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On Equality of Opportunity: Neighbourhood Effects, Childhood Exposure and Lifetime Income in Sweden

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Abstract

Earlier research has found unequal access to beneficial residential contexts to be an important cause for increasing inequality and segregation in the developed world. In this thesis, we empirically study the importance of neighbourhood effects, i.e. geographically bounded, contextual aspects of a childhood residential area, in influencing economic outcomes in adulthood. We use a methodology proposed by Chetty and Hendren (2017), where we study a sample of movers who relocate to a more affluent neighbourhood in childhood and compare the outcomes for those who were younger at the time of moving to those who were older. Using a sample of individuals born in the 1950s and growing up in Stockholm, Sweden, we find indicative evidence for a neighbourhood effect, as displayed by increased lifetime income for individuals who move to a higher quality neighbourhood. We also find support for our hypotheses that the length of exposure to a better (worse) neighbourhood is important in steering lifetime outcomes, as well as the impact being larger the earlier in childhood exposure takes place. However, we cannot exclude the possibility that selection on parental characteristics or some other unobserved factor is partly driving the results.

Keywords: Neighbourhood effects, equality of opportunity, income inequality, segregation, exposure effects

JEL: C23, J31, J68, R23, Z13

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1 Introduction

A growing body of economic research is attempting to shed new light on the concept of inequality in the developed world, whether it be related to income, health, or opportunity. In a time of demographic shifts, with the rich world experiencing an ageing population and increasing low-skilled immigration, research on equal access to opportunity for all citizens has a crucial role to play. Among the most pressing causes for rising inequality is residential segregation between poorer and more affluent neighbourhoods, which hampers the scope for some citizens to improve their own situation, as well as entrenches inequality across generations (Musterd and Andersson, 2006; Brännström, 2012; Chetty et al., 2014; Robling and Pareliusson, 2017).

When studying the literature on segregation and inequality, it is imperative to have an understanding of the concept of neighbourhood effects. The phenomenon refers to geographically bounded, contextual aspects of an individual's childhood residential area that affect the accumulation of human capital, which in turn impacts outcomes later in life. Neighbourhood effects work through various channels, with some of the more frequently studied being quality of schooling, institutional development and peer effects (Ellen and Turner, 1997; Andersson and Subramanian, 2006; Galster et al., 2010; Sykes and Musterd, 2011; Schwartz, 2012; Sharkey et al., 2014). These channels all affect the possibility a child has to improve her own life outcomes, through the acquisition of skills, pursuit of higher education, establishment of a favourable social network and future access to the labour market.

In this thesis, we will empirically evaluate the importance of neighbourhood effects for individuals born in the 1950s in Stockholm, Sweden, during a time of broad expansion of the country's welfare state (Lundberg and Åmark, 2001). Sweden provides an interesting context for studies of inequality, being a politically egalitarian society, while also, a bit paradoxically, having one of the world's most unequal distributions of wealth (The Economist, 2019). Previous studies have emphasised how early childhood context is critical for intellectual and emotional development, as well as the prospect for future skills and achievements (Duncan et al., 1994; Heckman, 2007; Duncan et al., 2012; Chetty et al., 2016). We will therefore follow the individuals in our sample throughout their entire childhood in order to investigate how childhood residential context impacts lifetime income. Seeing that the quality of schooling is an important channel to consider, and that Swedish schools are administered at the municipal level, we will use municipalities as the definition of a neighbourhood.

We will be using a novel methodology proposed by Raj Chetty and Nathaniel Hendren (2017) to study individuals who move from a less affluent neighbourhood to a more affluent one. We then compare the economic outcomes of children who were younger at the time of the move to those who were older, thus examining the effect from exposure to a high quality neighbourhood rather than the effect from the move per se. To do this, we use longitudinal data from the SBC Multigen at Stockholm University on 14,608

individuals born in 1953 and growing up in Stockholm county. For these individuals, we can track the area of residence at a granular level during their entire childhood, as well as access income data from Statistics Sweden between 1990-2015. Considering the conclusions from previous research, we hypothesise that the length of exposure will be important in steering economic outcomes in adulthood (Ellen and Turner, 1997; Wodtke et al., 2011). We also expect exposure to have a larger impact the earlier during childhood it takes place (Duncan et al., 1998).

We find a significant neighbourhood effect as demonstrated by better economic outcomes in adulthood for those children who move to a higher quality neighbourhood. Although decreasing in magnitude, the effect persists when controlling for a number of observable parental characteristics, at least for individuals moving early in childhood, before starting school. There is also some evidence for an exposure effect, as measured by the differing rates at which lifetime income improves depending on how much time a child spends in the more affluent neighbourhood. However, we cannot entirely exclude the possibility that the results are driven by selection on parental socioeconomic status or some other unobservable factor.

Our study contributes to the existing body of research in a number of methodological and contextual ways. First, there is to our knowledge no previous study conducted in Sweden that uses a similar methodology to look at neighbourhood effects on lifetime income. Sweden offers a very different institutional setting compared to the United States, the context most frequently studied in the literature, which makes it an interesting setting to look at more carefully. Moreover, our definition of neighbourhoods will be significantly smaller than that of e.g. Chetty and Hendren (2017), who look at commuting zones in the USA. Lastly, the data we have allows us to follow the individuals in the sample throughout virtually their entire worklife. This means we can use lifetime income as the outcome variable of interest, contributing to previous studies that have evaluated outcomes at a static point in time, much earlier in adulthood when individuals could still be in education or training[1].

The remainder of this thesis will be organised as follows: Section 2 gives an overview of the relevant literature for our topic, while Sections 3 and 4 describe the data and our empirical strategy. Section 5 walks the reader through the main results, with Section 6 adding some further analysis. After discussing the robustness and validity of our findings in Sections 7 and 8, Section 9 will conclude.

2 Literature Review

The concept of neighbourhood effects is a widely studied topic with a variety of attempts in the literature to empirically quantify the magnitude this effect has on long-term life outcomes. There is far from a consensus among scholars, with debates ranging from methodological choices of such things as how to define

[1]For example, Chetty and Hendren (2017) use income at age 24 as their outcome of interest.

a neighbourhood, to the basic question of whether childhood residential contexts matter or not (Sharkey and Faber, 2014). In an American setting, several studies have found neighbourhoods in childhood to be important in steering adulthood life outcomes (Schwartz, 2012; Chetty et al., 2016; Chetty and Hendren, 2017). Meanwhile, in a Swedish setting, most studies have found small or negligible effects (Brännström, 2004, 2005, 2012; Hedman and Galster, 2013). This implies that institutional context is important for the interpretation and generalisability of results.

Sweden provides a particularly interesting context for the study of neighbourhood effects. There are a number of characteristics of the Swedish system that makes it unique, whether it be the institutionalised welfare policies, redistributive tax system, free education or the availability of affordable housing (Andersson and Subramanian, 2006; Brännström, 2012; OECD, 2017; Swedish Ministry of Finance, 2017). This way, the state has taken considerable responsibility for citizens' economic security, making Swedish residents less dependent on their families, with the ultimate goal of creating a society of more equal opportunity. Still, a number of studies find that this egalitarian state does not level the playing field entirely, with particular ethnic or socioeconomic groups facing less economic opportunity as well as being less likely to pursue higher education, and that these differences are not only attributable to individual characteristics (Andersson and Subramanian, 2006; Musterd and Andersson, 2006). Moreover, there is a strong correlation between individual economic success in adulthood and the socioeconomic status of a childhood neighbourhood as well as the family context that an individual is born into (Andersson and Malmberg, 2018; Hedman et al., 2019). Thus, there is evidence for neighbourhood effects having an impact on the availability of opportunity and the entrenchment of economic inequality also in a Swedish context. However, when it comes to the magnitudes and the channels that drive these effects, the evidence remains inconclusive.

Regardless of context and plausible channels, virtually all studies of neighbourhood effects face a similarly challenging obstacle in establishing causality: selection bias, stemming from inherently different individuals deciding to cluster in different areas. There are several promising avenues in the literature using exogenous variation to alleviate this bias. One such approach is to use displacement shocks caused by e.g. a large employer shutting down or natural disasters (Eliason and Storrie, 2006; Sacerdote, 2012; Chetty and Hendren, 2017). By taking the decision to move out of the individual's hands, the identifying assumption does not hinge on people moving away from a poor neighbourhood being ex ante comparable to those staying put. In a Swedish context, one study exploits the natural experiment created by a refugee placement policy where the officer making the allocation did not meet the refugees in person, and therefore had to make the decision based on observable characteristics that could be controlled for in the econometric specification (Åslund et al., 2011).

However, the most common approach to deal with selection bias is to make use of random allocation. In the American context, projects like these date as far back as the 1976 Supreme Court case Hills vs Gatreaux

where some public housing families in Chicago were moved to more affluent neighbourhoods (Rubinowitz and Rosenbaum, 2000). In a modern setting, the most well-known intervention of this kind is the Moving To Opportunity project. This experiment, which was conducted by the US Department of Housing and Urban Development, randomly assigned low-income families living in public housing in areas with particularly high poverty rates to a treatment group granting them highly subsidised housing if they decided to move to a less disadvantaged neighbourhood. Evaluations of the project have reached a number of conclusions that resonate well with earlier research (Kling et al., 2007; Clampet-Lundquist and Massey, 2008; Ludwig et al., 2013; Chetty et al., 2016). The following findings are well established in the literature, and constitute the conceptual framework for this thesis. They are instrumental in steering our methodological considerations, as well as shaping our hypotheses. They constitute that:

1. The length of exposure to a higher quality neighbourhood is crucial in steering outcomes later in life (Duncan et al., 1994; Ellen and Turner, 1997; Wodtke et al., 2011).
2. The earlier exposure starts, the more likely it is to have a lasting effect (O'Connor et al., 2000; Cunha and Heckman, 2010).
3. In order to see any impact from childhood neighbourhood effects, a certain time needs to pass. Evaluations of adulthood outcomes cannot be too close in time to an intervention (Chetty et al., 2016).

In absence of exogenous variation from either randomised allocation or natural experiments, other methodological approaches are required in order to make causal claims about neighbourhood effects. This brings us to the literature studying movers, a body of research dating back at least to a paper by Daniel Aaronson (1998), in which the author looks at families moving within the United States. Aaronson employs family fixed effects in order to compare the effect from exposure to a better neighbourhood across siblings of different ages. This way, he eliminates selection bias from unobservable characteristics that are constant within a family. He ultimately finds that there are differences in the outcome of interest (educational attainment) even after family fixed effects have been employed, implying that there are neighbourhood effects extending beyond family unobservables and self-selection into an area of residence[2].

For this thesis, two papers are of particular methodological importance. As mentioned in Section 1, we will be using the approach proposed by Chetty and Hendren (2017). In their paper, the authors define the quality of a neighbourhood by the mean income percentile rank in adulthood for the individuals who spent their entire childhood in an area[3]. Using a sample of Americans born between 1980-1988, they study movers, i.e. children who moved across neighbourhood borders in childhood[4] and look at individual movers'

[2]For examples of more recent papers using movers to alleviate bias in observational data, although to look at different outcomes, see e.g. Chetty et al. (2013) and Finkelstein et al. (2016).

[3]From here on referred to as permanent residents.

[4]Neighbourhoods are defined as so called commuting zones. There are 741 commuting zones in the US, with an average size of 380,000 residents.

adulthood income percentile rank as the outcome variable[5]. Neighbourhood quality in a mover's destination is the independent variable of interest. The authors find that for each additional childhood year a mover is exposed to a neighbourhood where the mean income rank for permanent residents is 1 percentile point higher, there is a 0.04 percentile point increase in the mover's adulthood income rank. Thus, one more year spent in the higher quality neighbourhood increases adulthood income by 4%. This treatment effect is linear and robust to a number of checks for selection bias, including controlling for time-varying observables such as parental income, employing family fixed effects and using displacement shocks as a source of exogenous variation.

Lastly, Evelina Björkegren (2018) employs Chetty and Hendren's methodology to study neighbourhood effects on adolescent health using Swedish administrative data for children born between 1984-1992. She uses municipalities as her definition of a neighbourhood and lets the average health outcome for permanent residents at age 16-19 define neighbourhood quality. Her results point to there being significant neighbourhood effects on health, but that these effects are instantaneous, and thus she finds no evidence for any exposure effect. Again, these results are robust to controlling for family observable characteristics as well as family fixed effects.

3 Data

For the empirical analysis we will be using the data set SBC Multigen belonging to a research program called RELINK (Reproduction of inequality through linked lives) at Stockholm University. It consists of two main parts[6] (Almquist et al., 2019):

1. The first is the Stockholm Metropolitan Study (SMS). This was a survey-based project tracking a cohort of individuals born in 1953 and living in the greater Stockholm area in 1963 (n=15,117). The data was being updated and expanded up until 1986 when the debate on personal integrity in Sweden led the project to be discontinued.

2. The second part is based on Swedish administrative data from a number of national registers on health, income, education, and work for all individuals born in 1953 and living in Sweden in either 1960, 1965 and/or 1968, as well as their parents, siblings and children (n=2,390,753).

These two anonymous data sets have then been combined through a probability matching procedure[7], meaning the researchers used a number of variables to connect an individual in the first data set with the

[5] Adulthood income is evaluated at age 24.

[6] We are most grateful to Ylva B. Almquist at the Department of Public Health Sciences at Stockholm University for allowing us access to the data set.

[7] For details on the procedure, see the original papers outlining the data collection (Stenberg et al., 2007; Almquist et al., 2019).

matching observation in the second. This rendered the final data set, the SBC Multigen, a multigenerational data set containing a cohort of 14,608 individuals growing up in Stockholm county that can be followed from their birth in 1953 until reaching their retirement age in 2018, as well as some sibling and parental data. The data includes variables that have been collected both through surveys and questionnaires, as well as administrative data from some of Sweden's extensive registers. The sources include the National Board of Health and Welfare, Statistics Sweden, the Social Insurance Agency, the National Crime Register, the Swedish Defence Recruitment Agency and Swedish Population and Housing Censuses. Seeing that the focus of this paper will be on economic outcomes in connection to geographical locations, much of our focus will be on the geographical census data and income data from Statistics Sweden.

3.1 Descriptive statistics

In order to employ our empirical strategy, we first need to distinguish between permanent residents and movers. Permanent residents are those individuals who spent their entire childhood from age 0-18 (1953-1971) in the same municipality, while movers are those who moved across municipal borders exactly once during this observation period. For ease of analysis and to avoid spurious correlations, we exclude those individuals who spent any part of their childhood outside of Stockholm county. This leaves us with a baseline sample of 11,188 individuals, of whom 8,546 are considered permanent residents and 2,269 are movers[8]. Since the analysis will be focused only on the subset of movers, the methodology does not require movers and permanent residents to be ex ante comparable. However, in order to get a sense of the individuals in the sample, Table 1 includes summary statistics on a number of observable parental characteristics including the main variables that will later be included in the analysis. We note that movers on average seem to have parents of stronger socioeconomic status, as shown by higher income and education levels for the parents. Moreover, movers' parents seem to be slightly younger than those of permanent residents. None of these observations are particularly surprising, since it is likely that parents who decide to move together with their children are inherently different from those who decide to stay put. It is plausible that the decision to move signals some kind of inherent ambition or drive. What is somewhat surprising is that movers are seemingly less likely to have had a divorce than permanent residents, seeing that previous studies have found divorce to be one of the main drivers behind a decision to move (Mincer, 1978; Heidrich, 2016). This could be because divorce in general was a rare phenomenon in Sweden at this time, as implied by the mean divorce rate being just 3.67% among movers and 4.76% among permanent residents. Thus, with very few people getting a divorce and large standard deviations, we cannot draw any conclusions about this characteristic's differing effect on the two groups.

