

Childhood Trajectories of Neighbourhood Disadvantage and Educational Attainment: Towards a Longitudinal Machine-Learning Approach*

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Abstract: Prior research has shown that deprived neighbourhoods negatively impact educational attainment, including when individual and family characteristics are controlled for, but still remains unclear on the temporal dimensions of this effect. Using Swedish administrative data over 20 years, and drawing on bespoke neighbourhoods, we examine two questions on the relationship between residential context and high school graduation: 1) Does enduring exposure to disadvantaged neighbourhoods matter more than brief exposure? And 2) During which childhood period does it matter the most? We address the challenge of dynamic residential selection using inverse probability weighting within a marginal structural model of high school graduation and neighbourhood exposure. To flexibly model the complex, non-linear processes underlying residential selection, we use Bayesian additive regression trees to estimate propensity scores, providing a more flexible alternative to conventional parametric models. Findings suggest that cumulative exposure to disadvantaged neighbourhoods linearly reduces the likelihood of high school graduation, with effects varying across childhood stages. The negative effect is strongest during late adolescence and, to a lesser extent, in early childhood, with prolonged exposure reducing graduation rates by up to 34 percent. These findings indicate that both the duration and developmental timing of neighbourhood exposure are consequential for educational attainment, even in the context of Sweden's strong welfare state. More broadly, the study highlights how longitudinal approaches that integrate flexible machine-learning methods with causal inference can improve our understanding of how residential trajectories contribute to educational inequality.

Keywords: Neighbourhood effects • Childhood • High school graduation • Machine learning • Dynamic selection

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1 Introduction

The role of neighbourhoods in contributing to socioeconomic inequalities has gained considerable attention since the 1980s (Mayer/Jencks 1989; Wilson 1987). Several decades of studies have thoroughly documented the effect of residential context, typically measured at one point in time using administrative boundaries, on outcomes such as cognitive ability (Sharkey/Elwert 2011), educational attainment (Sanbonmatsu *et al.* 2006; Wodtke *et al.* 2011), income (Chetty/Hendren 2018), marriage and childbirth (Harding 2003; South/Crowder 1999), and health (Arcaya *et al.* 2016; Clarke *et al.* 2014). Studies of neighbourhood effects conclude that living in socioeconomically disadvantaged neighbourhoods results in less favourable individual outcomes, beyond individual or family characteristics (Galster 2012).

While the existence of neighbourhood effects is well established, most research has treated neighbourhood exposure as static, ignoring how cumulative exposure and age-specific effects may complicate these associations. Recent studies have underscored the importance of examining neighbourhood poverty dynamically using longitudinal data (Sharkey/Faber 2014; van Ham/Tammaru 2016). Two temporal dimensions are considered key: the amount of time spent in disadvantaged contexts – the *duration* of exposure – and the age at which it occurred – the *timing* of exposure. Although not explicitly modelled, this framing acknowledges that residential mobility and neighbourhood change drive variation in exposure over time, and therefore in estimated neighbourhood effects. The growing availability of longitudinal data has allowed for exploring these dimensions over extended periods (Sharkey/Elwert 2011; Wodtke *et al.* 2011), but studies tend to focus on one temporal dimension at a time.

In this article, we investigate how childhood trajectories of neighbourhood disadvantage shape high school graduation in Sweden. First, we examine how prolonged (as against brief) exposure to a disadvantaged neighbourhood in childhood affects educational attainment. Second, we explore the developmental periods during which the residential context is most influential. We use data from Swedish population registers, a high-quality data source that provides detailed information of all individuals residing legally in Sweden (Ludvigsson *et al.* 2016, 2019). The use of these registers offers several advantages. First, while most neighbourhood-effect studies rely on administrative units to define neighbourhoods, we construct individualised, scalable neighbourhoods. Unlike administrative units, our neighbourhoods have bespoke boundaries centred on the individual's residence, thereby ensuring that they represent more accurately everyday local experiences (Hennerdal/Nielsen 2017). Second, registers allow us to track individuals over a 14-year period with a very low attrition rate. We thus address the need for high-standard longitudinal studies on neighbourhood effects, improving the measurement of exposure duration and neighbourhood boundaries.

Our empirical approach addresses two key identification challenges: separating short from long-term neighbourhood exposure and accounting for the fact that residential mobility and subsequent neighbourhood exposure are themselves partly a product of prior neighbourhood exposure. First, by relying on longitudinal data,

we avoid the pitfall of cross-sectional studies that combine children who recently arrived in a disadvantaged neighbourhood with those who experienced long-term exposure. This distinction is crucial, as recent movers may differ systematically from long-term residents in terms of family resources and aspirations for future mobility. Doing so could underestimate the effects of sustained exposure to disadvantaged residential contexts on individual outcomes.

Secondly, in longitudinal analyses, conventional regression models yield biased estimates when past treatment affects the covariates that predict future treatment (Robins 1999; Wodtke *et al.* 2011). This is precisely the case with residential mobility: past neighbourhood exposure shapes families' circumstances, which in turn determine residential mobility patterns. Naively controlling for these time-varying covariates therefore risks absorbing part of the neighbourhood effect itself. It is therefore essential for researchers to make explicit the theoretical model guiding their analysis. To account for this dynamic selection, we estimate a marginal structural model using inverse probability of treatment weighting (IPTW), a method that provides unbiased estimates of the cumulative treatment effect under no unobserved confounding (Robins *et al.* 2000). While most studies using IPTW estimate weights with linear or logit models, we use a machine learning algorithm – Bayesian additive regression trees (BART). This approach yields better covariate balance, as measured by standardised mean differences, than logistic regression.

Our use of machine learning is particularly innovative in the context of mobility research, as it allows us to account for complex, non-linear patterns of residential change and selection into neighbourhoods. To our knowledge, only one article has used machine learning to improve the quality of inverse probability weights in longitudinal settings, but the research it reported focused on the effect of living in social housing on mental health (Bentley *et al.* 2018). In a context of rising interest in machine learning applications in empirical social science (Athey 2018; Hofman *et al.* 2021; Molina/Garip 2019), we provide an original example of the complementarity between machine learning and statistical methods by using the best of both worlds in the case of neighbourhood effects: machine learning for prediction tasks and conventional statistics for estimation tasks. This approach is especially valuable for understanding how residential trajectories, instead of static neighbourhood characteristics, shape educational inequality.

2 Previous research

Prior research evidenced the pernicious consequences of growing up in disadvantaged areas for child development (Brooks-Gunn *et al.* 1993; Mayer/Jencks 1989) beyond family characteristics (Sastry 2012). Neighbourhood effects are typically explained by two sets of mechanisms. One set is related to the physical attributes of the neighbourhood, the other to its population composition (Galster 2012).

