Temporal Dimensions

and the Measurement of Neighbourhood Effects

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ABSTRACT

We conduct a panel analysis quantifying the degree to which the mixture of low-, middle- and high-income males in the neighbourhood affects the subsequent labour income of individuals, and test the degree to which these effects vary by timing (lagging up to three years) duration (one to four years), and cumulative amount of exposure and to what extent the effects are persistent. We employ a fixed effects model to reduce the potential bias arising from unmeasured individual characteristics leading to neighbourhood selection. The empirical study applies individual-level data for the working-age population of the three largest cities in Sweden covering the period 1991-2006. We find that there are important temporal dimensions in the effect of neighbourhood income mix: recent, continued or cumulative exposure yields larger effects than lagged, temporary ones, and there is distinct time decay (though some persistence) in the effect after exposure ceases, though with some gender differences.

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I. Introduction

Over the past decades neighbourhood effect research has matured rapidly. The use of richer, longitudinal data (Oberwittler 2007; Andersson et al. 2007; Buck 2007; Galster et al. 2008, 2011, Van Ham and Manley 2010; Sykes 2011), the application of statistical methods to overcome selection bias (Weinberg et al. 2004; Cuttler et al. 2008; Galster et al. 2007; Galster et al. 2008, 2011) and, albeit rarely, more focus on the non-linear relationship between neighbourhood characteristics and individual outcome variables (Duncan et al. 1997; Vartanian 1999a, b; Weinberg et al. 2004; Musterd et al. 2003; Galster et al 2008), have enriched the insights in this field. Simultaneously, more qualitative research has enhanced our understanding of the mechanisms through which the neighbourhood would affect individuals. Through these contributions we have learned more about the potential influences of socialization and social control (for example Friedrichs et al. 2003; Pinkster 2009); social networks (Farwick 2004; Pinkster 2009; Kleit 2008); social disorder (Sampson and Raudenbusch 1999); and stigmatization (Hastings 2004; Hastings and Dean 2003; Permentier 2009).

However, what has received limited attention so far are temporal issues about exposure to neighbourhood environments and resulting individual consequences. More research on precisely this point was recently advocated by Briggs and Keys (2009: 451). How long an exposure does it take before a particular type of neighbourhood effect manifests itself? Is the effect stronger if the particular contextual condition persists over time? Do exposures from the past still have an effect currently?

It is important to know more about impacts of timing, duration, and cumulative exposure and the durability of these impacts, because of academic interest in building stronger theory, and because policy makers are searching for interventions that will promote the most efficacious neighbourhood environment for human well-being. In particular, the formulation and evaluation of programs to socially diversify neighbourhoods through place-based housing schemes or tenant-based rental subsidies or vacancy allocation devices may benefit from the insights to be gained. If we were to find that neighbourhood effects take hold only after a substantial period of sustained exposure, we should expect few short-term benefits from place-based social mix strategies and from only temporary exposures of subsidized renters to low-poverty neighbourhood (as experienced by participants in the Moving To Opportunity demonstration in the U.S.). Or, if we were to find that neighbourhood effects were virtually indelible once substantial exposure had occurred, spatial policies would have to focus on preventing children, youth, and adults from ever experiencing such permanently deleterious environments, not "curing" those who already have been so exposed.

In this paper we will address the temporal dimensions of neighbourhood exposure. We build upon earlier work as far as the neighbourhood context variables and individual outcome variables are concerned (authors' self-citations redacted). Neighbourhood context will be defined in terms of mixtures of three income categories, whereas the outcome variable will represent social mobility opportunities, measured through labour income of working-age adults.

We intend to find answers to the following research questions:

- How does the timing (contemporaneous, lagging one, two or three years) of when exposure to a particular neighbourhood income mix occurs relate to the labour incomes of individual adults in the neighbourhood?
- How does the duration (number of continuous years) of exposure to a particular neighbourhood income mix relate to the labour incomes of individual adults in the neighbourhood?

- How does the cumulative exposure (contact with a particular income group in the neighbourhood cumulated over four continuous years) relate to the labour incomes of individual adults in the neighbourhood?
- Does exposure to a particular neighbourhood income mix create a persistent effect, or one that quickly decays over time after the exposure changes? If the latter, does the rate of time decay depend on the duration of original exposure?

More specifically, our study aims to contribute to the scholarly literature by providing new empirical evidence from a panel study quantifying the degree to which the mixture of low-, middle-, and high-income males in the neighbourhood affects the subsequent labour earnings of working-age individuals in three metropolitan areas in Sweden--Stockholm, Gothenburg and Malmö--and investigating the degree to which these effects vary by timing, duration, and cumulative amount of exposure. We employ a fixed effects specification of econometric model to reduce potential bias arising from unmeasured individual characteristics leading to neighbourhood selection and also affecting income.

II. The Temporal Dimension of Neighbourhood Effects: Theory and Evidence

Temporal dimensions and mechanisms of neighbourhood income mix effects: Theoretical considerations

Neighbourhood income mix might affect individual adult residents through a variety of causal mechanisms that can occur either through social interactions within the neighbourhood and/or by actions of others located outside of the neighbourhood; for extended discussion, see especially Jencks and Mayer (1990), Duncan, Connell and Klebanov (1997), Gephart (1997), Friedrichs (1998), Dietz (2002), Sampson, Morenoff, and Gannon-Rowley (2002), Friedrichs, Galster and Musterd (2003); Ioannides and Loury (2004), and Pinkster (2009). The potential intra-neighbourhood mechanisms include socialization (collective norms, peers, role models), social networks, and exposure to violence and disorder. The potential extra-neighbourhood mechanisms include stigmatization, local institutional resources and public services, and job accessibility. While current scholarship is not decisive, it suggests that several intra- and extra-neighbourhood mechanisms associated with neighbourhood income mix may be relevant; see especially Van Kempen (1997); Dietz (2002); Sampson, Morenoff and Gannon-Rowley (2002); Ellen and Turner (2003); and Galster (2005, 2011). Our purpose in this section is first to speculate for these mechanisms why one might expect variations in their power to influence residents' labour earnings depending on the timing, duration, and cumulative exposure, and then to review the scant empirical literature related to these issues.

First, consider how quickly a neighbourhood effect might occur once an adult becomes exposed to it. Socialization processes associated with particular income mixes likely take time before wielding influence. Therefore, it might be deduced that those who are exposed only briefly to an environment that is trying to re-shape their behaviours will experience little if any effect from it compared to those who are exposed to the same socializing environment for a longer period of time. A similar deduction holds for the impacts that operate through local social networks; it takes time for these networks to develop after an individual moves in (or evolve if the neighbourhood is changing around the individual). It thus follows that some minimum duration of exposure to this new context will be required before new local social networks will produce any measurable differences in job-related information conveyed by them. Finally, effects of local institutions like job placement, counselling, and skill development centres will be felt only after some period elapses, insofar as the services provided have slow, cumulative impacts. This implies that recent, short-term neighbourhood exposures will yield very small impacts compared to sustained durations producing substantial cumulative exposure, as has been argued before (Leventhal and Brooks-Gunn, 2000; Wheaton and Clarke, 2003).

However, whereas socialization processes, the development of social networks, and local institutions likely take some time before a noticeable effect can be expected, the impacts of contextual changes in stigmatization, social disorder, and accessibility may manifest themselves more rapidly, almost instantly. A person's move to a stigmatized neighbourhood may imply that the image of the neighbourhood will be immediately connected by external decision-makers to the person concerned. Similarly, the psychological and behavioural impacts from social disorder may be quickly felt. Finally, geographic challenges for the unemployed and underemployed in gaining information about and easily commuting to higher-paying jobs should manifest themselves almost immediately if the accessibility characteristics of a neighbourhood in which the individual resides change. Yet, even through these fast-acting mechanisms a stronger cumulative effect may be expected from sustained, longer-term exposure.

The final consideration relates to the persistence or durability of impact. Is a neighbourhood effect mechanism reversible? In some mechanisms, namely socialization, networks, accessibility, and stigmatization, this is likely. A change in any of these contextual dimensions could produce a comparable change in outcome, regardless of the starting value and the direction of change. However, for other mechanisms this symmetric reversibility is less likely. For example, if one replaces a weak institutional education-training infrastructure that had retarded residents' opportunities with a far superior one, one would expect (after a lag) an improvement in residents' human capital, thus rendering the initial impact transitory. By contrast, the opposite situation of a superior institutional structure producing strong human capital is likely to produce persistent effects since a hypothetical, new, inferior set of institutions will do little to erode the human capital previously attained. As another example, the benefits to mental health produced by a violence-free environment will quickly dissolve if the context turns violent, yet the psychological harms caused by exposure to a violent environment can persist for a considerable period even when the individual is placed in a safe environment. Of course, we recognize that even if in principle the mechanism is reversible (either symmetrically or asymmetrically) the impact may not be reversible if the initial context triggered behavioural changes that were durable. Should an initial neighbourhood context result in individuals making choices that adversely affected their education, job-training, or criminal record, for instance, the consequences on their income could be long-lasting even when the current neighbourhood environment had changed dramatically.