[8]Moreover, 373 observations are excluded since they moved more than once during the period of interest.

Table 1: Descriptive statistics for movers and permanent residents

Variables	(1) Movers	(2) Permanent residents
Sex (Female)	0.507	0.500
	(0.500)	(0.500)
Parents Income Percentile (1963)	54.66	48.62
	(29.39)	(28.48)
Years of Education (Head of Household)	8.966	8.458
	(2.045)	(1.832)
Married	0.860	0.838
	(0.347)	(0.368)
Divorced	0.0367	0.0476
	(0.188)	(0.213)
Mother YoB	1925	1924
	(5.349)	(5.756)
Father YoB	1922	1920
	(7.292)	(7.741)
Average Lifetime Income	2,647	2,611
	(2,126)	(1,864)
Observations	2,269	8,546

Notes: The first row of each variable presents the mean value. Standard deviations are presented in parentheses.

3.2 Geographical variables

We choose to follow Björkegren (2018) and use municipalities (the smallest administrative unit in Sweden) as the definition of a neighbourhood in our baseline analysis. The data set includes information on different levels of geographical aggregation for different years and to be able to extend the analysis by using the smaller aggregation called parish, we choose to limit the observation period to those years where we have information on both municipalities and parishes. This leaves us with 1953 (when the individuals in the sample were born), 1960, 1963, 1967 and 1971 (when the individuals in the sample turned 18) as the years of observation[9]. Ideally, we would have access to administrative data on a yearly basis, which would enable us to quantify the exposure effect from spending one additional year in a higher quality neighbourhood. However, due to the sensitivity of individual data and the difficulties that therefore arise in acquiring it, the data set has limitations and we face a trade-off between frequency and geographical granularity of observations. When faced with such a choice, we have decided to prioritise smaller administrative units.

One important aspect to consider is that the geographical areas that we study have not been stable over time. Sweden had two large municipal reforms taking place in 1952 (before the cohort were born) and in 1971 (the year the cohort turned 18), thus leaving the childhood period mostly untouched by large alterations

[9]The exact date for when the geographical data is collected varies across the years. For 1953 it is the day the individuals were born, 1 November for 1960 and 1963, the last week of December for 1967 and week 16 for 1971.

(Nilsson and Forsell, 2013). However, in addition to these two major reforms, there have been a number of smaller changes where municipalities have either been absorbed into a larger one, or have been recoded to belong to another county. In the cases where municipal definitions changed during the observation period we have aggregated upwards, meaning we have used the definition at the point of observation where the municipality was at its largest. To keep the geographical distinctions comparable across all years, we have adhered to the following conditions:

1. First, we use the tool REGINA provided by Statistics Sweden to establish what the municipalities in Stockholm county looked like after the reform in 1971. We then aggregate upwards so that this regime holds for every year of observation, thereby recoding some municipalities in earlier years.

2. Second, we follow the criteria set up by the original researchers of the Stockholm Metropolitan Study in defining the Stockholm Metropolitan Area (Martens, 1975). These conditions meant that some municipalities belonging to Stockholm county were not included in the data set. They constituted that apart from the municipality of Stockholm, the only contiguous municipalities that should be included were those where:

    - more than 50% of the population lived in localities with at least 200 inhabitants,
    - more than 15% of the working population had employment in Stockholm,
    - and no more than $\frac{1}{3}$ of the working population were employed in agriculture or similar lines of work.

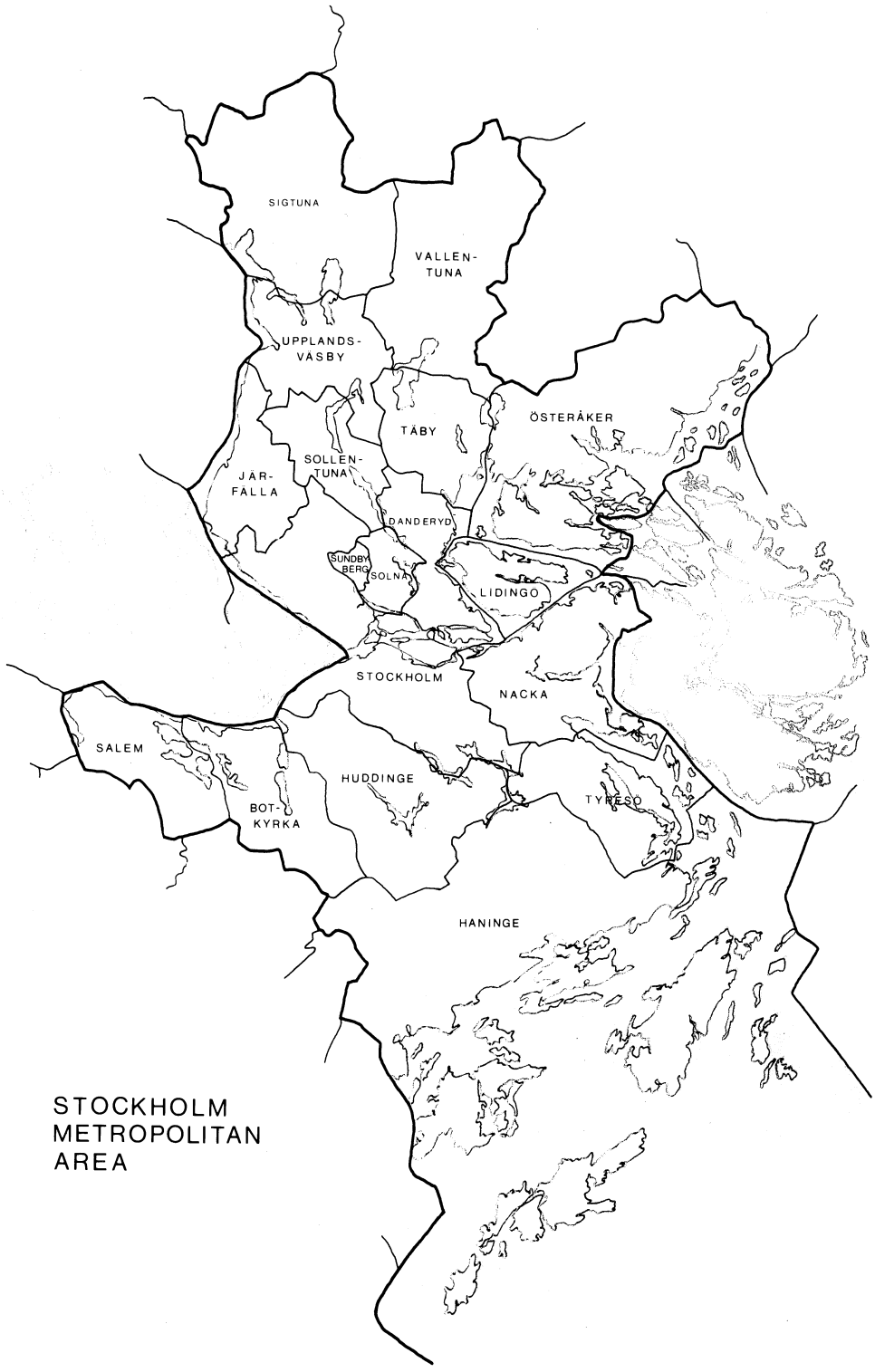
This leaves us with a total of 18 municipalities in the greater Stockholm area at the beginning of the observation period, out of which Stockholm municipality is by far the largest one in terms of where the individuals in the sample reside, as shown by Table 2.

Table 2: Municipalities in Stockholm county, 1953

	Frequency	Share
Stockholm	8,369	74.80%
Solna	499	4.46%
Nacka	312	2.79%
Sundbyberg	306	2.74%
Lidingö	266	2.38%
Huddinge	245	2.19%
Sollentuna	219	1.96%
Danderyd	180	1.61%
Täby	156	1.39%
Järfälla	122	1.09%
Haninge	103	0.92%
Botkyrka	96	0.86%
Upplands Väsby	84	0.75%
Sigtuna	68	0.61%
Vallentuna	51	0.46%
Österåker	49	0.44%
Tyresö	40	0.36%
Salem	23	0.21%
Total	11,188	100%

To get a sense of the moving patterns across municipalities for the sample of movers, we direct the attention to Table 3. We note a clear pattern of the most common move going from Stockholm to one of the suburban municipalities, as shown by Stockholm being the origin for 1,458 moves but the destination for only 409 of them. We take this observation as suggestive evidence that moving is a natural part of most families' life courses, and that many of the moves observed here are from apartments in the inner city to residential districts in the suburbs. This type of moving often takes place interdependently with a family growing in size, or with changes to family wealth (Mincer, 1978; De Jong and Graefe, 2008; Geist and McManus, 2008). Among the recipients, Järfälla and Huddinge are the municipalities with largest absolute inflow of movers leaving Stockholm during the observation period. Moreover, we note that moves in general take place between nearby municipalities, as shown by the frequent moving between e.g. neighbouring Solna and Sundbyberg or Danderyd and Täby.

Figure 1: Map of municipalities in Stockholm county



Source: Authors' rendering of a Stockholm Metropolitan Study map (Martens, 1975).

Table 3: Moving patterns across municipalities

	Origin																		
Destination	Stockholm	Uppl. Väsby	Vallentuna	Österåker	Järfälla	Huddinge	Botkyrka	Salem	Haninge	Tyresö	Täby	Danderyd	Sollentuna	Nacka	Sundbyberg	Solna	Lidingö	Sigtuna	Total
Stockholm	0	7	4	6	17	50	6	0	14	5	16	14	24	54	53	95	37	7	409
Uppl. Väsby	15	0	1	0	0	0	0	0	0	1	0	0	3	1	2	4	0	3	30
Vallentuna	15	0	0	0	1	1	1	0	0	0	2	0	0	0	2	2	1	0	25
Österåker	26	0	0	0	0	1	0	0	0	0	1	1	0	1	0	2	1	0	33
Järfälla	161	1	0	0	0	0	2	1	0	0	1	3	2	3	13	14	1	1	203
Huddinge	176	0	1	1	2	0	4	1	5	0	1	2	1	4	0	1	1	0	200
Botkyrka	37	0	0	0	0	5	0	2	1	0	0	1	1	2	0	1	1	0	51
Salem	22	0	0	0	0	1	0	0	0	0	0	0	0	0	0	0	0	0	23
Haninge	89	1	0	0	0	4	1	1	0	1	2	1	1	1	2	1	1	3	109
Tyresö	101	0	0	1	0	2	0	0	3	0	0	2	1	5	1	2	0	0	118
Täby	122	0	5	1	2	3	0	0	0	0	0	16	7	1	7	9	3	2	178
Danderyd	105	0	1	0	0	3	1	0	2	0	9	0	0	1	4	9	2	1	138
Sollentuna	101	4	4	0	1	1	1	1	0	0	1	4	0	2	8	14	3	0	145
Nacka	135	1	0	1	0	1	0	0	0	0	1	1	1	0	0	3	5	1	150
Sundbyberg	39	0	0	0	4	1	1	0	0	0	1	0	0	3	0	25	1	0	75
Solna	152	1	1	0	1	0	1	0	0	0	1	3	3	4	14	0	6	2	189
Lidingö	143	0	0	0	2	4	0	0	0	0	2	1	1	1	6	5	0	0	165
Sigtuna	19	1	0	1	0	0	1	0	0	0	0	3	2	0	0	1	0	0	28
Total	1,458	16	17	11	30	77	19	6	25	7	38	52	47	83	112	188	63	20	2,269

In order to extend the analysis, we will also conduct our empirical study on a smaller geographical aggregation called parishes within Stockholm municipality. Similarly to the main analysis, we face a difficulty in that definitions of parishes changed during the observation period. However, by using the original documentation from the Swedish censuses during this time as well as the code book from the Stockholm Metropolitan Study we manage to conclude which definitions of parishes were used in constructing the data set, leaving us with a total of 27 parishes where individuals in the sample reside (Janson, 1980)[10] [11].

[10]The reason for the different number of total observations in Table 4 of parishes and the number of individuals living in the city of Stockholm in 1953 according to Table 2 is that we, when conducting the analysis only on parishes, restrict the sample to only include those individuals who remain within Stockholm municipality for the entire observation period, 1953-1971.

[11]For this part of the data collection, we are most grateful to Sten-Åke Stenberg at SOFI, Stockholm University for his generous help and guidance.

Table 4: Parishes in Stockholm municipality, 1953

	Frequency	Share
Hägersten	735	11.01%
Farsta	667	9.99%
Bromma	545	8.16%
Skarpnäck	539	8.07%
Enskede	503	7.53%
Brännkyrka	491	7.35%
Högalid	286	4.28%
Katarina	275	4.12%
Oskar	272	4.07%
St Göran	264	3.95%
Vantör	255	3.82%
Matteus	233	3.49%
Spånga	229	3.43%
Västerled	206	3.09%
Sofia	167	2.50%
Kungsholm	137	2.05%
Maria	132	1.98%
Essinge	126	1.89%
Engelbrekt	123	1.84%
Hedvig Eleonora	103	1.54%
Johannes	100	1.50%
Gustav Vasa	91	1.36%
Adolf Fredrik	89	1.33%
Nikolai	40	0.60%
Klara	31	0.46%
Hässelby	25	0.37%
Jakob	12	0.18%
Total	6,676	100%

3.3 Income variables

This study is aiming to evaluate long-term economic outcomes for individuals growing up in Stockholm county and connect these to the neighbourhood in which they grew up. Thus, the next step becomes to define our income measure of choice. We do this using administrative data from Statistics Sweden's LISA registry[12], giving us income data for the individuals in the sample between 1990-2015 (at ages 37-62). This allows us to study income on a yearly basis for a large part of the working life for each individual in the data set. We sum earned income, defined as the sum of labor income and income from running a business, over all the years for which the values are non-missing, and take the yearly average to find our preferred income measure[13]. For ease of analysis, we follow Chetty et al. (2014) and assign percentile ranks rather than absolute values in nominal terms for our income measures to each individual in the subset of movers.

[12]The SBC Multigen contains this information for the 1953, Stockholm cohort.

[13]We choose to use earned income in order to include business income, seeing that this is a plausible channel of additional lifetime income for successful individuals. The results are not sensitive to the choice of income variable, as shown in Appendix B.2.

This then constitutes the outcome variable of choice. Seeing that we are comparing individuals who are all born in the same year, we do not have to adjust for inflation or other fluctuations to the individual's macroeconomic context.