The former includes theories on the socioeconomic implications of the geographical isolation of some neighbourhoods, e.g., spatial mismatch theory (Kain 1968), and theories on social infrastructures (Klinenberg 2018), which explain neighbourhood

effects by the uneven quality and presence of public and social services and infrastructures, like roads, parks, or public transport. Poor neighbourhoods also lack good schools, with often higher teacher turnovers and shortages (*Hansson/Gustafsson* 2016; *Karbownik* 2020) and under-resourced facilities (*Siebecke/Jarl* 2022). In Sweden, poverty-concentrated neighbourhoods were also found to be associated with less social mixing in local schools (*Malmberg/Andersson* 2021). The latter group of mechanisms focuses on the role of social networks and interactions, considering neighbourhoods as sites of social contact and exposure (*Crane* 1991). In disadvantaged neighbourhoods, role models and peer influences are typically considered negative, promoting anti-social norms and deviant behaviours (*Galster* 2012). Within schools, peer influences can lead to a devaluation of educational norms, of school as an institution, and of its possibilities to provide a successful career path (*Ceballos et al.* 2004; *Flouri et al.* 2012). Both groups of mechanisms predict negative effects of residing in disadvantaged neighbourhood contexts on educational outcomes. However, these effects are likely moderated by patterns of residential mobility and the timing of exposure, as families' ability to move in or out of neighbourhoods is not random but shaped by structural constraints and resources.

Most early neighbourhood-effects research relies on cross-sectional or short-term longitudinal designs, which fail to capture the dynamic nature of neighbourhood effects and its consequences. Static measures of neighbourhood disadvantage ignore how mobility trajectories – such as frequent moves, downward mobility into poorer areas, or prolonged immobility – shape cumulative exposure and outcomes. For instance, a child who moves into a disadvantaged neighbourhood in adolescence may experience different effects than one who has lived there since birth, due to differences in social integration, school quality, and peer networks. Moreover, selective mobility means that families who move may differ systematically from those who stay, both in terms of resources and reasons for moving (e.g., job loss, family disruption, or housing policy changes). Thus, understanding neighbourhood effects requires a life-course perspective that accounts for the timing, duration, and context of residential mobility. We therefore seek to add to the growing literature focusing on cumulative exposure to disadvantaged neighbourhoods using longitudinal data (*Alvarado* 2016; *Crowder/South* 2011; *Sampson et al.* 2008; *Wodtke et al.* 2011).

2.1 Cumulative exposure to neighbourhood poverty

Prolonged exposure to disadvantaged neighbourhoods may be more detrimental than short-term exposure (*Musterd et al.* 2012). The longer children remain in neighbourhoods lacking essential resources – such as quality schools, public services, or social infrastructure – the more likely they are to experience the adverse effects of local deprivation (*Chetty et al.* 2016; *Sharkey* 2013). This is especially true for children whose families lack the resources to move, leading to entrenched disadvantages and limited access to opportunities elsewhere (*Sharkey* 2013). Conversely, youth living durably in affluent areas benefit from the positive opportunities those environments offer. Similarly, social-interactive mechanisms, like peer group socialisation, depend

on continuous, long-term interactions that might not operate with little exposure. Finally, the longer one lives in disadvantaged neighbourhoods, the more likely one is to be exposed to violence or delinquent behaviours or to be a victim of physical assault, all of which can undermine educational outcomes (*Schwartz et al.* 2022).

Early cross-sectional studies provided mixed evidence on the impact of neighbourhood deprivation on education, probably because these studies failed to consider neighbourhood effects temporally (*Brooks-Gunn et al.* 1993; *Vartanian/Gleason* 1999). However, more recent research using longitudinal data offers robust evidence of neighbourhood effects on children's educational outcomes (*Andersson* 2004; *Chetty et al.* 2016). *Sampson and colleagues* (2008) found that living in a disadvantaged neighbourhood reduces verbal skills among children years after exposure, while *Alvarado* (2016) found similar effects on cognitive ability. Studies with data on childhood residential trajectories found that these effects are exacerbated by the length of residence (*Crowder/South* 2011; *Wodtke et al.* 2011). While most of these studies have been conducted in the US context, similar results were found in Europe (*Kauppinen* 2007; *Lund* 2020; *Nieuwenhuis et al.* 2021).

In light of the above, we hypothesise that residents of disadvantaged neighbourhoods will be less likely to finish high school (H1a) and that the negative effect of the neighbourhood will be more pernicious as exposure to disadvantaged neighbourhoods increases over time (H1b).

2.2 Timing of exposure to neighbourhood poverty

There is reason to believe that certain mechanisms contributing to neighbourhood effects may be more influential during specific childhood stages than in others (*Brooks-Gunn et al.* 1993). *Leventhal* (2018) details three models of developmental timing. The *early childhood* model contends that neighbourhood context is more important during a child's first years. Early childhood is a formative period of social construction, where primary socialisation occurs – a crucial phase for the development of cognitive and educational skills. On the contrary, the *adolescence* model asserts that the environment experienced during adolescence is more determinant. During adolescence, *Leventhal* argues, children become more autonomous and explorative, making extrafamilial contexts like the neighbourhood more important. Adolescence is also associated with risky behaviours that peers and social disorganisation may encourage. Furthermore, as decisions on school continuation usually happen during adolescence, living in a disadvantaged neighbourhood might have stronger effects at this time on educational outcomes. Finally, the *cumulative exposure* model advances that rather than timing, duration of exposure matters most in children's development. This model suggests that the compound effect of exposure to disadvantage during childhood weighs more than exposure during a specific life stage.

Few studies explore the differential effect of timing of exposure on high school achievement or cognitive skills, and results are mixed. In the UK, *Nieuwenhuis and colleagues* (2021) found no effect of exposure to neighbourhood poverty on educational attainment between the ages of 10 and 16. *Anderson et al.* (2014) found

that exposure during early childhood is more harmful to educational attainment, while other studies suggest that exposure in adolescence is more important (*Alvarado 2016; Sharkey/Faber 2014; Troost et al. 2023; Wodtke et al. 2016*). Recent research that jointly considers duration and timing supports this nuanced view. For example, *Kleinepiers/van Ham (2018)* found that children who moved into disadvantaged neighbourhoods during adolescence faced a higher risk of school dropout than those who lived in disadvantaged neighbourhoods throughout childhood. These findings suggest that residential mobility can shape both the timing and accumulation of exposure, meaning that not only where but also when children move matters for their educational outcomes.

Given the mixed evidence on the role of timing of exposure on the magnitude of neighbourhood effects, we propose two sub-hypotheses. According to the early childhood model, we would expect exposure to neighbourhood poverty to have a stronger effect during early childhood (H2a). On the other hand, the adolescence model would predict a stronger effect during adolescence (H2b).

2.3 Neighbourhood effects and selective mobility

Neighbourhood effects are inherently dynamic because children's environments change over time, either when they move to a new neighbourhood or when the neighbourhood composition itself changes as other children develop and residents move in and out (*Hedman 2011; Sharkey 2013; Wodtke et al. 2011*). Moreover, residential mobility is not random: families with greater resources are more likely to move out of disadvantaged neighbourhoods (*Kuyvenhoven/Haandrikman 2025; Sampson/Sharkey 2008*). Empirical evidence suggests that early childhood moves out of deprived neighbourhoods often reflect upward mobility (*Kuyvenhoven et al. 2025*), granting access to better schools and safer environments, while moves later in childhood or adolescence may be downward or lateral, sometimes driven by economic hardship or family disruption.

