



## UvA-DARE (Digital Academic Repository)

### Neighborhood Social Mix and Adults' Income Trajectories: Longitudinal Evidence from Stockholm

Galster, G.; Andersson, R.; Musterd, S.

**DOI**

[10.1111/geob.12096](https://doi.org/10.1111/geob.12096)

**Publication date**

2016

**Document Version**

Final published version

**Published in**

Geografiska Annaler. Series B. Human Geography

**License**

Article 25fa Dutch Copyright Act (<https://www.openaccess.nl/en/policies/open-access-in-dutch-copyright-law-taverne-amendment>)

[Link to publication](#)

**Citation for published version (APA):**

Galster, G., Andersson, R., & Musterd, S. (2016). Neighborhood Social Mix and Adults' Income Trajectories: Longitudinal Evidence from Stockholm. *Geografiska Annaler. Series B. Human Geography*, 98(2), 145-170. <https://doi.org/10.1111/geob.12096>

**General rights**

It is not permitted to download or to forward/distribute the text or part of it without the consent of the author(s) and/or copyright holder(s), other than for strictly personal, individual use, unless the work is under an open content license (like Creative Commons).

**Disclaimer/Complaints regulations**

If you believe that digital publication of certain material infringes any of your rights or (privacy) interests, please let the Library know, stating your reasons. In case of a legitimate complaint, the Library will make the material inaccessible and/or remove it from the website. Please Ask the Library: <https://uba.uva.nl/en/contact>, or a letter to: Library of the University of Amsterdam, Secretariat, Singel 425, 1012 WP Amsterdam, The Netherlands. You will be contacted as soon as possible.

UvA-DARE is a service provided by the library of the University of Amsterdam (<https://dare.uva.nl>)

# NEIGHBORHOOD SOCIAL MIX AND ADULTS' INCOME TRAJECTORIES: LONGITUDINAL EVIDENCE FROM STOCKHOLM

by

George Galster, Roger Andersson and Sako Musterd

GALSTER, G., ANDERSSON, R. and MUSTERD, S. (2016): 'Neighborhood social mix and adults' income trajectories: longitudinal evidence from Stockholm', *Geografiska Annaler: Series B, Human Geography* 98 (2): 145–170.

**ABSTRACT.** We investigate the relationship between neighborhood income composition and income trajectories of adults, employing annual panel data from Stockholm over the 1991–2008 period and multiple measures of neighborhood income mix. We advance the human geography literature in three ways by quantifying neighborhood effects that: (1) are unusually precise due to our large sample size; (2) are arguably causal and unbiased due to the econometric techniques employed; (3) are potentially heterogeneous, varying according to gender, income group, and ethnicity. Our innovative, fixed-effect change modeling indicates that neighborhood income mix affects subsequent one- and five-year income trajectories of residents in highly heterogeneous ways according to gender, income and ethnicity, and for some groups this effect is substantial. The evidence supports on Pareto improvement grounds a social mix policy that attempts to reduce the incidence of lower-income dominant neighborhood environments and replace them with more mixed or middle-income dominant ones.

**Keywords:** neighborhood effects, social mixing, fixed effects models, neighborhood income mix, Pareto improvement, Stockholm

## Introduction

Several urban policy initiatives have emerged during recent decades in Europe, Australia and the US that aim to encourage or require “social mixing” within neighborhoods as an antidote to the perceived social evils associated with “concentrated disadvantage” (Galster 2013; Galster and Friedrichs 2015). Programmatic examples include: urban regeneration measures that replace concentrations of social housing with more diverse housing stocks; social housing management and tenant allocation reform; tenant-based housing allowances; and land-use planning rules requiring mixed developments; see Murie and Musterd (2004), Berube (2005), Briggs (2005), Musterd and Andersson (2005), Norris (2006), Andersson (2008), Andersson *et al.* (2010) and Bergsten (2010).

Seven causal pathways have been hypothesized for linking neighborhood social mix and labor market outcomes; see especially Jencks and Mayer (1990),

Manski (1995), Duncan *et al.* (1997), Gephart (1997), Sampson (2001), Dietz (2002), Sampson *et al.* (2002), Ioannides and Loury (2004) and Galster (2012). The *socialization mechanism* suggests that residents can develop different attitudes, values, behaviors and expectations about skill acquisition, educational credentials, labor force participation, and criminal activities as a result of interactions with neighborhood peers and role models; see Sullivan (1989), Anderson (1990, 1991, 2004); Case and Katz (1991), Diehr *et al.* (1993), Ginther *et al.* (2000) and South and Baumer (2000). The *collective social control mechanism* suggests that pervasive community norms regarding education, work and criminality can shape residents' attitudes, values, and behaviors in these realms because they do not wish to risk the potential social sanctions (such as ostracism) associated with violation of these norms (e.g. Simmel 1971; Weber 1978; South 2001; Pinkster 2008). The *localized social networks mechanism* suggests that residents may gain different amounts of information about skill-enhancing and employment opportunities, depending on the degree to which they rely on spatially localized, intra-neighborhood social networks and the degree to which such networks help individuals to access income-enhancing resources (Granovetter 1995). Limited social ties with employed and better-educated people who may possess such income-enhancing information is an often-observed characteristic of non-employed and lower-income people, especially when the latter reside amid concentrations of similarly disadvantaged residents (Wilson 1987, 1996; Fernandez and Harris 1992; Tigges *et al.* 1998; Bertrand *et al.* 2000; Buck 2001; Farwick 2004; Pinkster 2008). The *social disorder mechanism* suggests that eroding neighborhood safety may: (1) signal residents (especially males) that superior economic gains (and less chance of arrest) may be had by substituting participation in illegal activities in the neighborhood for participation in the legal labor force (Skogan 1990); (2)

alter residents' ability to take advantage of a different range of skill-enhancing and employment opportunities depending on the degree to which they feel secure leaving their homes and traversing their neighborhoods (Sampson *et al.* 1997; Sampson and Raudenbush 1999); and (3) create psychological damage to residents that may impair their income-earning potential (Fitzpatrick and Boldizar 1993; Singer *et al.* 1995). The *stigmatization mechanism* suggests that prospective employers in the metropolitan area may evaluate job applicants residing in certain locales based on the disrepute of the place; see especially Kirschenman and Neckerman (1991), Wacquant (1993), Power (1997), Taylor (1998), Atkinson and Kintrea (1998), Dean and Hastings (2000), Hastings and Dean (2003), Martin and Watkinson (2003), Hastings (2004), and Permentier (2009). The *institutional resources mechanism* suggests that public and private institutions controlling important services and facilities geographically vary their quantity and quality on the basis of neighborhood economic status, thereby differentially affecting residents' opportunities to acquire and use human capital and labor market information accessed through these institutional resources (Kozol 1991; Card and Krueger 1992; Rasmussen 1994; Bauder 2001; Lankford *et al.* 2002; Condrón and Roscigno 2003; Hastings 2007, 2009a, 2009b). The *job accessibility mechanism* suggests that neighborhoods offer different degrees of access to employment information and work sites themselves, due to the combination of the degree of geographical proximity and public transportation disparities (Ihlanfeldt 1999; Zenou *et al.* 2006). It is important for our purposes to note that a neighborhood's access to jobs may change endogenously with changes in its income mix. For example, gentrification may lead to a substantial increase in local retail and entertainment job opportunities or, conversely, severe downward income succession may lead retailers to close if local disposable incomes fall sufficiently in the aggregate. In sum, extant theory argues that at least seven, non-mutually exclusive mechanisms may produce an individualistic labor market effect stemming from neighborhood income mix. Qualitative studies have provided some qualified support for all of these potential mechanisms, though it is currently impossible from the extant literature to conclude which mechanisms might be the dominant contributors to the economic relationships we are investigating here.

On the contrary, recent work implies that there may *not* be a uniformly "dominant" mechanism but

instead the influence of each mechanism will vary across residential groups within any given neighborhood depending on their social identity and their embeddedness in local social life (Pinkster 2014). Several of the above mechanisms suggest that effects are heterogeneous by gender and income group, though not necessarily in unambiguous ways (Galster *et al.* 2010; Burdick-Will *et al.* 2011; Harding *et al.* 2011; Sanbonmatsu *et al.* 2011; Small and Feldman 2012; Sharkey and Faber 2014). The key linkages rely upon the notion that intra-neighborhood mechanisms (the first four listed above) have stronger effects to the extent that people: (1) spend more time in the neighborhood; (2) are more locally oriented in their social interactions; and (3) do not marshal sufficient resources to insulate themselves from these (negative) effects. Gender and economic characteristics can serve as proxies for these three conditions. We would expect, for example, local social control in areas with more traditional, patriarchal norms would limit the ability and willingness of women to look for employment opportunities outside of the neighborhood (Pinkster 2008). However, these same social controls may produce strict monitoring of the behaviors of women, thus potentially insulating them from neighborhood peer effects and negating their greater time spent in the neighborhood (Pinkster 2008). Women with child-care responsibilities would also be more likely to develop a denser network of relationships that is more focused on the neighborhood (Kleinhans 2004; Kleit 2008). Given their more localized job search processes (Granovetter 1995; Waldinger and Lichter 2003) and more geographically localized social networks (Fischer 1982), we could infer that those who have lower incomes would be more vulnerable to this neighborhood effect mechanism, though again there may be gender differences (Pinkster 2008). We would predict that women would be more vulnerable to neighborhood social disorder. Lower-income residents would similarly be more vulnerable, inasmuch as they would have fewer resources available for buffering themselves from the disorder, as in being forced to use public transportation instead of a private auto to commute to work.

The impacts from extra-neighborhood mechanisms (the last three noted above) may also be contingent on gender and income. Area-based stigmatization may reinforce pre-existing class and gender biases of prospective employers. Inferior local public institutions and services are more likely to harm those families with fewer economic resources available to substitute private services. 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 – lower-income (due to lower auto ownership rates and more peripheral residential locations) and those with child-care responsibilities (who require transportation to day care) – would feel the greatest impact.

The international evidence from non-experimental and experimental quantitative studies indeed suggests that different mechanisms may have varying salience across different groups (Turley 2003; Bergsten 2010; Galster *et al.* 2010; Burdick-Will *et al.* 2011; Clampet-Lundquist *et al.* 2011; Ludwig 2012; Musterd *et al.* 2012; Andersson and Malmberg 2015; Sharkey and Faber 2014).

Despite its theoretical justification and widespread adoption in urban planning and public policy circles, the goal of socially mixed neighborhoods has been challenged on conceptual and empirical grounds by a wide range of scholars; see Atkinson and Kintrea (2000, 2001), Ostendorf *et al.* (2001), Kearns (2002), Musterd (2002, 2003), Musterd *et al.* (2003), Meen *et al.* (2005), Delorenzi (2006), Joseph (2006), Joseph *et al.* (2006), Cheshire (2007), Van Kempen and Bolt (2009), Darcy (2010) and Galster and Friedrichs (2015). Perhaps most fundamental to this critique is the empirical argument that disadvantaged households economically fare about the same, regardless of their residential environments.

Our research reported here tries to respond to the obvious need to understand more about whether there is any statistically and economically significant relationship between neighborhood social mix context and resulting labor market outcomes for adult residents, whether they be disadvantaged in some ways or not (Andersson 2008; van Ham *et al.* 2012). Specifically, we employ annual panel data on working-age adults residing in Stockholm over the 1991–2008 period and measure neighborhood social mix both by the four-year average exposures to low- and high-income neighbors and by distinctive neighborhood income-mix “types” revealed by a cluster analysis. We address these research questions:

- Is there a strong relationship between neighborhood income mix and the one- and five-year trajectories of labor incomes subsequently earned by working-age adults in the neighborhood?
- Does the answer vary depending upon gender, income, and/or ethnic group; that is, are these effects heterogeneous?

Our modeling framework leads to an original specification that integrates a change model and a fixed-effect panel model in a way that minimizes potential biases from both time-varying and invariant unobservables and thus permits valid causal inferences. We advance the scholarly literature in three ways by quantifying neighborhood effects that: (1) are unusually precise due to our large sample size; (2) are arguably causal and unbiased due to the econometric techniques employed; (3) are potentially heterogeneous, varying according to gender, income group, and ethnicity.

### **Prior scholarship quantifying neighborhood effects on individual incomes and the challenge of geographic selection bias**

The empirical challenges associated with obtaining accurate estimates of the independent causal effect of neighborhoods on individual residents has been the subject of numerous scholarly reviews; see Manski (1995, 2000), Gephart (1997), Leventhal and Brooks-Gunn (2000), Dietz (2002), Sampson *et al.* (2002), Ellen and Turner (2003), Friedrichs *et al.* (2003), Durlauf (2004), Galster (2008) and Ross (2012). Arguably the central methodological challenge that researchers confront in obtaining an unbiased estimate of the magnitude of neighborhood effects is geographic selection bias (Ginther *et al.* 2000). The most basic selection issue is that certain types of individuals who have certain (unmeasured) characteristics will move from/to certain types of neighborhoods and these same unobservables may also affect the outcome in question. Any observed relationship between neighborhood conditions and outcomes for such individuals or their children may therefore be biased because of this systematic spatial selection process, *even if all the observable characteristics are controlled* (Duncan *et al.* 1997). As Weinberg *et al.* (2004), Bolster *et al.* (2007) and Gennetian *et al.* (2011) show, these biases can be substantial enough to seriously distort conclusions about the magnitude and even the direction of neighborhood effects if not controlled appropriately. Scholarly skepticism about the importance of neighborhood effects remains (e.g. Cheshire 2007; Smolensky 2007); it is thus incumbent upon quantitative research to address such methodological challenges.

There have been three general empirical approaches adopted in response to the challenge of geographic selection bias. The most common approach

consists of a variety of econometric techniques applied to observational (non-experimentally generated) longitudinal datasets. The two other, less common approaches use natural or experimental designs to generate quasi-random or random assignments of households to neighborhoods. None of the three broad approaches as thus far applied have proven limitation free and unambiguously superior, as amplified below.

#### *Econometric models based on observational data*

Most studies of neighborhood effects have used cross-sectional or longitudinal data collected from surveys of individual households residing in a variety of neighborhoods as a result of mundane factors associated with normal market transactions. The subset that has tried to overcome geographic selection bias employs one or more of the following: (1) difference (change) models; (2) fixed-effect models; (3) instrumental variables; (4) sibling comparisons; (5) propensity score matching; (6) comparisons of individuals on the same block.

