (male); 1584.5 (female) 100 SEK.

males and females who have no children and do not work fulltime evince substantial gains under these circumstances.

Of course, the above conclusions are based on a model that assumes a log-linear relationship for the interaction terms. To test this assumption, we estimate the model for each of 48 strata, as reported next. We find several important caveats to the above generalizations.

B. The Difference Model with Stratifications for Different Types of Individuals

Table 5 shows the coefficient parameter estimates of the neighborhood income mix variables for 24 strata of females described above that were produced by our difference specification [2]; Table 6 does the same for 24 strata of males. Overall, the comparison of strata again indicates large differences in effect magnitudes (and directions) of neighborhood income mix, though in more subtle ways than revealed by the interaction model above. For ease of summarizing results, we adopt the shorthand L, M, H to represent proportions of low-, middle-, and high-income males in neighborhood, respectively. We use inequality signs to indicate a relative difference in individual income gains associated with alterations in these neighborhood mixes, based on our econometric results; e.g.: M>L means M neighbors confer more subsequent income gains than and equivalent share of L ones, whereas M (≈L) means they are not significantly different.

[Tables 5, 6 about here]

Consider first females in Table 5, and their patterns of which type of neighbors convey the greatest benefits to their individual incomes. We see that, contrary to the implications of the gender interaction specification above, no female groups evince any statistically significant effect from neighborhood income mix when they are ages 24-30 (regardless of any other characteristic). However, for lower-income females in both 31-

46 and 47-60 age groups, the strong pattern is M>H>L, regardless of family or employment status. These inequalities are stronger for non-fulltime women and for older women (i.e., M>>H>>L), regardless of family status. Interestingly, these inequalities are stronger for women with children (than for those with no children) in the oldest group, but just the opposite in the middle age group, regardless of employment status within the lower-income group.

For high-income females the patterns are quite different; for them there are few neighborhood effects for any age, employment status, or parenthood category. The noteworthy exception is the middle aged, fulltime workers with children, who are the only stratum to evince a pattern of H>M (≈L).

For a summary sense of the magnitude of effects whose ordinal patterns were just described, we present Figure 1, which plots the statistically significant coefficients of both proportion low-income and proportion high-income male neighbor variables for each stratum in Table 5. The type of font in Figure 1 denotes which (or both) of the coefficients are statistically significant, and the key shows the relevant stratum associated with each pair of coefficients. Dark-shaded diamonds indicate low-income females; grey-shaded squares indicate high-income females. A dashed 45-degree reference line is superimposed to aid interpretation.

[Figure 1 about here]

Figure 1 makes several conclusions regarding females immediately clear. The impact of low-income neighbors is typically greater than for an equivalent share of high-income neighbors, inasmuch as seven of the nine coefficient pairs lie above the reference line. The magnitude of impacts is greater for low-income women, especially those over age 46.

For males, neighborhood effects are more prevalent but less consistent in nature compared to those for females; see Table 6. For low-income males, the pattern is consistently M>L, thought he relative magnitude of effects from H and M vary according to age. For low-income males under age 31, M>L>H, but for those over age 30 it is M>H>L. In all ages, low-income males who work less than full time seem more strongly affected by neighborhood income mix.

For high-income males there are strong neighborhood effects but they differ in their nature. The pattern M>L is observed only for those under age 47 who do not have children. Typically M≈H, with only one exception. Finally, for high-income, fulltime males over age 46 we have the most unexpected results, H≈L>M.[14]

Figure 2 provides a summary sense of the magnitude of effects whose ordinal patterns were just described; it follows an identical format as Figure 2 except that it applies to results for males presented in Table 6.

[Figure 2 about here]

Figure 2 shows that, as in the case of females, effects from low-income neighbors are generally stronger than from high-income ones, though this pattern is far less dominant for males. It is also difficult to generalize from this Figure about patterns related to individual male income, age, or parental status. What does emerge, however, is that males who are not employed fulltime appear much more strongly affected by neighborhood income mix; indeed the five largest coefficient pairs in absolute value are associated with such males. The outlier here is clearly young, childless, not fulltime yet high-income males (see key 10 in Figure 2); this is an exceptionally unusual group (only 88 observations) so this result should not be over-emphasized.