Ignoring these non-random patterns may lead studies to underestimate or misrepresent the true impact of neighbourhood contexts (*Crowder/South 2011; Wodtke et al. 2011*). Accurately capturing neighbourhood effects therefore requires considering both the cumulative duration of exposure and the selective nature of residential mobility across the life course. Our empirical strategy addresses both by using a cumulative measurement of neighbourhood disadvantage and accounts for the dynamic relationship between neighbourhood composition and time-varying characteristics that shape residential mobility.

2.4 Cumulative disadvantage in the Swedish context

Boasting one of the most generous welfare states in the world, Sweden provides a strong, universal safety net of social assistance, including benefits such as free education and healthcare, unemployment insurance, family allowances, and housing subsidies. This regime substantially reduces the share of households at risk of poverty and mitigates overall income disparities (*Andersson et al. 2010*).

More recently, Sweden has witnessed growing levels of income inequality and socioeconomic residential segregation (*Mutgan/Mijs 2023*) comparable to other European countries (*Haandrikman et al. 2023; Musterd et al. 2017*). It has also experienced an increase in ethnic residential concentration (*Malmberg et al. 2018*). This is in part a result of national housing policies, which have contributed to housing-tenure segmentation across local areas. Poor areas concentrate rental units in large housing estates while more affluent areas provide smaller, owner-occupied dwellings (*Andersson et al. 2022*). As immigration to Sweden increased in the 1990s, migrant families settled in the large housing estates in poor neighbourhoods. Urban riots in 2009 have contributed to the visibility of ethnically segregated neighbourhoods and the dearth of socioeconomic opportunity in these areas (*BråmÅ 2006; Malmberg et al. 2013*). Mounting evidence shows that residential inequalities are durable over time, as neighbourhood disadvantage is reproduced across the life course, particularly among ethnic minorities (*Hedman/van Ham 2021; van Ham et al. 2014*).

Prior studies have investigated the consequences of neighbourhood deprivation on educational outcomes in Sweden (*Andersson 2004*). *Hedefalk/Dribe (2020)* explored the effect of nearest neighbours on educational achievements in a Swedish medium-sized city and reported a significant effect of the neighbourhood's social class composition, even after controlling for school and individual characteristics. Other studies have focused on the long-term consequences of growing up in a poor neighbourhood for educational attainment using longitudinal data (without a cumulative perspective) and found similar results (*Andersson/Malmberg 2015; Brännström/Rojas 2012; Lindahl 2011*). To our knowledge, no study has examined both the effects of prolonged residence in, and the timing of exposure to, disadvantaged neighbourhoods on high-school completion among urban residents in Sweden.

3 Data and measurements

We use yearly Swedish administrative data, combining multiple longitudinal registers from 1990 to 2017. The register of the total population includes demographic variables like gender, year, and place of birth as well as place of residence. Information on income and occupational status is derived from the taxation register. Education-related variables come from educational registers. Individuals were matched across registers using a pseudonymised personal identifier.

3.1 Sample

We focus our analysis on the 1989 Swedish birth cohort and limit our sample to individuals that have a complete residential trajectory. Swedish population registers include the entire population of legal residents, and therefore have a very low attrition rate (*Ludvigsson et al. 2019*). Incomplete or incorrect information may still occur when an individual has moved outside of Sweden or when they are unregistered (e.g., because of an unregistered move, or illegal residency). Attrition analysis is conducted in Online Appendix A and shows no substantial differences

in key variables, primarily because attrition is low. Out of the 116,744 individuals born in 1989 and registered in Sweden in 1990, 112,436 were still registered in the subsequent 15 years, and 111,996 have complete residential trajectories available. Missing values in covariates account for another drop of 5,677 individuals.

The final sample is restricted to urban residents, as we restrict the sample to individuals whose 500 nearest neighbours are located within a 1000-metre radius of their residence. This corresponds to areas where the population density is more than 160 inhabitants/km², which covers every large Swedish city, their agglomeration as well as medium- and small-sized cities. This constraint is motivated by the fact that neighbourhood effects in rural areas are conceptually different than in urban neighbourhoods (Marco/Marco 2010; Petrović *et al.* 2022). Even though the 1000-metre threshold is arbitrary, it corresponds to the high range of perceived neighbourhoods' size and population (Coulton *et al.* 2013; Pebley/Sastry 2009) in urban or periurban contexts and is also likely to capture proximity to schools and educational resources. Robustness checks presented in Online Appendix B show that changing the radius threshold does not affect the results. The final sample consists of 61,315 individuals whose residential trajectories are complete from 1990 to 2004.

3.2 Exposure to disadvantaged neighbourhoods

Neighbourhoods are constructed using the *geocontext* algorithm (Hennerdal 2019). *Geocontext* computes the k -nearest neighbours for each individual-year based on 100×100 metres geocoordinates. Methods based on the k -nearest neighbours approach mitigate the zoning component of the modifiable areal unit problem (MAUP) by replacing fixed, externally imposed areal units with individualised neighbourhoods, thereby reducing boundary-induced statistical dependence between observations (Hennerdal/Nielsen 2017; Petrović *et al.* 2022). Furthermore, by centring neighbourhoods on individuals rather than on externally imposed boundaries, these approaches better approximate individuals' immediate social environments and lived contexts. They also make the choice of spatial scale explicit, as the number of neighbours becomes a theoretically and analytically transparent parameter rather than an artifact of predefined administrative zoning. Studies using bespoke neighbourhoods rather than administrative units find stronger neighbourhood effects (Andersson/Malmberg 2015; Hipp/Boessen 2013).

We set k to 500, meaning that we construct individual neighbourhoods based on the 500 nearest neighbours. This size represents the number of people one may encounter in their immediate surrounding (Östh *et al.* 2014) and is therefore a plausible scale at which neighbourhood effects can operate. This specification is consistent with other studies using ego-centred neighbourhoods (Brandén *et al.* 2023). We use three variables to evaluate neighbourhood disadvantage: unemployment, low income, and social assistance use. As data on the active population is not available throughout the observation period, we look at non-employed people between 35 and 64 years old to estimate unemployment rates. Low income is defined as

belonging to the lowest quintile of disposable income for a given year. Finally, social assistance use is defined as any use of social assistance during a given year.