Unfortunately, there is no empirical consensus about neighborhood effects on labor market outcomes when using one of the aforementioned econometric techniques on non-experimental, observational datasets.<sup>1</sup> Several studies using US data (Weinberg *et al.* 2004; Dawkins *et al.* 2005; Vartanian and Buck 2005; Bayer *et al.* 2008; Cutler *et al.* 2008; Sharkey 2012), several using Swedish data (Galster *et al.* 2008, 2010, 2015a; Musterd *et al.* 2012; Hedman and Galster 2013), one Scottish study (van Ham and Manley 2009) and one French study (Sari 2012) find non-trivial neighborhood effects on various labor market outcomes. On the other hand, three UK-based analyses (Bolster *et al.* 2007; Propper *et al.* 2007; van Ham and Manley 2010) and one from the US (Plotnick and Hoffman 1999) find minor, if any, neighborhood effects, and instead suggest selection dominates.

There are several obvious potential reasons for these discrepancies, including differences in: (1) nation-specific variations in neighborhood conditions and public services and social welfare policies; (2) variations in the labor market outcomes measured (e.g. employment, earnings, income from all sources); (3) how neighborhood conditions are operationalized (e.g. disadvantage index, poverty rates, income mix, tenure mix)<sup>2</sup>; (4) timing of neighborhood effects (e.g. contemporaneous or cumulative); and (5) geographic size of the

neighborhood (e.g. census tract, ward, bespoke area of 500 m radius). Another basic reason is that all of the aforementioned econometric approaches have their distinctive limitations, though it is beyond the scope of this article to present them in any detail.<sup>3</sup> Suffice it to note the ongoing debate about whether individual economic outcomes are best measured as *changes* or *levels* (i.e. the relative superiority of the difference and fixed-effect models). Bolster *et al.* (2007), Propper *et al.* (2007) and van Ham and Manley (2010) model temporal changes in outcomes as a function of initial level of neighborhood conditions, whereas all the studies finding strong neighborhood effects model the level of individual outcome as a function of (contemporaneous, lagged, or cumulative) levels of neighborhood indicators, except Galster *et al.* 2008, 2010.<sup>4</sup> Galster and Hedman (2013) demonstrated using the same dataset that substantially different conclusions are reached about how much neighborhood income mix affects individuals' incomes when variants of the first three econometric approaches above are used, even when all of the prior reasons for discrepancies are held constant. Of interest, they found a substantial neighborhood effect, regardless of method employed.

#### *Quasi-random assignment natural experiments*

It is sometimes possible to observe non-market interventions into households' residential locations that mimic random assignment. In the US such experiments have been based on court-ordered, public housing racial-ethnic desegregation programs; elsewhere they have involved allocation of tenants to social housing or placement of refugees in particular locales. Rosenbaum (1991, 1995), Rubinowitz and Rosenbaum (2000), Edin *et al.* (2003), Åslund and Fredricksson (2009), Piil Damm (2009, 2014), DeLuca *et al.* (2010) Galster *et al.* (2015b, 2015c, 2015d), and Chyn (2016) find evidence of neighborhood effects on labor market outcomes in their analyses of natural experiments; only Oreopolos (2003) does not.

Although these natural experiments may indeed provide some exogenous variation in neighborhood locations, the geographic selection problem is unlikely to be avoided completely. In most cases, program participants have some non-trivial latitude in which locations they choose, both initially and subsequent to original placement. Moreover, if the programs involve the use of rental vouchers, there will



be selection in who succeeds in locating rental vacancies in qualifying locations and signing leases within the requisite period. These selection processes raise the possibility that those who succeed in living in low-poverty neighborhoods were especially motivated and resourceful ... traits poorly measured by researchers but likely ones that would help them succeed economically irrespective of their neighborhoods. Additional empirical problems can arise if sampled subjects move quickly from their randomly assigned dwellings to another location, thereby minimizing exposure to neighborhood context and potentially confounding consequences because moving itself can be disruptive. Finally, there often are limitations in the range of neighborhoods to which study participants moved because of where subsidized housing was located, thereby reducing the ability of statistical tests to discern neighborhood effects.

#### *Random assignment experiments*

Many researchers advocate a random assignment experimental approach for best avoiding biases from geographic selection. Data on outcomes that can be produced by an experimental design whereby individuals or households are randomly assigned to different neighborhoods is indeed, *in theory*, the preferred method. In this regard, the Moving To Opportunity (MTO) demonstration has been touted conventionally as *the* study from which to draw conclusions about the magnitude of neighborhood effects (e.g. Gennetian *et al.* 2011). As explained below, however, MTO proves to be a better experimental design for evaluating voucher-based housing policy impacts than measuring neighborhood effects. The MTO research design randomly assigns public housing residents who volunteer to participate to one of three experimental groups: (1) controls that get no voucher but can stay in public housing in disadvantaged neighborhoods; (2) recipients of rental vouchers; and (3) recipients of rental vouchers and relocation assistance who had to move to neighborhoods with less than 10% poverty rates and remain for at least a year. Most investigations of MTO data uncovered no substantial neighborhood effects on adult labor market outcomes (e.g. Katz *et al.* 2001; Goering and Feins 2003; Orr *et al.* 2003; Ludwig *et al.* 2005, 2008; Kling *et al.* 2007; Ludwig 2012). Based on this, it has been claimed that 'MTO is the gold standard ... [and] its results ... have proven discouraging ... neighborhood quality

... [has] little effect on desirable and measurable outcomes ...' (Smolensky 2007, p. 1016).

Such a sweeping conclusion with regards to adult labor market outcomes is inappropriate given the substantial debate over the power of MTO as a test of neighborhood effects (cf. Clampet-Lundquist and Massey 2008; Briggs *et al.* 2008, 2011; Sampson 2008; Briggs *et al.* 2010; Burdick-Will *et al.* 2011; Sanbonmatsu *et al.* 2011; Ludwig 2012). The debate focuses on five domains. First, although MTO randomly assigns participants to treatment groups, it neither randomly assigns characteristics of neighborhoods initially occupied by voucher holders (except maximum poverty rates for the experimental group) nor characteristics of neighborhoods in which participants in all three groups may move subsequently. Thus, there remains considerable question about the degree to which geographic selection on unobservables persists. Second, MTO may not create adequate duration of exposure to neighborhood conditions by any group at any location to observe much treatment effect.<sup>5</sup> Third, MTO overlooks the potentially long-lasting and indelible developmental effects upon adult experimental group participants who spent their childhoods in disadvantaged neighborhoods. Fourth, it appears that even experimental MTO movers rarely moved out of predominantly black-occupied neighborhoods near those of concentrated disadvantage and achieved only modest changes in school quality and job accessibility. Thus, they may not have experienced sizable enhancements in their opportunity structures. Fifth, many participants in MTO may not have been expected to evince much labor market activity in *any* neighborhood context without additional assistance. About one-quarter of the MTO families were headed by an adult unable to work because of disabling, chronic illness, while many more needed childcare and transportation that, likewise, were not in the package of supports offered in the experiment. Sixth, impacts may be contingent on age of assignment. Chetty *et al.* (2015) analyzed the subset of MTO experimental children who moved to low-poverty neighborhoods before they were age 13 and observed that they subsequently exhibited significantly higher earnings as young adults. Thus, despite its theoretical promise and conventional wisdom notwithstanding, MTO may not have provided definitive evidence about the potential effects on the poor from prolonged residence in multiply advantaged neighborhoods.

## Modeling framework

Such uncertainties in the empirical literature motivate our further exploration of a rich observational panel dataset. Our outcome of interest is the individual's annual income from work (measured in Swedish kronor; \$1=8.6 kronor).<sup>6</sup> Since this indicator encapsulates the net impact of 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 performance. We specify the annual income from work ("income" hereafter) earned during year  $t+1$ <sup>7</sup> for individual  $i$  as:

$$I_{t+1ij} = \alpha + \beta[P_{it}] + \gamma[P_i] + \theta[N_{ij}] + \mu[L_{t+1}] + \hat{\alpha}_i + \pi_{t+1i} + \varepsilon_{t+1i} \quad (1)$$

where  $I_{t+1ij}$  = income from work observed for individual  $i$  earned during course of year  $t+1$  after being exposed to neighborhood environment  $j$  observed at end of year  $t$ ;  $[P_{it}]$  = characteristics for individual  $i$  that can vary over time, observed at end of year  $t$  (e.g. marital or fertility status, educational attainment);  $[P_i]$  = observed characteristics for individual  $i$  that do not vary over time; (e.g. year and country of birth);  $[N_{ij}]$  = characteristics of neighborhood  $j$  where individual resides, observed at end of year  $t$  and for three years prior (e.g. average share of low-income neighbors);  $[L_{t+1}]$  = characteristics of the regional labor market observed during the course of year  $t+1$  that potentially affect incomes of all working-age adults throughout  $t+1$ ;  $\hat{\alpha}_i$  = error term associated with unobserved characteristics for individual  $i$  that do not vary over time after start of analysis period that may affect income (e.g. childhood experiences, certain beliefs and work habits);  $\pi_{t+1i}$  = error term associated with unobserved characteristics for individual  $i$  that vary over time after start of analysis period that may affect income (e.g. psychological state, health, genetic attributes that express themselves during different periods);  $\varepsilon_{t+1i}$  = a random error term with usual assumed statistical properties (such as measurement error).

The aforementioned geographic selection bias occurs when unobserved heterogeneity  $\hat{\alpha}_i$  and/or  $\pi_{t+1i}$  is not statistically controlled and proves correlated with  $[N_{ij}]$ , thereby producing a violation of the standard independence assumptions for the aggregate error term  $\varepsilon_{t+1i}$  in Equation (1). We delineate these two potential sources of selection to justify the two steps we have taken to minimize the likelihood of their introducing bias into

our estimates. The annual panel nature of our data provides a foundation for our two-step solution. First, following Bolster *et al.* (2007), Propper *et al.* (2007), Galster *et al.* (2008, 2010) and van Ham and Manley (2009, 2010), we difference Equation (1) over two periods of span  $\tau$  (alternatively, one- and five-year changes in our analyses), thereby eliminating the influence of fixed observed and unobserved personal characteristics because their differences over time are zero:

$$\Delta I_{ij} = \beta[\Delta P_i] + \theta[\Delta N_j] + \mu[\Delta L] + \Delta \pi_i + \Delta \varepsilon_i \quad (2)$$

As in Bolster *et al.* (2007), we allow changing neighborhood conditions to which the individual is exposed to follow a repeated Markov process described as a linear function of initial conditions of the neighborhood and the individual at time=0:<sup>8</sup>

$$\Delta N_j = \psi_1[P_{0i}] + \psi_2[N_{0j}] + \psi_3[\Delta P_i] + \psi_4 \Delta \pi_i + \lambda_i \quad (3)$$

but, unlike them, we permit not only initial but changing observed and unobserved personal characteristics to have influences on both whether the individual switches neighborhoods and, if so, what characteristics the new neighborhood(s) will possess. For example, during our observation period the individual may gain more educational credentials (which we observe) and networks providing labor market information (which we do not observe) that may subsequently affect what sort of future neighborhood they would be willing and able to occupy. We assume that  $\lambda_i$  errors are uncorrelated with  $[N_{ij}]$  and each other. Substituting (3) into (2) yields:

$$\Delta I_{ij} = (\beta + \theta \psi_3)[\Delta P_i] + \theta \psi_1[P_{0i}] + \theta \psi_2[N_{0j}] + \mu[\Delta L] + (1 + \theta \psi_4) \Delta \pi_i + \theta \lambda_i + \Delta \varepsilon_i \quad (4)$$

Unlike Bolster *et al.* (2007), who assume that errors in Equation (4) are uncorrelated with  $\Delta I_{ij}$  and  $[N_{0j}]$ , we do not. On the contrary, we think it highly plausible that initial neighborhood conditions may affect a variety of unobserved changes in individuals over time (such as mental and physical health, which has been demonstrated by MTO research; see Ludwig 2012) which, in turn, could strongly influence their subsequent income and, perhaps, residential choices. We attempt to control for this remaining potential source of bias with the second step in our method: individual-specific proxies for these *time-varying* unobservables; that is, a fixed-effect model

applied to the change model (4). Our fixed effects model assumes that each individual has a particular intercept differing from the mean *change* in income by some constant value; that is,  $\alpha_i$ , which we would argue serves as a proxy for the  $\Delta^* \pi_i$  term.<sup>9</sup> Thus, Equation (4) can be rewritten as a *fixed-effects change model*:

$$\Delta^* I_{ij} = \alpha_i + (\beta + \theta \psi_3) [\Delta^* P_i] + \theta \psi_1 [P_{0i}] + \theta \psi_2 [N_{0i}] + \mu [\Delta^* L_i] + \theta \lambda_i + \Delta^* \varepsilon_i \quad (5)$$

We believe that our integration of both a change model and a fixed-effect panel model advances the science of quantifying neighborhood effects because it minimizes potential biases from *both* time-varying and invariant unobservables.<sup>10</sup> Our formal test for neighborhood effects becomes one of testing whether  $\theta \psi_2$  is significantly different from zero, and whether its values differ substantially across various gender and income groups. We model (5) alternatively specifying  $\Delta^* I_{ij}$  as a change over one year and then over five years.<sup>11</sup> More specifically, our panel for the one-year change model comprises income changes beginning in 1994–1995 and continuing annually through 2006–2007; the corresponding range for the five-year income change model is 1994–1999 through 2003–2008.<sup>12</sup> We do not explicitly model selection into employment but treat this as an implicit intervening variable in our model of neighborhood effects, in the same way as we treat hours worked and the wage per hour. We regard these as behind-the-scenes aspects of labor force activity that may be affected by neighborhood and ultimately may emerge as an income effect. In this article we do not look into this “black box” of all potential intervening variables.

Because our specification is unusual, we offer a heuristic explanation, using our five-year change model of (5) as an illustration. We are investigating whether the change in an individual's income over the succeeding five years is related to the income mix of that person's neighborhood as experienced over the previous four years, controlling for both personal characteristics that are fixed and those that change over the five-year period, as well as changing local labor market conditions. We have reduced the potentially biasing effect of time-invariant unobserved individual characteristics by specifying income changes, but we also want to reduce the potentially biasing effect of unobserved individual characteristics that may vary across the 1994–2008 period. We do so by adding an individual fixed effect

that we obtain by estimating the five-year change model for repeated years, in a conventional panel context.

## Data and variables

### Swedish data files

The variables we employ are constructed from data contained in the GeoSweden database. This database contains a large amount of information on all individuals that is assembled from a range of administrative statistical registers (income, education, labor market, and population). We merged selected information about individuals age 15 and above from annual *Louise/Lisa* files to create a longitudinal database 1991–2008<sup>13</sup> for all adults residing in Sweden's largest metropolitan area, Stockholm, in 1991.<sup>14</sup> Since we focus on labor income, we confine our analysis to these adults who remain in prime working ages throughout the balanced panel (i.e. ages 20–49 in 1991). Since we also wish to maintain a reasonably consistent notion of urban neighborhood, we further confine our analysis to those 1991 Stockholm residents who were residents of Stockholm or (after moving, resided in) another Swedish metropolitan area in each year from 1992 to 2008.<sup>15</sup> This restriction meant that we analyze about 70% of the 1991 Stockholm population ages 20–49 and more than half of the resident population in this range for any given year in our panel.