The independent variable of interest is the affluence of a neighbourhood. Rather than making an ex ante assumption on how to rank municipalities in the analysis, we follow Chetty and Hendren (2017) and let the data do the ranking for us. We do this using a variable that assigns a destination municipality to every mover in the data set. Next, we compute the lifetime income measure for each permanent resident and create percentiles of these economic outcomes. We then find the mean income percentile within each municipality, which in practice ranks the neighbourhoods based on how well the permanent residents do in terms of annual earned income during the most productive years of their working life. We are thus left with our independent variable of choice, the average lifetime income rank for permanent residents in each municipality, which we will be referring to as neighbourhood quality (see Table 5). By using the destination variable, we can then assign this value to each individual mover in the sample based on the municipality they move to.

Table 5: Neighbourhood quality for each municipality under observation in Stockholm county

Municipality	Neighbourhood quality
Danderyd	59.03
Salem	56.80
Solna	54.53
Sollentuna	54.27
Täby	54.16
Lidingö	52.93
Huddinge	52.85
Tyresö	50.96
Stockholm	50.16
Sundbyberg	50.08
Österåker	49.55
Haninge	49.38
Nacka	49.36
Upplands Väsby	48.90
Järfälla	48.74
Sigtuna	48.24
Vallentuna	44.86
Botkyrka	42.76

In addition to these main variables of interest, we construct a measure of parental income to use as a control variable in the econometric specifications. This is constructed by simply adding the reported net income in 1963 for the mother of the individual in the sample to the reported net income for the father in the same year. Since missing values of these variables are coded as 0, we are left with observations for every individual in the data set, with 651 of them taking on the value of 0. However, it remains ambiguous whether the value is actually missing or if the reported net income is equal to 0. We therefore attempt to differentiate between these two cases using variables on the marriage status of the sample individuals' parents. In brief, we

follow a regime where if there is uncertainty, we assume that an individual lived with her mother, unless the father is a widower or if the marital status of the mother is missing but not for the father, in which case the child is assumed to live with her father. Moreover, if one of the parents is married, but the marital status of the other parent differs, we assume that there has been a divorce and remarriage. Since we cannot establish the income of the new partner of the remarried parent, we treat these cases as missing values in order to avoid using misleading information. Finally, we construct ranks of the parental income variable to employ as a control variable in the empirical specification. We specifically choose to construct parental income quintiles, in line with Björkegren (2018), to obtain sufficiently many observations in each group while maintaining variation. Figure 4 in Appendix A.1 shows the positive, linear relationship between parental income and the lifetime income of a mover, hinting it is a suitable control variable for our econometric specification.

4 Empirical Approach

4.1 Baseline specification

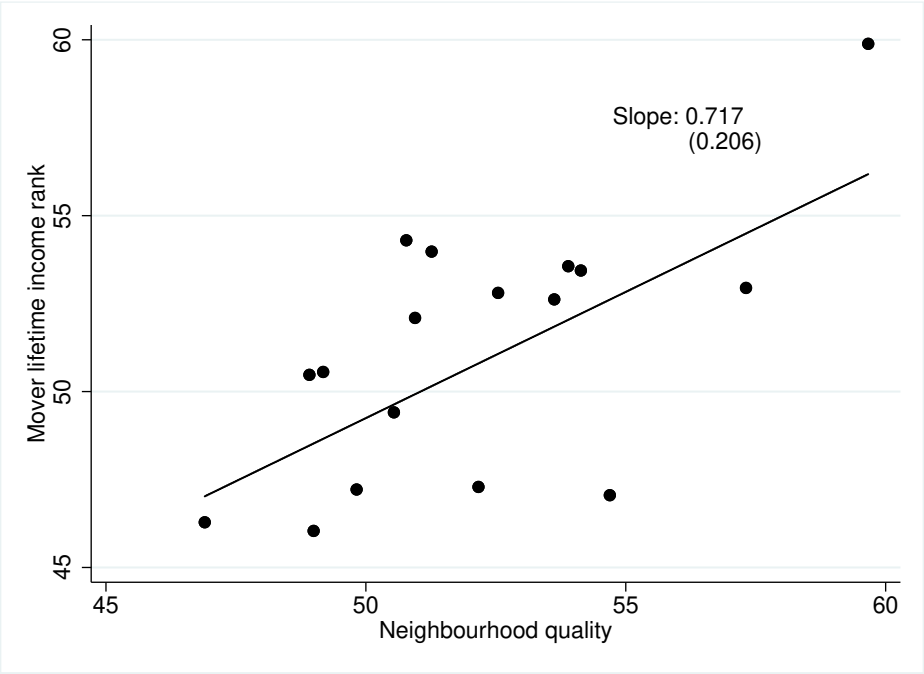
We estimate several versions of our empirical model on the subset of movers, starting with the following specification:

$$y_i = \alpha_o + \beta \bar{y}_d + \epsilon_i \tag{1}$$

where y_i is the preferred measure of lifetime income for mover i and α_o is capturing a fixed effect for the origin municipality of this individual[14]. The variable $\bar{y}_d$ corresponds to the independent variable of interest, i.e. neighbourhood quality in the destination municipality, β is the treatment effect from moving to a higher quality municipality where the mean lifetime income rank for permanent residents is one percentile point higher and ϵ is the error term. Seeing that we run this model on the full sample of movers without taking the timing of the move into consideration, the coefficient β is the average effect in this sample from moving to a more beneficial destination municipality. In this very simple model, β would capture the causal neighbourhood effect under the identifying assumption that there is no selection on the choice of destination for a mover that affects individual economic outcomes later in life, given origin municipality. In other words, individuals who have the same origin municipality and decide to move to different destinations are in expectation similar. This is an unreasonably strong assumption, but it gives us a starting point for the remainder of our analysis. Figure 2 plots the positive relationship between neighbourhood quality in the destination municipality and the lifetime income rank for movers, illustrating the basic premise for the empirical study.

[14]For an extensive discussion on applications of panel data methods, see Wooldridge (2012).

Figure 2: Relationship between movers' lifetime income rank and neighbourhood quality in the destination



4.2 Exposure effects

Studies of exposure time is a promising avenue for research on neighbourhood effects using observational data. This is mainly because it relaxes identifying assumptions when it comes to estimating causal effects. With random assignment, the coefficient β in Equation (1) would give an unbiased estimate of the effect from living in a particular neighbourhood, since randomisation means that the error term ϵ is uncorrelated with the independent variable $\bar{y}_d$, the neighbourhood quality in the destination municipality. In observational data, however, this is highly unlikely to hold, since there are factors that make movers inherently different from one another and that will affect both a family's choice of destination as well as the lifetime income rank of the children in the family. Thus, there is a correlation between the independent variable of interest and the error term, biasing the results. Taking this endogeneity into consideration, the regression coefficient from Equation (1) can be thought of as

$$b = \beta + \delta$$

where δ is the standard selection effect corresponding to parental characteristics and other factors that affect children's outcomes while also being correlated with the destination choice of the moving parents. However, by estimating exposure effects, the identifying assumption is relaxed to not require the choice of destination to be unrelated with children's potential outcomes, but only that the timing of the move is. This allows for systematic differences between parents that choose to move to a more affluent neighbourhood and

those who choose to move to a less affluent one in terms of characteristics such as inherent ambition and the effort they put into their children's upbringing. The identifying assumption is instead that these differences cannot vary with the age of a child when her parents decide to move, i.e. that $\delta_m = \delta$ for all moving ages m (Chetty and Hendren, 2017). This is still quite a strong assumption as it is possible that families who move when their children are young are more aware of the impact that good (bad) peers can have, or invest in their children in other ways. We will therefore take measures in an attempt to validate this assumption.

Due to limitations of the geographical data for this project, we are not able to pinpoint the exact timing of a move. Instead, we are left to divide the sample of movers into four different categories M_i based on the five years for which we have geographical data (1953, 1960, 1963, 1967 and 1971)[15]:

1. *Preschool movers*, those children whose families move exactly once between 1953-1960 (age 0-7).
2. *Early movers*, those children whose families move exactly once between 1960-1963 (age 7-10).
3. *Youth movers*, those children whose families move exactly once between 1963-1967 (age 10-14).
4. Lastly, *adolescent movers*, those children whose families move exactly once between 1967-1971 (age 14-18)[16].

Table 6 shows the distribution of moving status across the total sample, including those who are excluded due to having moved across municipalities more than once during the observation period, as well as permanent residents. We note that permanent residents are by far the most common category.

Table 6: Moving status, total sample

Moving status	Frequency	Share
Moved more than once	373	3.33%
Permanent resident	8,546	76.39%
Moved once, age 0-7	1,398	12.50%
Moved once, age 7-10	390	3.49%
Moved once, age 10-14	221	1.98%
Moved once, age 14-18	260	2.32%
Total	11,188	100%

We then create four different dummy variables, one for each age interval of movers, and interact these with the independent variable $\bar{y}_d$ of neighbourhood quality in the destination municipality (Björkegren,

[15]This is possible because we are studying a single birth cohort, meaning we can group individuals into moving age intervals without knowing exactly which year they moved.

[16]As mentioned in Section 3, the location data is not collected on 31 December, meaning we do not know for certain if for example an early mover has already turned 7 or not. We thus choose not to make the age intervals mutually exclusive, since an individual can in fact be 7 and be considered both a preschool mover and an early mover, depending on the location variable for a given year.

2018). That leaves us with the following specification:

$$y_i = \alpha_{om} + \beta_1 \bar{y}_d + \beta_2 M_i \bar{y}_d + \epsilon_i. \tag{2}$$

As before, α_{om} corresponds to a fixed effect, only in this case it has the additional subscript m which refers to the moving age interval for individual i. The variable y_i refers to lifetime income rank for mover i, $\bar{y}_d$ to the neighbourhood quality of the destination municipality and M_i to the age interval at the time of move for individual i. Since M_i is interacted with $\bar{y}_d$, we are allowing for different magnitudes of the coefficient β_2 depending on which age interval a mover belongs to, thus evaluating whether exposure time makes a difference to the rate at which a mover's lifetime income rank converges to that of a permanent resident in a more affluent municipality.

4.3 Observable family characteristics

Next, we expand the model by controlling for observable family characteristics that could bias our results. In the baseline case, this would be factors that correlate with both the choice of destination municipality as well as the lifetime income ranks of children in a particular family. In the model looking at exposure effects, we should consider factors that would change the family environment for a child in proportion to exposure time in such a way that it has an effect on adulthood income, but are independent of other neighbourhood characteristics. This could e.g. be a job promotion forcing the family to relocate. In that case, a child would be exposed to the positive downfall that comes from an income shock induced by the promotion. This would in turn coincide with the timing of the move, but only be related to the job promotion, not the neighbourhood per se. In order to control for this, we would ideally have yearly information related to such potentially confounding factors. However, when it comes to variables related to parents and the family environment, the data set is somewhat scarce. We will therefore not be able to directly control for these factors in a completely satisfactory way. Both Chetty and Hendren (2017) and Björkegren (2018) have access to parental income shocks and marital status on a yearly basis, two factors that have long been considered important in the literature when it comes to triggering a decision to move (Mincer, 1978; Jencks and Mayer, 1990; Heidrich, 2016). We will instead include a set of static variables related to observable family characteristics in an attempt to alleviate omitted variable bias as best we can. This gives us the following specification in the baseline case:

$$y_i = \alpha_o + \beta \bar{y}_d + \gamma \mathbf{X}_i + \epsilon_i \tag{3}$$

where the interpretation of y_i, α_o, β and $\bar{y}_d$ are as before. $\mathbf{X}_i$ is a vector of variables controlling for a child's sex, whether her parents were married throughout the entire childhood, the father's social class in

1963, the level of finished education as measured in years for the head of household (usually assumed to be the father in the data set) and categorical variables for parental income quintiles in 1963[17]. These are all factors that could bias the results. For example, it is plausible that more educated parents invest more in their children by moving to a neighbourhood where the schools are considered to be better. Moreover, the socioeconomic status of parents, as proxied in our specification by parental income and social class of the father, is plausibly a consequence of some inherent traits that are likely to affect both the choice to move to a high quality neighbourhood and the child's economic outcomes. For instance, these traits could be the ability to plan long-term, being disciplined or putting a premium on educational attainment. This causes a correlation between the error term and our independent variable of interest, making it a source of omitted variable bias. At the same time, we can expect these traits to also translate into higher socioeconomic status for the parent as well, meaning we alleviate the bias by controlling for it. Lastly, we note that divorce is one of the most important factors in triggering a decision to move, while also having an independent effect on economic outcomes for the children (Heidrich, 2016; Frimmel et al., 2016). Excluding this variable would therefore bias our estimates. The interpretation thus becomes that β estimates the causal neighbourhood effect under the assumption that there is no selection on destination choice given origin municipality, holding a child's sex as well as the parental socioeconomic and marital statuses fixed. Though still a strong assumption, this alteration drastically eases the restrictions on our first specification.

We make the same adjustments to the model of exposure effects, leaving us with our preferred specification:

$$y_i = \alpha_{om} + \beta_1 \bar{y}_d + \beta_2 M_i \bar{y}_d + \gamma \mathbf{X}_i + \epsilon_i. \tag{4}$$

This model estimates the causal neighbourhood effect given an origin municipality and a child's age interval at the time of moving, holding a child's sex as well as socioeconomic and marital statuses of parents fixed. The identifying assumption is that the unobservable characteristics that could bias our estimates and that we are unable to control for cannot systematically differ with the age interval M_i of a child when her parents decide to move, a substantially less restrictive assumption than what we started out with in our first, baseline specification. Moreover, since we are studying only one particular birth cohort, we do not have to account for a number of unobservable, potentially confounding factors that could have biased the results if indiviudals were born at different times, e.g. the general state of the economy or societal trends. This makes the analysis more straightforward.

[17]This is one aspect where we differ from Chetty and Hendren (2017). They include parental income as a fixed effect rather than a control variable in their regressions. The reason why we choose to do it differently is that we do not focus on intergenerational mobility as they do, and are thus less concerned with the differing convergence rates (as shown by different β) between movers with varying parental income. We instead use the variable mostly to avoid omitted variable bias. In Table 14, Appendix A.2 we show that this choice does not affect the size of the coefficient, but yields a more precise estimate.