The foregoing discussion is summarized in Table 1.

[Table 1 about here]

Empirical literature on the temporal dimensions of neighbourhood effects

There has been a sizable literature devoted to measuring the independent magnitude of the effect of a neighbourhood's socioeconomic composition on adult economic outcomes, employing multivariate statistical analyses on both cross-sectional and longitudinal databases of individuals; see O'Regan and Quigley (1996); Buck (2001); Weinberg, Reagan and Yankow (2004); Musterd and Andersson (2005, 2006); Andersson et al. (2007); Dawkins, Shen and Sanchez (2005), Galster, et al. (2007, 2008, 2011). These studies typically have observed nontrivial partial correlations between various measures of the economic composition of neighbourhood residents and several measures of lagged or contemporaneous adult labour market performance, though there have been some exceptions; see: McCulloch (2001); Musterd, Ostendorf and de Vos (2003); and Drever (2004).

In almost all of the longitudinal studies the authors have constructed the data in such a way that the neighbourhood variables were measured some time before the outcome variable was measured. For example, Galster et al. (2007) found for young adults that higher neighbourhood poverty rates averaged over all years of their childhood were associated with a lower probability of graduating from college and lower annual earnings, all else equal, implicitly suggesting a durable, lagging effect. A few other studies have explored the question of non-linearity and threshold effects (Weinberg et al. 2004; Galster et al. 2008). Unfortunately, none of the studies of labour market outcomes tested for sensitivity of neighbourhood effects to different temporal aspects of exposure.

Only six studies have explicitly paid attention to how *variations* in the timing and duration of exposure modified the observed relationship with several individual outcomes that indirectly may affect labour outcomes because they involve human capital acquisition. They paint a consistent portrait that neighbourhood effects seem to be stronger if the exposure is cumulative, and sometimes effects appear only after a lag. Aaronson (1998) examined how neighbourhood poverty rates affected teen's school dropout rates, and found that the average (cumulative) neighbourhood conditions experienced during years 10-18 were much stronger predictors than contemporaneous conditions. Guerra, Huesmann and Spindler (2003) investigated consequences of exposure to violence, and found that it had an immediate effect on youths' aggressive tendencies, but a substantially lagged effect associated with the development of social cognitions related to violence. Wheaton and Clarke (2003) investigated the temporal dimension of neighbourhood disadvantage effects on the mental health of young adults. They found that current neighbourhood had no effect, but earlier neighbourhood disadvantage experienced as a child had a lagged effect that grew stronger as cumulative exposure intensified. Turley (2003) found that white (though not black) children's school test scores and several behavioural indicators grew more efficacious the greater the mean income of their neighbourhoods. These relationships were strongest for children who had lived in their neighbourhoods for three years or more, suggesting either a lagged and/or cumulative effect process. Kauppinen (2007) observed little impact of neighbours' social status on type of secondary school chosen unless the students were in the neighbourhood two or more years. Finally, Sampson, Sharkey and Raudenbush (2008) examined reading abilities of black children who grew up in Chicago at three later points in their life. Their findings indicated that there was a cumulative, durable penalty from extended childhood residence in neighbourhoods with concentrations of low socioeconomic status households, which grew stronger after several years of residence in such places.

Thus, both theory and the extant smattering of empirical evidence points strongly to the conclusion that temporal dimensions of neighbourhood effects must be taken into account explicitly. We do so comprehensively in this paper and make four unique contributions to the literature:

- we apply unusually rich longitudinal data over a 15 year panel
- we investigate annual variations in exposure (timing, duration, cumulative) to neighbourhood income mix and the durability of effects once exposure ceases
- we measure these effects for individual income
- we minimise bias from geographic selection by applying fixed effects.

III. Data and Empirical Model

The Swedish Data Files

The variables we employ are constructed from data contained in the Statistics Sweden *Louise* files, which are produced annually. These files contain a large amount of information on all individuals age 15 and above and represent compilations of data assembled from a range of statistical registers (income, education, labour market, and population). We have laboriously merged selected information about individuals from annual *Louise* files to create a unique, longitudinal database 1991-2006 for all adults residing in 1991 in three of Sweden's large, but to some extent contrasting metropolitan areas, Stockholm, Gothenburg and Malmö. Since we focus on labour earnings, we confine our analysis to prime working-age individuals (ages 20-49 in 1991). Since we also wish to maintain a reasonably consistent notion of urban neighbourhood, we further confine our analysis to those who were residents of (any of) these three metropolitan areas in each year from 1991 to 2006. This restriction meant that we analyze somewhat more than half of the Stockholm, Gothenburg, and Malmö populations within the desired age and residency range. Characteristics of our sample are provided in the descriptive statistics of Table 2a.

[Table 2a on webpage x]

Our Model of the Determinants of Individual Labour Incomes

Our outcome of interest is the individual's annual income from work (measured in Swedish *kronor,* SEK; $1=7.40 SEK).[1] Since this indicator encapsulates the net impact of educational credentials, labour force participation, employment regularity, and hourly compensation, we believe it to be the most comprehensive single measure of an individual's economic performance. We model in conventional, log-linear form[2] the annual income from work during year t (with the current year t=0) for individual i residing in neighbourhood j in metropolitan area k[3] as:

$$\ln(I_{tijk}) = \alpha + \beta[P_{ti}] + \gamma[P_i] + \partial[UP_i] + \theta[N_{tij}] + \mu[L_{tk}] + \varepsilon_{ti} \qquad [1]$$

where:

I_{ti} = annual income from work observed for individual i in year t

$[P_{ti}]$ = observed personal characteristics in year t for individual i that can vary over time (e.g., marital or fertility status, educational attainment)

$[P_i]$ = observed personal characteristics for individual i that do not vary over time (e.g., gender and country of birth)

$[UP_i]$ = unobserved personal 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)

$[N_{tj}]$ = observed economic characteristics of neighbourhood(s) j where individual resides during year t and three years prior (e.g., shares of low-income neighbours)

$[L_{tk}]$ = observed characteristics of local labour market k in which the individual resides during t (e.g., mean earnings of all workers)

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

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

[3] There are several local labour market areas specified within each metropolitan area in Sweden.

ε_{ti} = a random error term with statistical properties discussed below

i = individual

j = neighbourhood

k = metropolitan labour market

t = year

As amplified below, we will alter the specification of the [N_t] variables, experimenting with different temporal structures.

In this study we operationalize “neighbourhood” as a “SAMS,” which is defined by Statistics Sweden as a relatively small, homogeneous area taking into account housing type, tenure and construction period. We recognize that scale of neighbourhood chosen may affect results, as found by Buck (2001), Bolster et al. (2004), van Ham and Manley (2010), and Andersson and Musterd (2010). The last found strongest Swedish neighbourhood effects at the 100 meter squared scale, but the effects were nearly as strong at the SAMS level (which is on average 20 hectares). We chose the SAMS for our analyses because of their housing homogeneity and their greater likelihood of meeting minimum population criteria, even though they vary somewhat in terms of population within and among the three metropolitan areas (ranging from around 500 people in the smallest SAMS in Gothenburg to approximately 5000 people in the largest SAMS in Stockholm).

The three metropolitan areas we focus on also differ in their economic history and educational profiles. Malmö and Gothenburg used to represent rather typical Fordist-style industrial economies but have undergone rapid de-industrialization. However, Gothenburg keeps its key position as the country’s main port city and as the focal point of the Swedish car and truck manufacturing industry, and Malmö has seen an economic revival since the construction of the Öresund Bridge (connecting Malmö and Copenhagen) in the year 2000. Stockholm still represents a more developed post-industrial, service-dominated economy. In terms of income, a larger proportion of Stockholm residents earns high incomes compared to Malmö and Gothenburg residents. Cross-city comparisons must be interpreted with caution, but we will present results per city to see whether there is a robust general pattern of timing effects across these cities[4].

We focus on the income mix of neighbourhood as the [N_t] variable of importance for three reasons. First, this is the aspect of neighbourhood 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 U.S. 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 neighbourhood income mix was more strongly correlated with subsequent levels of individual incomes than neighbourhood mix defined by education, ethnicity, family status, or housing tenure (Andersson et al. 2007). As our measure of neighbourhood 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” neighbours. In the database we have constructed we observe these neighbourhood conditions annually from 1991 to 2006. Because of space restrictions, in this paper the empirical focus will be on exposure to shares of low income neighbours in various temporal patterns. Our prior work (self-citation redacted) has shown that variations in the low-income composition of Swedish neighbourhoods are much more strongly related to individuals’ subsequent earnings than variations in the high-income share. We will employ in all models the percentages of high-income neighbours experienced in each of the prior four years individually so that our key dummy variable measures of exposure to low-income

[4] City is defined as the more or less continuously built-up core area of each metropolitan region. For Stockholm this includes the municipalities of Stockholm, Solna and Sundbyberg, for Gothenburg, the municipalities of Gothenburg and Mölndal, and for Malmö: Malmö municipality.

neighbours (explained below) can be interpreted unambiguously using the share of middle-income neighbours as omitted reference category.