Characteristics of our sample are provided in the descriptive statistics of Table 1. Males and females in our sample are virtually identical in all characteristics, though females are more likely to take maternal leave or sick leave and have smaller income gains. Both genders during our panel are, on average, 41 years old and have one child under age seven. Almost a quarter have 15 or more years of education. Half are in married or official couple relationships, and 2–3% change coupling status in any given year. Eleven percent of the females and 12% of the males are non-Western immigrants. During the period males evinced a mean annual increase in labor income of 13,426 kronor and 65,816 kronor over a five-year period; corresponding figures were 10,197 and 51,544 for females.

### Neighborhood variables

*Scale* In this study we operationalize the scale of neighborhood as a SAMS (Small Area Market Statistics) area, as defined by Statistics Sweden.



Table 1. Descriptive statistics for Stockholm males and females being analyzed.

	Males		Females	
	Mean	Std dev.	Mean	Std dev.
<i>Control variables</i>				
No. children under age 7	1.07	2.28	1.18	2.36
Marital status: coupled or married (1=yes)	0.50	0.50	0.50	0.50
Pre-retirement status (1=yes)	0.05	0.22	0.07	0.25
Parental leave during year (1=yes)	0.21	0.40	0.31	0.46
Sick leave during year (1=yes)	0.10	0.30	0.17	0.38
Student during year (1=yes)	0.04	0.19	0.07	0.25
12 years of education (LT 12 is omitted category)	0.16	0.37	0.16	0.36
13–14 years of education	0.18	0.38	0.21	0.41
15+ years of education	0.25	0.43	0.25	0.43
Changed from couple to single prior year (1=yes)	0.02	0.14	0.02	0.14
Changed from single to couple prior year (1=yes)	0.03	0.17	0.03	0.16
Non-Western immigrant	0.12	0.33	0.11	0.31
Immigrant no. years in Sweden (0 if Swedish)	4.75	10.58	4.94	10.78
Age	41.46	8.14	41.19	8.22
Age squared	1785.0	682.10	1764.55	686.20
<i>Neighborhood variables</i>				
% Low-income neighbors ave. last 4 years	31.42	8.86	31.16	8.57
% High-income neighbors ave. last 4 years	37.41	12.10	37.82	12.01
Cluster “high-income dominance”	0.12	0.33	0.13	0.33
Cluster “low-income dominance”	0.04	0.20	0.04	0.18
Cluster “middle-income and high-income dominance”	0.12	0.33	0.12	0.33
Cluster “low-income and high-income dominance”	0.05	0.22	0.06	0.23
Cluster “low-income and middle-income dominance”	0.18	0.38	0.17	0.37
Cluster “mixed-income”	0.49	0.50	0.49	0.50
<i>Dependent variables (in 100 SWE kronor)</i>				
1-year change in income from work	134.26	1667.27	101.96	789.88
5-year change in income from work	658.16	2759.44	515.44	1335.01

On average, the 184 Stockholm SAMS areas comprise 20 hectares and house 4,259 people. We adopt SAMS as our operational definition of neighborhood for three reasons. First, the SAMS classification scheme is designed to identify distinctive, physically homogeneous areas by taking into account housing type, tenure and construction period. Second, the SAMS scale in Stockholm closely matches that of census tracts, the geography upon which virtually all of the vast neighborhood effects research has been conducted in the US. Third, SAMS are typically employed by Swedish policymakers when formulating, implementing and evaluating area-based policies, so research done on the appropriate “policy scale” proves most relevant.

We recognize that SAMS are not the only potential ways of delineating neighborhoods (cf. Bolster *et al.* 2007; van Ham and Manley 2009; Andersson and Musterd 2010; Andersson and Malmberg 2015), and indeed they may represent too large an area to

correspond to what residents perceive as their neighborhood or socially interact within (Galster 2008). Thus, we would expect any measured effects at this SAMS scale of neighborhood to be underestimated, given that Buck (2001), Bolster *et al.* (2007), van Ham and Manley (2009), Andersson and Musterd (2010) and Andersson and Malmberg (2015) consistently found stronger neighborhood effects at smaller spatial scales.

*Income mix* We focus on the income mix of neighborhood as the  $[N_{ij}]$  variable of importance for five reasons. First, this is the aspect of neighborhood that has been the dominant focus of the international 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 US and Western Europe; see Murie and Musterd (2004), Berube (2005), Briggs (2005), Musterd and Andersson

(2005), and Norris (2006). 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.* 2007). Fourth, when multiple aspects of the residential environment are collapsed into a single index of neighborhood conditions (e.g. Bolster *et al.* 2007; Propper *et al.* 2007; van Ham and Manley 2009, 2010; Brännström and Rojas 2012) it is less clear which particular aspect of the context is generating the observed correlation with individual outcomes, and thus it provides less clear guidance to policy-makers. Fifth, in most nations espousing “social mix” principles it is legal to tailor public policies to have differential impacts on the basis of income, but illegal to do so on the basis of gender, ethnicity, religion or other potential dimensions of neighborhood diversity.

Having justified why we focus on the income composition of the neighborhood, we next consider how to operationalize “social mix” based on resident incomes. Several measures might be specified; in this article we consider two. The first involves continuous measures of the percentages of low-income and high-income male residents, a measure that has been employed often in the literature. The second is a new formulation of discrete neighborhood income mix types based on a cluster analysis. We emphasize that both measures have their limitations and therefore we essentially view our investigations with these alternatives as robustness checks. When discussing holistically our findings and their policy implications we will focus only upon results that are general across both specifications of neighborhood income mix.

*Continuous measure of neighborhood income composition* As one 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 reference category. For brevity we will refer to these groups as “lower-income”, “middle-income”, and “higher-income” neighbors.<sup>16</sup> In the database we have constructed we computed these neighborhood conditions annually based on the entire adult male population in Stockholm, even though our modeling utilizes a subset of these observations, as explained above.

Several observational studies have paid attention to how *variations* in the duration of exposure modified the observed relationship between neighborhood income composition and several individual outcomes related to labor market performance or human capital acquisition; see Aaronson (1998), Turley (2003), Wheaton and Clarke (2003), Kauppinen (2007), Clampet-Lundquist and Massey (2008), Sampson *et al.* (2008) and Musterd *et al.* (2012). Several others have focused on the cumulative effects on adults from sustained exposure to disadvantaged neighborhoods during childhood (Galster *et al.* 2007; Sundlöf 2008; Bergsten 2010; Crowder and South 2011; Wodtke *et al.* 2011; Hedman *et al.* 2012). These studies paint a consistent portrait that neighborhood effects seem to be stronger if the exposure is sustained, though the veracity of most (except Aaronson 1998; Galster *et al.* 2007; Musterd *et al.* 2012) can be challenged due to their failure to control for geographic selection bias. We therefore measure income composition as *four-year moving averages* experienced by the individual for each year.<sup>17</sup>

As seen in Table 1, working-age adults in Stockholm on average experience a mixed residential (SAMS) environment composed of 31% lower-income, 32% middle-income and 37% higher-income neighbors.<sup>18</sup> Of course these means might have been produced by roughly a third of the sample each experiencing homogeneously lower-, middle-, or higher-income neighborhoods. As documented further in the next section, such was not the case because virtually all Stockholm neighborhoods are “mixed” to some extent. Nevertheless, there was substantial variation in such exposures in our analysis sample. Males experienced a range of 5–93% in their four-year average percentages of lower-income neighbors and 0–84% in their four-year average percentages of higher-income neighbors. The corresponding figures for females were 5–92% and 0–79%.

*Cluster type measure of neighborhood income composition* The limitation with the aforementioned continuous specification of neighborhood income mix is that the coefficient for the percentage of lower-income neighbors variable can only be interpreted as the impact resulting from a one percentage-point larger value of this variable and a corresponding one percentage-point smaller value of the percentage middle-income neighbors variable, *holding the percentage of higher-income neighbors*

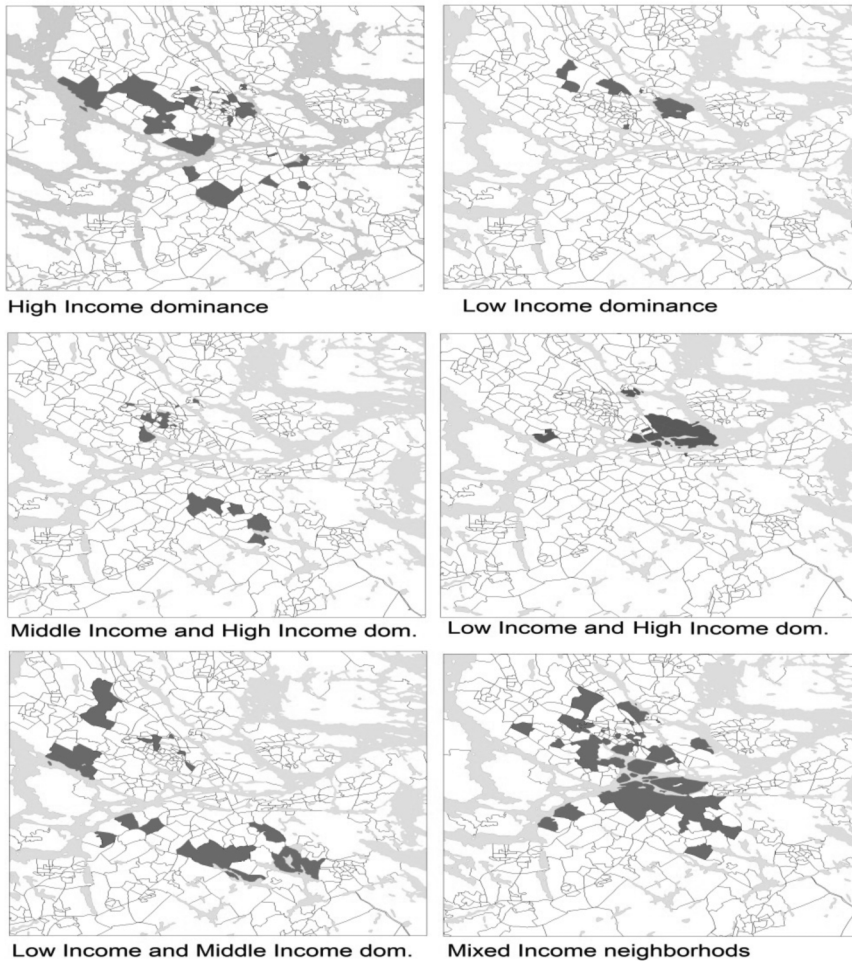


Figure 1. Location of neighborhood types of income mix in Stockholm, 2005.

*constant*. (An equivalent interpretation applies to the higher-income neighbor variable's coefficient.) This specification thus assumes that the marginal impact of trading off middle-income for lower-income neighbors is invariant to the percentage of higher-income neighbors (and for higher-income neighbors is invariant to the percentage of lower-income neighbors). Another way to view this limitation is that marginal increases in the percentage of lower-income neighbors do not consistently imply increases in overall neighborhood income mix; it will depend upon the original percentage upon which this change is measured. To avoid this limitation we specify an alternative way to operationalize neighborhood income mix that relies upon the

particular, "lumpy" variants in composition actually experienced by Stockholm households.

We employed a cluster analysis for delineating various types of urban neighborhoods, which has become standard practice.<sup>19</sup> Specifically, we applied K-means cluster analysis to identify relatively homogeneous groups of cases based on ranges of our aforementioned continuous income variables: the four-year averages of the percentages of lower-income, middle-income, and higher-income neighbors experienced by those in our analysis sample. We created six classes for each of these variables representing the 0–10, 10–25, 25–50, 50–75, 75–90 and 90–100 percentiles.<sup>20</sup> Permutations of these classes can be thought of as types of possible neighborhood

Table 2. Income composition of neighborhood types experienced in Stockholm.

Cluster name	Males				Females			
	Mean proportions within cluster				Mean proportions within cluster			
	% Lower	% Middle	% Higher	Total	% Lower	% Middle	% Higher	Total
"High-income dominance"	20.8	22.0	57.3	100.0	20.8	22.0	57.2	100.0
"Low-income dominance"	58.8	27.8	13.5	100.0	58.4	28.0	13.6	100.0
"Middle-income and high-income dominance"	23.0	32.0	45.0	100.0	23.1	32.0	44.9	100.0
"Low-income and high-income dominance"	32.8	21.5	45.8	100.0	32.7	21.5	45.8	100.0
"Low-income and middle-income dominance"	38.8	38.3	23.0	100.0	38.6	38.3	23.1	100.0
"Mixed-income"	31.2	32.1	36.7	100.0	31.2	32.0	36.8	100.0

income mixes experienced by those in our analysis sample. Eighty-five of these combinations contained observations in our dataset; we used these in the cluster analysis. We experimented with several numbers of clusters and finally constructed six to represent the alternative residential contexts experienced by those in our analysis sample.<sup>21</sup> In Table 2 we present these clusters, with their heuristic labels and average percentage of lower-, middle- and higher-income neighbors that males and females in our analysis sample experienced. Apart from a cluster with high-income dominance and a cluster with low-income dominance, we also distinguished three clusters where two categories were dominant and one "mixed" cluster with approximately equal representations of all three income categories. Not surprisingly, there are no gender differences in the mean exposures to these various residential contexts: see Table 2.

Figure 1 shows the location of these neighborhood types in Stockholm in 2005. It is immediately clear that the clusters occupy distinct ecological niches, with the low-income areas located in a band just north of the city center, low-income and high-income dominant areas just east of the city center, high-income dominant areas in the west and southwest, etc.

One vital fact emerges from our analysis of neighborhood income distributions experienced by Stockholm residents: by international standards there are very few, if any, that approach homogeneity; virtually all evince some non-trivial degree of mixing. From the information presented in Table 2 it becomes clear that all types of residential

experiences we have identified, including the clusters that are "dominated" by higher-income or lower-income neighbors, show a substantial mixture of the income categories. Unlike experiences in countries such as the US, the poorest neighborhoods experienced in Stockholm still include over 40% middle- and higher-income groups.<sup>22</sup> Thus, it is important to view our study as one that is probing the economic consequences of experiencing different degrees and patterns of essentially mixed residential environments, not the consequences of what in other national contexts might be concentrated disadvantage or advantage.