5 Main Results

Next, we move on to the results from the different specifications as described by Equation (1)−(4). These are shown in Table 7. Starting with the simple regression case in Model 1, we see evidence for a neighbourhood effect in the sample of movers. The statistically significant coefficient of 0.72 implies that by moving to a higher quality municipality where the average lifetime income rank for permanent residents is one percentile point higher, the average mover in the sample will improve her lifetime income rank by 0.72 percentile points. This effect could be thought of as the average speed with which movers in the sample converge to the outcomes of permanent residents, meaning that the average mover makes up for 72% of the initial head start that a permanent resident of a more affluent neighbourhood has, a substantial effect. Directing our attention to Model 2, however, we note that this effect virtually disappears when controlling for the child's sex, family characteristics and socioeconomic background. Instead, the control variables are mostly significant and have strong effects. We note that having parents ranked in the bottom income quintile indicates a decrease in lifetime income rank by 8.03 percentile points, compared to having parents ranked in the top income quintile. Also, having parents within the upper class or upper middle class suggests an increase in lifetime income rank by 7.40 percentile points, compared to having parents that are classified within the unskilled working class. However, we do not see a significant effect from the head of household having one additional year of completed education. We further note that having parents that are married throughout the entire childhood has a positive effect on lifetime income rank, increasing it by 4.57 percentile points. Being female corresponds to a decrease in lifetime income rank by 13.13 percentile points. This implies that much of the explanatory value in Model 1 is attributable to one or more of the socioeconomic variables that we control for in Model 2, signalling selection based on parental characteristics.

It is worth noting that when we add control variables to the regressions, the number of observations drops from 2023 to 1651. This is due to there being missing values in the control variables for some observations. In order to make sure that this does not cause systematic differences to our estimates, we rerun Model 1 and Model 3 using the smaller, more restrictive sample. The results from this robustness check can be found in Table 19 of Appendix B.1. The pattern is the same and does not alter our conclusions from the main analysis[18].

[18]However, we note that the estimated coefficient for movers in the age interval 7-10 is enhanced when reducing the sample, giving us reason to be somewhat cautious when interpreting the magnitude of this estimate.

Table 7: Main results

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Neighbourhood quality	0.72***	0.14	0.83***	0.35*
	(0.06)	(0.11)	(0.12)	(0.17)
Sex (Female)		-13.13***		-12.63***
		(1.22)		(1.21)
Parental Income: 1st quintile (bottom)		-8.03***		-7.86***
		(1.76)		(1.97)
Parental Income: 2nd quintile		-4.25***		-4.00***
		(1.20)		(1.37)
Parental Income: 3rd quintile		-4.87**		-4.84**
		(1.96)		(2.24)
Parental Income: 4th quintile		-1.55		-0.92
		(1.20)		(1.64)
Omitted variable: 5th quintile (top)		-		-
Married - Parents		4.57*		3.33
		(2.51)		(2.80)
Upper and upper middle class		7.40***		7.41***
		(1.85)		(2.08)
Lower middle class, officials		5.44***		5.09***
		(1.50)		(1.51)
Lower middle class, entrepreneurs		-0.38		-0.50
		(1.86)		(1.77)
Working class, skilled workers		-0.94		-0.46
		(1.90)		(1.53)
Omitted variable: Working class, unskilled workers		-		-
Years of Education (Head of Household)		0.31		0.31
		(0.22)		(0.22)
Move age 7-10*NQ			-0.66***	-1.16***
			(0.20)	(0.25)
Move age 10-14*NQ			0.11	0.09
			(0.37)	(0.30)
Move age 14-18*NQ			0.20	0.35
			(0.31)	(0.26)
Observations	2,023	1,651	2,023	1,651
R-squared	0.01	0.10	0.05	0.13
Origin FE	YES	YES	YES	YES
Age interval FE	NO	NO	YES	YES
Controls	NO	YES	NO	YES

Notes: Each column represents one regression. Model 1 displays the regression results from the specification in Equation (1), Model 2 corresponds to Equation (3), Model 3 to Equation (2) and Model 4 to Equation (4). The second part of the interaction terms, "NQ", stands for neighbourhood quality. Robust standard errors are presented in parentheses, and clustered on origin municipality. * p<0.10, ** p<0.05, *** p<0.01.

Moving on to the analysis of exposure effects, we find these results in Model (3) and (4). Since both regressions include interaction terms, the base case of preschool movers (age 0-7) is omitted. This means that the coefficients in the first row are referring to the neighbourhood effect for this first age interval of movers, and that the following coefficients (on the interaction terms between moving age intervals and neighbourhood quality) should be interpreted as the difference in neighbourhood effect for each age interval of movers relocating later during childhood, compared to that base case. This indicates that in Model 3, preschool movers who move to a higher quality municipality where lifetime income ranks of permanent residents is one percentile point higher will on average have lifetime income ranks that are 0.83 percentile points higher, a result that is significant at the 1% level. The fact that this coefficient is larger than in Model 1 makes sense, since we would expect the effect to be stronger the younger a child is at the time of moving, and the coefficient in Model 1 is an average effect for the entire sample of movers (Chetty et al., 2016; Chetty and Hendren, 2017). Early movers (7-10 years old) will converge at a rate that is 0.66 percentile points lower than in the base case of preschool movers, meaning that the magnitude of their neighbourhood effect is on average 0.17, a substantially smaller effect. This is in line with the theory of exposure effects and our hypotheses, while hinting that there might be a disruption effect for someone moving during her early school years compared to moving before starting school (Wood et al., 1993; Gibbons and Telhaj, 2011). For the last two age intervals, youth movers (10-14 years old) and adolescent movers (14-18 years old), we cannot reject the null hypothesis that the effects are the same as in the preschool case at any conventional level of significance. This implies that we do not find evidence for the exposure effect being linear and continuously decreasing throughout childhood.

Turning to Model 4, we see a similar pattern but with different magnitudes when controlling for the child's sex, family characteristics and socioeconomic background. The neighbourhood effect for preschool movers drops to 0.35 when holding fixed the child's sex and observable parental characteristics, but is only significant on the 10% level giving us a low level of statistical certainty. The variables controlling for a child's sex, as well as parental income, social class, and education level, show similar coefficients as in Model 2, and have the same interpretation. However, there is a notable change in the exposure effect for early movers. With a coefficient of -1.16, the interpretation becomes that individuals who move at 7-10 years of age to a higher quality municipality where the mean lifetime income rank of permanent residents is one percentile point higher, will on average have a lifetime income rank that is $|0.35 - 1.16| = 0.81$ percentile points lower. This significantly negative effect means that despite ending up in a better neighbourhood, individuals who move at this age are worse off in absolute terms, possibly implying an even stronger disruption effect than in Model 3[19]. We cannot draw any definitive conclusions about the exposure effects for the last two age intervals in this model either.

[19] Although we take a conservative stance in interpreting the magnitude of this coefficient, and focus more on the general pattern.

6 Further Analysis

6.1 Parish as the definition of a neighbourhood

Next, we investigate whether the definition of a neighbourhood has implications for the empirical results. It is plausible that depending on which geographical aggregation is chosen, neighbourhood effects will work through different channels. For example, since Swedish schools are administrated at the municipal level, we would expect larger variation in the quality of schooling across municipalities than across parishes. Meanwhile, peer effects and other mechanisms stemming from social interactions between children may be at play when comparing parishes, but not when using larger geographical areas (Andersson and Musterd, 2010). To evaluate the impact from changing the neighbourhood definition, we rerun the analysis presented in Section 4, but using the geographical aggregation of parishes within Stockholm municipality as the unit of observation. In 1965, Stockholm county had a total population of 1,381,921 inhabitants, 788,503 out of whom were living in Stockholm municipality, making it the home of more than half of the county's total population (Statistics Sweden, 1966). From Section 3 we know that this relationship holds for the individuals in the sample as well, with Stockholm municipality being particularly dominant as the place of residence throughout the observation period. This makes it an interesting area to look at more carefully. The mean number of residents across parishes within Stockholm municipality was thus 29,204 in 1965, compared to the average municipality under observation for this study, which had 66,500 inhabitants [20]. For ease of analysis, we restrict the sample to only include individuals who spent their entire childhood (age 0-18) in one of the parishes under observation. Next, we follow the approach in Section 4 and divide the sample into different groups depending on their mover status during the observation period. The distribution is shown in Table 8.

Table 8: Moving status, sample within Stockholm municipality

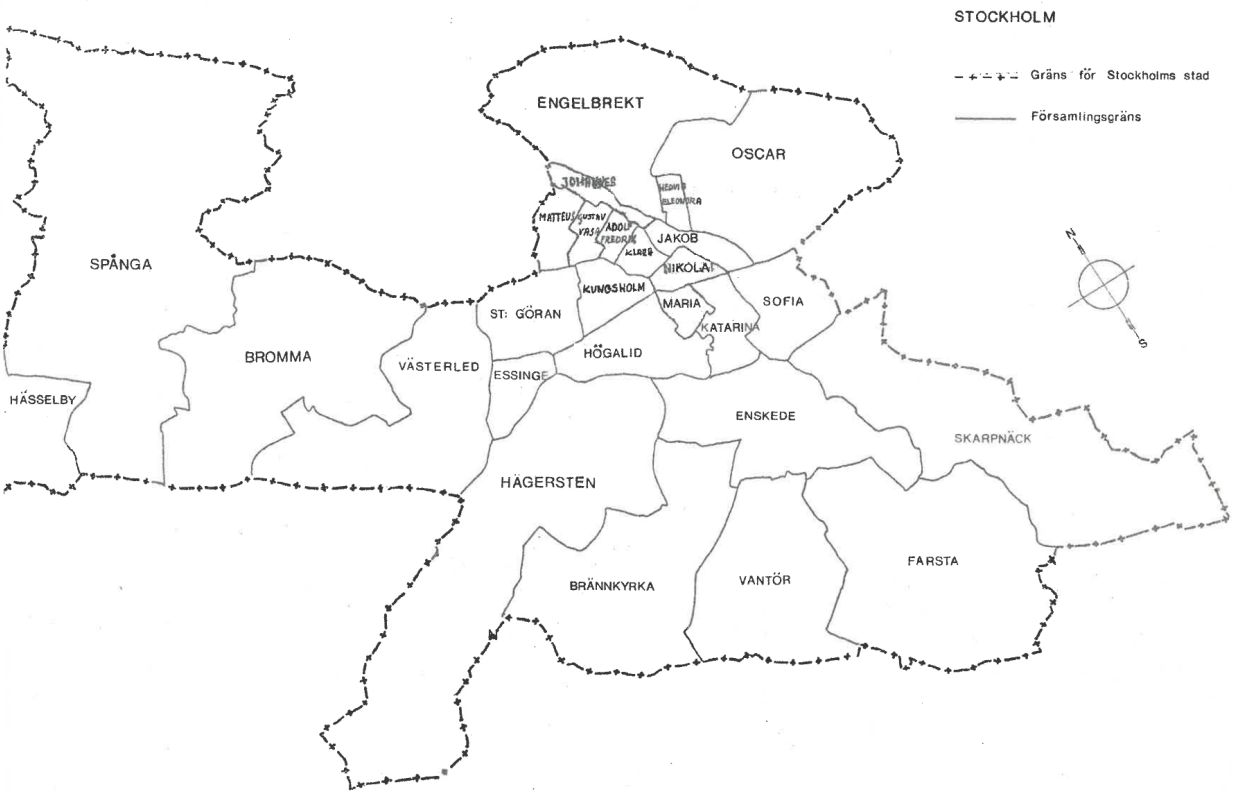
Moving status	Frequency	Share
Moved more than once	1,229	18.41%
Permanent residents	2,336	34.99%
Moved once, age 0-7	2,348	35.17%
Moved once, age 7-10	369	5.53%
Moved once, age 10-14	190	2.85%
Moved once, age 14-18	204	3.06%
Total	6,676	100%

The first thing to note is that the moving behaviour is quite different across parishes compared to municipalities (Table 6 for reference). Most striking is that people move more frequently between parishes, with individuals coded as moving more than once constituting approximately 18% of the parish sample compared to approximately 3% in the municipality case. Meanwhile, permanent residents make up only

[20] Note that this average includes Stockholm municipality.

about 35% of the parish sample, compared to 76% in the municipality case. This makes intuitive sense, seeing that all individuals who move within a municipality will be considered permanent residents in the main analysis and that many moves happen across short distances.

Figure 3: Map of parishes in Stockholm municipality



Source: Authors' rendering of a Länsstyrelsen map (Nyman and Hardeberg, 1971).

The moving patterns across origin and destination parishes are shown descriptively in Table 9. One noteworthy observation is that moves from a suburban parish[21] generally goes to another nearby, suburban parish. For example, if we direct the attention to Bromma in the north of Stockholm as the origin, we note that a vast majority of moves go to either Spånga or Hässelby, two parishes close by. The same can be said about suburban Enskede in the south of Stockholm, from which most moves go to one of the neighbouring parishes of Farsta, Vantör or Skarpnäck. The pattern is somewhat different for areas in the city centre[22], with many of the moves originating in e.g. Matteus, Katarina or Högalid having a suburban parish as its destination. This observation is in line with the reasoning in Section 3.2 about moves from the city to the suburbs being a part of families' life cycles. If, however, movers remain in the central areas, they generally

[21]The parishes to the north, west and south of St Göran, Högalid, Katarina and Sofia.
[22]The parishes north and east of Skarpnäck, Enskede, Hägersten, Essinge and Västerled.

choose to not go far, as in the case with moves between e.g. Hedvig Eleonora and Oskar or Katarina and Sofia being frequent.