As for the control variables in our models, we operationalize the observed personal characteristics of individuals $[P_t]$ and [P] with a set of variables describing their demographic and household characteristics, educational attainments, immigrant status, and features of their employment status during the period that will affect their income but are likely not related to neighbourhood context (such as parental leave, illness, or attending school). We operationalize $[L_t]$ with the mean labour income for prime-age workers during year t in the metropolitan area in which the individual resided during the period in question. See Table 2a for complete listing of these variables and their descriptive statistics, by metropolitan area and gender. It might be interesting to split the models by tenure category as well, because effects appear to differ for owners and renters (see Oreopolous, 2003; Van Ham and Manley 2010); however, the required data were not available in our datasets.

We cannot, of course, directly measure [UP]. Indeed, the aforementioned geographic selection bias occurs when this unobserved heterogeneity is not statistically controlled and proves correlated with the [N] variables, producing thereby a violation of the standard independence assumptions for ε_{ti}. However, the panel nature of our data provides a well-known vehicle for overcoming part of this problem with a proxy for time-invariant unobservables: fixed-effect models (Galster 2008). The fixed effects model assumes that each individual has a particular intercept differing from the mean by some constant value, i.e. α_i, which we would argue serves as a proxy for the [UP] terms. Thus, [1] can be rewritten as a fixed effects model:

$$\ln(I_{tijk}) = \alpha_i + \beta[P_{ti}] + \gamma[P_i] + \theta[N_{ti\,j}] + \mu[L_{tik}] + \varepsilon_{ti} \quad [2]$$

We recognise that the fixed effect model does not eliminate potential bias arising from time-varying unobservables. In particular, our measures of length of exposure to neighbourhood attributes may be correlated with a person's age and education, because rate of income growth and (unobserved) *expectations* of such growth that may affect residential mobility behaviours may be related to these variables. We attempt to control for this by adding interaction variables between education and age, thereby permitting different income-age profiles by educational level.

We do not explicitly model selection into employment but treat this as an implicit intervening variable in our model of neighbourhood effects, in the same way as we treat hours worked and the wage per hour. These are regarded as behind-the-scenes aspects of labour force activity that may be affected by neighbourhood and ultimately will end up as an income effect. In this paper we do not look into the "black box" of all potential intervening variables.

Strategy for Estimating Temporal Variations in the Effect of Neighbourhood Income Mix

Our strategy for investigating the degree to which the impacts of neighbourhood income mix varies across time, duration and cumulative amount of exposure involves two prongs. The first involves creating a set of dummy variables, [N%LOW], which describe the timing of the individual's exposure to a particular minimum percentage (expressed as a dichotomous condition) of one or other income group in their neighbourhood over each of the prior four years (i.e., the current year plus the three previous ones).[5] The set consists of 15 mutually exclusive and exhaustive dummy variables denoting alternative sequences of whether the particular minimum percentage of group X was absent (=0) or present (=1) during a given year for the individual. For a four-year period (including the year contemporaneous to when earnings are measured: year0), there are 16 possible combinations of patterns. One

[5] We recognize that four years is arbitrary and represents a compromise: longer periods place higher requirements on how many years we must compute [N] and thus the number of permutations of patterns possible; shorter periods reduce the length of duration we can test for.

combination (which will serve as the reference category excluded from the regression) is: year0=0, year1 =0, year2 =0, year3= 0 (henceforth designated 0-0-0-0). This is the case where at no time during the past four years the individual has been exposed to neighbourhood condition X. The other extreme case is when minimum percentage X was present all four years (1-1-1-1). Every other possible pattern of four zeros and ones is denoted by a separate dummy variable corresponding to that pattern. Descriptive statistics of these 15 patterns are shown in Table 2b, here just presented for the extreme values of X, those who experienced exposure to a neighbourhood with at least 50% low incomes.

[Table 2b on webpage y]

We experimented with a variety of values for X, although in this paper we report results obtained with X specified as 50 per cent for the percentage of low-income males in the neighbourhood. We emphasize that our conclusions about the *temporal nature* of neighbourhood effects are not sensitive to this specification of X. The *magnitude* of estimated neighbourhood effects for any given temporal pattern is, however, sensitive to the choice of X; the ones we report appear to be the values associated with very large (and most statistically and substantively significant) magnitudes. In Table 3 we present, by gender, and just for the year 2000, the share of residents in each of the three cities that qualifies for exposure to different levels of poor residents in their neighbourhood. Notice that exposure to a high share (above 50%) of poor neighbours is rather moderate in Stockholm and high in Malmö.

[Table 3 approx. here]

Careful interpretation of coefficients of these [N%LOW] dummies provides the answers to our research questions. Consider the following illustrations:

Example 1 Timing of Exposure and Subsequent Effects: Coefficient of (1-0-0-0) dummy can be compared to those of (0-1-0-0), (0-0-1-0), and (0-0-0-1) to see if a one-year exposure has any effect and, if so, if it is strongest after a one-, two-, or three-year lag. This logic can be extended to look at one- and two-year lag timing effects for two-year durations of exposure: compare coefficients of (1-1-0-0), (0-1-1-0), and (0-0-1-1). Finally, one can examine one-year lag timing effects for three-year durations of exposure by comparing coefficients of: (0-1-1-1) and (1-1-1-0).

Example 2 Duration of Exposure Effects: Coefficient of dummy (1-0-0-0) can be compared to coefficients of three other dummies (1-1-0-0), (1-1-1-0), and (1-1-1-1) to see effect of exposure to characteristic X only contemporaneously, after one year, after two continuous years, and after three continuous years, respectively.

Example 3 Time Decay Effects: The same comparisons described in example 1 above can be interpreted as measures of time decay of effect once exposure ceases. Comparison of the coefficients of (1-0-0-0) and (0-0-0-1) dummies will reveal the degree to which the contemporaneous impact of a single-year exposure has attenuated after three years.

The second prong of our strategy involves a test for *cumulative* exposure. We recognize that the aforementioned duration tests can also be considered tests of cumulative exposure insofar as particular type of exposure embodied in X is being varied across a number of consecutive years. However, this approach has the disadvantage employing only one degree of exposure intensity: X. We therefore operationalize a more general cumulative exposure measure that does not use either a dichotomous measure of neighbourhood condition or require continuous exposure to such. Our measure of cumulative exposure to low-income neighbours is the sum of the percentages of low-income male neighbours in the individual's neighbourhood over each of the years t=0, 1, 2, and 3. Note that when our measure of cumulative intensity of exposure equals 200 or more it is not equivalent to the (1-1-1-1) dummy variable above, because it does not necessarily imply that the exposure to the particular income group in question equals (or exceeds) 50% *each* of the four years, as the dummy formulation does, only that it *averages* 50% per year.

IV. Findings

We estimated parameters of our fixed effects models using STATA's GLS estimator and report robust standard errors. We estimated equation [2] separately for Malmö, Gothenburg, and Stockholm. Because our earlier work (self-citation redacted) has suggested its importance, we further stratified our estimations by gender.

A representative example of the results for the control variables is presented in Appendix Table 1 (webpage z). We selected Stockholm, but findings for the other two metropolitan regions allow for similar conclusions, except as noted below. We present results for both genders, for those who experienced at least 50 per cent low incomes in their neighbourhood in year t. The control variables of time-varying personal characteristics perform as expected. Incomes are greater for those who are not currently studying or took advantage of the generous Swedish benefits for sick leave or parental leave. Those who are phasing into retirement or who have an increase in the number of children under age 7 see lower incomes. For males, college education was associated with higher incomes, though less so for older cohorts in Stockholm and Gothenburg. For females, having 13-14 years of education was associated with lower income unless one was in an older age cohort, in Stockholm and Gothenburg. Local labour markets with greater average incomes subsequently convey analogous gains to individual residents, presumably by its association with expanding local employment opportunities. All the subsequent results regarding neighbourhood income mix variables should be interpreted in the context of models containing these control variables.

Before we interpret the neighbourhood income mix findings, we would stress that the contemporaneous effects should be evaluated with caution, since they might be influenced by potential endogeneity problems, i.e., a result of reversed causality. Individual income for year t is measured Dec. 31st of that year and represents the labour earnings accumulated during that year; neighbourhood income mix of year t is also measured as of Dec. 31st but only represents that moment's mix. Thus, cause and effect for year t can be blurred if the person moved into a quite different income mix near the end of the year due to some change in income earlier during the year. This ambiguity is not present with the lagged income mix variables, however, because income earned during year t can only be the effect of income mix experienced during years t-1, t-2, etc. and not *vice versa*, whether moves occurred during those earlier years or not.