For each neighbourhood-year, we compute the share of unemployment, low income, and social assistance use among the nearest neighbours. We then use principal component analysis to reduce these variables into one composite yearly measure of neighbourhood poverty. Across all years, this measure accounts for around 60 percent of the variance. The neighbourhood disadvantage measure is then divided into quintiles whereby the first quintile represents affluent neighbourhoods and the fifth disadvantaged ones. Descriptive statistics of the fifth quintile composition according to the three measures of disadvantage is provided in Online Appendix D (Table D1). The cumulative measure of neighbourhood deprivation is a continuous variable that sums the number of years individuals have spent in neighbourhoods belonging to the fifth quintile of neighbourhood disadvantage. Robustness checks provide evidence that changing the threshold of the quantile (e.g., to the last decile or quartile) does not change our results (Online Appendix B, Table B1).

Table 1 provides summary statistics for our measure of neighbourhood disadvantage. The average number of years of living in a disadvantaged neighbourhood is 2.92, with a minimum of 0 and a maximum of 15. Table 2 also indicates that a majority of the sample has never lived in the fifth quintile of neighbourhood poverty, with 37,080 individuals out of 61,315 being in that case. Nonetheless, 6.4 percent of the sample has never lived elsewhere than in a disadvantaged neighbourhood.

3.3 High school graduation

High school graduation is the dependent variable in all models, measured using a dummy indicating whether individuals completed high-school education (*gymnasium*). Typically, individuals in Sweden complete high school by the time they are 1819 years old (Gustafsson *et al.* 2017; Halldén 2008), but we also include individuals who completed *gymnasium* as late as 25 years old in order to include as many graduates as possible. Reports from the Swedish Bureau of Statistics indicate no significant change in the number of graduates after 25 years old (Statistics Sweden 2015). Table 1 shows that 83 percent of our sample graduated from high school.

3.4 Covariates

We control for variables that might affect both educational achievement and neighbourhood history. Hedman *et al.* (2011) indicates that individuals or households are more likely to move to neighbourhoods whose residents match their social, economic, and demographic characteristics. Specifically, they find that income, education, social benefits, and immigration status of movers are strongly associated with the characteristics of the neighbourhood they move into. Because these characteristics are also determinants of educational attainment, we need to control for those variables to remove confounding. Beyond measured covariates, for which we can account in the model, some characteristics that might affect both

Tab. 1: Descriptive statistics

	Overall	1990	1997	2004
<i>Time-varying variables</i>				
<i>Single parents</i>				
No	82%	92%	81%	73%
Yes	18%	8%	19%	27%
<i>Number of parents employed</i>				
0	5%	3%	6%	4%
1	21%	18%	23%	19%
2	77%	79%	71%	77%
<i>Parents' disposable income (in 1000SEK)</i>				
Mean	305	216	314	436
Min.-Max.	2-6,764	3-8,518	3-48,632	.7-116,980
<i>Number of children in household</i>				
Mean	2	2	2	2
Min.-Max.	1-13	1-11	1-15	1-13
<i>Time-invariant variables</i>				
<i>Years of residence in disadvantaged neighbourhoods</i>				
Mean	2.92			
Min.-Max.	0-15			
<i>High school graduation</i>				
Did not graduate	17%			
Graduated	83%			
<i>Gender</i>				
Men	52%			
Women	48%			
<i>Foreign background</i>				
No foreign background	79%			
Children of immigrant	21%			
<i>Parents' highest education</i>				
Compulsory schooling (< 9 years)	0.8%			
Compulsory schooling (9 years)	3.3%			
Secondary education	43%			
Post-secondary education (< 2 years)	7.3%			
Post-secondary education (2+ years)	43%			
Graduate program	2.8%			
<i>Number of moves</i>				
0	25.4%			
1	31.5%			
2	20.5%			
3+	22.6%			

Source: Authors' calculations based on Swedish population registers, 1990-2017.

the treatment and the outcome are unobserved (e.g., parental involvement in their children's education). Unobserved confounding variables are assessed using the Evaluated method (VanderWeele/Ding 2017).

Our analysis accounts for both timevarying and timeinvariant covariates. Time-invariant controls include gender, foreign background, parental education, and the number of moves. Gender is a dichotomised variable. Foreign background is defined based on whether respondents are children of at least one parent born outside Sweden or Swedes with no foreign background.¹ Parental education uses parents' highest educational achievement, and is scaled from 1 to 6, 1 being less than primary education and 6 being a graduate program. The number of moves is the number of annual changes in residential location during the period of observation. Multiple moves within the same year are not recorded. Table 1 summarises the timeinvariant covariates. Children of immigrants represent 21 percent of the sample. 53.1 percent of individuals have at least one parent with some postsecondary education, while only 4.1 percent have both parents with at most primary education. The average number of moves is 1.62, with 25 percent of never-movers and 32 percent of one-time movers.

Time-varying covariates are measured each year from 1990 to 2014. They include parental income, parental employment status, single-parent household, and number of children in the household. Parental income is the unweighted sum of parents' disposable income. Parental employment status refers to the number of employed parents. Single-parent household is coded 1 for single-parent households and 0 for two-parent households (including biological parents, stepparents, or adoptive parents residing together). Finally, we retrieve the number of children living in the same household: these are likely siblings but can also be half-siblings or other children.

Table 1 presents summary statistics for the time-varying covariates for three years: 1990, 1997, 2004. In the sample, 8.1 percent live in single-parent households at one year of age in 1990, rising to 27 percent living in single-parent households in 2004, when the individuals are 15 years old. Parental employment significantly decreases after childbirth, with an eight-percentage point decline in dual-worker families between 1990 and 1995, followed by a reverse trend in the following years. Finally, parental income steadily increases from 1990 to 2004. Table 1 demonstrates the importance of accounting for time. Time-varying variables account for the evolution of the child's household, which might dramatically affect parents' residential mobility and children's education.

¹ As our sample includes only people born in Sweden, first-generation immigrants are not part of the sample.

4 Methods

4.1 Model specification

The target population is individuals born in Sweden and who have lived in urban areas in Sweden between 0 and 15 years old. Because Swedish administrative data has a very low attrition rate and a low share of unregistered inhabitants (*Ludvigsson et al.* 2016), our sample constitutes a near-complete register-based population sample.

To compute the effect of neighbourhood disadvantage trajectories on educational outcomes, we need to estimate the difference in the probability of high school graduation between all individuals had they experienced a specific neighbourhood trajectory compared to another neighbourhood trajectory. In other words, let $Y_i(\bar{a})$ be individual i 's potential outcome Y (high school graduation) under the complete neighbourhood history $\bar{a} = (a_1, \dots, a_T)$, with a_t being 1 when i is exposed to a disadvantaged neighbourhood at year t , and 0 otherwise. Let n be the target population. For two neighbourhood histories $\bar{a}$ and $\bar{a}'$, the average treatment effect of neighbourhood disadvantage on high school graduation therefore equals

$$\frac{1}{n} \sum_{i=1}^n (Y_i(\bar{a}') - Y_i(\bar{a})) \quad (1)$$

This average treatment effect represents the mean difference in high school graduation probability if all individuals in the population experienced neighbourhood trajectory $\bar{a}$ versus trajectory $\bar{a}'$. This counterfactual quantity is the causal estimand of interest.