[14] We do not have a plausible explanation for this result, though we have identified a particular group for which it appertains: males who have not moved from neighborhoods experiencing small changes in the proportions of low-income residents (i.e., they are not "gentrifiers."). Perhaps in these stable areas the low-income group disproportionately constitutes longtime-resident homeowners with whom other older males have built valued social networks.

C. Discussion

How do the foregoing results comport with the expected magnitudes of impact summarized in Table 1 that we derived from the theories of neighborhood effect mechanisms? Two of our expectations regarding family status and employment are strongly supported. Regardless of gender, those with children and those who do not work fulltime appear more vulnerable to effects from neighborhood economic mix. This is certainly consistent with our suggestion that such people will tend to spend more time in the neighborhood and develop more localized social networks as a result. Our expectations regarding age and income were not consistently evinced across genders, family statuses, or employment groups. Seemingly different conclusions drawn from the interaction model thus prove oversimplified in the more nuanced context of the stratification models. Though on the basis of theoretical arguments we could make no clear predictions about the role of gender, our stratified models indicate that employment status strongly affects this relationship. For fulltime workers, females (at least over age 30) appear more affected by neighborhood income mix than males, whereas the opposite holds (especially the impact of the proportion of low-income neighbors) for those not employed fulltime. This suggests that fulltime females over age 30 may more heavily use networks accessed in the neighborhood to secure better-paying jobs, while not fulltime males may be more influenced by neighborhood social norms, peers, and or networks or perhaps external forces of stigmatization related to neighborhood mix. Their means of obtaining employment may also be systematically more informal that those employed by Swedish females.

We think it of special interest to highlight a distinct difference in relationships across income groups. Lower-income males typically gain more income from a situation with more middle-income male neighbors than either lower- or higher-income ones. However, higher-income males rarely evince these relationships; typically they gain more from high-income neighbors than either middle-income or low-income ones. To us this indicates the importance of the combination of social distance and social resources in shaping the nature and consequence of social interactions within neighborhoods. If the social distance between neighbors is perceived as too great, there will be minimal social interaction, and thus the potential for any sort of affect transmitted through this mechanism (norms, peers, role modeling, networks) will be minimized, regardless of the potential benefits or harms associated with this interaction. By contrast, for low-income males the social distance with other low-income males neighbors will be little, but they apparently provide through such social interactions less helpful resources for future income gains than middle-income male neighbors. Our claims are further supported by the strong positive relationship between shares of high-income male neighbors and subsequent income gains for males who begin with higher incomes.[15]

Conclusion

In this paper we have attempted to contribute to the burgeoning neighborhood effects literature by investigating the question of how the nature and magnitude of the impact of neighborhood income mix on adult labor incomes differs across individuals based on their gender, age, family status, employment status, and income. We have employed a difference model econometric specification to remove the potential influence of geographic selection on observed magnitude of neighborhood effect, and conduct analyses using interaction variables and stratification by various individual groups. Based on our analysis of 1.67 million adults consistently residing in the three Swedish

[15] Although we do not observe this for high income women, we do not view this as contradictory evidence because there still may be considerable social distance between the average high-income female (starting at 158,450 SEK) and average high-income male used for computing neighborhood income mix (starting at 214,425 SEK).

metropolitan areas from 1991 to 1999, we have found strong evidence of variation in neighborhood effects depending on personal context, with many relationships being statistically and economically significant.

Specifically, lower-income metropolitan Swedish males and females over age 30 experience a gain in their labor income when either lower-income (i.e., males in the lowest 30th percentile) neighbors or (although to a smaller degree) *higher*-income (i.e., males in the highest 30th percentile) neighbors are replaced by an equivalent share of middle-income (i.e., males in the 31st to 70th percentile) neighbors. This relationship is stronger for males not working fulltime and females working fulltime, the presence of children makes for still stronger impacts, regardless of employment status or gender. Males ages 24-30 (especially those not employed fulltime) seem similarly affected by neighborhood income mix (i.e., gaining from middle-income neighbors), whereas females of this age do not. By contrast, higher-income metropolitan Swedish males of any age rarely evince the aforementioned relationship; typically they gain more from high-income neighbors than middle-income or low-income ones. Overall, we see a consistent pattern of neighborhood mix effects being stronger for parents and those who do not work fulltime, independently of other individual dimensions, as comports with our theoretical expectations regarding a variety of potential neighborhood effect mechanisms.