Our final introductory comment is that space constraints require that we focus only on the estimated relationships for the low-income share in the neighbourhood.

Timing of Exposure and Magnitude of Effects

Figure 1 shows the estimated coefficients from selected dummy variables operationalizing the neighbourhood income mix, as described above, indicating a one-time (i.e., year-long) exposure, but varied in timing from current year to three years before. This and subsequent figures only report coefficients that are statistically significant at $p<.05$, otherwise they are plotted as zero. Figures 2 and 3 present similar portraits of coefficient magnitudes at different lags, but for exposures of two and three consecutive years, respectively.

We would remark about three salient patterns revealed consistently in these figures. First, in all three metropolitan areas, for both genders, regardless of duration of exposure, exposure to a relatively high share of low-income neighbours (instead of middle-income ones) has in most cases a statistically significant negative impact on an individual's labour income. Second, this impact is generally larger if the exposure occurred more recently, even disregarding the ambiguous estimates for contemporaneous values. Third, this effect is stronger for Stockholm than for Gothenburg and Malmö.

[Figures 1, 2, 3 about here]

Duration of Exposure and Magnitude of Effects

Figure 4 shows the estimates of the neighbourhood low-income dummy variables for a comparison involving an exposure that has persisted from one to three years previously through the current year. First, for Stockholm and Gothenburg, increasing duration of exposure produces larger negative neighbourhood effects, as one would expect. For Malmö there appears to be an initial flat negative effect of exposure to more than 50 per cent low-income neighbours. For all metropolitan areas there also appear to be 'saturation levels' with regard to duration of exposure. After an initial increase in the size of the negative effect, the effect slowly decreases again after two or three years of consecutive exposure, yet remains significantly negative (except for females in Malmö). Both males and females evince roughly similar patterns (Table 4).

[Figure 4 about here]

[Table 4 about here]

Stockholm's low-income percentage coefficient was -.31 for males who experienced only a contemporaneous exposure, but increased in absolute magnitude to a value of -.35 when males experienced two years of continuous exposure and -.41 for three years continuous exposure. For Gothenburg these figures were -.25, -.28, and -.29. Malmö was the exception, with a negative coefficient of -.19 evinced for the first two types of exposure, and -.17 evinced for the third type. After three, or in the case of Malmö two, years of consecutive exposure, the exposure effect apparently reached its limit. The same generally applied for females; in Gothenburg the limit was reached with a year less exposure than it was reached by males. Table 4 also shows the calculated percentages in income reduction that would result due to these various durations of exposure, relative to those who were not exposed to these low income neighbourhoods in any of the four years, all other things being equal. The income differences are substantial, ranging from almost 18 per cent for contemporaneously exposed male individuals in Malmö, to 33 and 34 per cent for females and males, respectively, in Stockholm who had been exposed to low income neighbourhoods during three consecutive years and continuing through the current year.

Cumulative Exposure and Magnitude of Effects

Table 5 summarizes the results for our variables operationalizing the cumulative exposure as the four-year sum of percentages of low-income neighbours. All the coefficients are highly statistically significant for both genders and all metropolitan areas, and in each area the values for males and females are statistically different, though not always with the same relative magnitudes. In interpreting the sizes of these coefficients in comparison to those reported in Figures 1-4 above, it should be recalled that those in Table 5 will appear considerably smaller because: (1) they represent four-year summations of exposures, not measures of an annual exposure and (2) they are estimated over the entire range of potential neighbourhood income mixes, not extreme dichotomous values as the prior variables. Nevertheless, these estimates are impressive.

[Table 5 approx. here]

The average coefficient for males across the three metropolitan areas is -.00278, meaning that a male who has experienced a ten percentage point-higher share of low-income neighbours (and an equivalently lower share of middle-income neighbours) *on average* for the last four years would be predicted to have an 11.1 per cent lower income this year than an otherwise-identical male. The equivalent estimate for females is 7.7 per cent.

Persistence of Effects once Exposure Has Changed

The final question we intend to answer is whether exposure to neighbourhoods with more than 50 per cent low-income neighbours creates a persistent, durable effect on incomes earned, or one that quickly decays over time after the initial exposure. If the latter, does the rate of time decay depend on the duration of exposure?

We refer to the same set of figures presented before (Figures 1, 2, and 3), but focus on Stockholm and Gothenburg only, since Malmö figures were not statistically significant. Three observations are of relevance here. First, those who are exposed to such a low income neighbourhood generally see a substantial decrease of the negative effect after exposure has stopped. Secondly, recovery seems to be somewhat stronger for males than for females. Thirdly, in both cities, both for males and females, a (small) negative effect persists, even when the exposure was three years before the current year. For exposure to high percentages of low-income neighbours, after three years of non-exposure about three quarters (for males) to two-thirds (for females) of the initial effect has disappeared. Stockholm females continue to carry a substantial amount of the income effect of an earlier exposure to a neighbourhood environment years after such exposure has ceased. The greatest time decay was experienced by females who were exposed to high percentages of low-incomes during two consecutive years.

The duration of exposure seems to have a systematic additional effect on the durability of impact. This can best be detected by investigating those exposure situations with different duration of exposure, but not in the current year, and then compare the situation of one year exposure three years ago, with one year exposure two years ago, with one year exposure one year ago with each other; the same can be done for two consecutive years of exposure two and three years ago, and one and two years ago. Table 6 shows the estimates.

[Table 6 approx. here]

With a few small exceptions, the pattern is clear: when exposure has been longer ago, the attenuation of initial impact is larger. However, there is a lasting effect as well. There is no example of full recovery from initial exposure to low-income neighbours within the span of four year we investigated, even when exposure has been short and relatively long ago.

V. Discussion

Neighbourhood income mix clearly matters for the future income-earning prospects of working-age Swedes who reside in the nation’s three biggest metropolitan areas. This finding is consistent with prior work and robust to a variety of econometric model specifications (self-citation redacted). The current paper extends this work by revealing that more recent, sustained and cumulative exposures to neighbourhood income mix context create larger impacts on individuals’ incomes than episodic or lagged exposures, and that (though decaying over time after exposure changes) some impacts persist after several years.

We cannot, of course, be definitive about which of the aforementioned mechanisms of neighbourhood impact might be predominantly responsible for producing this relationship. Indeed, we think it probable that multiple causal processes are in operation and that what we observed is some amalgam or “net” relationship produced by the interaction of multiple mechanisms, Nevertheless, it is instructive to draw some inferences based on the foregoing findings; cf. Table 1. Both stigmatization and lack of accessibility that may be associated with neighbourhoods of low-income concentrations should in theory produce the fast-acting yet not very durable effects as those observed here. Analogously, the persistence of some effect after several years after changed exposure (especially for women) is consistent with how exposure to violence and disorder in such neighbourhoods is thought to operate to discourage labour force participation and psychological health. Such disorder used to be rather rare in Sweden's poorest neighbourhoods but there has been growing rates of such problems over the last decade. It is widely believed that neighbourhoods having a concentration of low-income residents are indeed stigmatized in the three metropolitan areas and few doubt that stigmatization poses a real problem for people residing in the poorest housing estates, often located in the urban periphery.

VI. Conclusions, Implications, Caveats and Future Directions

Our study probed a hitherto underdeveloped realm within the burgeoning field of neighbourhood effects: temporal patterns between exposure and outcomes. Our results strongly confirmed the importance of this temporal realm and the fact that we found these results in all three rather different metropolitan areas, adds to the robustness of the findings. A series of empirical conclusions can be drawn that contribute to societal debate on neighbourhood social mix. First, for both genders in all three Swedish metropolitan areas investigated, we have found that exposure to a neighbourhood with at least 50 per cent low-income male neighbours (and a correspondingly lower share of middle-income ones) has a significant negative impact upon an individual's income from work. Second, this impact is larger when the exposure occurred more recently, is of longer duration, and/or was greater in cumulative intensity. Third, prior exposure to low-income neighbours has rapidly diminishing impacts on males once exposure has ceased; three quarters of the effect disappears within three years of cessation of exposure. However, even though negative effects of exposure decrease over time after exposure has stopped, there are persistent and significant effects for both genders after four years.