Table 9: Moving patterns across parishes

	Origin																											
Destination	Klara	Adolf Fredrik	Gustav Vasa	Matteus	Jakob	Johannes	Engelbrekt	Hedvig Eleo.	Oskar	Kungsholm	St Göran	Nikolai	Katarina	Sofia	Maria	Högalid	Hägersten	Brännkyrka	Vantör	Enskede	Skarpnäck	Farsta	Bromma	Västerled	Essinge	Spånga	Hässelby	Total
Klara	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Adolf Fredrik	0	0	1	1	0	0	2	2	1	2	0	0	0	0	1	0	0	0	0	2	0	0	0	0	1	0	0	13
Gustav Vasa	0	0	0	5	0	3	0	1	3	2	1	1	0	0	0	0	1	1	1	1	0	0	0	0	0	0	0	20
Matteus	0	6	3	0	1	3	1	1	1	1	4	0	2	2	0	0	2	0	0	0	3	3	1	2	2	0	0	38
Jakob	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
Johannes	0	2	1	3	0	0	3	1	2	0	0	0	0	0	0	0	2	0	0	0	0	0	0	0	0	0	0	14
Engelbrekt	0	0	1	1	0	2	0	2	11	1	5	1	1	1	0	3	0	0	0	2	2	2	0	1	1	0	0	37
Hedvig Eleo.	0	2	3	3	0	2	0	0	10	0	0	0	0	0	0	2	2	1	0	0	1	0	3	1	0	0	0	30
Oskar	0	4	0	3	1	1	8	13	0	4	13	2	1	1	0	3	6	1	1	5	3	7	2	3	2	2	0	86
Kungsholm	0	1	1	3	0	0	1	2	3	0	6	0	1	0	1	0	2	0	1	0	0	3	0	1	1	1	0	28
St Göran	0	1	1	2	0	1	3	1	5	3	0	1	0	0	3	3	0	1	0	1	2	3	6	3	2	1	0	43
Nikolai	0	1	0	0	0	0	0	0	1	0	0	0	0	2	0	0	0	0	2	0	0	0	0	0	0	0	0	6
Katarina	0	0	1	0	0	1	1	2	3	0	1	0	0	7	3	3	0	2	1	2	4	4	0	1	0	0	0	36
Sofia	0	0	0	0	0	1	0	0	2	0	0	0	8	0	1	0	0	1	2	1	2	0	2	1	2	0	0	23
Maria	1	1	0	3	0	1	0	2	1	0	0	1	1	1	0	4	1	1	1	1	2	1	2	0	0	0	0	25
Högalid	0	1	0	5	1	4	1	1	2	2	0	0	6	3	4	0	3	5	3	2	6	4	1	0	1	1	0	56
Hägersten	2	3	2	10	0	1	3	0	2	2	7	1	9	11	8	21	0	55	14	30	32	24	13	0	7	4	0	261
Brännkyrka	1	2	2	7	0	2	5	3	3	5	7	1	15	3	9	17	52	0	10	26	19	20	7	3	2	2	0	223
Vantör	2	8	9	19	0	6	3	9	11	13	20	4	42	18	12	35	96	40	0	59	46	87	18	3	15	4	1	580
Enskede	0	0	1	1	0	1	2	0	12	6	7	1	11	7	3	7	8	9	9	0	19	11	1	1	2	1	0	120
Skarpnäck	3	3	4	8	0	3	5	2	13	6	10	2	24	14	12	18	25	11	6	31	0	47	6	5	3	1	1	263
Farsta	2	5	10	17	1	6	12	7	4	9	9	2	27	11	9	21	44	19	19	54	59	0	11	3	2	11	0	374
Bromma	0	1	0	3	0	1	3	1	3	3	8	0	3	1	0	3	6	3	4	0	3	4	0	12	2	7	2	73
Västerled	0	0	3	5	0	2	2	3	6	3	4	0	2	1	1	2	3	1	0	6	4	2	11	0	3	5	1	70
Essinge	0	0	0	0	0	0	1	1	0	0	1	0	1	0	0	1	0	2	0	1	0	0	1	0	0	0	0	9
Spånga	1	10	9	23	0	7	8	3	12	13	24	1	12	6	4	13	24	5	6	9	15	25	100	27	14	0	5	376
Hässelby	1	3	1	8	0	6	6	1	8	5	21	1	8	3	4	10	27	14	6	8	13	20	77	23	12	21	0	307
Total	13	54	53	130	4	54	70	58	119	80	148	19	174	92	75	166	304	172	86	241	235	267	262	90	74	61	10	3,111

Next, we follow the same empirical strategy as in Section 4 in order to evaluate the average neighbourhood effect for all movers in the baseline case (Model 1) as well as look at the exposure effects (Model 3). In both these cases we also include controls for observable parental characteristics (Models 2 and 4). Table 10 tells us that by moving to a parish where the average lifetime income rank for permanent residents is one percentile point higher, the average mover in the sample will improve their lifetime income rank by 0.38 percentile points. This implies that a mover makes up for 38% of the initial head start that a permanent resident of a more affluent neighbourhood has, when excluding controls. Thus, compared to the analysis on municipalities (see Table 7), there is a smaller effect from moving to a more affluent parish on lifetime income, that is significant on the 10% level. This seems intuitive since parishes are smaller in size and plausibly have less variation between them. This is also what we see when conducting an F-test of the null hypothesis that the fixed effect across all units is the same, where the p-value is somewhat higher when comparing differences between parishes. When we include controls for parents' socioeconomic and marital statuses, the effects practically disappear.

Table 10: Results, parish analysis

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Neighbourhood quality	0.38*	0.17	0.22	0.03
	(0.20)	(0.23)	(0.22)	(0.26)
Move age 7-10*NQ			0.42	0.16
			(0.57)	(0.60)
Move age 10-14*NQ			1.53**	2.01**
			(0.66)	(0.76)
Move age 14-18*NQ			0.57	-0.08
			(0.67)	(0.74)
Observations	2,798	2,145	2,798	2,145
R-squared	0.01	0.11	0.05	0.14
Origin FE	YES	YES	YES	YES
Age interval FE	NO	NO	YES	YES
Controls	NO	YES	NO	YES

Notes: Each column represents one regression. Model 1 displays the regression results from the specification in Equation (1), Model 2 corresponds to Equation (3), Model 3 to Equation (2) and Model 4 to Equation (4). Control variables are the same as in Table 7. The geographical unit of observation has been changed to parishes within Stockholm municipality. Robust standard errors are presented in parentheses, and clustered on origin parish. * p<0.10, ** p<0.05, *** p<0.01.

In contrast to the analysis on municipalities, we see no evidence of a stronger neighbourhood effect from moving to a higher quality neighbourhood for preschool movers (0-7 years old). Instead, we note a significantly positive effect from moving between ages 10-14 when including interaction terms to study the exposure effects. The coefficient on this age interval interacted with neighbourhood quality is large in both Model 3 and Model 4, implying that a move to a parish where neighbourhood quality is one percentile point higher at age 10-14 will result in a lifetime income rank that is 1.5 or 2 percentile points higher than for a child who moved at age 0-7. One possible explanation for this pattern is that school spots in Sweden were generally allocated based on geographical proximity during the time when the individuals in the sample were growing up (Andersson et al., 2012; Edmark and Wondratschek, 2014). Consequently, if parents with high ambitions wanted to place their children in a better school, they would in essence have to move to the neighbourhood in which the school was located. This specific age interval is then particularly relevant, seeing that the children would be of an age where they are about to start applying to upper secondary school. Moves of this nature would not be captured by the municipality analysis, seeing that they take place within Stockholm municipality, meaning the individuals are not treated as movers[23]. On the other hand, with a higher number of parishes than municipalities, we acknowledge that we have large standard errors and rather few observations within each age interval after origin fixed effects are employed. We can therefore not exclude the possibility that the surprisingly strong estimates are driven by a few informative observations. Figure 6 in

[23]Keeping in mind the definition of a mover as someone who moved across municipality or parish borders exactly once during childhood.

Appendix A.5 plots the relationship between neighbourhood quality and the lifetime income rank of a mover. We note substantial outliers among the highest quality parishes in a positive direction for movers' income in the age interval 10-14, indicating that a few observations might indeed drive this particular estimate.

6.2 Sibling analysis and family fixed effects

As previously mentioned, the key identifying assumption to evaluate exposure effects is that selection effects do not vary with the age of a child at the time of moving. In other words, that inherent differences in the characteristics of parents who choose to move to a more affluent neighbourhood do not vary with the timing of a decision to move. To validate this assumption, we follow Chetty and Hendren (2017) and Björkegren (2018) in utilizing sibling data and applying family fixed effects. By applying family fixed effects, we are in practice comparing siblings and controlling for unobservable family characteristics that are constant within the family. We thereby exploit that the only thing differing between siblings of different ages is their exposure time to the higher quality neighbourhood they move to. Thus, we alleviate potential selection based on a child's age and other omitted variable bias, since these factors ought to affect two siblings in the same way.

Table 11: Moving status, sibling sample

Moving status	Frequency	Share
Moved more than once	840	3.52%
Permanent resident	18,089	75.76%
Moved once, age 0-7	3,061	12.82%
Moved once, age 7-10	853	3.57%
Moved once, age 10-14	477	2.00%
Moved once, age 14-18	556	2.33%
Total	23,876	100%

In a first step, we limit the original data set to include only those individuals born in 1953 who have siblings. Next, we add the sample of full siblings (sharing both parents with the 1953 cohort) to the data set. This leaves us with a sample of 23,876 individuals out of whom 8,653 are part of the original sample born in 1953, and 15,223 are their siblings. Consequently, each family in the sample has an average of 2,76 children sharing both parents. In Table 11 we see that 18,089 are considered permanent residents, while 4,947 moved exactly once across municipalities within Stockholm county. The distributions between each moving status group are similar to the distributions in the original sample, as shown by Table 6.

As a next step, we would ideally estimate the exposure effect as the extent to which the difference in moving siblings' lifetime income, $y_1 - y_2$, covaries with the difference in siblings' ages at the move interacted with the improved neighbourhood quality (Chetty and Hendren, 2017). However, given that we only have geographical data at five different points in time during childhood, we cannot know with certainty the age

of a sibling at the time of the move[24]. To still get an idea of whether siblings' outcomes covary with the difference in their ages, we estimate Equation (1) on the extended sample including siblings and compare the cohort members born in 1953 to younger and older siblings. We exclude siblings who are more than three years older or younger than the 1953 cohort, to account for differences in the macro environment across birth years that may prevail if we allow for too large age differences. Table 12 shows that younger siblings, who moved to a more affluent neighbourhood at a younger age, experience stronger neighbourhood effects compared to the 1953 cohort and older siblings. The interpretation is that the younger siblings benefit more from moving to a more affluent area, given the origin municipality, which we take as additional suggestive evidence for the importance of exposure effects[25].

Table 12: Baseline specification, comparing siblings and the 1953 cohort

	(1)	(2)	(3)
Mover lifetime income rank	Cohort member	Younger siblings	Older Siblings
Neighbourhood quality	0.51***	1.08***	0.40
	(0.14)	(0.16)	(0.27)
Observations	1,004	699	531
R-squared	0.02	0.03	0.02
Origin FE	YES	YES	YES
Family FE	NO	NO	NO
Controls	NO	NO	NO

Notes: All columns show regression results for Equation (1), but displaying the predicted neighbourhood effects for different subgroups. Column (1) shows the results for the 1953 cohort members, Column (2) for younger siblings, and Column (3) for older siblings. Robust standard errors are presented in parentheses, and clustered on origin municipality.
* p<0.10, ** p<0.05, *** p<0.01.

Lastly, we change the approach to once again compare older and younger siblings, this time by using age interactions while employing family fixed effects. Specifically, the first independent variable is constructed as an interaction between neighbourhood quality in the destination, $\bar{y}_d$, and a dummy taking on the value of 1 if an individual is an older sibling (born in 1950, 1951 or 1952) and 0 otherwise (born in 1953, 1954, 1955 or 1956). The second independent variable is an interaction between neighbourhood quality and a dummy taking on the value of 1 if an individual is a younger sibling (born in 1954, 1955 or 1956) and 0 otherwise (born in 1953, 1952, 1951 or 1950). Once again siblings who are more than three years younger or older than the 1953 cohort are excluded. Thus, we get one estimate for the strength of neighbourhood effects for older siblings compared to their younger family members, and one estimate for younger siblings compared to their older family members[26]. Table 13 shows the results. We find that older siblings benefit significantly

[24]For example, even if we know that a sibling is 2 years older than the individual born in 1953, and we know that the family moved between 1960-1963, we do not know for certain which year the move happened. Thus, we do not know if the older sibling belongs to the mover age interval that is 7-10 years old, or 10-14 years old.

[25]Appendix B.3 shows that the pattern is similar if we include siblings that are further separated in age.

[26]With the comparison group for both dummies including the 1953 cohort from the original sample.

less from moving to a more affluent neighbourhood than their younger family members, whereas younger siblings benefit significantly more compared to their older family members. More importantly, this pattern seems to hold also when we apply family fixed effects, and thereby control for unobservable characteristics that are constant within a family (e.g. parental characteristics or household context). The interpretation is that younger individuals experience stronger neighbourhood effects also within a family, implying that longer exposure to a more affluent neighbourhood in childhood has a long-term impact on lifetime income that extends beyond selection on family characteristics or other omitted variable bias[27].

Table 13: Exposure effects, comparing siblings within a family

Mover lifetime income rank	(1)
Older siblings*NQ	-0.96*
	(0.46)
Younger siblings*NQ	0.90**
	(0.32)
Observations	2,277
R-squared	0.60
Origin FE	YES
Family FE	YES
Controls	NO

Notes: The interaction terms consist of a dummy for older (younger) siblings, and the variable for neighbourhood quality, allowing for different magnitudes of the coefficient depending on the age of a mover. Robust standard errors are presented in parentheses, and clustered on origin municipality.
* p<0.10, ** p<0.05, *** p<0.01.

7 Robustness of the Results

To evaluate how sensitive the results are, we conduct a set of robustness checks related to our empirical approach. These can all be found in Appendix B. We alter our model's assumptions when it comes to the construction of the income variable, the cutoffs we choose for movers' age intervals, which geographical units to include, as well as which age span we allow for when comparing the economic outcomes for siblings. We note that changing the underlying income variable from the original data set when constructing our preferred measure of lifetime income does not alter the conclusions. Both when using a measure of disposable income

[27]It is worth noting that there is a risk that an unobserved driver of the more beneficial economic outcomes for younger siblings is the general economic development in the post-war Sweden of this time. This is the reason why we choose to restrict the sample to siblings who are quite close in age. We refer to Appendix B.3, which shows that the pattern is similar also for siblings who are further separated in age, implying that the relationship extends beyond macro trends that affect children of different ages in different ways.

(see Table 20 in Appendix B.2), and when using only labor income, i.e. excluding the part that is accumulated from running a business (see Table 21 in Appendix B.2), the results are qualitatively unchanged, showing the same pattern across specifications as in Section 5.

Similarly, we find support for our approach when it comes to including as many age intervals for movers as the data would allow us. As a robustness check, we try different binary cutoffs, i.e. having only a preschool interval and a later than preschool interval (before age 7 vs after age 7), early movers interval and a later than early movers interval (before age 10 vs after age 10) as well as a youth movers interval and a later than youth movers interval (before age 14 vs after age 14). The accompanying results can be found in Table 23, Appendix B.4. We note that were we to use the binary cutoffs, we would lose much of the informative nuance that we get from having several different intervals in our main analysis, as captured most importantly by the negative effect that moving at 7-10 years of age has on lifetime income rank compared to moving at 0-7 years of age. This effect essentially disappears when applying a cutoff for moving before and after the age of 7, as the negative effect of moving at 7-10 years of age is seemingly consumed by the positive effect from moving after the age of 14. We therefore do not see a reason to alter our approach in the main analysis, but conclude that all age intervals should be kept to provide a comprehensive picture.

Furthermore, as we note in Section 3, Stockholm municipality is by far the most dominant municipality when it comes to where individuals in the data set choose to reside, as well as being either the origin or destination municipality in a large share of the moves that we focus on in this study. Hence, we rerun the analysis excluding Stockholm city to evaluate whether this makes a difference to our conclusions. The results from these regressions can be found in Table 24, Appendix B.5. The pattern is overall similar to the main analysis, if anything with even stronger coefficients. Most notably, we find a strongly negative and statistically significant effect from moving to a higher quality municipality between 10-14 years of age on a mover's lifetime income rank. This is likely due to the smaller sample size opening up for more volatile results. To compare, we also rerun the analysis on movers that have Stockholm as either their origin or destination municipality, see Table 25 in Appendix B.6. Once again, we find similar patterns as in the main analysis. However, when looking at the effect of moving to a higher quality municipality between 10-14 years of age, we now see a positive and statistically significant estimate. Due to the limited sample size, we cannot exclude the possibility that a few individuals are driving this estimate as well. Table 26 in Appendix B.6 verifies that there are only a few individuals moving to the highest quality neighbourhoods at 10-14 years of age[28].