There are at least two main implications for scholarship and policy that follow from our conclusion that the neighborhood effect size is highly contingent on individual characteristics. From a scholarly perspective, this conclusion raises the possibility that the current set of oft-conflicting findings regarding neighborhood impacts may at least partially be due to the failure to account for and distinguish which groups are being investigated. We have found, for example, that the income prospects of low-income males over age 30 are strongly enhanced by the increased presence of middle-income male neighbors (compared to either high-income or, especially, low-income ones), but increases in middle-income neighbors will have the opposite effect on high-income males if they substitute for high-income neighbors. A study of neighborhood income mix effects that aggregates males thus will tend to obscure and minimize the true effects. From a public policy perspective, we have noted above that neighborhood income mixing has assumed crucial importance for many current debates and programmatic formulations. Unfortunately, this unfortunately has occurred under the implicit assumption that neighborhood income mix will substantially and similarly affect labor market outcomes for all residents (or, perhaps, all “socially disadvantaged” residents). This paper shows that this assumption should be strongly rejected, especially on the basis of gender, family status, and employment status and, in a more nuanced way, on age and income as well. The pragmatic implications for planner and policymakers are daunting indeed, for it suggests much more explicit consideration of the populations that should be targeted for gains through mixed income neighborhoods, and amore finely tuned strategy to achieve the optimal mix for these target groups. Our findings also raise the uncomfortable political prospect that the consequences from the often standardized, “one size fits all” programs for neighborhood mixing underway today will vary significantly among target groups, with some perhaps being unforeseen and unwanted.

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Table 1. Predicted Associations Between Magnitude of Neighborhood Effect and Individual Characteristics, by Neighborhood Effect Mechanism

	Adults Differing By:				
Neighborhood Effect Mechanism	Gender (Males)	Age (Years)	Family (# Children)	Employment (# Hours)	Income (Money Unit)
Socialization	**?**	**-**	**+**	**-**	**-**
Networks	**?**	**-**	**+**	**-**	**-**
Competition	**?**	**?**	**+**	**-**	**-**
Stigmatization	**?**	**-**	**?**	**-**	**-**
Institutional Resources	**?**	**-**	**?**	**-**	**-**
Job Accessibility	**?**	**-**	**+**	**?**	**-**

Table 2. Descriptive Statistics for Sample

Outcome Variable	Mean	Std. Dev.
Annual mean labor income, 1996-1999	1848.95	1433.06
(100 Swedish *kroner*, SEK)		
Neighborhood variables		
proportion in lowest 3 male income deciles	.300	.120
proportion in highest 3 male income deciles	.350	.139
Control Variables		
males	.501	.500
# children under age 7, 1995	.330	.676
# child-years under 7, 1996-99	1.184	2.328
Some sick leave during 1995 (1=yes)	.161	.365
Pre-retired during 1995 (1=yes)	.062	.239
Parental leave during 1995 (1=yes)	.232	.419
Studying during 1995 (1=yes)	.064	.236
# years with pre-retirement, 1996-99 (1=yes)	.312	1.031
# years studying, 1996-99	.213	.701
# years with parental leave, 1996-99	.811	1.395
# years with sick leave, 1996-99	.562	1.020
Immigrants w/ < 5 years in Sweden (1=yes)	.010	.087
No formal education (1=yes)	.012	.101
< 10 years education (1=yes)	.093	.282
10 years education (1=yes)	.121	.329
13 years, some post-secondary (1=yes)	.084	.265
14+ years, but no PhD (1=yes)	.243	.426
PhD attained (1=yes)	.012	.104
Education rose LT 11-12 to 11-12+(1=yes)	.020	.134
Education rose 11-12 to higher (1=yes)	.040	.199
Age in years	41.06	10.29
Civil status in 1995: couple	.583	.493
Single 1991 but couple 1995 (1=yes)	.093	.283
Couple 1991 but single 1995 (1=yes)	.080	.277
Mean income in local labour market, 1995 (100 SEK)	1452.8	94.67
Mean income in local labour market, 1999 (100 SEK)	1770.8	143.88
N	1,667,641	

Table 3. Regression Results of Difference Model of Labor Incomes

[Dependent variable = change in ln(mean annual labor income) 1991-94 to 1996-99]