In closing, we would emphasize several caveats to our work. First, our results are based on data from Swedish metropolitan areas and need not necessarily apply in other national contexts, with their potentially distinctive housing markets, social structures, class inequality, and political-economic features. Second, we have focused on only one aspect of neighbourhood context (income mix) and one outcome (individual labour incomes); other contextual variables and/or outcomes may produce different relationships than those produced here. Third, though we have suggested that our findings are suggestive at particular neighbourhood effect mechanisms at work behind the scenes, we think it likely that multiple mechanisms may be operative and that different mechanisms may predominate when different neighbourhood contexts and/or individual outcomes are investigated. Fourth, on a methodological point, we must say that although the panel structure of our data allowed for applying fixed effects models, which helps to overcome potential bias due to unobserved variables that do not change over time, there may be some remaining selection bias due to unobserved individual variables that are changing over time. Our additional analyses indicated, however, that including education-specific age-income profiles had no effect on the neighbourhood coefficients, suggesting that time varying unobserved expectations of income growth were not strongly correlated with mobility behaviour and thus were not a large source of bias. We intend to continue our efforts in search of alternative ways to address these issues (see Couch and Placzek, 2010 for a recent example of a combined fixed effects and time trends analysis that may offer new perspectives). Interesting recent work by Bayer, Ross and Topa (2008) in which they compared block level and wider 'block-level group' information may also provide new avenues out of selection effect bias in neighbourhood effect research. The required geographical data were not available in the dataset we applied, but their insights offer opportunities for future research. Fifth, we recognize that our results may be influenced by endogeneity bias, wherein those with different incomes may sort across neighbourhoods with different income mixes as well as be influenced by the mix once they are residing there (Hedman, forthcoming). Last, our selection of those who remained within Sweden's three metropolitan areas might limit the generality of our findings.

For future research it would also be revealing to probe temporal effects of additional dimensions of neighbourhood context beyond income mix and additional sorts of individual outcomes besides incomes. Exploring why there are distinctive gender differences in temporal patterns of exposures and outcomes should also be on the agenda, as well as further stratifications based on age and tenure. We also would advise exploring potential threshold effects associated with particular critical masses of low-income (or high-income) neighbours, because identification of such thresholds holds crucial implications for the precise formulation of neighbourhood social mix policy (Galster, 2007a, b).

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Figure 1. Timing Effects: Estimated Coefficients for Exposure to 50%+ Low-Income Neighbourhoods; One-Year Only Exposure, by Various Lags

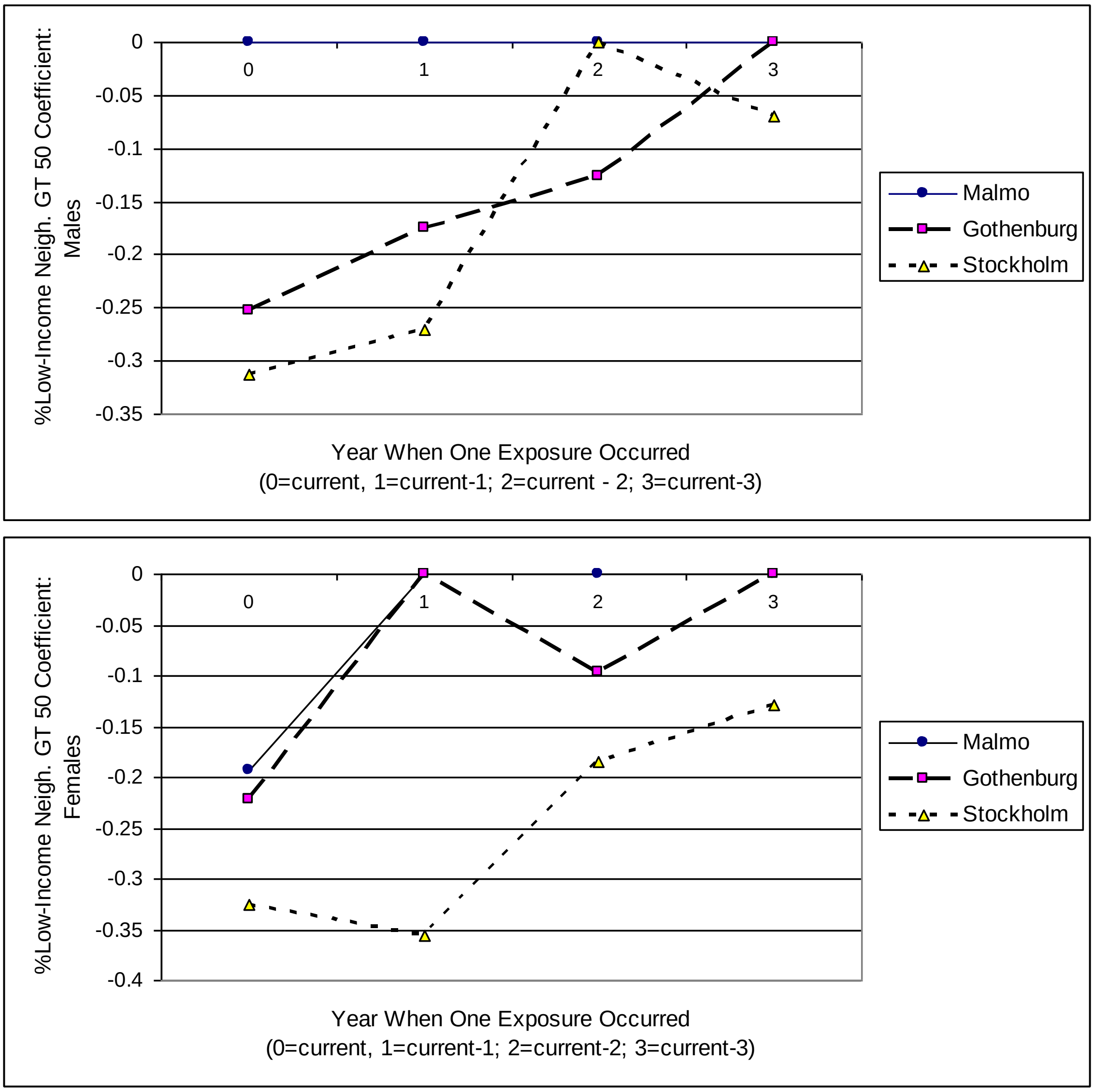


Figure 2. Timing Effects: Estimated Coefficients for Exposure to 50%+ Low-Income Neighbourhoods: Two-Year Consecutive Exposure, by Various Lags

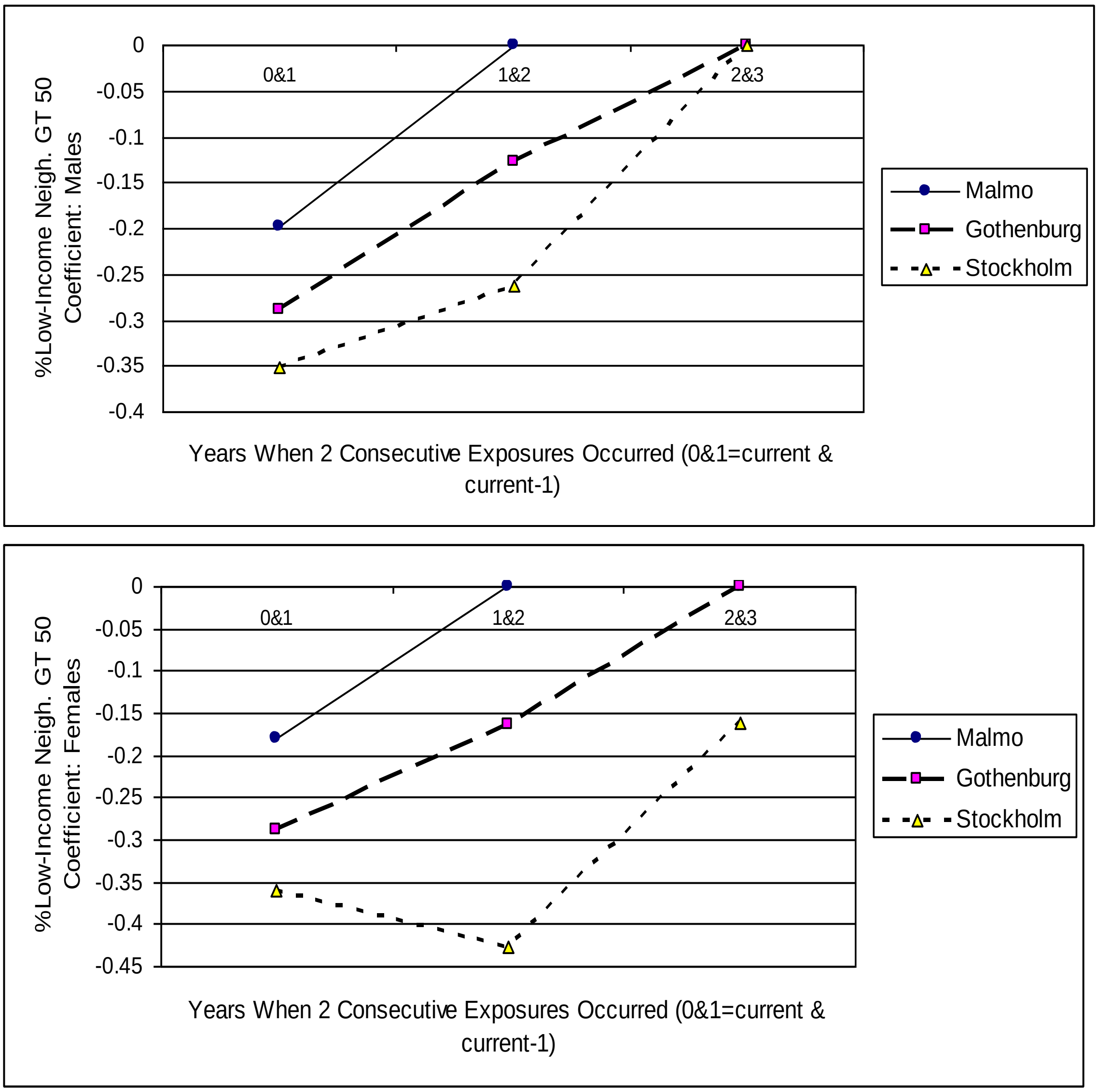
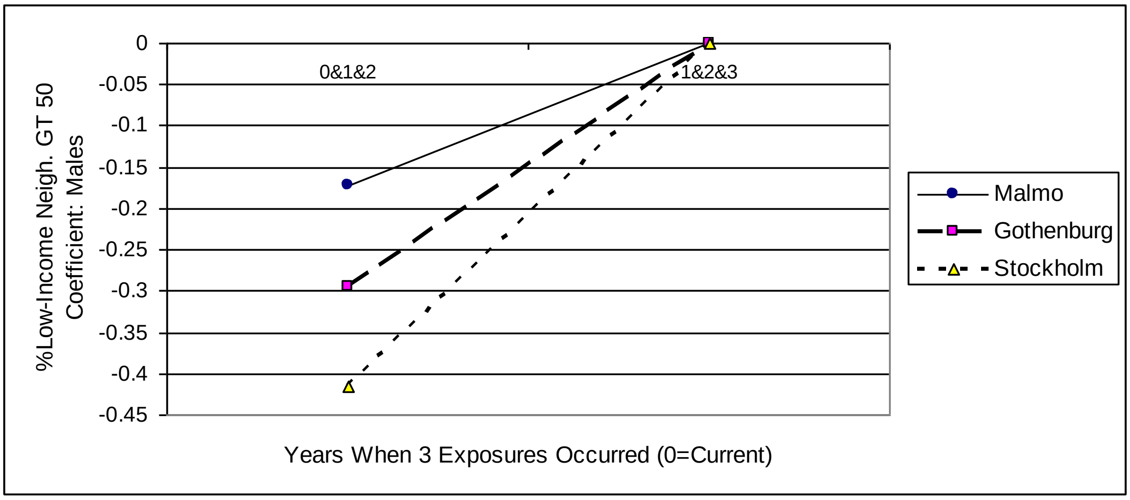