### *Control variables*

As for the control variables in our models, we included demographic and household characteristics at the beginning of each baseline year during which succeeding income changes were measured (number of children under age 7, age, age squared,<sup>23</sup> educational attainments, non-Western immigrants'<sup>24</sup> tenure in Sweden), and other life events during the period that likely will affect their income growth (such as parental leave, illness, attending school, or changing coupling/marital status). We also include a series of year dummy variables to control for the (unspecified) time-varying local labor market conditions experienced during the period of analysis. Gender and non-Western immigrant status are controlled via stratification. See Table 1 for a complete listing of these variables and their descriptive statistics.

Table 3. Estimated parameters for fixed effect, income change models using continuous measures of income mix, by gender.

	1-Year earnings change				5-Year Earnings Change			
	Males		Females		Males		Females	
	Coeff.	Std error	Coeff.	Std error	Coeff.	Std error	Coeff.	Std error
No. children under age 7	0.25	(1.09)	6.00***	(0.34)	-8.00*	(3.21)	22.1***	(1.02)
Marital status: coupled or married (1=yes)	22.4***	(5.04)	43.7***	(2.54)	23.1	(18.5)	264.8***	(9.03)
Pre-retirement status (1=yes)	33.0***	(4.60)	-25.4***	(3.27)	752.9***	(15.0)	641.3***	(10.4)
Parental leave during year (1=yes)	-62.0***	(5.70)	-30.1***	(2.06)	-34.7**	(11.4)	101.7***	(5.40)
Sick leave during year (1=yes)	-110.3***	(3.36)	-140.1***	(1.83)	-51.9***	(6.47)	-73.8***	(3.65)
Student during year (1=yes)	256.7***	(4.19)	293.0***	(2.63)	1001.0***	(13.4)	957.0***	(6.40)
12 years of education (LT 12 is omitted category)	116.8***	(8.68)	89.1***	(4.52)	251.8***	(36.5)	173.5***	(19.3)
13-14 years of education	272.6***	(8.88)	245.9***	(4.96)	686.6***	(37.4)	635.5***	(24.8)
15+ years of education	398.7***	(11.0)	372.6***	(5.50)	723.5***	(40.0)	499.0***	(32.5)
Changed from couple to single prior year (1=yes)	-3.34	(11.0)	29.5***	(4.78)	-20.1	(20.9)	113.3***	(9.11)
Changed from single to couple prior year (1=yes)	-37.4***	(10.1)	-158.3***	(5.07)	-12.1	(18.5)	-208.4***	(8.35)
Immigrant no. years in Sweden (0 if Swedish)	1.48**	(0.51)	0.99***	(0.28)	16.6***	(2.55)	-2.78*	(1.36)
Age	21.2***	(1.53)	20.7***	(0.81)	42.7***	(7.14)	78.0***	(3.59)
Age squared	-0.19***	(0.017)	-0.20***	(0.0090)	-0.75***	(0.088)	-0.97***	(0.043)
1995 (1994 is omitted category)	52.3***	(4.24)	23.7***	(2.27)	175.1***	(6.84)	61.7***	(3.34)
1996	46.9***	(4.46)	8.84***	(2.22)	214.3***	(8.01)	103.7***	(4.01)
1997	56.1***	(4.37)	4.10	(2.35)	84.4***	(9.07)	64.5***	(4.53)
1998	55.1***	(5.40)	22.7***	(3.07)	-106.0***	(9.59)	-4.14	(4.90)
1999	131.3***	(6.16)	36.1***	(3.03)	-207.8***	(11.1)	-66.5***	(5.05)
2000	61.7***	(6.92)	37.2***	(2.88)	-342.6***	(11.1)	-127.6***	(4.79)
2001	-120.6***	(7.37)	-37.6***	(2.99)	-338.8***	(10.9)	-153.3***	(4.69)
2002	-169.3***	(7.52)	-66.5***	(3.03)	-173.8***	(9.45)	-86.6***	(3.93)
2003	-96.0***	(6.57)	-58.8***	(3.14)	-75.4***	(9.87)	-45.7***	(3.53)
2004	-46.5***	(6.89)	-44.2***	(3.41)	N/A	N/A	N/A	N/A
2005	-3.91	(7.89)	-12.7***	(3.78)	N/A	N/A	N/A	N/A
2006	-2.33	(6.55)	-5.88	(4.34)	N/A	N/A	N/A	N/A
Mean % lower-income neighbors prior 4 years	-1.25**	(0.46)	-0.23	(0.24)	-7.45***	(2.11)	1.76	(1.04)
Mean % higher-income neighbors prior 4 years	-2.34***	(0.40)	-0.51**	(0.19)	-14.2***	(1.96)	1.10	(0.87)
Constant	-443.6***	(47.4)	-540.1***	(23.6)	539.9**	(209.1)	-1622.1***	(99.5)
Observations	1,513,741		1,552,819		1,085,260		1,113,742	
N_clusters	124,269		127,461		124,269		127,461	
rho	0.037		0.058		0.24		0.32	
r <sup>2</sup>	0.0034		0.015		0.015		0.056	
F	374.0		1119.2		517.1		1472.2	

Standard errors in parentheses; all models include fixed effects.

\* $p < 0.05$ , \*\* $p < 0.01$ , \*\*\* $p < 0.001$ .

## Findings

### Control variables

Results for the control variables for one-year income change models as produced by Stata's XTREG/FE procedure (stratified by gender) are presented in Table 3; as these do not differ appreciably

with variations in how neighborhood income mix is measured, we present them only once to conserve space.<sup>25</sup> The control variables of time-varying individual characteristics performed as expected and in the same qualitative fashion for both males and females, with a few exceptions. Annual income gain



Table 4. Estimated coefficients for neighborhood type measures of income mix in fixed effect, income change models; by gender.

	One-year income change model					Five-year income change model				
	Neighborhood cluster type dominant group					Neighborhood cluster type dominant group				
	Low	Middle –high	Low –high	Low –middle	Mixed	Low	Middle –high	Low –high	Low –middle	Mixed
Males	47.1***	25.5***	–11.5	33.6***	34.1***	298.2***	205.5***	86.9	215.4***	230.8***
Females	6.26	–7.48*	–25.9***	–5.88	–11.8***	–6.28	–61.4***	–117.7***	–69.9***	–88.1***

Note: all models include fixed effects and controls as shown in Table 3; excluded neighborhood type is high-income dominant.

\* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$ .

was greater for those who had progressively better educational credentials, remained married or partnered in their civil union status, did not get married in the prior year, were students, and were not on sick or parental leave. The quadratic results for age indicated that increasing labor market experience raised annual income gain until approximately age 50. Immigrants gained a small amount from more years in Sweden. Females (but not males) evinced greater one-year income gains if they had an increase in the number of children under age seven or if they changed civil union status from couple to single. Females who were nearing retirement evinced lower income gains but males evinced the opposite. Years 1995 through 2000 represented a period of strong Stockholm labor markets that conveyed income gains to individuals, presumably through expanding labor demands. All the subsequent results regarding neighborhood income mix variables should be interpreted in the context of models containing these control variables (as well as person-specific fixed effects), though for brevity these are not reported.

#### *Continuous measure of neighborhood income mix*

Of more relevance to our enquiry are the results for the neighborhood income mix variables. The bottom section of Table 3 presents these results for the continuous measures, which indicate a statistically and economically significant negative effect of both percentages of low- and high-income neighbors on males' one- and five-year income trajectories. A male experiencing a one standard deviation higher percentage of lower-income neighbors (and correspondingly lower percentage of middle-income neighbors) over the prior four years would be predicted by our model to evince a 9% smaller change in both one- and five-year income compared

with an otherwise identical male. A male experiencing a one-standard deviation-higher percentage of higher-income neighbors (and correspondingly lower percentage of middle-income neighbors) over the prior four years would be predicted by our model to evince a 24% smaller change in both one- and five-year income compared with an otherwise identical male. The relationships for females were weak to nonexistent. Prior studies using our measure of neighborhood income mix have found similar results, even though they employed the *level* (not the change) of income as the dependent variable, and different econometric techniques (cf. Galster *et al.* 2008, 2010; Galster and Hedman 2013; Hedman and Galster 2013). Given the apparent robustness of this result across many specifications, we think it appropriate to conclude that there is a nontrivial effect on Stockholm male income from neighborhood income composition measured in this continuous way. However, as we will amplify below, these effects are heterogeneous across income and ethnic groups, so we shall postpone interpretation of these results until they are presented in their entirety.

#### *Cluster type measure of neighborhood income mix*

Coefficients estimated for the various types of neighborhoods are presented in Table 4; for brevity standard errors and results for control variables are omitted. This specification reinforces the conclusions above but adds nuance. Males benefit from higher annual and five-year income gains if they reside in any type of neighborhood (surprisingly, even low-income dominant ones) compared with the high-income dominant ones. The increments in male annual income range across the neighborhood types from 19% to 35% of the mean annual gain; the corresponding range for five-year increase is even

larger: 31–45%. The pattern is quite different for females, whose income trajectories suffer in several of the more mixed-income contexts compared with the high-income dominant context. As before, the magnitudes of impact for females are smaller: decrements in annual income ranging across the types from 7% to 26% of the mean and corresponding figures for five-year gains ranging from 12% to 23%.

#### *Heterogeneity of effects by income and ethnicity*

The aggregate story for males and females gets considerably more complicated when we stratify by income group (using the same 30th and 70th percentile breaks to create three classes as we did in defining the continuous neighborhood income mix) and by Swedish/non-Western immigrant status. What immediately becomes apparent in examining Table 5 is that the aggregate results portrayed in Table 3 for the continuously measured neighborhood mix do not appertain generally across the various groups.

Consider first the results for males. The apparent aggregate male benefit from residing among more middle-income neighbors is revealed in Table 5 to be primarily produced by higher-income Swedes and is completely absent for non-Western immigrants. A Swedish higher-income male experiencing a one standard deviation higher percentage of lower-income neighbors (and correspondingly lower percentage of middle-income neighbors) over the prior four years would be predicted by our model to evince a 26–27% smaller change in both one- and five-year income compared with an otherwise identical male. A Swedish higher-income male experiencing a one standard deviation higher percentage of higher-income neighbors (and correspondingly lower percentage of middle-income neighbors) over the prior four years would be predicted by our model to evince a 21% smaller change in annual income and a 37% smaller change in five-year income compared with an otherwise identical male.

A new result for males also emerges for lower-income immigrants, who gain somewhat over a five-year horizon from a higher share of higher-income neighbors. A non-Western immigrant male experiencing a one standard deviation higher percentage of higher-income neighbors (and correspondingly lower percentage of middle-income neighbors) over the prior four years would be predicted by our model to evince a 9% larger change in five-year income compared with an otherwise identical male immigrant.

As for females, the aggregate relationships using the continuous measures of neighborhood income mix were weak to nonexistent, but Table 5 reveals some interesting disaggregated patterns. Middle-income Swedish females benefit modestly from more higher-income neighbors: 10% larger five-year income gain for a one standard deviation increase. Unexpectedly, lower- and middle-income immigrant females apparently benefit over the five-year span from *both* more lower-income and (especially) higher-income neighbors (compared with middle-income ones). A lower-income immigrant female experiencing a one-standard deviation-higher percentage of lower-income neighbors (and correspondingly lower percentage of middle-income neighbors) over the prior four years would be predicted by our model to evince a 12% larger change in five-year income, *ceteris paribus*, the corresponding increment for a standard deviation increase in higher-income neighbors is 18%. These neighborhood impacts are considerably stronger for middle-income immigrant females: their corresponding gains in five-year income are 34% and 60% at the mean for standard deviation increases in lower- and higher-income neighbors, respectively.

When the cluster-analysis-based neighborhood income mix types are employed as predictors, yet a different picture of heterogeneity arises. Compared with results using the continuous measures of neighborhood income mix, the models using types reveal many more statistically significant differences among Swedish groups but fewer statistically significant differences among non-Western immigrant strata. See Table 6.

Again considering males first, the prior conclusions that Swedish higher-income males benefit from more middle-income neighbors is supported by the five-year change results for neighborhood type. The prior conclusion about immigrant males benefitting from more higher-income neighbors was not replicated, however. Indeed, none of the neighborhood types proved statistically significant predictors in any models involving male immigrants. Nevertheless, several new relationships emerged for Swedish males. Compared with residing in high-income-dominant contexts, lower-income Swedish males gained more income in both the short and longer run if they resided in the more mixed or even lower-income-dominant neighborhood types. Just the opposite proved the case for middle-income Swedish males, however, for five-year income changes.

Table 5. Estimated coefficients for continuous neighborhood measures of income mix in fixed effect, income change models; by gender, income group, ethnicity

		Swedish ethnicity					
		Mean income change (100s kronor)		One-year income change model by neighborhood income group %		Five-year income change model by neighborhood income group %	
	No. obs.	One year	Five years	% Lower income	% Higher income	% Lower income	% Higher income
<i>Males</i>							
Lower income	47,739	275.1	909.1	1.18	0.58	-1.15	-1.86
Middle income	62,754	73.8	435.2	0.61	0.10	1.18	1.68
Higher income	71,694	129.2	785.7	-3.85*	-2.27*	-22.9***	-24.0***
<i>Females</i>							
Lower income	62,147	230.2	766.9	0.40	-0.66	1.06	1.55
Middle income	82,790	59.0	389.0	-0.02	0.79*	1.12	3.36***
Higher income	51,263	56.3	560.0	-0.53	0.57	-7.51	-1.49
		Non-Western immigrant ethnicity					
		Mean income change (100s kronor)		One-year income change model by neighborhood income group %		Five-year income change model by neighborhood income group %	
	No. obs.	One year	Five years	% Lower income	% Higher income	% Lower income	% Higher income
<i>Males</i>							
Lower income	12,234	159.6	603.9	-0.55	-0.78	3.77	4.29*
Middle income	10,447	-4.2	163.4	-0.50	0.28	2.29	3.67
Higher income	6238	1.5	335.0	-1.06	-2.91	9.81	-2.31
<i>Females</i>							
Lower income	12,007	129.8	511.6	0.09	0.34	6.61***	7.76***
Middle income	9983	3.5	174.5	0.53	0.77	6.56*	8.73***
Higher income	3706	13.2	425.1	-3.35	-3.05	11.6	9.19

Note: all models include fixed effects and controls as shown in Table 3.

\* $p < 0.05$ ; \*\* $p < 0.01$ ; \*\*\* $p < 0.001$ .

In the case of Swedish females, the earlier finding that middle-income females benefit in both the short and longer run from higher-income neighbors was replicated. Several new distinctions emerged when using the neighborhood types, however. Lower-income Swedish females gained higher annual income increases if they were not in higher-income-dominated areas (just as in the case of lower-income Swedish males). On the contrary, middle-income Swedish females gained higher annual and five-year income increases if they were in higher-income-dominated areas (just as in the case of middle-income Swedish males). Higher-income

Swedish females also gained higher annual and five-year income increases if they were in areas dominated by higher-income neighbors (which is opposite the case for higher-income Swedish males).