	B	std.error	beta	t	signif.
Constant	0.074	0.002	NA	39.47	0.000
Education rose from low level, 1991-95*	0.427	0.009	0.035	49.03	0.000
Education rose from medium level, 1991-95*	0.369	0.006	0.045	57.76	0.000
Change in # years with pre-retirement benefits^	-0.769	0.002	-0.296	-415.18	0.000
Change in # years with sick leave benefits^	0.162	0.001	0.115	160.98	0.000
Change in # years with parental leave benefits^	0.084	0.001	0.071	86.43	0.000
Change in # years studying^	-0.281	0.001	-0.16	-206.98	0.000
Change in # of child-years, children < 7 years, 1991-95	-0.050	0.001	-0.077	-90.78	0.000
Recent immigrant in 1991, not 1995	-0.009	0.006	-0.001	-1.51	0.132
Civil status changed from couple to single, 1991-95**	0.084	0.004	0.014	19.19	0.000
Civil status changed from single to couple, 1991-95**	0.160	0.005	0.028	34.89	0.000
Change in local labor market mean earnings, 1991-95	0.001	0.000	0.033	44.70	0.000
Change in neigh. prop. in lowest 3 male income deciles	1.109	0.085	0.062	12.00	0.000
[above] X fulltime employment 1995	0.690	0.046	0.035	15.01	0.000
[above] X male	-0.169	0.035	-0.008	-4.86	0.000
[above] X any children in 1995	-0.220	0.036	-0.008	-6.07	0.000
[above] X age 1995	0.004	0.002	0.01	2.52	0.012
[above] X ln income in 1995 (in 100,000 SEK)	-0.311	0.009	-0.121	-33.34	0.000
Change in neigh. prop. in highest 3 male income deciles	-2.006	0.087	-0.128	-22.96	0.000
[above] X fulltime employment 1995	-1.584	0.047	-0.09	-33.52	0.000
[above] X male	0.011	0.033	0.00	0.33	0.743
[above] X any children in 1995	-0.312	0.034	-0.013	-9.22	0.000
[above] X age 1995	0.040	0.002	0.098	25.33	0.000
[above] X ln income in 1995 (in 100,000 SEK)	0.254	0.011	0.111	23.29	0.000
R-squared	0.166				
F	14427				
N	1,667,641				

* excluded category = no change in education credentials between 1991 and 1995
** excluded category = no change in civil status between 1991 and 1995
^ Change between 1991-94 and 1996-99 periods

Table 4. Estimated Percentage Changes in Growth of Income Due to Changes in Neighborhood Income Mix (based on parameters in Table 3)

Results for Simulated Increase (Decrease) in Proportion of Low- (Middle-) Income Male Neighbors by ten percentage points

low income males, age 27				high income males, age 27			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
-10.7	-12.7	-4.3	-6.4	-12.5	-14.4	-6.3	-8.3
low income males, age 54				**high income males, age 54**			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
-9.7	-11.7	-3.3	-5.4	-11.6	-13.5	-5.3	-7.3
low income females, age 27				**high income females, age 27**			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
-8.5	-10.5	-2.0	-4.1	-10.2	-12.1	-3.8	-5.9
low income females, age 54				**high income females, age 54**			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
-7.5	-9.5	-0.9	-3.1	-9.2	-11.2	-2.7	-4.8

Results for Simulated Increase (Decrease) in Proportion of High- (Middle-) Income Male Neighbors by ten percentage points

low income males, age 27				high income males, age 27			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
10.0	6.7	-6.1	-9.0	10.9	7.5	-5.4	-8.3
low income males, age 54				**high income males, age 54**			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
22.6	18.8	4.6	1.4	23.5	19.7	5.4	2.2
low income females, age 27				**high income females, age 27**			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
8.3	4.9	-7.6	-10.4	9.9	6.5	-6.2	-9.1
low income females, age 54				**high income females, age 54**			
baseline	parents	fulltime	both	baseline	parents	fulltime	both
20.6	16.9	3.0	-0.2	22.5	18.7	4.5	1.3

baseline = not parents, not fulltime employees both = parents, fulltime employees
low income = 1109.25 (male); 876.5 (female) high income = 2144.25 (male); 1584.5 (female)