Figure 3. Timing Effects: Estimated Coefficients for Exposure to 50%+ Low-Income Neighbourhoods, Three-Year Consecutive Exposure, by Various Lags



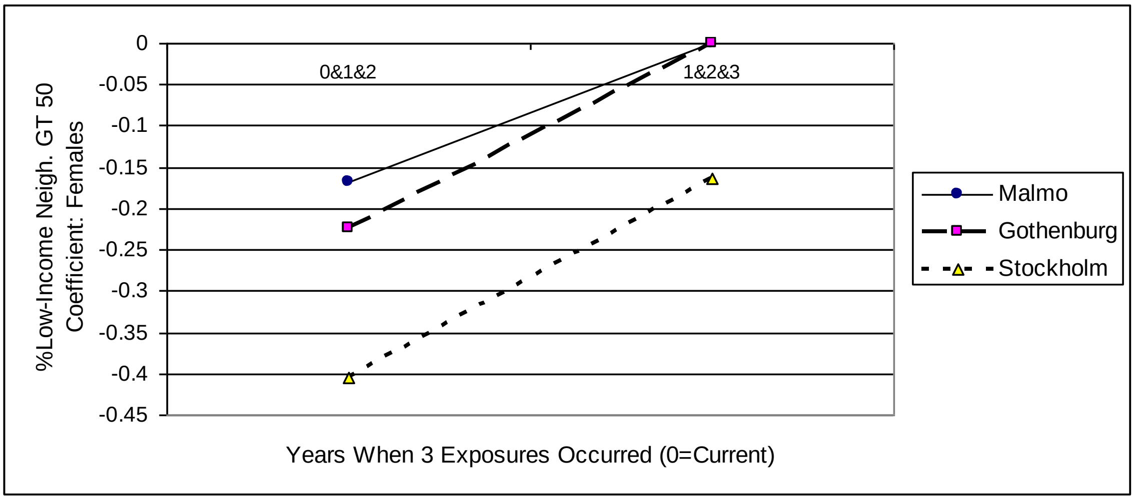
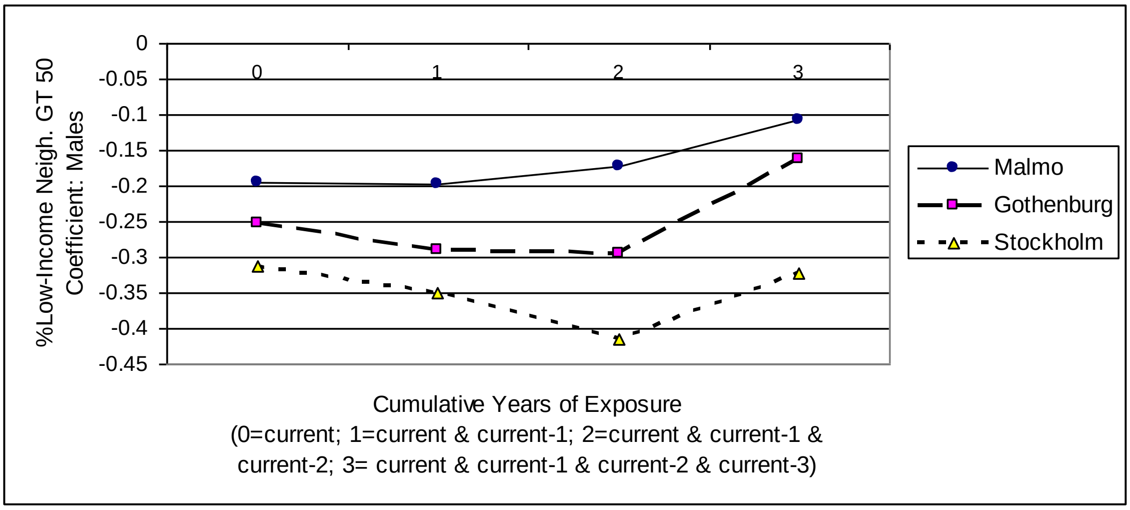


Figure 4. Duration Effects: Estimated Coefficients for Exposure to 50%+ Low-Income Neighbourhoods, Contemporaneous and Varied Numbers of Years of Consecutive Exposure



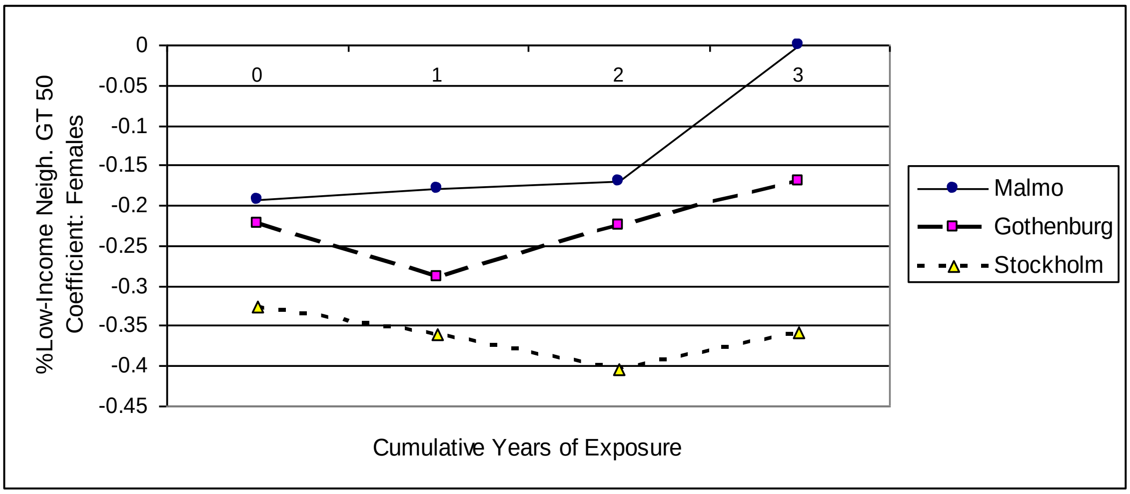


Table 1. Summary of Theoretical Predictions Regarding Temporal Aspects of Neighbourhood Effects Mechanisms