Lastly, we test whether we find a similar pattern as in Table 12 where we compare older and younger siblings, if we were to allow for a larger age span. Table 22 in Appendix B.3 displays the effect of moving to a higher quality neighbourhood on lifetime income rank for older as well as younger siblings, when limiting

[28]When scrutinizing the data, we also note that individual destination municipalities where movers' lifetime incomes are either particularly high or low within an age interval can indeed have a disproportionate effect on the results.

the sample to siblings that are at most 10 years older or younger than the 1953 cohort members. In line with the findings in Table 12, the results confirm that younger siblings experience stronger neighbourhood effects compared to older siblings.

8 Discussion

To briefly summarise our results, we find suggestive evidence for neighbourhood effects having an impact on economic outcomes in adulthood for individuals who move to a higher quality neighbourhood in childhood. The effect persists for the youngest subset of movers in our sample also when controlling for socioeconomic background, although the magnitude of the impact and the level of statistical certainty drops substantially. This indicates that the results are sensitive to selection on parental characteristics, but that early exposure to an affluent neighbourhood can still have a non-negligible influence over lifetime income. In particular, we find that movers who relocate before the age of 7 to a neighbourhood where permanent residents' lifetime income rank is one percentile point higher, will increase their own lifetime income rank by 0.35 percentile points in the most restrictive specification (see Model 4 in Table 7). This means that children moving to a higher quality neighbourhood before starting school will make up 35% of the head start that permanent residents got by being born into a more affluent area, holding parental observable characteristics fixed[29]. This thereby provides indicative support for our hypothesis about early exposure having the largest impact on outcomes later in life. For a rough comparison, Chetty and Hendren (2017) conclude in their study of the United States that an individual moving to a higher quality neighbourhood at birth will make up 80% of the adulthood income advantage of permanent residents, or the equivalent of 4% per year of childhood exposure[30], implying a larger effect in the American context than the Swedish. This is intuitive, considering the state of the Swedish welfare system, and the extensive residential segregation of modern-day America. Furthermore, the sibling analysis in Section 6.2 provides convincing evidence for the importance of the length of exposure to a high quality residential context in childhood. Table 12 shows how neighbourhood quality has a stronger impact on lifetime income for younger siblings compared to older ones. This finding is reiterated when comparing siblings within a family, as shown by Table 13. There thus seems to be a long-term impact from exposure to an affluent neighbourhood that goes beyond selection on family characteristics, giving support for our second hypothesis about the length of exposure.

Moreover, we find a negative effect on lifetime income from moving at 7-10 years of age compared to moving at age 0-7, even if the destination municipality is of a higher quality than the origin. This implies a disruption effect from moving during the early school years as opposed to moving before starting school. We cannot draw any conclusions about differing exposure effects for the two oldest moving age intervals (10-14

[29] Although this finding is only significant on the 10% level and should therefore be interpreted with some caution.

[30] Note that the sample in their study does not include children who were younger than 9 years old at the time of moving, meaning they have assumed linearity in the exposure effect. See the original paper for details.

and 14-18 years old). Chetty and Hendren (2017) find a continuously linear decrease in the magnitude of the neighbourhood effect throughout childhood, implying that every additional year a child is exposed to a higher quality neighbourhood increases her adulthood income. We cannot confirm this linear relationship in our main analysis.

Lastly, the conclusion from our analysis in Section 6.1 is that the definition of a neighbourhood has implications for the results. When rerunning the analysis on parishes instead of municipalities, we see a considerably different moving pattern, as well as substantial changes to the results. In the most restrictive specification (Model 4 in Table 10) we find no evidence of a neighbourhood effect, regardless of the exposure time.

8.1 Internal validity

This section will discuss the internal validity of our study, i.e. to what extent the empirical strategy can be thought of as estimating a causal effect. In order to evaluate this, we will in turn discuss three different issues: our main identifying assumption, the validity of our research design and the risk of reverse causality.

The main identifying assumption of our empirical strategy is that selection effects on family characteristics do not vary with the age of a child at the time of moving. The strongest evidence we have put forward to validate this assumption is the analysis in Section 6.2, where we show that exposure effects persist even when controlling for family fixed effects. However, this approach also has limitations. We can for example not control for the birth order of each individual sibling. It is therefore possible that there is in fact selection when it comes to parents' decision to move that varies with the age of their children, only that the selection is based on the age of another child than the one in the 1953 cohort. Furthermore, we cannot control dynamically for time-varying inputs such as income shocks or changes in marital status for the parents. For instance, a parental job promotion could bias the estimate of a child's neighbourhood effect upwards, as a move triggered by an increase in parental income may directly improve the child's outcomes relative to her time of exposure to the destination area. Moreover, we have to rely solely on observational data for our analysis as we do not have access to exogenous shocks that randomly allocate individuals to a neighbourhood. Ideally, we would have been able to include such a source of variation as a robustness check on our results[31].

Next, we turn to the validity of our research design. As shown by the analysis in Section 6.1, the chosen definition of a neighbourhood has implications for the results and conclusions that can be drawn from a study like this. Thus, our thesis has not provided a definitive answer to the debate on how to appropriately define a neighbourhood, leaving the question open for future studies. As an extension, it remains open

[31] Both Chetty and Hendren (2017) and Björkegren (2018) have access to some data of this nature.

for discussion how far an individual must move to be considered a mover. In the analysis on parishes, we note a large number of moves taking place within Stockholm municipality, meaning that those individuals will be considered permanent residents in the analysis on municipalities, despite their immediate residential context having changed. In addition, as we do not have geographical data for every year under observation, it is possible that some individuals in the sample have moved during a particular age interval, but then moved back to the same municipality before reaching the age of the next interval[32]. In general, the fact that observations in the data are not yearly makes our analysis less reliable when it comes to making causal claims, mainly because it becomes more difficult to estimate exposure effects in a precise manner.

Lastly, it is worth addressing the possibility of reverse causality in studies using our methodology. This concern stems from the construction of the main independent variable, which is based on the average lifetime income rank of permanent residents in the destination. It is possible that the neighbourhood effects work in both directions, i.e. that an area's permanent residents who go on to do well later in life have a positive influence on movers who come there, but also that the movers could in turn impact the outcomes of permanent residents. If so, the treatment would in part be an outcome. Although this is worth keeping in mind, we do not consider it an immediate concern for the validity of our research design. That is because peer effects that arise in the interaction between movers and permanent residents is part of the neighbourhood effects that we wish to capture with our analysis. In other words, if there is mutually beneficial interaction between inhabitants in one neighbourhood, but not in another, that is part of what we want to study. Moreover, permanent residents constitute such a clear majority in the sample for this thesis that any peer effects are on average more likely to be influencing the movers than the other way around.

As a final note, we generally choose to be cautious in drawing far-reaching conclusions regarding the magnitude of our estimates. This partly follows from the preceding discussion about the identifying assumption, the research design and the risk for reverse causality, but also from the fact that the number of movers within an age interval who move to a particular destination municipality is quite low. In Appendix A.7, we show how municipalities that are the destination of only a few moves tend to have the most volatile lifetime income ranks for movers in relation to the estimated neighbourhood quality. In order to more confidently estimate causal effects, we would ideally have had a larger sample size.

8.2 External validity

In this section we will discuss the extent to which our conclusions can be applied outside the setting of this study. We acknowledge that our results to a large extent are context specific. Both the geographical setting (Sweden in general and Stockholm in particular) and the time period (childhood years of 1953-1971) are two

[32]This behaviour of moving away from a municipality or parish and then moving back at a later point in time is something we observe in the data, although it is not frequent enough to alter our conclusions.

pronounced factors that are common for the individuals in our sample which limit the generalisability of our results. Hence, we would expect different outcomes if we were to replicate the study in a different context.

Starting with the geographical setting, we recognise that the specific institutional context of Sweden is quite particular, meaning that the results could differ in another country. Sweden's extensive welfare state and redistributive tax system likely has an effect in leveling the playing field across geographical areas, which would decrease the importance of childhood neighbourhoods. Furthermore, given the many differences between rural and urban areas, such as employment opportunities, income inequality and institutional development, it is unlikely that our results are applicable to smaller cities or towns, even within Sweden's borders.

Furthermore, it is unlikely that the results are directly applicable to Sweden's modern society. The individuals in our sample grew up in Stockholm county during the 1950s and 60s, at a time when Sweden was experiencing fast expansion of its welfare state, making it one of the most socially equal countries in the world (Lundberg and Åmark, 2001). However, since the 1980s, Sweden has experienced both rising income inequality and higher residential segregation, which in turn has reduced social mobility and equality of opportunity (Holmlund, 2016; OECD, 2017). Given these contextual changes, it is plausible that we would find different results if we studied the same geographical setting today. Moreover, the status of Stockholm's city centre has changed substantially since 1953. At the time, the inner city was mostly home to industries and working class families, and the 1950s and 60s saw a large exodus of families moving out to improved living conditions in the suburbs, as shown by the patterns in Table 3 (Stenberg, 2018). These days, Stockholm's city centre has become home to high income families, meaning many families instead move from the suburbs to the inner city as their wealth increases. This is also what we see in our data. Out of the 1953 cohort members in our sample, about 80% of those living in the city centre in 2009 were members of the upper social classes (Stenberg, 2018). As a last point, the childhood period for the 1953 cohort saw other policies that may have influenced the moving patterns of families. Before the 1990s, school spots in Sweden were offered based on proximity to a particular school (Andersson et al., 2012; Edmark and Wondratschek, 2014). Hence, it is possible that wealthy and ambitious parents moved to be able to put their children in a better school environment, an important difference to the Sweden of today where families face a free school choice regardless of where they decide to live.

9 Conclusion

This thesis has studied neighbourhood effects in childhood for individuals born in 1953 and growing up in Stockholm, Sweden. Using a sample of movers, we have evaluated the impact of childhood residential area on lifetime income by comparing individuals who spent longer time in a more affluent neighbourhood, to

those who spent less time there. Our hypotheses state that longer and earlier exposure to a high quality neighbourhood will increase the positive impact on adulthood outcomes. The quality of a neighbourhood has been defined as the average lifetime income for permanent residents who spent their entire childhood in the area.

The empirical analysis provides evidence for neighbourhood effects in this context as displayed by improved lifetime income for movers relocating to a higher quality neighbourhood. We also find indicative evidence for an exposure effect that is robust to controlling for family fixed effects, and that the impact is largest for the subset of movers who were youngest at the time of relocation. However, we are unable to exclude the possibility that selection effects on parental socioeconomic characteristics or some other unobservable factor are driving the main results. We test our hypotheses on two different definitions of a neighbourhood, municipalities in Stockholm county and parishes in Stockholm municipality, and find suggestive evidence that the definition of a neighbourhood has an impact on the results.

The study faces some limitations, leaving us conservative in our interpretation of the results, especially the magnitude of coefficients and the extent to which the effects should be thought of as causal. Most importantly, there are data constraints stemming from us not having access to residential or socioeconomic background data on a yearly basis. Moreover, we cannot make use of exogenous variation in the decision to move or the destination choice for individuals in the sample, yielding some endogeneity concerns. Lastly, the sample size is quite small, meaning the magnitude of all coefficients should be interpreted with care. In addition to these methodological limitations, the quite particular setting provided by Stockholm during the observation period is a cause for some caution when generalising the results to other contexts.

Our results shed new light on the topic of equality of opportunity, and has some important policy implications. In general, there seems to be a non-negligible positive effect from moving to a higher quality neighbourhood that in part makes up for the head start of an individual born there, especially if the move takes place in early childhood. This implies that it is possible to level the playing field throughout a child's life, but follows the general theme in the literature that targeted interventions to boost long-term outcomes should take place as early as possible. However, our reading of the results is that much of the observed neighbourhood effects are due to selection on parental characteristics, i.e. that socioeconomically strong families decide to cluster in the same areas, and that the positive lifetime outcomes that their children experience are not as much attributable to the residential context as it is to the family context. This makes intervention more difficult, since improvement to a neighbourhood, be it in terms of quality schooling, good housing or some other factor, could have only a marginal impact compared to the peer effects from living in an area where socioeconomically strong families have decided to locate, a decision that will ultimately be endogenous. The main focus of an egalitarian social planner thus becomes to make sure that the institutional quality is equally high across e.g. municipalities, in order to set a general standard that applies to all citizens.

Where affluent individuals then decide to reside is difficult to steer without opening up to paternalistic tendencies. Our recommendation would be to use the redistributive mechanisms in place in a modern society to improve equality of opportunity, rather than focusing on targeted interventions related to neighbourhood context.

We have contributed to the existing literature on neighbourhood effects in mainly three ways. First, we have taken an innovative methodology with observational data and applied it to the quite particular institutional context of Sweden. Second, we have used longitudinal data that allows us to follow an individual birth cohort throughout their entire worklife, meaning we can employ a measure of lifetime income as our outcome of interest. Third, we have attempted to evaluate if the definition of a neighbourhood matters for the interpretation of our results. However, there are plenty of remaining topics for other scholars to consider. Most importantly, our paper has not settled the debate on which factors in an individual's childhood context are the most decisive when it comes to channeling neighbourhood effects into lifetime income. Future studies should attempt to establish causal relationships between contextual input and personal output. This would make it easier to formulate policies to improve the lives of citizens. Moreover, the discussion on neighbourhood effects should be extended to more carefully account for the importance of family background. What are we actually referring to when we talk about a high quality neighbourhood? Is it really the quality of local institutions, or is it just the socioeconomic status of the inhabitants which was already inherent to them when they decided to live there? Lastly, it is of interest to deepen the contextual analysis, partly by looking at other institutional settings, but also by studying modern-day Sweden with more recent data. Have, for example, residential segregation become more severe in the last decades? How have the moving patterns changed, and how would this impact our conclusions? These are all questions that future researchers should direct their attention to.

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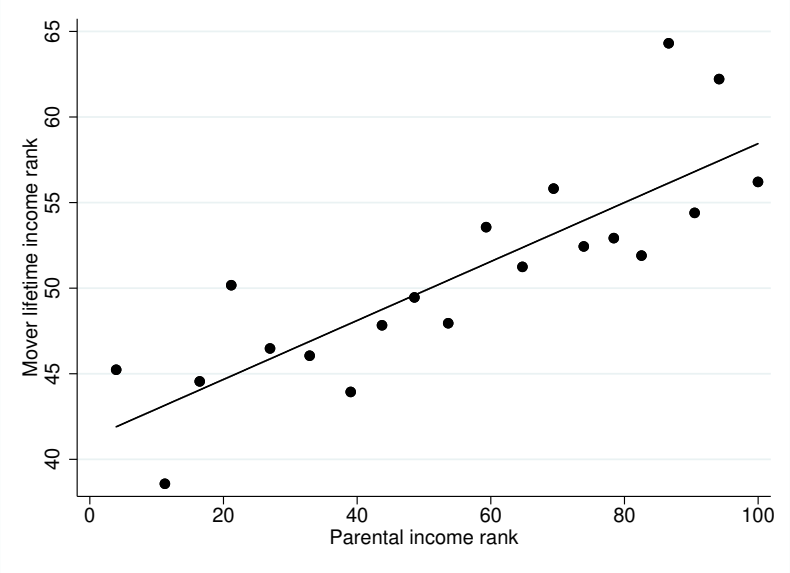
A Appendix 1: Additional Information

A.1 The importance of parental income

Figure 4: Relationship between movers' lifetime income rank and their parental income rank



Notes: The figure shows the relationship between parental income rank and movers' lifetime income rank, using percentiles. We note that it is a positive and linear relationship. This makes parental income an important control variable in the empirical specifications in order to alleviate bias.