Finally, the prior finding that lower-income, immigrant females gained in five-year changes by having either more lower-income or higher-income neighbors was not replicated when using neighborhood income mix types. The analogous prior finding for middle income, immigrant females was only partially replicated. The results in Table 6 indicate that they do substantially better in five-year income trajectory in higher-income-dominant neighborhoods

Table 6. Estimated coefficients for neighborhood type measures of income mix in fixed effect, income change models; by gender, income group, and ethnicity.

Swedish ethnicity										
	One-year income change models by neighborhood income mix type					Five-year income change models by neighborhood income mix type				
	Low	Middle –high	Low –high	Low –middle	Mixed	Low	Middle –high	Low –high	Low –middle	Mixed
<i>Males</i>										
Lower income	57.4**	27.4	38.4*	36.6*	45.1**	104.2*	104.9**	107.2	86.9	101.8*
Middle income	6.77	–3.62	1.76	–6.71	–9.89	–6.12	–36.2*	–36.2	–57.8**	–54.4**
Higher income	1.54	11.4	–94.2**	–15.4	0.95	89.2	219.7***	–108.7	157.7*	215.9***
<i>Females</i>										
Lower income	58.2***	33.8***	35.4***	42.8***	43.4***	33.5	–7.83	–12.5	–12.2	–10.9
Middle income	–38.6***	–26.3***	–40.4***	–34.1***	–34.4***	–82.6**	–71.4***	–111.0***	–87.4***	–95.6***
Higher income	–44.0	–42.2***	–74.2***	–37.7	–52.0***	–206.7**	–131.6***	–291.7***	–126.5**	–169.1***
Non-Western immigrant ethnicity										
	One-year income change models by neighborhood income mix type					Five-year income change models by neighborhood income mix type				
	Low	Middle –high	Low– high	Low– middle	Mixed	Low	Middle –high	Low –high	Low –middle	Mixed
<i>Males</i>										
Lower income	21.7	28.3	2.01	33.5	30.5	19.0	16.5	175.4	12.0	–1.31
Middle income	–35.0	–37.4	33.8	–25.7	–14.5	–103.4	–90.6	–13.8	94.3	–96.4
Higher income	89.5	–16.7	–36.7	83.8	8.56	354.5	14.5	–63.8	88.7	108.4
<i>Females</i>										
Lower income	–8.0	–14.3	8.76	–5.16	5.87	–8.96	–13.8	–13.2	–27.6	8.26
Middle income	–17.1	–35.4	–31.8	–18.1	–13.5	–179.5***	–134.0**	–142.5*	–213.5***	–166.6***
Higher income	5.21	–42.3	–58.8	–21.8	–24.7	–44.3	–187.1	–140.7	–252.3	–247.4*

Note: all models include fixed effects and controls as shown in Table 3; omitted neighborhood type is “higher-income dominant”.

\* $p < 0.05$ , \*\* $p < 0.01$ , \*\*\* $p < 0.001$ .

than either variously mixed ones or in the low-income dominant ones; the superiority of these higher-income contexts is roughly the same magnitude as the mean change for this group (cf. Table 1).

## Discussion

We note at the outset that we will focus our discussion upon results that are general across both specifications of neighborhood income mix. The preeminent finding of this article is the heterogeneity of neighborhood income mix effects on residents' income trajectories. For some groups these effects can be unquestionably large yet opposite in direction. One of the prime distinctions is based on gender, where females generally do better economically in neighborhoods with higher shares of high-income neighbors whereas males do better in those with higher shares of middle-income neighbors. If, for example, middle-income non-Western immigrant females were to reside in high-income-dominant Stockholm neighborhoods they could expect a much larger five-year income gain (roughly double the mean gain for this group overall) than if they were to reside in other environments. Substantial gains would also occur for middle- and higher-income Swedish females in such circumstances. On the other hand, if higher-income Swedish males were to reside in high-income-dominant Stockholm neighborhoods they could expect a much-attenuated five-year income change (by over one-quarter) than if they were to reside in more mixed environments.

National origin also proved another important source of heterogeneity of impact. Non-Western immigrant females exhibited much stronger effects from their neighborhoods on their five-year income trajectories than Swedish females, both in absolute and percentage terms. By contrast, our measures of neighborhood income mix demonstrated no robust predictive power when it came to income trajectories of non-Western immigrant males of any income, whereas for Swedish males numerous large impacts were manifest.

Perhaps most surprising was the lack of significant relationships for lower-income residents' income trajectories and the lower-income composition of their neighborhoods. Contrary to conventional wisdom, we could not identify any support for the notion that lower-income residents imposed negative economic externalities on other low-income neighbors, regardless of gender or ethnicity. We caution that this result may not be general beyond

Sweden, given that nowhere are there extreme concentrations of disadvantage, those who earn little nevertheless are entitled to a generous package of income, health, education and housing benefits, there are minimal fiscal capacity disparities among local jurisdictions, and national spatial policies have tried to focus compensatory public services and institutional supports on less advantaged places. Moreover, we hasten to add that our study does not address the issue of the self-assessed social and psychological integration into Sweden of lower-income people in general, and immigrant male youth in particular (Kelly 2013). As such, it should not be interpreted as implicit commentary on the recent civil disturbances in some Stockholm neighborhoods.

## *Comparison to other studies of economic impacts of neighborhood*

Our heterogeneous findings stand in clear contrast to those of other scholars who did not identify statistically or economically significant neighborhood effects on income or employment outcomes (cf. Bolster *et al.* 2007; Propper *et al.* 2007; van Ham and Manley 2010). One obvious potential reason for the disparity in results is that Bolster *et al.* (2007) allow for heterogeneity by gender and (via quantile regression) income group, van Ham and Manley (2010) allow for heterogeneity by tenure, and we allow for heterogeneity by gender, income, and ethnicity. Perhaps the finer-grained heterogeneity we discovered is blurred at a coarser level of aggregation, although Propper *et al.* (2007) observed no differences among social renters in the relationship between residence in the most disadvantaged neighborhood quartile and the individual's income level 10 years hence across age, gender, education, and ethnic strata. Another potential reason is that we measure neighborhood income mix (in two ways), whereas the others use a composite neighborhood disadvantage index and/or percentage of social rented housing. It may be that income mix serves as a superior proxy for the sorts of causal processes creating neighborhood effects than the other measures, as suggested by Andersson *et al.* (2007). Moreover, we use a four-year average neighborhood exposure measure, instead of the one-year measurements employed by Bolster *et al.* (2007), Propper *et al.* (2007), and van Ham and Manley (2010). If neighborhood environments require a modicum of sustained exposure before registering impacts (Musterd *et al.* 2012) our longer-term measure



may prove more efficacious. Finally, we employ an econometric method that arguably does a superior job of insulating results from geographic selection bias, whose impact may have been to minimize the apparent neighborhood effect (Brooks-Gunn *et al.* 1997).

#### *Heterogeneity of neighborhood causal processes and residents' responses*

Our heterogeneous results likely emerged because the variety of potential neighborhood causal processes at play (described earlier) generate different responses for different kinds of residents (Burdick-Will *et al.* 2011; Galster *et al.* 2010; Bergsten 2010; Clampt-Lundquist *et al.* 2011; Ludwig 2012; Musterd *et al.* 2012; Andersson and Malmberg 2015; Sharkey and Faber 2014). We cannot, of course, offer any firm claims about causal processes based on our statistical relationships, but we offer some speculations that would be consistent with our observed heterogeneity.

Among Swedes, higher-income males benefit economically from residing with relatively more middle-income and fewer higher-income neighbors, although middle-income males evince just the opposite relationship. Likewise, both middle- and higher-income females benefit from residing with relatively more middle-income and fewer higher-income neighbors. These results are consistent holistically with local social interaction propensities and network patterns that are strongly differentiated by class and gender. Higher-income Swedish males may spend relatively little time in their neighborhoods, and when they do, they would be less likely to engage in less-than-superficial neighboring compared with middle-income males or females of any income. These higher-income males may thus, ironically, become more socially isolated when residing in higher-income-dominant neighborhoods, to their unwitting economic detriment.<sup>26</sup> Swedish women and middle-class males, by comparison being more prone to interact and build more meaningful networks with neighbors, will gain economically as these networks bridge more higher-income members who presumably provide more resources of various sorts, including information about income-enhancing prospects and techniques.

Non-Western immigrant females consistently demonstrated that they had superior five-year income trajectories if they resided among large numbers of higher-income neighbors; this was most

dramatically shown in the case of middle-income immigrant females. In her study of an immigrant-dense neighborhood in The Hague, Pinkster (2008) showed how local social control could limit females' ability and willingness to look for employment opportunities outside of the neighborhood, especially in areas where more traditional, patriarchal norms dominate (as they likely do in similar Stockholm neighborhoods). This implies that immigrant females' economic success will be more sensitive to the role modeling, peer effects, information, and opportunities afforded by higher-income neighbors, perhaps because they are more tolerant of female labor force participation and more open to females being part of their networks.

Again, we stress that we can only offer conjectures about the reasons for the different apparent impacts that neighborhood social mix has on different types of residents. We thus urge additional research involving mixed methods in an effort to elucidate the underlying heterogeneity of these response processes.

#### *Implications for social mix policy*

Recent research indicates increasing levels of socioeconomic segregation in most European capital cities, and Stockholm is no exception (Andersson and Kährik 2015). It is highly likely that this development will spur intensified interest in how to reverse the trend and especially how to avoid negative neighborhood effects, in particular for those having less resources. By focusing on our results for lower-income groups we can draw several relevant implications for urban policies of encouraging a mixture of income groups in neighborhoods, particularly in Sweden. In overview, variations in the mix of lower-income and middle-income neighbors appears to be of little importance to lower-income individuals' income trajectories (regardless of gender or ethnicity), but variations in the mix of lower-income and *higher-income* neighbors may be. Moreover, it appears that middle-income females are economically harmed by exposure to lower-income-dominant contexts. Thus there may be justification on grounds of overall societal efficiency for a particular sort of social mix strategy.

First, it is clear from Tables 5 and 6 that there is not one statistically significant result indicating either that a higher percentage of lower-income neighbors (and corresponding reduction in middle-income ones) or residence in a low-income-dominant

neighborhood type (compared with any other type) has harmful effects on the annual or five-year trajectories of low-income individuals. To test the robustness of this conclusion we defined a new, extreme type of four-year neighborhood experience – an average of more than 50% lower-income neighbors – and re-estimated the models. The results reinforced the prior conclusion. This clearly runs counter to a large body of US-based neighborhood effects research noted previously, but is understandable in light of longstanding Swedish government actions that not only avoid extreme concentrations of lower-income individuals but also target a variety of employment-enhancing services and institutions in areas where concentrations are the highest.

This is not to say that neighborhood income mix is irrelevant to lower-income individuals, however; rather that the neighborhood composition of higher-income individuals may be more telling. Swedish males and females evince attenuated income gains when they reside in higher-income-dominant environments compared with any other type, which might be explained by the networking barriers of social distance that inhibit the transmission of valuable economic information from higher- to lower-income neighbors when the former predominate to the extent that they can isolate themselves from their less-fortunate neighbors. A different pattern emerges for lower-income immigrants of both genders, who gain from having higher percentages of higher-income (generally starting from a low base percentage) instead of middle-income neighbors, perhaps reflective of a more vibrant ethnic enclave with its enhanced attendant economic prospects (Andersson *et al.* 2014). We caution about drawing any firm policy directions for the results for immigrants, however, because we have not measured characteristics of the neighborhood environment that may pertain directly to their economic performance, such as the human capital and employment possessed by their co-ethnic neighbors (Andersson *et al.* 2014).

A final policy-relevant consideration here is the degree to which middle- and higher-income individuals might be economically harmed by a social mix policy that increased their residential exposure to lower-income neighbors. Such a consideration is vital for ascertaining the degree to which a social mix policy might not only improve the wellbeing of the less advantaged but increase the net wellbeing of the society overall after those whose wellbeing has declined are taken into consideration (Galster and Zobel 1998; Galster 2002, 2007a, 2007b, 2013).

Table 5 shows no evidence that the income trajectories of either middle- or higher-income individuals would be degraded by the marginal replacement of their own group by lower-income neighbors. The only hint of possible harm comes from Table 6, which shows that middle- and higher-income Swedish females and middle-income immigrant females perform worse in neighborhood types with dominant shares of lower-income residents, instead of dominant shares of middle and/or higher-income residents.

The foregoing provides support for a particular sort of social mix policy that attempts to reduce the incidence of lower-income-dominant neighborhood environments and replace them with more mixed or middle-income-dominant neighborhood environments, but for an unconventional reason. Based on our results, avoiding concentrations of disadvantage in Stockholm can be justified because it reduces exposure to a context in which some citizens (particularly middle-income females) are harmed, not solely because it will improve the income prospects of lower-income citizens. The alternative to such lower-income-dominant neighborhoods in this strategy should be mixed and lower-/middle-income-dominant neighborhoods, as these are clearly superior from the perspective of several lower-income groups than the high-income and high-/low-income-dominant neighborhood alternatives. Such a strategy could indeed be justified on grounds of aggregate economic efficiency since it would likely produce Pareto improvements, if our results from Stockholm can be generalized. In sum, our results reinforce recent arguments for a more nuanced social mix policy (e.g. Galster 2013; Galster and Friedrichs 2015) that, as per conventional wisdom, indeed tries to reduce the incidence of lower-income dominant neighborhoods and (less conventionally) replaces them with *particular types* of “mixed” alternatives. Moreover, policymakers must recognize that (1) low-income individuals will not likely reap homogeneous benefits of such a nuanced social mix policy; and (2) some middle-income individuals will likely reap some benefits as well, compared with the *status quo*.

## Conclusion

In this article we have tried to contribute to policy debates over the consequences of neighborhood social mix and scholarly debates over the economic importance of neighborhood effects. To do so we have

advanced an original econometric specification integrating a change model and a fixed-effect panel model in a way that minimizes potential biases from both time-varying and invariant unobservables and thus permits causal inferences with enhanced confidence. We have found that the neighborhood income mix experienced by working-age adults in Stockholm over the past four years affects subsequent income trajectories of residents in highly heterogeneous ways according to gender, income and ethnicity, and for some groups this effect can be substantial. Variations in the mix of lower-income and middle-income neighbors is of little importance to lower-income individuals' income trajectories (regardless of gender or ethnicity), but variations in the mix of lower-income and higher-income neighbors appear to be. Moreover, it appears that middle-income females are harmed by exposure to lower-income-dominant contexts. Thus, we find justification on Pareto improvement grounds for a social mix strategy that replaces lower-income-dominant contexts with more mixed or middle-income-dominant ones. Our results also hold a strong implication for the scholarly literature attempting to quantify neighborhood effects: specifications that permit heterogeneous impacts are a must.