Table 5 Coefficients and ρ Values for Neighborhood Income Mix Variables, Various Female Strata

	Females w/ Earnings Lowest 30th Percentile					Females w/ Earnings Highest 30th Percentile				
	N	% Low-Inc. Neighs.		% High-Inc. Neighs.		N	% Low-Inc. Neighs.		% High-Inc. Neighs.	
		B	ρ	B	ρ		B	ρ	B	ρ
Age 24-30										
Full-time										
Kids=0	32,483	0.098	0.174	0.069	0.324	12,085	-0.038	0.540	-0.005	0.917
Kids>0	15,027	0.179	0.278	0.261	0.089	5,634	-0.081	0.367	0.020	0.771
Not Full-time										
Kids=0	13,812	0.060	0.782	-0.155	0.505	294	1.476	0.229	0.230	0.824
Kids>0	15,832	0.160	0.464	-0.092	0.709	279	-2.129	0.103	0.019	0.984
Age 31-46										
Full-time										
Kids=0	11,044	-0.403	0.066*	-0.218	0.347	43,620	-0.096	0.019**	-0.025	0.475
Kids>0	39,804	-0.105	0.392	-0.103	0.380	61,953	0.003	0.920	0.069	0.007***
Not Full-time										
Kids=0	17,064	-0.450	0.047**	-0.229	0.275	12,999	-0.663	0.453	-0.935	0.230
Kids>0	35,879	-0.380	0.033**	-0.274	0.175	1,992	-0.671	0.271	-0.073	0.882
Age 47-60										
Full-time										
Kids=0	19,805	-0.867	0.000***	-0.658	0.004***	98,698	0.047	0.422	0.008	0.871
Kids>0	5,526	-1.030	0.011**	-0.748	0.060*	21,015	-0.098	0.153	-0.072	0.215
Not Full-time										
Kids=0	35,556	-0.301	0.106	-0.477	0.025**	2,949	1.370	0.136	1.103	0.175
Kids>0	5,924	-0.890	0.069*	-0.931	0.098*	429	1.190	0.495	-0.077	0.962

**** ρ<.10 ** ρ<.05 *** ρ<.01***

Note: age, employment, and earnings measured at beginning of analysis period
All results produced by regressions using full set of control variables

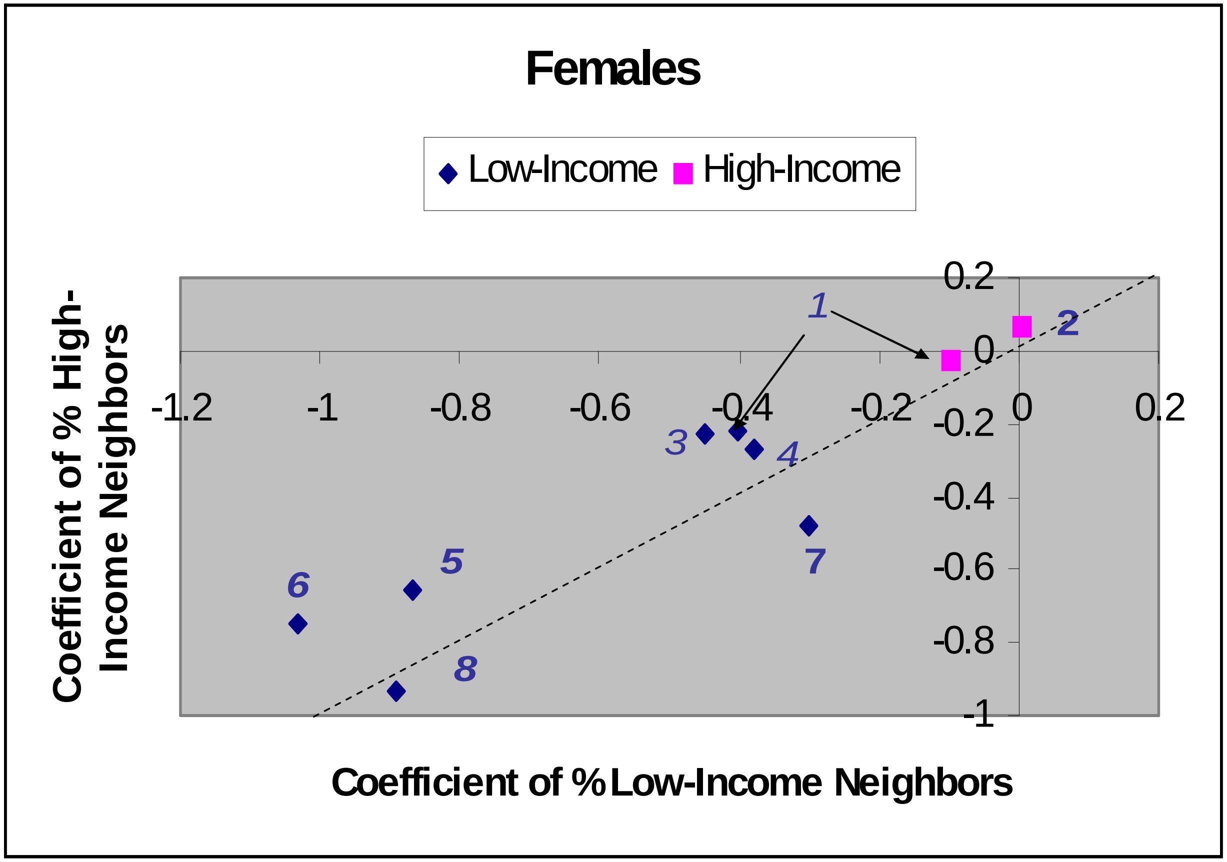
Table 5 Coefficients and ρ Values for Neighborhood Income Mix Variables, Various Male Strata