Because neighbourhood context is measured over $T = 15$ years with a binary indicator, there are 32,768 possible residential trajectories, making a fully saturated model infeasible. We therefore summarise each sequence as the total number of years in a disadvantaged neighbourhood. This is reasonable since about 80 percent of individuals experience at most one change in neighbourhood status, so cumulative exposure captures most of the relevant variation.

Our modelling strategy consists of two marginal structural models (MSM), a class of models that were designed to identify average treatment effects in longitudinal settings where time-varying confounding is affected by past treatments (*Robins et al.* 2000). We first present the unweighted models (Equations 2 and 3) and detail the weighting procedure used to account for time-varying confounding in the next section. The first model can be written as:

$$\text{logit}(P[Y(\bar{a}) = 1]) = \beta_0 + \beta_1 \text{cum}(\bar{a}) + \beta_2 c \quad (2)$$

where $P[Y(\bar{a}) = 1]$ is the probability of high-school graduation had students followed the neighbourhood trajectory $\bar{a}$ and c is a vector of time-invariant covariates. The cumulative function $\text{cum}(\bar{a})$ equals $\sum_{t=1}^T a_t$, the total number of periods a subject is

living in a disadvantaged neighbourhood. The log odds β_1 describes the effect of a one-unit increase of $cum(\bar{a})$ on high school graduation.

In the second model, we further explore how neighbourhood disadvantage affects high school graduation rates differentially based on the timing of exposure during childhood:

$$\text{logit}(P[Y(\bar{a}) = 1]) = \theta_0 + \theta_1 \text{cum}(\bar{a}_{t \in [1,5]}) + \theta_2 \text{cum}(\bar{a}_{t \in [6,10]}) + \theta_3 \text{cum}(\bar{a}_{t \in [11,15]}) + \theta_4 c \quad (3)$$

where $\text{logit}(P[Y(\bar{a}) = 1])$ and $\theta_4 c$ are defined as previously, but instead of one cumulative function, as in Equation 2, three functions specify the years a subject lived in a disadvantaged neighbourhood separately for ages 1 to 5, 6 to 10, and 11 to 15. θ_1 , θ_2 , and θ_3 are the log odd ratios associated with a marginal increase in exposure to high-poverty neighbourhood during respectively, early childhood, late childhood, and adolescence, keeping other variables constant.

Under the assumptions of consistency, positivity, and sequential exchangeability, an MSM yields a consistent estimate of the parameters of interest (*Robins 1999*). In the next section, we describe our use of inverse probability of treatment weighting (IPTW) to model time-varying control variables and ensure that sequential exchangeability is achieved.

4.2 Inverse probability of treatment weighting

With longitudinal data, conventional regression methods are unable to identify the causal estimand when time-varying variables simultaneously act as mediators of past treatment and confounders of future treatment (*Elwert/Winship 2014; Robins 1999; Robins et al. 2000*), even in the absence of unobserved confounding. Given the dynamic nature of neighbourhood selection (*Wodtke et al. 2011*), this problem is unavoidable: residential mobility is shaped by individual characteristics that are themselves influenced by prior neighbourhood exposure. Standard regression approaches cannot properly account for this feedback loop, where the same time-varying factors (e.g., income or family structure) both transmit the effects of past neighbourhoods and determine selection into future ones, leading to bias in estimated neighbourhood effects. We detail this problem using directed acyclic graphs (DAGs) in Online Appendix E.

IPTW resolves the problems of conventional methods without additional assumptions by generating a weighted pseudo-population in which the treatment at each time point is independent of past (observed) time-varying confounders. In IPTW, weights are computed at each time point as a function of past treatment and confounding history. We detail the weight computation in Online Appendix E. Combining MSM and IPTW, we get the following model for Equation 2:

$$\text{logit}(E[Y(\bar{a})]) = \beta_0^w + \beta_1^w \text{cum}(\bar{a}) + \beta_2^w c \quad (4)$$

with β^w being the weighted coefficients. The log odds β_1^w is the empirical estimand and describes the effect of a oneunit increase of $cum(\bar{a})$ on high school graduation,

conditional on both timevarying and timeinvariant observed confounders. We also apply these weights to Equation 3.

4.3 Estimation strategy: BARTgenerated probability weights

IPTW requires prediction of the probability of treatment at each time point given past treatment and covariate history (i.e., a propensity score). While previous studies have estimated propensity scores of IPTW with generalised linear models (e.g., *Sharkey/Elwert* 2011; *Wodtke et al.* 2011), we use Bayesian additive regression trees (BART; *Chipman et al.* 2010), as implemented in the *WeightIt* package (*Greifer* 2026) in R. Bayesian additive regression trees model a variable as the sum of the fit of many small regression trees, each contributing a part of the prediction. Following a Bayesian framework, BARTs use a regularisation prior that strongly favours small regression trees and minimises overfitting (*Hill et al.* 2020). BART tend to perform well, if not better, than other state-of-the-art algorithms in prediction tasks (*Chipman et al.* 2006; *Hill et al.* 2011).