We note in closing several limitations to our work that implicitly serve as guides for further research. First, we recognize that the way we have operationalized neighborhood context could be modified. Different spatial scales should be tested and other aspects of the neighborhood environment (such as employment rates, educational composition, own-ethnic group capital, etc.) explored. We recognize that our investigation has been limited to four-year maximum durations of exposure; this was a strategic choice on our part to preserve temporal degrees of freedom in our panel. Certainly, further investigations probing consequences of even longer-term exposures to disadvantaged Stockholm contexts would be appropriate. We have only touched here on the implications of neighborhood social mix for working-age adults; valuable payoffs from our modeling approach are also likely when applied to the impacts on children and adolescents (cf. Sundlöf 2008; Bergsten 2010; Andersson and Malmberg 2015). We emphasize that speculations we have offered about underlying causal mechanisms should be treated as highly tentative pending many more qualitative investigations into a variety of Swedish neighborhood contexts. Nevertheless, regardless of which mechanisms are producing the statistical

results, our findings support the notion forwarded by Wilson (1987) decades ago that there is a compelling public policy interest in focusing on areas of concentrated lower-income residence, even in a social welfare state like Sweden.

## Acknowledgements

A special debt of gratitude is owed to Eva Andersson, Zara Bergsten, Bo Malmberg and Fenne Pinkster, and three anonymous referees, whose comments on a preliminary draft of this article proved immensely helpful. The research assistance of Rob Mehregan is gratefully acknowledged. This work was supported by the University of Amsterdam and Uppsala University – Institute for Housing and Urban Research (IBF) and by the Swedish Research Council (grant VR 2008-1549 to RA), neither of whom had any influence on the methods, results or interpretations presented here.

*George Galster*  
Hilberry Professor  
Department of Urban Studies and Planning  
Wayne State University  
Detroit, MI 48202  
USA  
Email: george.galster@wayne.edu

*Roger Andersson*  
Professor  
Institute for Housing and Urban Research (IBF)  
Uppsala University  
PO Box 514  
SE-752 20 Uppsala  
Sweden  
Email: roger.andersson@ibf.uu.se

*Sako Musterd*  
Professor  
Department of Geography, Planning and  
International Development Studies  
University of Amsterdam  
PO Box 15629  
1001 NC Amsterdam  
The Netherlands  
Email: s.musterd@uva.nl

## Notes

1. Many other studies have investigated this issue but are not noted here because they do not attempt to overcome geographic selection bias.

2. Andersson *et al.* (2007) show with the same dataset that different neighborhood characteristics have vastly different correlations with individuals' incomes.
3. Difference models reduce statistical power by shrinking variation in the outcome variable and assume that change relationships are independent of starting conditions. Fixed-effect models assume that the individual dummies adequately capture the bundle of unobservables for all times during the panel and that the effect of this bundle remains constant during the panel. Instrumental variables must be both valid and strong. Micro-scale investigations are limited to neighborhood effect mechanisms that operate only at the small geographic scales and assume there is no residential sorting on unobservables at that scale.
4. Note that, unlike Galster *et al.* (2008, 2010) and Galster and Hedman (2013), Bolster *et al.* (2007), Propper *et al.* (2007) and van Ham and Manley (2009, 2010) do not specify pure "difference" models because they use initial levels of (not changes in) neighborhood and individual characteristics to predict the change in individual outcomes. This could provide yet another methodological reason for divergent results.
5. Nonexperimental analysis focusing on MTO families who resided for a majority of the study period in low-poverty and/or higher-education neighborhoods revealed their substantially better adult employment and earnings than in the control group (Turner *et al.* 2012).
6. Formally, income from work is computed here as the sum of cash salary payments, income from active businesses, and the value of 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).
7. This notation is required because in our dataset neighborhood and personal characteristics are measured every December and we use these to predict income earned during the following year.
8. Time zero begins for our income data in 1994.
9. We recognize that this is an imperfect control, insofar as the fixed effect represents some average of the unobserved personal changes occurring over the entire analysis period.
10. Galster and Hedman (2013) evaluate alternative econometric models for estimating neighborhood effects, including change models and fixed-effect models. They do not consider an integrated version of these two approaches as we have developed here.
11. We thought our panel too short to estimate reliably a 10-year change fixed-effects model.
12. We could not estimate the annual change model using 2007–8 because we lacked some individual characteristics for 2006.
13. Our data on neighborhood and personal characteristics cease as of 2006, but we have earnings data for 2007 and 2008 that we employ as end points for the dependent variable in the one- and five-year change models, respectively.
14. We define the core metropolitan area as the municipalities of Stockholm, Solna and Sundbyberg.
15. This restriction also means that we do not analyze international immigrants in Stockholm or any residents who emigrated from Sweden after 1991. Seventy-eight percent of our original 1991 sample of working age adults from the core municipalities of Stockholm remained in this area by 2006, 21% remained in the rest of the Stockholm metropolitan region, and less than 2% moved to other Swedish metropolitan regions. (We did not wish to exclude those in this last group for fear of introducing any sample selection biases.) Thus, we can accurately describe our analysis as being based on experiences of Stockholm-area neighborhoods.
16. We recognize that the choice of male income distribution as the means of defining neighborhood income mix is arbitrary. We believe this choice is the best feasible option, however. Although there are virtually no gender differentials in labor force participation rates in Stockholm, there are substantial differences in income, due primarily to variations in annual hours worked. Since our aim here is to measure a dimension of neighborhood social status, male income distribution is a superior measure to female income distribution. In principle, household income distribution would have been a preferable measure, but this proved impractical to compute, given the complexities of tracking coupling and decoupling both over time and space and the fact that cohabitation in multi-family housing is not known unless the couple had children together. To the extent that certain neighborhoods are dominated by female-headed households and/or that the relationship between gendered income distributions is unusual in particular neighborhoods, our specification of neighborhood income mix will introduce measurement errors that will bias our findings toward finding no statistically significant neighborhood effects.
17. We recognize that perhaps an even longer record of exposures might prove influential, but we were forced to balance concerns over adequate numbers of repeated observations in our panel. Given 18 years of data and five-year income changes, we clearly must measure neighborhood context for a shorter duration than 13 years to get any temporal variation in this context for any given individual and thereby estimate a fixed effect. By using a four-year measurement window for constructing (moving) averages of neighborhood income mix this gives us nine discrete observations of temporal variation for each individual, which we think is appropriate to get a robust estimate of our fixed effects, which recall we use to control for time-varying unobserved individual characteristics that might affect both neighborhood selection and income growth.
18. The slight over (under) representation of the higher (lower) group reflects the fact that the groups were specified based on the national Swedish male income distribution, and Stockholm is richer than the average locale.
19. For recent illustrations of cluster analysis applied to identifying neighborhood typologies, see the 2011 Special Issue of *Urban Geography* (32.3) and Williams *et al.* (2014).
20. We chose to divide the upper and lower quartiles into the extreme deciles given the past attention given by the literature to potential threshold effects associated with extremes of neighborhood composition.
21. Sundlöf (2008) also performs a cluster analysis to find neighborhoods of similar socioeconomic status composition in Sweden and finds five groups, thus giving some independent confirmation that our numbers of clusters is reasonable.
22. For a comparison with US neighborhood income distributions, see Booza *et al.* (2006) and Galster and Booza (2007).
23. We also experimented with a spline specification of age to test if results were sensitive to the age quadratic specification. We found no substantive difference on measured neighborhood effects using these alternatives.
24. Non-Western immigrants consist overwhelmingly of those who entered as refugees and thus might be expected to face the most challenges working in Sweden. They exclude those immigrating from Western Europe, North America and Australia/New Zealand.
25. We report only the results for the model using the continuous measures if neighborhood income mix.
26. We admit that this finding may also be the result of time-varying unobservables we did not fully control in our model. For instance, suppose that high-income males for status reasons strive

to establish themselves in high-income-dominated neighborhoods. If they are satisfied when they reach this goal they might prioritize other things besides increasing income, which would result in slower income increases observed in such places compared with more middle-income-dominant places where “strivers” were more predominant.

## References

- AARONSON, D. (1998): ‘Using sibling data to estimate the impact of neighborhoods on children’s educational outcomes’, *Journal of Human Resources* 33 (4): 915–946.
- ANDERSON, E. (1990): *Streetwise: Race, Class and Change in an Urban Community*. University of Chicago Press, Chicago, IL.
- ANDERSON, E. (1991): ‘Neighborhood effects on teenage pregnancy’, in JENCKS, C. and PETERSON, P. (eds): *The Urban Underclass*. Brookings Institution, Washington, DC, pp. 375–398.
- ANDERSSON, E. (2004): ‘From the valley of sadness to the hill of happiness: the significance of surroundings for socioeconomic career’, *Urban Studies* 41 (3): 641–659.
- ANDERSSON, E.K. and MALMBERG, B. (2015): ‘Contextual effects on educational attainment in individualised, scalable neighbourhoods: Differences across gender and social class’, *Urban Studies* 52 (12): 2117–2133.
- ANDERSSON, R. (2008): ‘Neighbourhood effects and the welfare state: towards a European research agenda?’, *Journal of Applied Social Science Studies [Schmollers Jahrbuch/ Zeitschrift für Wirtschafts- und Sozialwissenschaften]* 128: 1–14.
- ANDERSSON, R., BRÅMÅ, Å. and HOLMQVIST, E. (2010): ‘Counteracting segregation: Swedish policies and experiences’, *Housing Studies* 25 (2): 237–256.
- ANDERSSON, R. and KÄHRİK, A. (2015): ‘Widening gaps: segregation dynamics during two decades of economic and institutional change in Stockholm’, in: TAMMARU, T., MARCINCA, S., VAN HAM, M. and MUSTERD, S. (eds.): *Socio-Economic Segregation in European Capital Cities: East meets West*. Routledge, London and New York, pp. 110–131.
- ANDERSSON, R. and MUSTERD, S. (2010): ‘What scale matters? Exploring the relationships between individuals’ social position, neighbourhood context and the scale of neighbourhood’, *Geografiska Annaler B, Human Geography* 92 (1): 1–21.
- ANDERSSON, R., MUSTERD, S. and GALSTER, G. (2014): ‘Neighborhood ethnic composition and employment effects on immigrant incomes’, *Journal of Ethnic and Migration Studies* 40 (5): 710–736.
- ANDERSSON, R., MUSTERD, S., GALSTER, G. and KAUPPINEN, T. (2007): ‘What mix matters? Exploring the relationships between individuals’ income and different measures of their neighbourhood context’, *Housing Studies* 22 (5): 637–660.
- ÅSLUND, O. and FREDRICKSSON, P. (2009): ‘Peer effects in welfare dependence: quasi-experimental evidence’, *Journal of Human Resources* 44 (3): 798–825.
- ATKINSON, R. and KINTREA, K. (1998): *Reconnecting Excluded Communities: Neighbourhood Impacts of Owner Occupation*. Edinburgh, Scottish Homes.
- ATKINSON, R. and KINTREA, K. (2000): ‘Owner-occupation, social mix and neighborhood impacts’, *Policy and Politics* 28 (1): 93–108.
- ATKINSON, R. and KINTREA, K. (2001): ‘Area effects: what do they mean for British housing and regeneration policy?’, *European Journal of Housing Policy* 2 (2): 147–166.
- BAUDER, H. (2001): ‘“You’re good with your hands, why don’t you become an auto mechanic”: neighborhood context, institutions and career development’, *International Journal of Urban and Regional Research* 25 (3): 593–608.
- BAYER, P., ROSS, S. and TOPA, G. (2008): ‘Place of work and place of residence: informal hiring networks and labor market outcomes’, *Journal of Political Economy* 116 (6): 1150–1196.
- BERGSTEN, Z. (2010): *Bättre framtidsutsiker? Blandade bostadsområden och grannskapsfaktorer*. Geografiska Regionstudier 85, PhD dissertation, Department of Social and Economic Geography, Uppsala University.
- BERTRAND, M., LUTTMER, E., and MULLAINATHAN, S. (2000): ‘Network effects and welfare cultures’, *Quarterly Journal of Economics* 115 (3): 1019–1055.
- BERUBE, A. (2005): *Mixed Communities in England: A U.S. Perspective on Evidence and Policy Proposals*. Joseph Rowntree Foundation, York.
- BOLSTER, A., BURGESS, S., JOHNSTON, R., JONES, K., PROPPER, C. and SARKER, R. (2007): ‘Neighborhoods, households and income dynamics: a semi-parametric investigation of neighborhood effects’, *Journal of Economic Geography* 7 (1): 1–38.
- BOOZA, J., GALSTER, G. and CUTSINGER, J. (2006): *Where Did They Go? The Decline of Middle-Income Neighborhoods in Metropolitan America*. Brookings Institution-Metropolitan Studies Center, Washington, DC.
- BRÄNNSTRÖM, L. and ROJAS, Y. (2012): ‘Rethinking the long-term consequences of growing up in a disadvantaged neighbourhood: lessons from Sweden’, *Housing Studies* 27 (6): 729–747.
- BRIGGS, X. (ed.) (2005): *The Geography of Opportunity*. Brookings Institution Press, Washington, DC.
- BRIGGS, X., COVE, E., DUARTE, C. and TURNER, M. A. (2011): ‘How does leaving high-poverty neighborhoods affect the employment prospects of low-income mothers and youth?’, in NEWBURGER, H., BIRCH, E. and WACHTER, S. (eds): *Neighborhood and Life Chances: How Place Matters in Modern America*. University of Pennsylvania Press, Philadelphia, pp. 179–203.
- BRIGGS, X., FERRYMAN, K., POPKIN, S. and RENDON, M. (2008): ‘Why did the moving to opportunity experiment not get young people into better schools?’, *Housing Policy Debate* 19 (1): 53–91.
- BRIGGS, X., PPOKIN, S. and GOERING, J. (2010): *Moving To Opportunity*. Oxford University Press, New York.
- BROOKS-GUNN, J., DUNCAN, G. J. and ABER, J. L. (1997): *Neighborhood Poverty: Vol. 1 Context and Consequences for Children*. Russell Sage Foundation, New York.
- BUCK, N. (2001): ‘Identifying neighborhood effects on social exclusion’, *Urban Studies* 38 (12): 2251–2275.
- BURDICK-WILL, J., LUDWIG, J., RAUDENBUSH, S., SAMPSON, R., SANBONMATSU, L. and SHARKEY, P. (2011): ‘Converging evidence for neighborhood effects on children’s test scores: An experimental, quasi-experimental, and observational comparison’, in DUNCAN, G. and MURNANE, R. (eds): *Whither Opportunity: Rising Inequality, Schools, and Children’s Life Chances*. Russell Sage, New York; Spencer Foundation, Chicago, IL, pp. 255–276.
- CARD, D. and KRUEGER, A. (1992): ‘Does school quality matter?’, *Journal of Political Economy* 100 (1): 1–40.
- CASE, A. and KATZ, L. (1991): *The Company You Keep: The Effects of Family and Neighborhood on Disadvantaged Youth*. NBER Working Paper 3705, National Bureau of Economic Research, Cambridge, MA.