	Males w/ Earnings Lowest 30th Percentile					Males w/ Earnings Highest 30th Percentile				
	N	% Low-Inc. Neighs.		% High-Inc. Neighs.		N	% Low-Inc. Neighs.		% High-Inc. Neighs.	
		B	ρ	B	ρ		B	ρ	B	ρ
Age 24-30										
Full-time										
Kids=0	46,907	-0.030	0.635	-0.167	0.009***	6,016	-0.239	0.009***	-0.090	0.217
Kids>0	8,705	-0.047	0.769	0.039	0.803	4,107	-0.151	0.103	0.058	0.402
Not Full-time										
Kids=0	23,563	-0.318	0.065*	-0.056	0.011**	88	-6.99	0.039**	-3.849	0.205
Kids>0	4,791	-0.221	0.549	-0.769	0.083*	50	2.494	0.491	1.278	0.673
Age 31-46										
Full-time										
Kids=0	24,423	-0.228	0.082*	-0.035	0.818	38,089	-0.028	0.512	0.052	0.147
Kids>0	21,543	-0.136	0.329	0.062	0.666	79,965	-0.009	0.743	0.211	0.000***
Not Full-time										
Kids=0	38,050	-0.756	0.000***	-0.677	0.000***	822	-2.151	0.048**	-1.204	0.268
Kids>0	16,918	-0.772	0.001***	-0.946	0.002***	906	-1.026	0.318	-0.856	0.378
Age 47-60										
Full-time										
Kids=0	17,466	-0.438	0.045**	-0.415	0.078*	84,149	0.387	0.000***	0.391	0.000***
Kids>0	5,710	-0.969	0.010***	-0.439	0.264	34,412	0.326	0.000***	0.269	0.000***
Not Full-time										
Kids=0	32,722	-0.304	0.101	-0.288	0.217	2,826	1.486	0.17	1.811	0.048**
Kids>0	5,995	-2.608	0.000***	-1.958	.001***	724	1.262	0.603	1.082	0.550

***** ρ<.10 ** ρ<.05 *** ρ<.01***

Note: age, employment, and earnings measured at beginning of analysis period
All results produced by regressions using full set of control variables