A.2 Controlling for parental income in two different ways

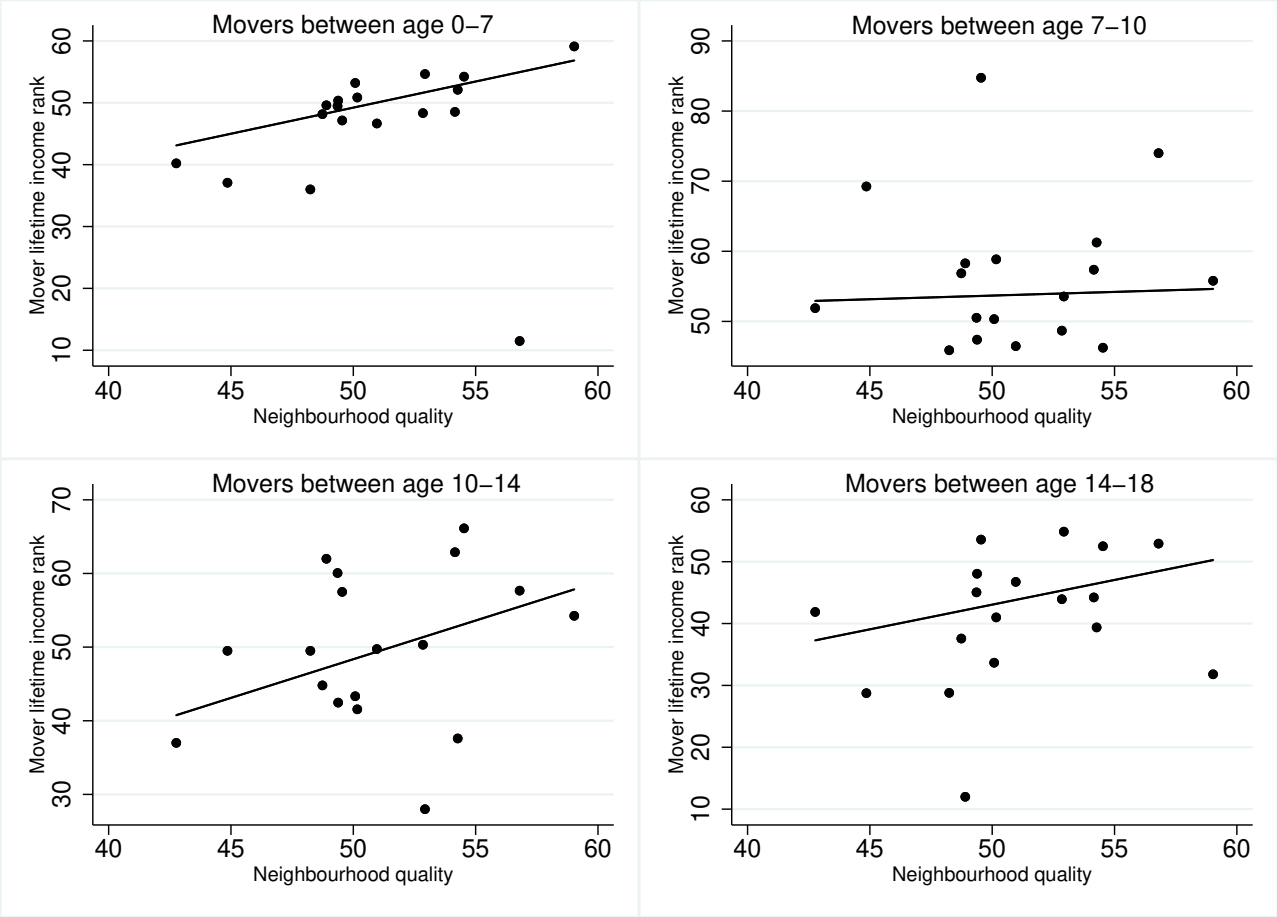
Table 14: Baseline results with different controls for parental income quintile

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Neighbourhood quality	0.30**	0.32***	0.42**	0.44***
	(0.11)	(0.09)	(0.16)	(0.13)
Parental Income: 1st quintile (bottom)		-12.50***		-11.51***
		(1.31)		(1.31)
Parental Income: 2nd quintile		-9.77***		-9.29***
		(1.53)		(1.67)
Parental Income: 3rd quintile		-9.34***		-9.31***
		(1.72)		(1.80)
Parental Income: 4th quintile		-3.54**		-3.04**
		(1.35)		(1.39)
Omitted variable: 5th quintile (top)		-		-
Move age 7-10*NQ			-0.88**	-0.91***
			(0.37)	(0.24)
Move age 10-14*NQ			0.18	0.35
			(0.22)	(0.21)
Move age 14-18*NQ			0.42**	0.48
			(0.19)	(0.34)
Observations	1,865	1,865	1,865	1,865
R-squared	0.06	0.04	0.13	0.07
Origin FE	YES	YES	YES	YES
Parental income FE	YES	NO	YES	NO
Age interval FE	NO	NO	YES	YES

Notes: We control for parental income in two different ways. Each column is based on one regression. Model 1 shows the regression results of Equation (1), with the addition of parental income as fixed effects. Model 2 represents results from Equation (3), but using only parental income as a control variable. Model 3 represents Equation (2), with the addition of parental income as fixed effects. Model 4 represents Equation (4), but using only parental income as a control variable. From this table, we can verify that changing the way we control for parental income does not change our results drastically. Robust standard errors are presented in parentheses, and clustered on origin. * p<0.10, ** p<0.05, *** p<0.01.

A.3 Visual representation of neighbourhood effects across moving age intervals

Figure 5: Relationship between movers' lifetime income rank and neighbourhood quality, by age interval at the point of moving



Notes: The upper left figure (age 0-7) displays a clearly positive relationship between moving to a higher quality neighbourhood and a higher income rank in the future. Note that the slope in the upper right figure (age 7-10) is significantly different from the upper left figure (age 0-7), implying that children moving between age 7-10 experience significantly weaker neighbourhood effects than children moving before age 7. Also note that the observations are much more scattered in the upper right figure (age 7-10), causing more volatile estimates. In both the bottom left (age 10-14) and the bottom right (age 14-18) figures the slopes are not significantly different from the upper left one (age 0-7). However, the observations are clearly more scattered in both these figures compared to the first one, leading to less precise estimates.

A.4 Full regression output, parish analysis

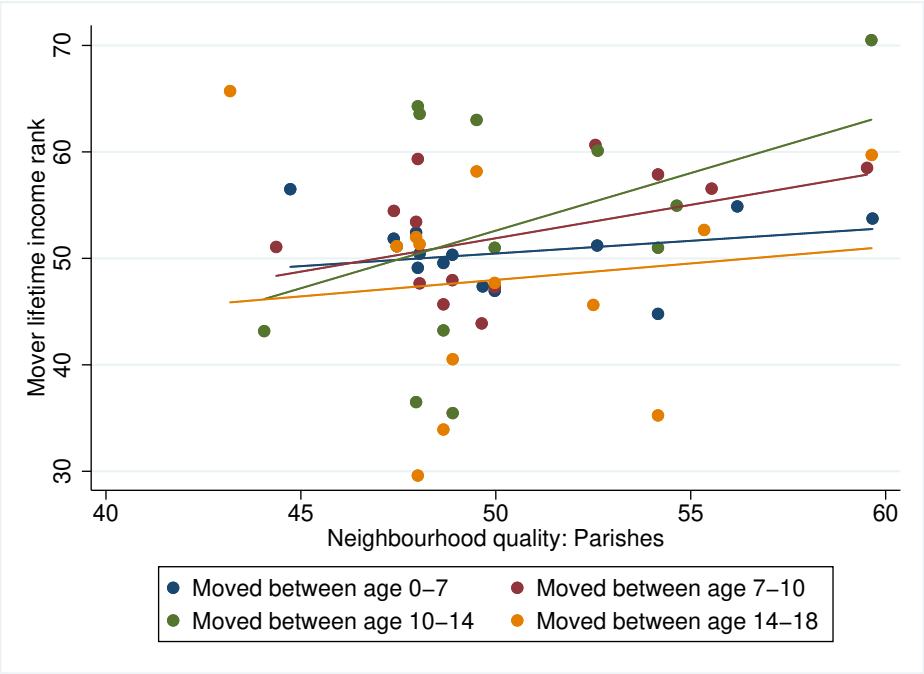
Table 15: Regression results, parish analysis displaying controls

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Neighbourhood quality	0.38*	0.17	0.22	0.03
	(0.20)	(0.23)	(0.22)	(0.26)
Sex (Female)		-13.18***		-13.18***
		(1.07)		(1.12)
Parental Income: 1st quintile (bottom)		-10.76***		-10.48***
		(2.66)		(2.54)
Parental Income: 2nd quintile		-5.24**		-5.14*
		(2.54)		(2.52)
Parental Income: 3rd quintile		-5.13**		-4.80**
		(2.03)		(1.91)
Parental Income: 4th quintile		-2.05		-2.04
		(2.35)		(2.36)
Omitted variable: 5th quintile (top)		-		-
Married		4.65**		3.86
		(2.20)		(2.31)
Upper and upper middle class		6.78**		5.30*
		(2.83)		(2.77)
Lower middle class, officials		6.55***		5.48***
		(1.91)		(1.82)
Lower middle class, entrepreneurs		3.57		2.61
		(2.39)		(2.42)
Working class, skilled workers		2.63		1.70
		(2.53)		(2.59)
Omitted variable: Working class, unskilled workers		-		-
Years of Education (Head of Household)		0.53*		0.70**
		(0.30)		(0.32)
Move age 7-10*NQ			0.42	0.16
			(0.57)	(0.60)
Move age 10-14*NQ			1.53**	2.01**
			(0.66)	(0.76)
Move age 14-18*NQ			0.57	-0.08
			(0.67)	(0.74)
Observations	2,798	2,145	2,798	2,145
R-squared	0.01	0.11	0.05	0.14
Origin FE	YES	YES	YES	YES
Age interval FE	NO	NO	YES	YES
Controls	NO	YES	NO	YES

Notes: The table shows the same results as Table 10, but displaying all included controls. Each column represents one regression. Model 1 displays the regression results from the specification in Equation (1), Model 2 corresponds to Equation (3), Model 3 to Equation (2) and Model 4 to Equation (4). Control variables are the same as in Table 7. The geographical unit of observation has been changed to parishes within Stockholm municipality. Robust standard errors are presented in parentheses, and clustered on origin.
* p<0.10, ** p<0.05, *** p<0.01.

A.5 Illustration of moving patterns across parishes

Figure 6: Relationship between movers' lifetime income rank and neighbourhood quality in parishes, by age interval at the point of moving



Notes: The figure illustrates the relationship between mover's lifetime income rank and neighbourhood quality in parishes, and shows how it varies when looking at moves at different ages. The positive and statistically significant estimate of moving between age 10-14 on movers' lifetime income rank is illustrated by the green line. However, it is possible that the outlier in the top right corner is driving the results.

A.6 Neighbourhood quality of parishes within Stockholm municipality

Table 16: Neighbourhood quality rank for each parish under observation within Stockholm municipality

Parish	Neighbourhood quality
Adolf Fredrik	63.30
Västerled	59.64
Kungsholm	57.81
Oskar	56.94
Johannes	56.38
Sofia	54.58
Högalid	54.51
St Göran	54.16
Spånga	52.61
Nikolai	51.25
Enskede	49.97
Skarpnäck	49.70
Matteus	49.31
Brännkyrka	48.89
Essinge	48.71
Vantör	48.66
Farsta	48.05
Hässelby	48.00
Katarina	47.96
Hägersten	47.48
Maria	46.63
Bromma	46.18
Hedvig Eleonora	45.70
Engelbrekt	44.58
Gustav Vasa	36.50

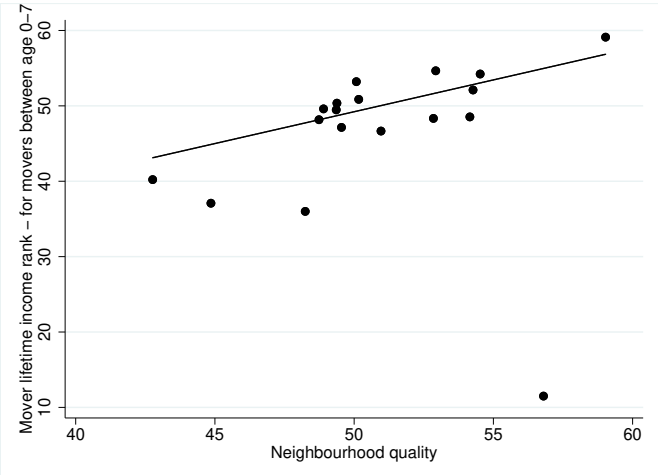
A.7 Displaying outliers in the sample of movers

Table 17: Number of movers aged 0-7, per destination municipality

Neighbourhood quality	Move age 0-7
59.03 (Danderyd)	95
56.80 (Salem)	4
54.53 (Solna)	126
54.27 (Sollentuna)	103
54.16 (Täby)	122
52.93 (Lidingö)	122
52.85 (Huddinge)	99
50.96 (Tyresö)	42
50.16 (Stockholm)	338
50.08 (Sundbyberg)	54
49.55 (Österåker)	14
49.38 (Haninge)	48
49.36 (Nacka)	80
48.90 (Upplands Väsby)	15
48.74 (Järfälla)	86
48.24 (Sigtuna)	10
44.86 (Vallentuna)	12
42.76 (Botkyrka)	28
Total	1,398

Notes: The table displays the distribution of movers per destination municipality within the youngest age interval of movers, 0-7 years old.