Mechanism	Effect occurs quickly	Effect stronger if continuous/cumulative	Effect is durable
Socialization	no	yes	No
Social Networks	no	yes	No
Exposure to Violence	yes	yes	Yes
Stigmatization	yes	yes	No
Institutional Resources	no	yes	?
Job Accessibility	yes	yes	No

Note: ? signifies that answer depends on starting context and direction of context change

Table 2a Descriptive Statistics for Males and Females in Stockholm, Gothenburg, and Malmö

	Stockholm				Gothenburg				Malmö			
	Males		Females		Males		Females		Males		Females	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
# Children Under Age 7	1.066	2.283	1.180	2.359	1.099	2.317	1.181	2.374	1.020	2.246	1.106	2.311
Marital Status: Coupled/Married	0.497	0.500	0.502	0.500	0.565	0.496	0.584	0.493	0.547	0.498	0.560	0.496
Pre-Retirement Status	0.054	0.226	0.068	0.253	0.066	0.247	0.095	0.293	0.055	0.227	0.080	0.271
Parental Leave During Year	0.206	0.404	0.304	0.460	0.206	0.404	0.300	0.458	0.180	0.384	0.276	0.447
Sick Leave During Year	0.097	0.296	0.172	0.378	0.109	0.312	0.186	0.389	0.112	0.315	0.182	0.386
Student During Year	0.037	0.189	0.067	0.251	0.037	0.189	0.065	0.247	0.043	0.203	0.072	0.258
12 Years of Education	0.164	0.370	0.157	0.363	0.146	0.354	0.151	0.358	0.147	0.354	0.145	0.352
13-14 Years of Education	0.178	0.382	0.209	0.407	0.170	0.376	0.184	0.387	0.150	0.357	0.174	0.379
15+ Years of Education	0.239	0.427	0.247	0.431	0.205	0.403	0.212	0.408	0.149	0.356	0.163	0.370
12 Years Education * Age	5.337	12.445	4.777	11.462	4.781	11.931	4.519	11.103	4.821	11.974	4.398	11.064
13-14 Years Education * Age	5.520	12.234	6.667	13.362	5.233	11.939	5.822	12.648	4.641	11.390	5.512	12.402
15+ Years Education * Age	8.010	14.695	8.152	14.679	6.083	13.851	7.009	13.950	4.999	12.253	5.338	12.448
Changed from Couple to Single Prior Year	0.020	0.139	0.019	0.137	0.020	0.138	0.019	0.135	0.020	0.141	0.019	0.138
Changed from Single to Couple Prior Year	0.030	0.171	0.027	0.161	0.027	0.162	0.024	0.152	0.027	0.164	0.024	0.152
Mean Local Labour Market Earnings for People Aged 20-64 (in 100 SWE kroner)	1961.057	313.117	1961.012	313.125	1772.194	295.404	1772.032	295.215	1609.052	249.274	1608.895	249.026
Sum of % Low-Income Neighbors Experienced over Prior 4 Years	125.657	35.427	124.603	34.286	128.708	53.007	125.336	50.452	154.285	60.147	150.395	59.013
Sum of % High-Income Neighbors Experienced over Prior 4 Years	149.637	48.459	151.375	48.087	137.592	59.454	141.161	59.146	104.708	58.324	108.502	58.252
Perc. Low-Income Neighbors contemporary	31.211	9.493	30.967	9.115	31.553	13.984	30.737	13.221	38.417	15.834	37.474	15.372
Perc. Low-Income Neighbors contemp - 1	31.344	9.445	31.085	9.054	32.003	14.092	31.163	13.332	38.538	15.940	37.560	15.503
Perc. Low-Income Neighbors contemp - 2	31.479	9.418	31.209	9.013	32.401	14.126	31.545	13.366	38.638	15.993	37.649	15.584
Perc. Low-Income Neighbors contemp - 3	31.622	9.458	31.342	9.030	32.750	14.130	31.891	13.370	38.692	15.993	37.712	15.614
Perc. High-Income Neighbors contemporary	37.813	13.046	38.239	12.848	35.093	15.653	35.972	15.477	26.566	15.396	27.497	15.282
Perc. High-Income Neighbors contemp - 1	37.476	12.838	37.912	12.661	34.598	15.611	35.496	15.458	26.271	15.311	27.229	15.217
Perc. High-Income Neighbors contemp - 2	37.253	12.644	37.691	12.483	34.161	15.562	35.063	15.433	26.045	15.238	27.005	15.167
Perc. High-Income Neighbors contemp - 3	37.095	12.450	37.532	12.346	33.739	15.522	34.630	15.413	25.826	15.167	26.771	15.122
Dependent Variable: ln (Income from Work. in 100 SWE kronor)	6.831	2.628	6.649	2.496	6.736	2.666	6.412	2.626	6.314	2.909	6.097	2.831
N (across all sites and genders=467.266)	124269		127461		73304		74270		33555		34407	

Table 2b Rates of exposure to 50%+ (1=yes; 0=no) low-income neighbours in year t, and 3 prior years

	Stockholm				Gothenburg				Malmo			
	Males		Females		Males		Females		Males		Females	
Exposure Pattern	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
1111	0.0050	0.0708	0.0043	0.0652	0.0163	0.1267	0.0139	0.1171	0.0442	0.2055	0.0432	0.2033
1110	0.0015	0.0385	0.0012	0.0344	0.0045	0.0670	0.0034	0.0583	0.0087	0.0928	0.0070	0.0833
1100	0.0018	0.0418	0.0013	0.0365	0.0052	0.0721	0.0040	0.0630	0.0094	0.0967	0.0075	0.0860
1101	0.0001	0.0079	0.0000	0.0052	0.0006	0.0239	0.0004	0.0195	0.0017	0.0415	0.0014	0.0371
1000	0.0011	0.0324	0.0005	0.0223	0.0061	0.0780	0.0044	0.0663	0.0112	0.1050	0.0087	0.0928
1001	0.0000	0.0066	0.0000	0.0040	0.0005	0.0232	0.0004	0.0192	0.0010	0.0311	0.0006	0.0253
1010	0.0000	0.0055	0.0000	0.0036	0.0003	0.0170	0.0002	0.0130	0.0012	0.0340	0.0011	0.0334
1011	0.0001	0.0074	0.0000	0.0048	0.0005	0.0234	0.0003	0.0176	0.0012	0.0341	0.0007	0.0262
0111	0.0018	0.0424	0.0013	0.0366	0.0040	0.0628	0.0027	0.0522	0.0080	0.0888	0.0067	0.0813
0110	0.0004	0.0208	0.0002	0.0155	0.0014	0.0370	0.0009	0.0307	0.0027	0.0518	0.0019	0.0436
0101	0.0000	0.0058	0.0000	0.0036	0.0003	0.0169	0.0001	0.0107	0.0007	0.0261	0.0005	0.0220
0100	0.0006	0.0239	0.0003	0.0174	0.0028	0.0532	0.0021	0.0456	0.0049	0.0697	0.0039	0.0627
0011	0.0024	0.0484	0.0017	0.0409	0.0049	0.0701	0.0034	0.0584	0.0091	0.0949	0.0074	0.0859
0010	0.0006	0.0252	0.0003	0.0182	0.0030	0.0546	0.0021	0.0461	0.0046	0.0675	0.0035	0.0590
0001	0.0032	0.0564	0.0021	0.0462	0.0075	0.0865	0.0053	0.0726	0.0122	0.1100	0.0100	0.0997

Note: the 1=yes/0=no sequence corresponds to exposures during years t, t-1, t-2, t-3, respectively

Table 3 Percentage of sample exposed to different percentages of low-income neighbours in 2000

	Gothenburg		Malmö		Stockholm	
2000 % low	Males	Females	Males	Females	Males	Females
GE 0	100	100	100	100	100	100
GE 10	99.8	99.7	100.0	100.0	100.0	100.0
GE 20	79.2	78.2	91.8	91.5	88.0	87.8
GE 30	44.3	42.3	63.8	61.8	45.3	44.6
GE 40	19.9	17.8	36.4	33.2	11.4	10.7
GE 50	9.4	7.7	21.2	18.5	4.0	3.4
N	73304	74270	33555	34407	124269	127461

Table 4 Fixed effect model estimates of impact of exposure to neighbourhoods with > 50% low income males with various durations of exposure (1=exposed that year)