Figure 1: Magnitudes of Coefficients for Neighborhood Variables

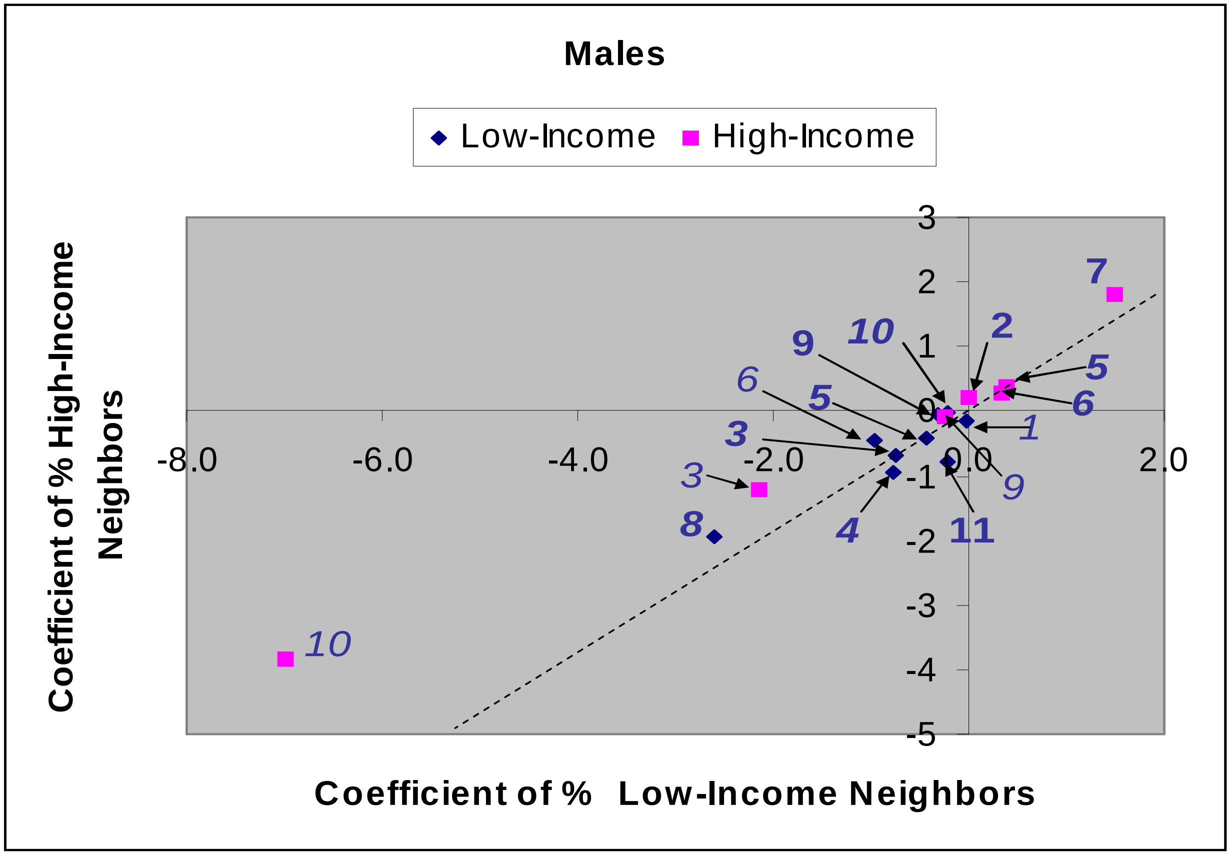


KEY
1: Age 31-46; FT; kids=0
2: Age 31-46; FT; kids>0
3: Age 31-46; NFT; kids=0
4: Age 31-46; NFT; kids>0
5: Age 47-60; FT; kids=0
6: Age 47-60; FT; kids>0
7: Age 47-60; NFT; kids=0
8: Age 47-60; NFT; kids>0

[FT=full-time]
[NFT=not full-time]

Statistical Significance of Neighborhood Coefficients Shown (p<.10): *Italics* = %low; **Bold** = %high; ***Bold Italics*** = both

Figure 2: Magnitudes of Coefficients for Neighborhood Variables



KEY
1: Age 31-46; FT; kids=0
2: Age 31-46; FT; kids>0
3: Age 31-46; NFT; kids=0
4: Age 31-46; NFT; kids>0
5: Age 47-60; FT; kids=0
6: Age 47-60; FT; kids>0
7: Age 47-60; NFT; kids=0
8: Age 47-60; NFT; kids>0
9: Age 24-30; FT; kids=0
10: Age 24-30; NFT; kids=0
11: Age 24-30; NFT; kids>0

[FT=full-time]
[NFT=not full-time]

Statistical Significance of Neighborhood Coefficients Shown (p<.10): *Italics* = %low; **Bold** = %high; ***Bold Italics*** = both