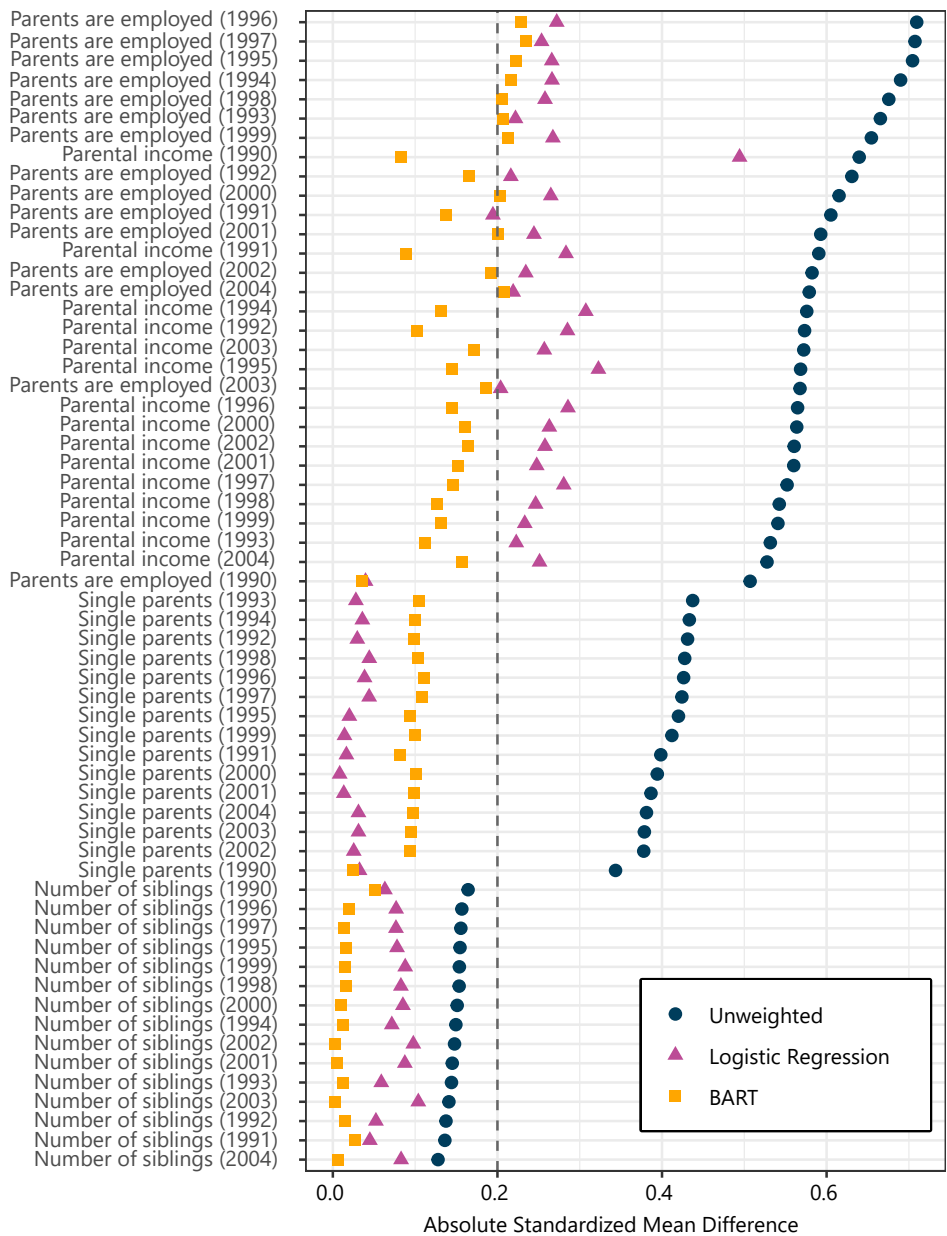
BART offers several advantages over linear models for predicting propensity scores. First, BART is not a parametric model and can accommodate for non-linearities in a flexible way. In our case, with up to 14 covariates in the numerator and up to 74 covariates in the denominator, BART provides a good way to include nonlinearities without prior knowledge of the possible interactions between the predictors while keeping high computational efficiency. Second, the specification of a BART model does not require hyperparameter tuning, as default parameters provide the best performance (*Hill et al.* 2011), thus reducing the number of arbitrary decisions to take when fitting the model.

We assess the performance of BARTgenerated weights using standardised mean difference (SMD), as suggested by *Austin/Stuart* (2015). SMD compares the average values of the covariates for individuals living in disadvantaged versus non-disadvantaged neighbourhoods. For a specific covariate, a value of 0 would mean exchangeability between the two groups. As IPTW simulates sequential randomisation, we should expect the SMD to be close to 0 for each covariate. Figure 1 shows the absolute SMD for the unweighted sample and two weighted samples, one using weights generated by logistic regression and the other using weights generated by BART. Both weighting approaches largely improve balance relative to the unweighted sample. Overall, BART performs better than logistic regression, particularly for continuous variables across all time points, while logistic regression seems to perform better for binary variables.

4.5 Sensitivity analysis

While our models account for the dynamic feedback between neighbourhood composition and time-varying confounding, it assumes that no additional unobserved traits may cause confounding between neighbourhood disadvantage and high school graduation. To assess the extent to which this assumption is held, we report E-values of our key parameters of interest (*VanderWeele/Ding* 2017). An

Fig. 1: Absolute standardised mean difference of the time-varying covariates according to three weighting methods



Note: Results are reported using the cobalt package in R (Greifer 2026). Covariates are presented in decreasing order of their absolute standardised mean differences in the unweighted model. Source: Authors' calculations based on Swedish population registers, 1990-2017.

E-value is the minimum association needed between unobserved confounding and both treatment and outcome to negate the observed treatment-outcome association. Further details are provided in Online Appendix C.

5 Results

5.1 Duration of exposure

Table 2 illustrates the crude relationship between the duration of exposure to disadvantaged neighbourhoods and high school graduation rates.² It highlights a negative correlation between cumulative exposure and graduation outcomes. Among children with no exposure, 88 percent successfully graduate. This rate declines progressively with increasing exposure: for those exposed for 1 to 5 years, the graduation rate drops to 80 percent, and for 6 to 10 years of exposure, it falls further to 74 percent. Children with uninterrupted exposure have a 69 percent likelihood of not graduating from high school, 19 percentage points less than children with no exposure. These findings suggest that prolonged exposure to disadvantaged neighbourhoods may have a compounding effect on educational attainment.

Figure 2 displays the predicted probability of graduating from high school by the number of years spent in a disadvantaged neighbourhood, as specified in Model 1, estimated with IPTW (Equation 4). Full results of the model are presented in Table D2 (Online Appendix D). The weighted estimate indicates that, in line with H1a and H1b, sustained exposure to poor neighbourhoods significantly decreases the probability of graduating from high school ($\beta_1^w = -0.028, p < 0.001$). Compared to growing up in non-disadvantaged neighbourhoods, subjects living for five years in disadvantaged neighbourhoods are about 13 percent less likely to graduate from

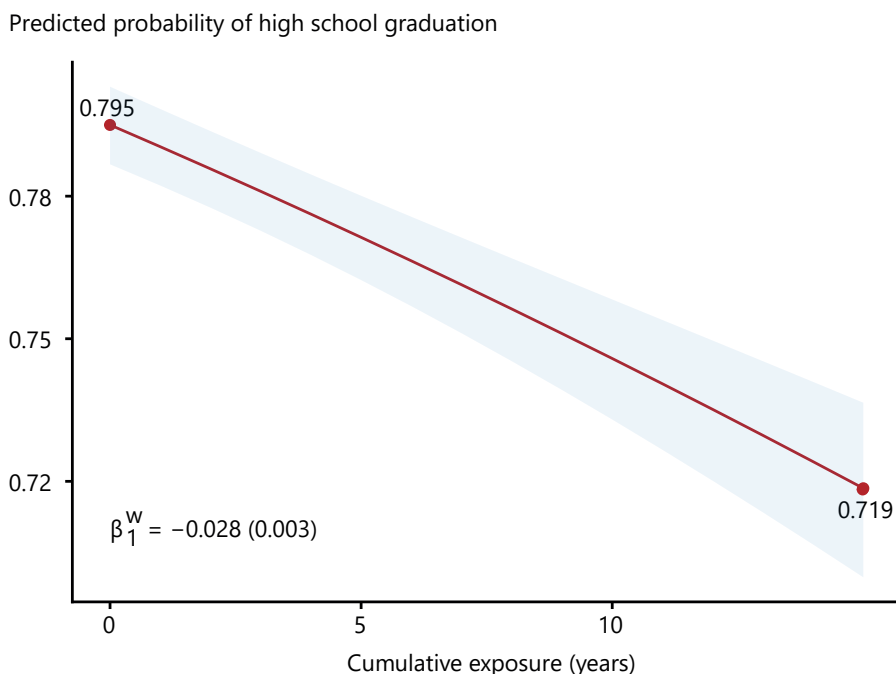
Tab. 2: High school graduation rates by duration of residence in disadvantaged neighbourhoods

Cumulative exposure to disadvantaged neighbourhoods	High-school graduation		Total
	Did not graduate	Graduated	
0 year	4,452 (12%)	32,538 (88%)	37,080 (100%)
1 to 5 years	2,276 (20%)	8,896 (80%)	11,172 (100%)
6 to 10 years	1,295 (26%)	3,765 (74%)	5,060 (100%)
11 to 14 years	1,152 (28%)	2,922 (72%)	4,074 (100%)
15 years	1,222 (31%)	2,707 (69%)	3,929 (100%)
Total	10,487 (17%)	50,828 (83%)	61,315 (100%)

Source: Authors' calculations based on Swedish population registers, 1990-2017.