- CHESHIRE, P. (2007): *Are Mixed-Income Communities the Answer to Segregation and Poverty?* Joseph Rowntree Foundation, York.
- CHETTY, R., HENDREN, N. and KATZ, L. (2015): *The Effects of Exposure to Better Neighborhoods on Children: New Evidence from the Moving to Opportunity Experiment*. NBER Working Paper 21156, National Bureau of Economic Research, Cambridge, MA.
- CHYN, E. (2016): Moved to Opportunity: The Long-Run Effect of Public Housing Demolition on Labor Market Outcomes of Children. Unpublished Working Paper, Department of Economics, University of Michigan, Ann Arbor, MI.
- CLAMPET-LUNDQUIST, S., EDIN, K., KLING, J. and DUNCAN, G. (2011): 'Moving at-risk youth out of high-risk neighborhoods: why girls fare better than boys', *American Journal of Sociology* 116 (4): 1154–1189.
- CLAMPET-LUNDQUIST, S. and MASSEY, D. (2008): 'Neighborhood effects on economic self-sufficiency: a reconsideration of the moving to opportunity experiment', *American Journal of Sociology* 114 (1): 107–143.
- CONDON, D. and ROSCIGNO, V. (2003): 'Disparities within: unequal spending and achievement in an urban school district', *Sociology of Education* 76 (1): 18–36.
- CROWDER, K. and SOUTH, S. (2011): 'Spatial and temporal dimensions of neighborhood effects on high school graduation', *Social Science Research* 40 (1): 87–106.
- CUTLER, D., GLAESER, E. and VIGDOR, J. (2008): 'When are ghettos bad? Lessons from immigrant segregation in the United States', *Journal of Urban Economics* 63 (3): 759–774.
- DARCY, M. (2010): 'Deconcentration of disadvantage and mixed income housing: a critical discourse approach', *Housing, Theory and Society* 27 (1): 1–22.
- DAWKINS, C., SHEN, Q. and SANCHEZ, T. (2005): 'Race, space and unemployment duration', *Journal of Urban Economics* 58 (1): 91–113.
- DEAN, J. and HASTINGS, A. (2000): *Challenging Images: Housing Estates, Stigma and Regeneration*. The Policy Press, Bristol and Joseph Rowntree Foundation, York.
- DELORENZI, S. (2006): 'Introduction', in: DELORENZI, S. (ed.): *Going Places: Neighbourhood, Ethnicity and Social Mobility*. Institute for Public Policy Research, London, pp. 1–11.
- DELUCA, S., DUNCAN, G., MENDENHALL, R. and KEELS, M. (2010): 'Gautreaux mothers and their children: an update', *Housing Policy Debate* 20 (1): 7–25.
- DIEHR, P., KOEPEL, T., CHEADLE, A., PSATY, B., WAGNER, E. and CURRY, S. (1993): 'Do communities differ in health behaviors?', *Journal of Clinical Epidemiology* 46 (10): 1141–1149.
- DIETZ, R. (2002): 'The estimation of neighborhood effects in the social sciences', *Social Science Research* 31 (4): 539–575.
- DUNCAN, G., CONNELL, J. and KLEBANOV, P. (1997): 'Conceptual and methodological issues in estimating causal effects of neighborhoods and family conditions on individual development', in: BROOKS-GUNN, J., DUNCAN, G. J. and ABER, J. L. (eds): *Neighborhood Poverty: Vol. 1. Context and Consequences for Children*. Russell Sage Foundation, New York, pp. 219–250.
- DURLAUF, S. (2004): 'Neighborhood effects', in: HENDERSON, V. and THISSE, J. (eds): *The Handbook of Regional and Urban Economics, Vol. 4: Cities and Geography*. Elsevier Science/North Holland, Amsterdam, pp. 2174–2242.
- EDIN, P., FREDRICKSSON, P. and ÅSLUND, O. (2003): 'Ethnic enclaves and the economic success of immigrants: evidence from a natural experiment', *The Quarterly Journal of Economics* 118 (1): 329–357.
- ELLEN, I. and TURNER, M. (2003): 'Do neighborhoods matter and why?', in: GOERING, J. and FEINS, J. (eds.): *Choosing a Better Life? Evaluating the Moving To Opportunity Experiment*. Urban Institute Press, Washington, DC, pp. 313–338.
- FARWICK, A. (2004): 'Spatial isolation, social networks, and the economic integration of migrants in poverty areas', Paper presented at the 'Inside Poverty Areas' conference, University of Köln, November.
- FERNANDEZ, R. and HARRIS, D. (1992): 'Social isolation and the underclass', in: HARRELL, A. and PETERSON, G. (eds): *Drugs, Crime and Social Isolation*. Urban Institute Press, Washington, DC, pp. 257–293.
- FISCHER, C. (1982): *To Dwell Among Friends*. University of Chicago Press, Chicago, IL.
- FITZPATRICK, K. and BOLDIZAR, J. (1993): 'The prevalence and consequences of exposure to violence among African-American youth', *Journal of the American Academy of Child and Adolescent Psychiatry* 32 (2): 424–430.
- FRIEDRICH, J., GALSTER, G. and MUSTERD, S. (2003): 'Neighborhood effects on social opportunities: the European and American research and policy context', *Housing Studies* 18 (6): 797–806.
- GALSTER, G. (2002): 'An economic efficiency analysis of deconcentrating poverty populations', *Journal of Housing Economics* 11 (4): 303–329.
- GALSTER, G. (2007a): 'Neighbourhood social mix as a goal of housing policy: a theoretical analysis', *European Journal of Housing Policy* 7 (1): 19–43.
- GALSTER, G. (2007b): 'Should policymakers strive for neighborhood social mix? An analysis of the Western European evidence base', *Housing Studies* 22 (4): 523–546.
- GALSTER, G. (2008): 'Quantifying the effect of neighbourhood on individuals: challenges, alternative approaches and promising directions', *Journal of Applied Social Science Studies [Schmollers Jahrbuch/ Zeitschrift für Wirtschafts- und Sozialwissenschaften]* 128(1): 7–48.
- GALSTER, G. (2012): 'The mechanism(s) of neighborhood effects: theory, evidence, and policy implications', in: VAN HAM, M., MANLEY, D., BAILEY, N., SIMPSON, L. and MACLENNAN, D. (eds): *Neighbourhood Effects Research: New Perspectives*. Springer, Dordrecht, pp. 23–56.
- GALSTER, G. (2013): 'Neighborhood social mix: theory, evidence, and implications for policy and planning', in: CARMON, N. and FAINESTEIN, S. (eds): *Policy, Planning and People: Promoting Justice in Urban Development*. University of Pennsylvania Press, Philadelphia, pp. 307–336.
- GALSTER, G., ANDERSSON, R. and MUSTERD, S. (2010): 'Who is affected by neighbourhood income mix? Gender, age, family, employment and income differences', *Urban Studies* 47 (14): 2915–2944.
- GALSTER, G., ANDERSSON, R. and MUSTERD, S. (2015): 'Are males' incomes influenced by the income mix of their male neighbors? Explorations into nonlinear and threshold effects in Stockholm', *Housing Studies* 30 (2): 315–343.
- GALSTER, G., ANDERSSON, R., MUSTERD, S. and KAUPPINEN, T. (2008): 'Does neighborhood income mix affect earnings of adults? New evidence from Sweden', *Journal of Urban Economics* 63 (3): 858–870.
- GALSTER, G. and BOOZA, J. (2007): 'The rise of the bipolar neighborhood', *Journal of the American Planning Association* 73 (4): 421–435.
- GALSTER, G. and FRIEDRICH, J. (2015): 'The dialectic of neighborhood social mix: editors' introduction to the special issue', *Housing Studies* 30 (2): 175–191.

- GALSTER, G. and HEDMAN, L. (2013): 'Measuring neighbourhood effects non-experimentally: how much do alternative methods matter?', *Housing Studies* 28 (3): 473–498.
- GALSTER, G., MARCOTTE, D., MANDELL, M., WOLMAN, H. and AUGUSTINE, N. (2007): 'The influence of neighborhood poverty during childhood on fertility, education and earnings outcomes', *Housing Studies* 22 (5): 723–752.
- GALSTER, G., SANTIAGO, A. and LUCERO, J. (2015a): 'Adrift at the margins of urban society: what role does neighborhood play?', *Urban Affairs Review* 51 (1): 10–45.
- GALSTER, G., SANTIAGO, A. and LUCERO, J. (2015b): 'Employment of low-income African American and Latino teens: does neighborhood social mix matter?', *Housing Studies* 30 (2): 192–227.
- GALSTER, G., SANTIAGO, A., LUCERO, J. and CUTSINGER, J. (2015c): 'Adolescent neighborhood context and young adult economic outcomes for low-income African Americans and Latinos', *Journal of Economic Geography* (online 2015; DOI:10.1093/jeg/lbv004).
- GALSTER, G. and ZOBEL, A. (1998): 'Will dispersed housing programs reduce social costs in the U.S.?', *Housing Studies* 13 (5): 605–622.
- GENNETIAN, L., LUDWIG, J. and SANBONMATSU, L. (2011): 'Understanding neighborhood effects on low-income families', in NEWBURGER, H., BIRCH, E. and WACHTER, S. (eds.): *Neighborhood and Life Chances: How Place Matters in Modern America*. University of Pennsylvania Press, Philadelphia, pp. 163–178.
- GEPHART, M. (1997): 'Neighborhoods and communities as contexts for development', in BROOKS-GUNN, J., DUNCAN, G. J. and ABER, J. L. (eds.): *Neighborhood Poverty: Vol. 1. Context and Consequences for Children*. Russell Sage Foundation, New York, pp. 1–43.
- GINTHER, D., HAVEMAN, R. and WOLFE, B. (2000): 'Neighborhood attributes as determinants of children's outcomes', *Journal of Human Resources* 35 (4): 603–642.
- GOERING, J. and FEINS, J. (eds.) (2003): *Choosing a Better Life? Evaluating the Moving To Opportunity Experiment*. Urban Institute Press, Washington, DC.
- GRANOVETTER, M. (1995): *Getting a Job*. University of Chicago Press, Chicago, IL.
- HARDING, D., GENNETIAN, L., WINSHIP, C., SANBONMATSU, L. and KLING, J. (2011): 'Unpacking neighborhood influences on education outcomes: setting the stage for future research', in DUNCAN, G. and MURNANE, R. (eds.): *Whither Opportunity: Rising Inequality, Schools, and Children's Life Chances*. Russell Sage, New York; Spencer Foundation, Chicago, IL, pp. 277–296.
- HASTINGS, A. (2004): 'Stigma and social housing estates', *Journal of Housing and the Built Environment* 19(3): 233–254.
- HASTINGS, A. (2007): 'Territorial justice and environmental services: UK', *Environment and Planning C: Government and Policy*, 25 (6): 896–917.
- HASTINGS, A. (2009a): 'Neighbourhood environmental services and neighbourhood "effects": exploring the role of urban services in intensifying neighbourhood problems', *Housing Studies* 24 (4): 503–524.
- HASTINGS, A. (2009b): 'Poor neighbourhoods and poor services: evidence on the "rationing" of environmental service provision to deprived neighbourhoods', *Urban Studies* 46 (13): 2907–2928.
- HASTINGS, A. and DEAN, J. (2003): 'Challenging images: tackling stigma through estate regeneration', *Policy and Politics* 31 (2): 171–184.
- HEDMAN, L. and GALSTER, G. (2013): 'Neighborhood income sorting and the effects of neighborhood income mix on income: a holistic empirical exploration', *Urban Studies* 50 (1): 107–127.
- HEDMAN, L., MANLEY, D., VAN HAM, M. and ÖSTH, J. (2012): 'Cumulative exposure to disadvantage and the inter-generational transmission of neighborhood effects', Paper presented at the ENHR meetings, Lillehammer, Norway, July.
- IHLANFELDT, K. (1999): 'The geography of economic and social opportunity within metropolitan areas', in ALTSHULER, A., MORRILL, W., WOLMAN, H. and MITCHELL, H. (eds): *Governance and Opportunity in Metropolitan America*. National Academy of Sciences, Washington, DC, pp. 213–252.
- IOANNIDES, Y. and LOURY, L. (2004): 'Job information networks, neighborhood effects, and inequality', *Journal of Economic Literature* 42 (4): 1056–1093.
- JENCKS, C. and MAYER, S. (1990): 'The social consequences of growing up in a poor neighborhood', in LYNN, L. and MCGEARY, M. (eds): *Inner-City Poverty in the United States*. National Academy Press, Washington, DC, pp. 111–186.
- JOSEPH, M. (2006): 'Is mixed-income development an antidote to urban poverty?', *Housing Policy Debate* 17 (2): 209–234.
- JOSEPH, M., CHASKIN, R. and WEBBER, H. (2006): 'The theoretical basis for addressing poverty through mixed-income development', *Urban Affairs Review* 42 (3): 369–409.
- KATZ, L., KLING, J. and LIEBMAN, J. (2001): 'A moving to opportunity in Boston: early results of a randomized mobility experiment', *Quarterly Journal of Economics* 116 (2): 607–654.
- KAUPPINEN, T. (2007): 'Neighbourhood effects in a European city: secondary education of young people in Helsinki', *Social Science Research* 36 (1): 421–444.
- KEARNS, A. (2002): 'Response: from residential disadvantage to opportunity? Reflections on British and European policy and research', *Housing Studies* 17 (1): 145–150.
- KELLY, M. (2013): *Onward Migration: The Transnational Trajectories of Iranians Leaving Sweden*. PhD dissertation, Department of Social and Economic Geography, Uppsala University.
- KIRSCHENMAN, J. and NECKERMAN, K. (1991): 'Hiring strategies, racial bias, and inner-city workers', *Social Problems* 38 (4): 433–447.
- KLEINHANS, R. (2004): 'Social implications of housing diversification in urban renewal: a review of recent literature', *Journal of Housing and the Built Environment* 19 (4): 367–390.
- KLEIT, R. (2008): 'Neighborhood segregation, personal networks, and access to social resources', in CARR, J. and KUTTY, N. (eds): *Segregation: The Rising Costs for America*. Routledge, New York, pp. 237–260.
- KLING, J., LIEBMAN, J. and KATZ, L. (2007): 'Experimental analysis of neighborhood effects', *Econometrica* 75 (1): 83–119.
- KOZOL, J. (1991): *Savage Inequalities*. Harper, New York.
- LANKFORD, H., LOEB, S. and WYCKOFF, J. (2002): 'Teacher sorting and the plight of urban schools: a descriptive analysis', *Educational Evaluation and Policy Analysis* 24 (1): 37–62.
- LEVENTHAL, T. and BROOKS-GUNN, J. (2000): 'The neighborhoods they live in: the effects of neighborhood residence on child and adolescent outcomes', *Psychological Bulletin* 126 (2): 309–337.
- LUDWIG, J. (2012): 'Moving to opportunity: guest editor's introduction', *Citiescape* 14 (2): 1–28.
- LUDWIG, J., DUNCAN, G. and HIRSCHFELD, P. (2001a): 'Urban poverty and juvenile crime: evidence from a randomized housing-mobility experiment', *Quarterly Journal of Economics* 116 (2): 655–679.