	Males				Females		
	B	S.E.		% income loss [a]	B	S.E.	% income loss
Stockholm							
1000	-0.31185	0.06214		-26.8	-0.32325	0.08116	-27.6
1100	-0.35192	0.05519		-29.7	-0.36044	0.06452	-30.3
1110	-0.41786	0.05923		-34.2	-0.40327	0.06816	-33.2
1111	-0.32401	0.04985		-27.7	-0.35786	0.05797	-30.1
Gothenburg							
1000	-0.25303	0.03328		-22.4	-0.22123	0.03800	-19.8
1100	-0.28924	0.04016		-25.1	-0.28881	0.04425	-25.1
1110	-0.29448	0.04431		-25.5	-0.22353	0.04854	-20.0
1111	-0.16127	0.04432		-14.9	-0.17009	0.04773	-15.6
Malmö							
1000	-0.19500	0.03706		-17.7	-0.19263	0.03923	-17.5
1100	-0.19773	0.04435		-17.9	-0.17929	0.04831	-16.4
1110	-0.17175	0.04847		-15.8	-0.16885	0.05251	-15.5
1111	-0.10793	0.04831	*	-10.2	-0.08913	0.05192	ns

all estimates ρ< 0.001 unless stated otherwise; * p<0.05; ns=not significant
[a] percentage lower income due to exposure to neighbourhood with > 50% low incomes by duration relative to those who are not exposed to neighbourhoods with > 50% low incomes in any of the four years (0000), all other things being equal
Note: the 1=yes/0=no sequence corresponds to exposures during years t, t-1, t-2, t-3, respectively

Table 5 Fixed effect model estimates of impact of cumulative sum of exposure over four years to low income neighbourhoods

	Sum of % low income in Year t, t-1, t-2, t-3 [t runs from 1994-2006]	
	B	S.E.
Stockholm		
Males	-0.00302	.00025***
Females	-0.00251	.00027***
Gothenburg		
Males	-0.00241	.00021***
Females	-0.00128	.00022***
Malmö		
Males	-0.00292	.00030***
Females	-0.00197	.00032***

*** ρ< 0.001

Table 6. Fixed effect model estimates of impacts of exposure to neighbourhoods with > 50% low incomes, with various durations of exposure, no current exposure.

	Males			Females			Males	Females
	B	S.E.		B	S.E.		% income loss	% income loss
Stockholm								
0001 (t-3)	-0.06947	0.03092	*	-0.12883	0.03531	***	-6.7	-12.1
0010 (t-2)	-0.07312	0.07253		-0.18119	0.09479	*		-16.6
0100 (t-1)	-0.27172	0.08260	***	-0.35499	0.10507	***	-23.8	-29.9
0011 (t-2, t-3)	-0.02157	0.03871		-0.16125	0.04443	***		-14.9
0110 (t-1, t-2)	-0.26056	0.09060	**	-0.42463	0.11319	***	-22.9	-34.6
0111 (t-1, t-2, t-3)	0.00594	0.04481		-0.16335	0.05142	***		-15.1
Gothenburg								
0001 (t-3)	0.00359	0.02731		-0.02168	0.02953			
0010 (t-2)	-0.12578	0.04309	**	-0.09545	0.04842	*	-11.8	-9.1
0100 (t-1)	-0.17418	0.04579	***	-0.05289	0.04884		-16.0	
0011 (t-2, t-3)	-0.02722	0.03621		-0.04826	0.03928			
0110 (t-1, t-2)	-0.12570	0.06189	*	-0.16123	0.06962	*	-11.8	-14.9
0111 (t-1, t-2, t-3)	-0.02518	0.04216		-0.04931	0.04742			

* $p < 0.05$; ** $p < 0.01$; ***$p < 0.001$; estimates for all Malmö patterns are insignificant
[a] percentage lower income due to exposure to neighbourhood with > 50% low incomes by duration relative to those who are not exposed to neighbourhoods with > 50% low incomes in any of the four years (i.e. pattern 0000), all other things being equal.
Note: the 1=yes/0=no sequence corresponds to exposures during years t, t-1, t-2, t-3, respectively

Appendix Table 1 Estimated parameters for control variables and exposure patterns to 50%+ low-income neighbours; Stockholm, by gender

	Males Stockholm				Females Stockholm			
Fixed effects (within regression)	N of obs = 1615497				N of obs = 1656993			
Group variable: personal ID	N of groups = 124269				N of groups = 127461			
	Obs per group: 13				Obs per group: 13			
R-sq: within	= 0.0891				= 0.1117			
Between	= 0.4659				= 0.4244			
Overall	= 0.3091				= 0.2829			
	F(31,124268) = 1020.31				F(31,127460) = 1262.53			
Robust	Prob > F = 0.0000				Prob > F = 0.0000			
Dependent Variable:ln (Income from Work, in 100 SWE kronor)	Coef.	Std. Err	t	P>| t |	Coef.	Std. Err	t	P>| t |
# Children Under Age 7	-0.00791	0.00097	-8.20	0.000	-0.05578	0.00109	-50.95	0.000
Marital Status: Coupled/Married	0.05785	0.00860	6.73	0.000	-0.25270	0.00871	-29.00	0.000
Pre-Retirement Status	-2.75808	0.03052	-90.36	0.000	-2.60982	0.02445	-106.74	0.000
Parental Leave During Year	0.27916	0.00516	54.12	0.000	0.18526	0.00590	31.42	0.000
Sick Leave During Year	0.08456	0.00806	10.49	0.000	0.16740	0.00559	29.94	0.000
Student During Year	-1.53975	0.01495	-103.00	0.000	-1.28521	0.01032	-124.54	0.000
12 Years of Education	0.00561	0.15740	0.04	0.972	0.16149	0.11802	1.37	0.171
13-14 Years of Education	-0.16438	0.15430	-1.07	0.287	-0.44502	0.11698	-3.80	0.000
15+ Years of Education	1.12428	0.16666	6.75	0.000	0.47151	0.12052	3.91	0.000
12 Years of Education * Age	0.00547	0.00563	0.97	0.331	-0.00035	0.00431	-0.08	0.936
13-14 Years of Education * Age	0.00998	0.00549	1.82	0.069	0.01345	0.00424	3.17	0.002
15+ Years of Education * Age	-0.02154	0.00598	-3.60	0.000	0.00103	0.00436	0.24	0.814
Changed from Couple to Single Prior Year	0.05440	0.01076	5.06	0.000	-0.09985	0.01052	-9.49	0.000
Changed from Single to Couple Prior Year	-0.03251	0.00781	-4.16	0.000	0.04422	0.00907	4.88	0.000
Mean Local Labour Market Earnings for People Aged 20-64 (in 100 SWE kroner)	0.00041	0.00001	52.75	0.000	0.00050	0.00001	65.26	0.000

Appendix 1 continued

Dependent Variable:ln (Income from Work, in 100 SWE kronor)	Coef.	Std. Err	t	P>| t |	Coef.	Std. Err	t	P>| t |
1111 Low Pattern	-0.32401	0.04985	-6.50	0.000	-0.35786	0.05797	-6.17	0.000
1110 Low Pattern	-0.41786	0.05923	-7.05	0.000	-0.40327	0.06816	-5.92	0.000
1100 Low Pattern	-0.35192	0.05519	-6.38	0.000	-0.36044	0.06452	-5.59	0.000
1101 Low Pattern	-0.21650	0.26233	-0.83	0.409	-0.19268	0.33590	-0.57	0.566
1000 Low Pattern	-0.31185	0.06214	-5.02	0.000	-0.32325	0.08116	-3.98	0.000
1001 Low Pattern	-0.58220	0.30166	-1.93	0.054	-0.33375	0.43905	-0.76	0.447
1010 Low Pattern	0.39561	0.34509	1.15	0.252	-0.32917	0.56807	-0.58	0.562
1011 Low Pattern	-0.53319	0.27935	-1.91	0.056	-0.03384	0.37662	-0.09	0.928
0111 Low Pattern	0.00594	0.04481	0.13	0.895	-0.16335	0.05142	-3.18	0.001
0110 Low Pattern	-0.26056	0.09060	-2.88	0.004	-0.42463	0.11319	-3.75	0.000
0101 Low Pattern	0.13559	0.32384	0.42	0.675	0.14317	0.40271	0.36	0.722
0100 Low Pattern	-0.27172	0.08260	-3.29	0.001	-0.35499	0.10507	-3.38	0.001
0011 Low Pattern	-0.02157	0.03871	-0.56	0.577	-0.16125	0.04443	-3.63	0.000
0010 Low Pattern	-0.07312	0.07253	-1.01	0.313	-0.18119	0.09479	-1.91	0.056
0001 Low Pattern	-0.06947	0.03092	-2.25	0.025	-0.12883	0.03531	-3.65	0.000
Percent high income	0.00530	0.00031	17.14	0.000	0.00396	0.00033	12.11	0.000
Percent high income t-1	0.00111	0.00028	3.94	0.000	0.00050	0.00030	1.67	0.095
Percent high income t-2	0.00020	0.00027	0.74	0.459	0.00007	0.00029	0.25	0.802
Percent high income t-3	-0.00304	0.00027	-11.05	0.000	-0.00051	0.00029	-1.73	0.084
constant	5.86003	0.02710	216.20	0.000	5.75321	0.02348	245.05	0.000
	sigma_u	1.79023			sigma_u	1.65270		
	sigma_e	1.48676			sigma_e	1.47549		
	rho	0.59182			Rho	0.55647		