² Results are reported using the R packages *gtsummary* (Sjoberg et al. 2021) and *ggeffects* (Lüdtke 2018).

Fig. 2: Predicted probability of high-school graduation by duration of exposure to disadvantaged neighbourhoods



Note: The blue zone is the estimate's 95% confidence interval. The full regression table is available in the Online Appendix (Table D2). The rightmost point can be interpreted as the predicted probability of high-school graduation given 15 years of exposure to a disadvantaged neighbourhood.

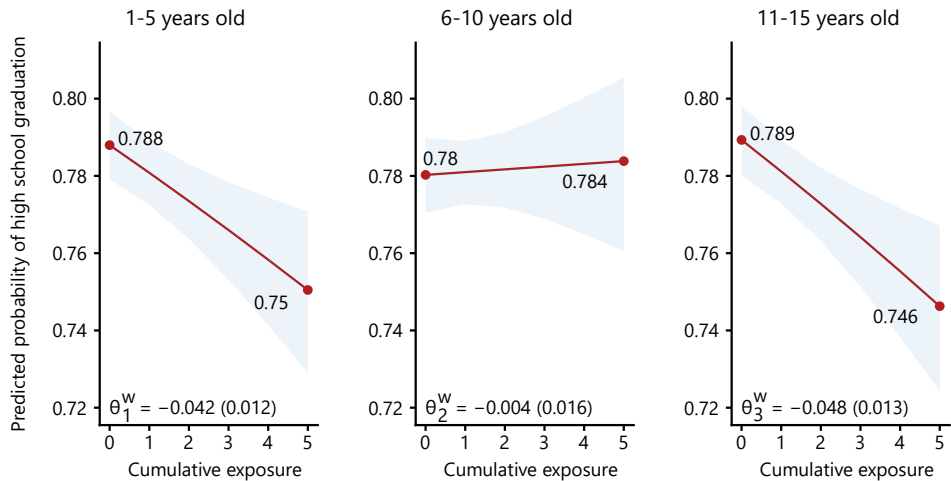
Source: Authors' calculations based on Swedish population registers, 1990-2017.

high school ($\exp(-0.028 \times 5) = 0.869$), and 34 percent less likely for always-exposed students ($\exp(-0.028 \times 15) = 0.657$). In terms of predicted probability, results suggest that while someone who has never lived in a disadvantaged neighbourhood has an 80 percent probability of graduating from high school, this proportion drops to 72 percent for individuals with continuous exposure during childhood.

5.2 Timing of exposure

Figure 3 presents the results of Model 2 (Equation 3) estimated using IPTW. The full output of the model is available in Online Appendix D (Table D3). In this model, we decompose cumulative exposure into three age-specific measures: 1-5 (early childhood), 6-10 (late childhood), and 11-15 (early adolescence). All measures are modelled simultaneously. This means θ_1^w , θ_2^w and θ_3^w each represents the effect of exposure during that specific period while holding constant exposure during the other periods. For example, θ_3^w estimates the effect of one additional year of exposure during adolescence for individuals with the same exposure history during

Fig. 3: Predicted probabilities of high school graduation by exposure to disadvantaged neighbourhoods, separated by life stages



Note: Results are detailed in Table D3 (Online Appendix D). The blue zone is the estimate's 95% confidence interval.

Source: Authors' calculations based on Swedish population registers, 1990-2017.

early and middle childhood. Results suggest a substantial effect of exposure to disadvantaged neighbourhoods on high school graduation between ages 1 to 5 ($\theta_1^W = -0.042$, $p < 0.001$) and 11 to 15 ($\theta_3^W = -0.048$, $p < 0.001$), but not between ages 6 to 10 ($\theta_2^W = -0.004$, $p = 0.79$).

As illustrated in Figure 3, children between 1 and 5 years old who experience five years of exposure to disadvantaged neighbourhoods during early childhood have a predicted probability of high school graduation that is 3.8 percentage points lower compared to those who are never exposed. Similarly, adolescents who experience five years of exposure to disadvantaged neighbourhoods have a predicted probability of high school graduation that is 4.3 percentage points lower than never-exposed students.

Consistent with both H2a and H2b, living in a disadvantaged neighbourhood for several years is particularly crucial to the educational outcomes of young children and adolescents. Further analysis (Online Appendix B, Table B1) shows that different age thresholds lead to similar conclusions while providing further insights. In Table B1, Model C displays coefficients for periods 1 to 9, 10 to 12, and 13 to 15 years old, while Model D for 1 to 3, 4 to 12, and 13 to 15. Both models show that the first and third periods are significantly associated with high school graduation and that the third period is more important than the first one. In other words, even though both early childhood and adolescence seem to be pivotal life stages for neighbourhood effects, adolescence tends to be the most important one. Furthermore, Model D has the highest coefficients for the first and third periods, showing that the earliest and latest years are the most critical.

5.3 Sensitivity analysis

To what extent is the influence of unobserved variables confounding the estimation of the cumulative exposure effect? As noted by *Levy (2019)*, parental expectations for their children's academic success may be confounding the association between children's high school completion and residential choice. Parents with high educational aspirations for their children may be disproportionately more likely to live in advantaged neighbourhoods, regardless of observed characteristics; at the same time, parental support directly affects children's educational attainment. While our model partially controls for these potential confounders to the extent that parental expectations or personality are correlated with sociodemographic variables (*Boonk et al. 2018*), we do not capture the part of these effects that are independent of those variables.

Estimates of the sensitivity analysis are provided in Table C1 in the Online Appendix. Results indicate that an association of 1.319 on the risk ratio scale would have to be found between unobserved confounding on the one hand and selection into neighbourhood disadvantage as well as high-school graduation on the other for the effect of living in deprived neighbourhoods during the whole period of observation to be ruled out. In other words, with Z being a vector of binary unobserved confounders, if individuals with $Z = 1$ are at least 31.9 percent more likely or less likely than individuals with $Z = 0$ to both graduate from high school (compared to not graduating) and to be exposed during the entire period to disadvantaged neighbourhoods (compared to never), then the neighbourhood effect can be ruled out by unmeasured confounding. The E-value analysis shows that the strongest association for a covariate in Models 1 and 2 is lower than the minimum association required to explain away the effect of 5 years of living in a disadvantaged neighbourhood. Further details on the sensitivity checks are provided in Online Appendix C.