- LUDWIG, J., DUNCAN, G. and PINKSTON, J. (2005): 'Neighborhood effects on economic self-sufficiency: evidence from a randomized housing-mobility experiment', *Journal of Public Economics* 89 (1): 131–156.
- LUDWIG, J., LADD, H. and DUNCAN, G. (2001b): 'The effects of urban poverty on educational outcomes: evidence from a randomized experiment', in GALE, W. and PACK, J. R. (eds): *Brookings-Wharton Papers on Urban Affairs*. Brookings Institution, Washington, DC, pp. 147–201.
- LUDWIG, J., LIEBMAN, J., KLING, J., DUNCAN, G., KATZ, L., KESSLER, R. and SANBONMATSU, L. (2008): 'What can we learn about neighborhood effects from the moving to opportunity experiment?', *American Journal of Sociology* 114 (1): 144–188.
- MANSKI, C. (1995): *Identification Problems in the Social Sciences*. Harvard University Press, Cambridge, MA.
- MANSKI, C. (2000): 'Economic analysis of social interactions', *Journal of Economic Perspectives* 14 (3): 115–136.
- MARTIN, G. and WATKINSON, J. (2003): *Rebalancing Communities: Introducing Mixed Incomes into Existing Rented Housing Estates*. Joseph Rowntree Foundation, York.
- MEEN, G., GIBB, K., GOODY, J., MCGRATH, T. and MACKINNON, J. (2005): *Economic Segregation in England*. Joseph Rowntree Foundation, York.
- MURIE, A. and MUSTERD, S. (2004): 'Social exclusion and opportunity structures in European cities and neighbourhoods', *Urban Studies* 41 (8): 1425–1443.
- MUSTERD, S. (2002): 'Response: mixed housing policy: a European (Dutch) perspective', *Housing Studies*, 17 (1): 139–144.
- MUSTERD, S. (2003): 'Segregation and integration: a contested relationship', *Journal of Ethnic and Migration Studies* 29 (4): 623–641.
- MUSTERD, S. and ANDERSSON, R. (2005): 'Housing mix, social mix and social opportunities', *Urban Affairs Review* 40 (6): 761–790.
- MUSTERD, S., ANDERSSON, R., GALSTER, G. and KAUPPINEN, T. (2008): 'Are immigrants' earnings influenced by the characteristics of their neighbours?', *Environment and Planning A* 40 (4): 785–805.
- MUSTERD, S., DEVOS, S., DAS, M. and LATTEN, J. (2012): 'Neighbourhood composition and economic prospects: a longitudinal study in the Netherlands', *Tijdschrift voor Economische en Sociale Geografie* 103 (1): 85–100.
- MUSTERD, S., GALSTER, G. and ANDERSSON, R. (2012): 'Temporal dimensions and the measurement of neighbourhood effects', *Environment and Planning A* 44 (3): 605–627.
- MUSTERD, S., OSTENDORF, W. and DE VOS, S. (2003): 'Neighborhood effects and social mobility', *Housing Studies* 18 (6): 877–892.
- NORRIS, M. (2006): 'Developing, designing and managing mixed tenure housing estates', *European Planning Studies*, 14 (2): 199–218.
- OREOPOLOS, P. (2003): 'The long-run consequences of living in a poor neighborhood', *Quarterly Journal of Economics* 118 (4): 1533–1575.
- ORR, L., FEINS, J., JACOB, R. and BEECROFT, E. (2003): *Moving to Opportunity: Interim Impacts Evaluation, Final Report*. US Department of Housing and Urban Development, Washington, DC.
- OSTENDORF, W., MUSTERD, S. and DE VOS, S. (2001): 'Social mix and the neighborhood effect: policy ambition and empirical support', *Housing Studies* 16 (3): 371–380.
- PERMENTIER, M. (2009): *Reputation, Neighbourhoods and Behaviour*. PhD dissertation, Department of Geography, Utrecht University.
- PIIL DAMM, A. (2009): 'Ethnic enclaves and immigrant labor market outcomes: quasi-experimental evidence', *Journal of Labor Economics* 27 (2): 281–314.
- PIIL DAMM, A. (2014): 'Neighborhood quality and labor market outcomes: evidence from a quasi-random neighborhood assignment of immigrants', *Journal of Urban Economics* 79 (1): 139–166.
- PINKSTER, F. (2008): *Living in Concentrated Poverty*. PhD dissertation, Department of Geography, Planning, and International Development Studies, University of Amsterdam.
- PINKSTER, F. (2014): 'Neighbourhood effects as indirect effects: evidence from a Dutch case study on the significance of neighbourhood for employment trajectories', *International Journal of Urban and Regional Research* 38 (6): 2042–2059.
- PLOTNICK, R. and HOFFMAN, S. (1999): 'The effect of neighborhood characteristics on young adult outcomes: alternative estimates', *Social Science Quarterly* 80 (1): 1–18.
- POWER, A. (1997): *Estates on the Edge: The Social Consequences of Mass Housing in Northern Europe*. Macmillan, London.
- PROPPER, C., BURGESS, S., BOLSTER, A., LECKIE, G., JONES, K. and JOHNSTON, R. (2007): 'The impact of neighbourhood on the income and mental health of British social renters', *Urban Studies* 44 (2): 393–415.
- RASMUSSEN, D. (1994): 'Spatial economic development, education and the new poverty', *International Regional Science Review* 14 (1–2): 107–117.
- ROSENBAUM, J. (1991): 'Black pioneers: do moves to the suburbs increase economic opportunity for mothers and children?', *Housing Policy Debate* 2 (4): 1179–1213.
- ROSENBAUM, J. (1995): 'Changing the geography of opportunity by expanding residential choice: lessons from the Gautreaux Program', *Housing Policy Debate* 6 (1): 231–269.
- ROSS, S. (2012): 'Social interactions within cities: neighborhood environments and peer relationships', in BROOKS, N., DONAGHY, K. and KNAAP, G. (eds): *The Oxford Handbook of Urban Economics and Planning*. Oxford University Press, Oxford and New York, pp. 203–229.
- RUBINOWITZ, L. and ROSENBAUM, J. (2000): *Crossing the Class and Color Lines: From Public Housing to White Suburbia*. University of Chicago Press, Chicago, IL.
- SAMPSON, R. (2001): 'How do communities undergird or undermine human development? Relevant contexts and social mechanisms', in BOOTH, A. and CROUTER, A. (eds): *Does it Take a Village? Community Effects on Children, Adolescents and Families*. Lawrence Erlbaum Publishers, London and Mahwah, NJ, pp. 3–30.
- SAMPSON, R. (2008): 'Moving to inequality: neighborhood effects and experiments meet social structure', *American Journal of Sociology* 114 (1): 189–231.
- SAMPSON, R., MORENOFF, J. and GANNON-ROWLEY, T. (2002): 'Assessing "neighborhood effects": social processes and new directions in research', *Annual Review of Sociology* 28: 443–478.
- SAMPSON, R. and RAUDENBUSH, S. (1999): 'Systematic social observations of public spaces: a new look at disorder in urban neighborhoods', *American Journal of Sociology* 105 (3): 603–651.
- SAMPSON, R., RAUDENBUSH, S. and EARLS, F. (1997): 'Neighborhoods and violent crime: a multilevel study of collective efficacy', *Science* 277 (5328): 918–924.

- SAMPSON, R., SHARKEY, P. and RAUDENBUSH, S. (2008): 'Durable effects of concentrated disadvantage on verbal ability among African-American children', *Proceedings of the National Academy of Sciences of the United States of America* 105 (3): 931–969.
- SANBONMATSU, L., LUDWIG, J., KATZ, L., GENNETIAN, L., DUNCAN, G., KESSLER, R., ADAM, E., MCDADE, T. and LINDAU, S. (2011): *Impacts of the Moving to Opportunity for Fair Housing Demonstration Program after 10 to 15 Years*. US Department of Housing and Urban Development, Office of Policy Development and Research, Washington, DC.
- SARI, F. (2012): 'Analysis of neighbourhood effects and work behaviour: evidence from Paris', *Housing Studies* 27 (1): 45–76.
- SHARKEY, P. (2012): 'An alternative approach to addressing selection into and out of social settings: neighborhood change and African American children's economic outcomes', *Sociological Methods and Research* 41 (2): 251–293.
- SHARKEY, P. and FABER, J. (2014): 'Where, when, why, and for whom do residential contexts matter? Moving away from the dichotomous understanding of neighborhood effects', *Annual Review of Sociology* 40: 559–579.
- SIMMEL, G. (1971): *Georg Simmel on Individuality and Social Forms*. University of Chicago Press, Chicago, IL.
- SINGER, M., ANGLIN, T., SONG, L. and LUNGHOFFER, L. (1995): 'Adolescents' exposure to violence and associated symptoms of psychological trauma', *Journal of the American Medical Association* 273 (6): 477–482.
- SKOGAN, W. (1990): *Disorder and Decline: Crime and the Spiral of Decay in American Neighborhoods*. University of California Press, Berkeley and Los Angeles, CA.
- SMALL, M. and FELDMAN, J. (2012): 'Ethnographic evidence, heterogeneity and neighbourhood effects after moving to opportunity', in VAN HAM, M., MANLEY, D., BAILEY, N., SIMPSON, L. and MACLENNAN, D. (eds.): *Neighbourhood Effects Research: New Perspectives*. Springer, Dordrecht, 57–78.
- SMOLENSKY, E. (2007): 'Children in the vanguard of the U.S. welfare state', *Journal of Economic Literature* 45 (4): 1011–1023.
- SOUTH, S. (2001): 'Issues in the analysis of neighborhoods, families, and children', in BOOTH, A. and CROUTER, A. (eds.): *Does it Take a Village? Community Effects on Children, Adolescents and Families*. Lawrence Erlbaum Publishers, London and Mahwah, NJ, 87–94.
- SOUTH, S. and BAUMER, E. (2000): 'Deciphering community and race effects on adolescent pre-marital childbearing', *Social Forces* 78 (4): 1379–1407.
- SUNDLÖF, P. (2008): Segregation och Karriärposition. Geografiska Regionstudier #78, PhD dissertation, Department of Social and Economic Geography, Uppsala University.
- SULLIVAN, M. (1989): *Getting Paid: Youth Crime and Work In The Inner City*. Cornell University Press, Ithaca, NY.
- TAYLOR, M. (1998): 'Combating the social exclusion of housing estates', *Housing Studies* 13 (6): 819–832.
- TIGGES, L. M., BROWNE, I. and GREEN, G. P. (1998): 'Social isolation of the urban poor', *Sociological Quarterly* 39 (1): 53–77.
- TURLEY, R. (2003): 'When do neighborhoods matter? The role of race and neighborhood peers', *Social Science Research* 32 (1): 61–79.
- TURNER, M., COMEY, J., KUEHN, D. and NICHOLS, A. (2012): *Residential Mobility, High-Opportunity Neighborhoods, and Outcomes for Low-Income Families: Insights from the Moving to Opportunity Demonstration*. US Department of Housing and Urban Development, Office of Policy Development and Research, Washington, DC.
- VAN HAM, M. and MANLEY, D. (2009): *The effect of neighbourhood housing tenure mix on labor market outcomes: a longitudinal perspective*. Institute for the Study of Labor Discussion Paper IZA DP no. 4094, Bonn, Germany, March.
- VAN HAM, M. and MANLEY, D. (2010): 'The effect of neighbourhood housing tenure mix on labor market outcomes: a longitudinal investigation of neighbourhood effects', *Journal of Economic Geography* 10 (2): 257–282.
- VAN HAM, M., MANLEY, D., BAILEY, N., SIMPSON, L. and MACLENNAN, D. (eds.) (2012): *Neighbourhood Effects Research: New Perspectives*. Springer, Dordrecht.
- VAN KEMPEN, R. and BOLT, G. (2009): 'Social cohesion, social mix, and urban policies in the Netherlands', *Journal of Housing and the Built Environment* 24 (4): 457–475.
- VARTANIAN, T. and BUCK, P. (2005): 'Childhood and adolescent neighborhood effects on adult income: using siblings to examine differences in ordinary least squares and fixed-effect models', *Social Service Review* 79 (1): 60–94.
- WACQUANT, L. (1993): 'Urban outcasts: stigma and division in the black American ghetto and the French periphery', *International Journal of Urban and Regional Research* 17 (3): 366–383.
- WALDINGER, R. and LICHTER, M. (2003): *How the Other Half Works*. University of California Press, Berkeley, CA.
- WEBER, M. (1978): *Economy and Society*, 2 Vols. University of California Press, Berkeley, CA.
- WEINBERG, B., REAGAN, P. and YANKOW, J. (2004): 'Do neighborhoods affect work behavior? Evidence from the NLSY79', *Journal of Labor Economics* 22 (4): 891–924.
- WHEATON, B. and CLARKE, P. (2003): 'Space meets time: integrating temporal and contextual influences on mental health in early adulthood', *American Sociological Review* 68 (5): 680–706.
- WILLIAMS, S., GALSTER, G. and VERMA, N. (2014): 'Home foreclosures and neighborhood crime dynamics', *Housing Studies* 29 (3): 380–406.
- WILSON, W. J. (1987): *The Truly Disadvantaged*. University of Chicago Press, Chicago, IL.
- WILSON, W. J. (1996): *When Work Disappears*. Knopf, New York.
- WODTKE, G., HARDING, D. and ELWERT, F. (2011): 'Neighborhood effects in temporal perspective: the impact of long-term exposure to concentrated disadvantage on high school graduation', *American Sociological Review* 76 (5): 713–736.
- ZENOU, Y., ÅSLUND, O. and ÖSTH, J. (2006): *How important is access to jobs? Old question – improved answer*. IFAU Working Paper, Uppsala University, Sweden.