6 Discussion and conclusion

This article used Swedish registry data to investigate how the duration and timing of childhood exposure to neighbourhood disadvantage shape educational attainment, while proposing a novel approach to the identification challenges posed by dynamic residential mobility into and out of disadvantaged neighbourhoods. To capture the complex processes linking residential trajectories with neighbourhood effects, we employed a marginal structural model with inverse probability of treatment weighting (IPTW), combined with Bayesian additive regression trees (BART) to account for time-varying confounders (*Robins et al. 2000*). Traditional approaches have struggled to model neighbourhood trajectories dynamically, because time-varying confounders – such as changes in family resources or household composition – can be affected by past neighbourhood exposures, creating bias in conventional models. Following *Wodtke et al. (2011)*, our approach explicitly addresses this issue by breaking the association between prior time-varying confounders and subsequent

neighbourhood exposure, allowing the effects of past neighbourhood exposures to be estimated more accurately. In doing so, we account for the complex dynamics of selective residential mobility.

Our approach departs from *Wodtke et al.* (2011) by integrating a state-of-the-art predictive model, BART, to predict the propensity scores needed to calculate inverse probability of treatment weights. BART outperformed the logistic model on all continuous variables and resulted in improved overall covariate balance after weighting, although it yielded slightly worse balance on binary covariates. Substantively, this has important implications for the interpretation of duration and timing effects. By modelling selection into neighbourhoods more flexibly than the parametric approach used in earlier work, BART reduces the risk that estimated effects are driven by residual selection rather than neighbourhood exposure itself. As a result, the age-graded pattern we observe is less likely to reflect selective residential mobility and more likely to capture differences in exposure to neighbourhood disadvantage.

While the issue of dynamic neighbourhood selection has long been recognised and addressed (*Quillian* 2003; *Sampson/Sharkey* 2008; *Wodtke et al.* 2011, 2020; *Zhou/Wodtke* 2020), research that explicitly accounts for it remains relatively rare. This study further illustrates how dynamic residential selection can be integrated into research on neighbourhood effects and residential mobility. By augmenting inverse probability of treatment weighting with a flexible non-parametric model, our approach also shows how complex longitudinal selection processes can be analysed using rich, individual-level data without simplifying the underlying dynamics or imposing strong parametric structure.

Our findings show that cumulative exposure to disadvantaged neighbourhoods linearly reduces the likelihood of completing high school, with effects varying by childhood stage: strongest in late adolescence and, to a lesser extent, in early childhood. Results show that the cumulative effect of neighbourhood disadvantage can lower graduation rates by up to 34 percent. These results align with US studies (e.g., *Chetty et al.* 2016; *Crowder/South* 2011; *Wodtke et al.* 2011) but are smaller in magnitude. Even in Sweden's strong welfare state, local environments exert durable influence on educational opportunities, and ongoing welfare retrenchment may amplify the concentration and impact of neighbourhood poverty (*Alm et al.* 2020). Using sensitivity analysis, we find that these effects are robust to high levels of unobserved confounding, increasing confidence that the observed associations reflect a relationship between neighbourhood disadvantage and educational outcomes rather than spurious bias.

We also provide evidence for different effect sizes across childhood. Consistent with recent research (*Alvarado* 2016; *Lund* 2020; *Wodtke et al.* 2016), we find strong neighbourhood effects on high school graduation during adolescence. Adolescence is a critical stage for making educational decisions, as it determines future paths for children; it is also the period when peer influences are most significant. Early childhood also proved to be an important period for a child's educational attainment, as cognitive, social, and educational skills are primarily learned during this period.

Neighbourhood deprivation thus exerts an independent effect on educational outcomes. Several mechanisms, which cannot be disentangled here, underscore this association: the quality of the physical environment and the material resources and opportunities available in these spaces, but also the social and symbolic resources they provide. The unequal distribution of job and housing opportunities, as well as geographical discrimination (*Besbris et al.* 2015), directly influence the parental socioeconomic status and the family environment, which feeds into children's educational outcomes. The school environment is another decisive mechanism that varies with neighbourhood quality: the quality of teaching, the socioeconomic composition of students, the infrastructure, etc. all influence academic performance (*Bourdieu/Passeron* 1977) and may be a strong mechanism explaining our results (*Kuyvenhoven/Boterman* 2021; *Sykes/Musterd* 2011). Physical environmental factors such as air and noise pollution, disorder, and violence, which are worse in deprived areas, could also affect children and adolescents (*Levy* 2019; *Sampson* 2012; *Wodtke et al.* 2022). Perhaps most importantly, processes of collective socialisation and peer group influence might promote anti-educational norms and behaviours and directly affect educational outcomes.

Our findings indicate that neighbourhood disadvantage is associated with lower probabilities of high-school graduation regardless of whether children moved between neighbourhoods or experienced changes while remaining in place. By addressing neighbourhood selection over time, we adjust for the non-random processes that shape children's neighbourhood trajectories and distinguish between children whose trajectories into more disadvantaged neighbourhoods reflect broader family-level disadvantages and the effects of neighbourhood conditions themselves. Furthermore, although children who grow up in disadvantaged neighbourhoods are more likely to experience residential mobility (*Kuyvenhoven et al.* 2025), our results show that cumulative exposure to neighbourhood disadvantage remains associated with high-school graduation even after accounting for these mobility dynamics.

While this study focuses on the effect of prolonged exposure to disadvantaged neighbourhoods during childhood, we call for future studies that examine individuals over longer periods and across multiple generations. Evidence remains scarce on the differential effect of residential context during childhood and adulthood, but also between parents and children. Although we know that parental neighbourhood poverty is transmitted to children during their adulthood (*McAvay* 2018; *Sharkey* 2008; *van Ham et al.* 2014) therefore potentially affecting the educational outcomes of the next generation, we lack empirical evidence and theory on intergenerational neighbourhood effects.

A related challenge concerns the disentanglement of school effects from neighbourhood effects. While our study does not separately model these two contexts, future research should address this distinction given that children's developmental environments encompass both residential neighbourhoods and school catchment areas, which may only partially overlap. The existing evidence on the independent contributions of schools versus neighbourhoods remains mixed, with some studies suggesting that school quality mediates partially or completely neighbourhood effects on educational outcomes (*Kuyvenhoven/Boterman* 2021;

Sykes/Musterd 2011), while others find persistent neighbourhood influences net of school characteristics (*Wodtke et al.* 2023). Methodologically separating these intertwined contexts requires careful identification strategies and rich data on both residential histories and school attendance patterns across childhood. Machine learning algorithms, like BART, could be a possible leverage to facilitate large-scale longitudinal analysis capable of handling the complexity of such multi-level, temporally dynamic processes.

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