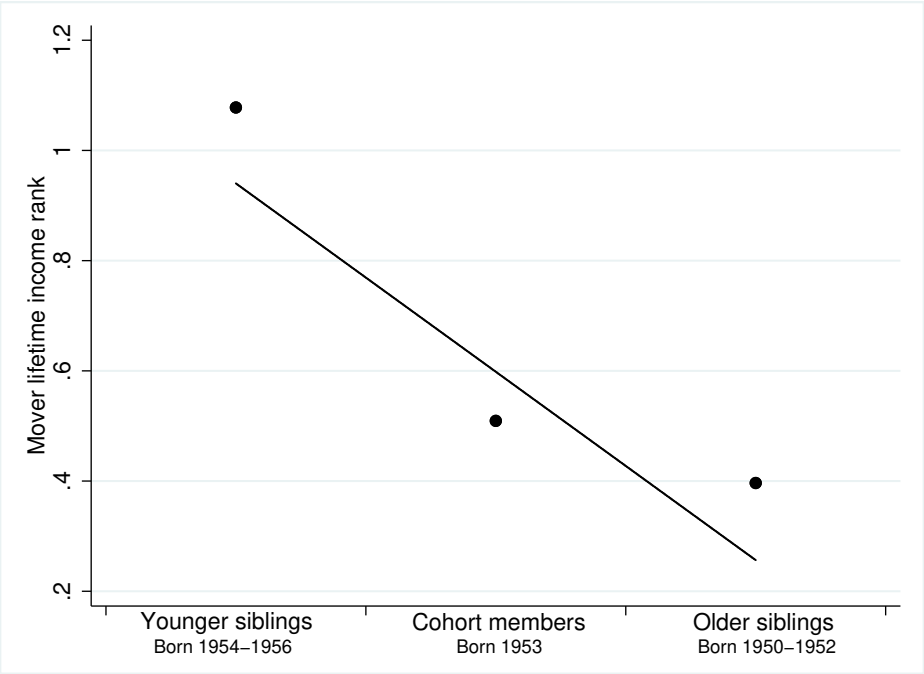
Figure 7: Relationship between movers' lifetime income rank and neighbourhood quality, movers aged 0-7



Notes: The figure shows the dispersion of movers' lifetime income rank plotted against the neighbourhood quality in the destination area. Combining the graph with Table 17, we note that the three largest outliers correspond to the three destination municipalities with the fewest observations (Salem, Sigtuna and Vallentuna). This serves to graphically display the risk for volatile estimates when having few observations.

A.8 Further sibling analysis

Figure 8: Relationship between the regression coefficients and age of siblings



Notes: The figure plots the regression coefficients from Table 12 against the age clusters of siblings, graphically illustrating the exposure effect.

Table 18: Baseline specification with family fixed effects

	(1)	(2)
Mover lifetime income rank	Origin FE	Family FE
Neighbourhood quality	0.28***	0.25***
	(0.04)	(0.05)
Observations	4,572	4,572
R-squared	0.02	0.50
Origin FE	YES	YES
Family FE	NO	YES
Controls	NO	NO

Notes: The sample in Columns (1) and (2) includes both the 1953 cohort members and their siblings. We redefine the independent variable and let neighbourhood quality vary with both the destination municipality and a child's year of birth. Column (1) represents regression results from Equation (1) using the reconstructed independent variable, and Column (2) from Equation (1) using the reconstructed independent variable while applying family fixed effects. We note that the coefficient in the baseline specification is very similar when including family fixed effects, implying that the results are stable to the inclusion of controls for family unobservables. It is worth mentioning that the much smaller coefficient compared to the one in the original estimation of Equation (1) stems from the two samples (including and excluding siblings) not being directly comparable. Nor is the measure of neighbourhood quality directly comparable since we let it vary across birth cohorts to create variation in the independent variable within a family. Standard errors are presented in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

B Appendix 2: Robustness Checks

B.1 Restricting the sample

Table 19: Regression results, restricted sample

	(1)	(2)
Mover lifetime income rank	Model 1 - Restricted Sample	Model 3 - Restricted Sample
Neighbourhood quality	0.64***	0.84***
	(0.11)	(0.17)
Move age 7-10*NQ		-1.07***
		(0.30)
Move age 10-14*NQ		0.02
		(0.30)
Move age 14-18*NQ		0.18
		(0.31)
Observations	1,651	1,651
R-squared	0.01	0.05
Origin FE	YES	YES
Age interval FE	NO	YES

Notes: Results from regressing Model (1) and Model (3) in the baseline specification using the restricted sample obtained when including controls in Model (2) and Model (4), to verify that the missing values do not systemically bias the results. Robust standard errors are presented in parentheses, and clustered on origin. * p<0.10, ** p<0.05, *** p<0.01.

B.2 Altering the income variable

Table 20: Regression results, constructing the outcome variable using disposable income

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Neighbourhood quality	0.73***	0.14	0.84***	0.31***
	(0.04)	(0.09)	(0.08)	(0.09)
Sex (Female)		-11.13***		-10.64***
		(1.11)		(1.07)
Parental Income: 1st quintile (bottom)		-9.40***		-9.13***
		(1.76)		(2.03)
Parental Income: 2nd quintile		-4.80***		-4.79***
		(1.20)		(1.37)
Parental Income: 3rd quintile		-6.00***		-6.14***
		(1.66)		(1.81)
Parental Income: 4th quintile		-2.59*		-1.99
		(1.30)		(1.82)
Omitted variable: 5th quintile (top)		-		-
Married - Parents		5.51**		4.67*
		(2.15)		(2.24)
Upper and upper middle class		10.52***		10.35***
		(2.48)		(2.89)
Lower middle class, officials		6.15***		5.55**
		(1.90)		(2.06)
Lower middle class, entrepreneurs		0.85		0.48
		(1.47)		(1.37)
Working class, skilled workers		-0.77		-0.55
		(1.58)		(1.48)
Omitted variable: Working class, unskilled workers		-		-
Years of Education (Head of Household)		0.80***		0.79***
		(0.23)		(0.22)
Move age 7-10*NQ			-0.61***	-0.92***
			(0.14)	(0.14)
Move age 10-14*NQ			0.30	0.30
			(0.29)	(0.21)
Move age 14-18*NQ			-0.11	0.11
			(0.19)	(0.10)
Observations	2,023	1,651	2,023	1,651
R-squared	0.03	0.13	0.05	0.15
Origin FE	YES	YES	YES	YES
Age interval FE	NO	NO	YES	YES
Controls	NO	YES	NO	YES

Notes: The table displays results from the main analysis, using disposable income as the underlying income measure. Each column represents one regression: Model 1 shows the regression results from Equation (1), Model 2 from Equation (3), Model 3 from Equation (2) and Model 4 from Equation (4). Robust standard errors are presented in parentheses, and clustered on origin. * p<0.10, ** p<0.05, *** p<0.01.

Table 21: Regression results, constructing the outcome variable using labor income

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Neighbourhood quality	0.56***	0.14	0.77***	0.41*
	(0.07)	(0.14)	(0.13)	(0.22)
Sex (Female)		-11.43***		-10.96***
		(0.86)		(0.80)
Parental Income: 1st quintile (bottom)		-6.46***		-6.38**
		(2.22)		(2.44)
Parental Income: 2nd quintile		-3.43**		-3.27*
		(1.35)		(1.59)
Parental Income: 3rd quintile		-4.28*		-4.38*
		(2.28)		(2.47)
Parental Income: 4th quintile		-1.10		-0.59
		(1.54)		(2.08)
Omitted variable: 5th quintile (top)		-		-
Married - Parents		5.04**		3.87
		(2.31)		(2.64)
Upper and upper middle class		7.50***		7.60***
		(1.79)		(2.06)
Lower middle class, officials		5.60***		5.28***
		(1.37)		(1.44)
Lower middle class, entrepreneurs		-1.68		-1.62
		(1.89)		(1.81)
Working class, skilled workers		-0.86		-0.38
		(1.63)		(1.27)
Omitted variable: Working class, unskilled workers		-		-
Years of Education (Head of Household)		0.11		0.10
		(0.30)		(0.30)
Move age 7-10*NQ			-0.94***	-1.44***
			(0.24)	(0.33)
Move age 10-14*NQ			0.03	0.01
			(0.32)	(0.29)
Move age 14-18*NQ			0.08	0.27
			(0.32)	(0.28)
Observations	2,023	1,651	2,023	1,651
R-squared	0.01	0.08	0.04	0.11
Origin FE	YES	YES	YES	YES
Age interval FE	NO	NO	YES	YES
Controls	NO	YES	NO	YES

Notes: The table displays results from the main analysis, using labor income as the underlying income measure. Each column represents one regression: Model 1 shows the regression results from Equation (1), Model 2 from Equation (3), Model 3 from Equation (2) and Model 4 from Equation (4). Robust standard errors are presented in parentheses, and clustered on origin. * p<0.10, ** p<0.05, *** p<0.01.

B.3 Altering siblings' age spans

Table 22: Regression results, sibling comparisons

	(1)	(2)
Mover lifetime income rank	Younger Siblings (-10 years)	Older Siblings (+10 years)
Neighbourhood quality	0.56***	0.42**
	(0.16)	(0.18)
Observations	1,657	1,112
R-squared	0.01	0.02
Origin FE	YES	YES
Family FE	NO	NO
Controls	NO	NO

Notes: Both columns show regression results from Equation (1), but displaying the predicted outcomes for different subgroups. Column (1) shows results for younger siblings, and column (2) for older siblings. In contrast to Table 12, only siblings that are more than 10 years older or younger than the 1953 cohort members are excluded from the sample, allowing a larger age span. Robust standard errors are presented in parentheses, and clustered on origin.
* p<0.10, ** p<0.05, *** p<0.01.

B.4 Moving age intervals, different cutoffs

Table 23: Regression results, varying moving age intervals

Mover lifetime income rank	(1) Cutoff age 7	(2) Cutoff age 7	(3) Cutoff age 10	(4) Cutoff age 10	(5) Cutoff age 14	(6) Cutoff age 14
Neighbourhood quality	0.83***	0.34*	0.62***	0.03	0.67***	0.08
	(0.11)	(0.17)	(0.08)	(0.13)	(0.07)	(0.12)
Sex (Female)		-12.83***		-12.97***		-12.85***
		(1.17)		(1.19)		(1.21)
Parental Income: 1st quintile (bottom)		-7.82***		-8.12***		-8.02***
		(1.84)		(1.72)		(1.80)
Parental Income: 2nd quintile		-4.35***		-4.32***		-4.15***
		(1.22)		(1.20)		(1.25)
Parental Income: 3rd quintile		-4.93**		-4.89**		-4.63**
		(1.97)		(1.98)		(2.11)
Parental Income: 4th quintile		-1.42		-1.37		-1.30
		(1.22)		(1.26)		(1.40)
Omitted variable: 5th quintile (top)		-		-		-
Married - Parents		4.47*		3.81		3.95
		(2.56)		(2.67)		(2.59)
Upper and upper middle class		6.98***		7.10***		7.25***
		(1.86)		(1.90)		(2.02)
Lower middle class, officials		5.37***		5.25***		5.27***
		(1.51)		(1.54)		(1.54)
Lower middle class, entrepreneurs		-0.54		-0.35		-0.11
		(1.91)		(1.92)		(1.99)
Working class, skilled workers		-0.77		-0.49		-0.60
		(1.85)		(1.69)		(1.69)
Omitted variable: Working class, unskilled workers		-		-		-
Years of Education (Head of Household)		0.35		0.33		0.32
		(0.25)		(0.23)		(0.22)
Move after age 7*NQ	-0.18	-0.38				
	(0.16)	(0.24)				
Move after age 10*NQ			0.31*	0.55**		
			(0.17)	(0.21)		
Move after age 14*NQ					0.36	0.60**
					(0.32)	(0.21)
Observations	2,023	1,651	2,023	1,651	2,023	1,651
R-squared	0.02	0.11	0.03	0.11	0.03	0.11
Origin FE	YES	YES	YES	YES	YES	YES
Age interval FE	YES	YES	YES	YES	YES	YES
Controls	NO	YES	NO	YES	NO	YES

Notes: The table displays the results when altering the moving age intervals, by introducing three different cutoff points. Columns (1) and (2) present the effect of moving to a higher quality neighbourhood before the age of 7, and the first interaction term shows the effect of moving to a higher quality neighbourhood after the age of 7. Columns (3) and (4) present the effect of moving to a higher quality neighbourhood before the age of 10, and the second interaction term shows the effect of moving to a higher quality neighbourhood after the age of 10. Columns (5) and (6) present the effect of moving to a higher quality neighbourhood before the age of 14, and the third interaction term shows the effect of moving to a higher quality neighbourhood after the age of 14. Robust standard errors are presented in parentheses, and clustered on origin. * p<0.10, ** p<0.05, *** p<0.01.

B.5 Excluding Stockholm municipality

Table 24: Regression results, excluding Stockholm municipality

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 4
Neighbourhood quality	0.79***	-0.06	1.15**	0.65
	(0.27)	(0.38)	(0.46)	(0.74)
Sex (Female)		-16.30***		-14.50***
		(3.77)		(4.47)
Parental Income: 1st quintile (bottom)		-5.11		-1.40
		(5.61)		(7.94)
Parental Income: 2nd quintile		-4.32		-0.94
		(7.04)		(8.42)
Parental Income: 3rd quintile		1.15		4.57
		(4.96)		(6.59)
Parental Income: 4th quintile		-2.17		2.64
		(5.17)		(6.46)
Omitted variable: 5th quintile (top)		-		-
Married - Parents		0.06		-5.09
		(6.50)		(7.09)
Upper and upper middle class		12.61*		12.41*
		(6.08)		(6.81)
Lower middle class, officials		7.31**		5.74
		(2.54)		(3.45)
Lower middle class, entrepreneurs		2.11		-0.71
		(7.67)		(9.34)
Working class, skilled workers		0.13		-0.01
		(3.95)		(4.30)
Omitted variable: Working class, unskilled workers		-		-
Years of Education (Head of Household)		0.34		0.46
		(1.14)		(1.33)
Move age 7-10*NQ			-0.91	-1.80
			(0.85)	(1.08)
Move age 10-14*NQ			-6.57**	-5.83***
			(2.86)	(1.27)
Move age 14-18*NQ			0.47	0.39
			(1.40)	(1.74)
Observations	360	306	360	306
R-squared	0.05	0.18	0.16	0.26
Origin FE	YES	YES	YES	YES
Age interval FE	NO	NO	YES	YES
Controls	NO	YES	NO	YES

Notes: The table shows the results from excluding the most dominant municipality, Stockholm municipality, from the main analysis. Each column represents one regression: Model 1 shows the regression results from Equation (1), Model 2 from Equation (3), Model 3 from Equation (2) and Model 4 from Equation (4), with the alteration of excluding Stockholm municipality. The results display similar patterns as the main analysis in Table 7. Most notably, we see a strongly negative and statistically significant estimate for moving at age 10-14 compared to moving at age 0-7, which was not found when including Stockholm municipality in the analysis. However, seeing that the observations reduce drastically, we refrain from reading too much into the strength of given estimates. Robust standard errors are presented in parentheses, and clustered on origin municipality. * p<0.10, ** p<0.05, *** p<0.01.

B.6 Allowing only moves to and from Stockholm municipality

Table 25: Regression results, including only moves to and from Stockholm municipality

Mover lifetime income rank	(1) Model 1	(2) Model 2	(3) Model 3	(4) Model 3
Neighbourhood quality	0.73***	0.22***	0.79***	0.36***
	(0.00)	(0.04)	(0.00)	(0.05)
Sex (Female)		-12.50***		-12.19***
		(0.69)		(0.69)
Parental Income: 1st quintile (bottom)		-9.02***		-9.95***
		(1.55)		(1.28)
Parental Income: 2nd quintile		-5.17***		-5.66***
		(1.22)		(1.52)
Parental Income: 3rd quintile		-7.03***		-7.20***
		(0.65)		(0.56)
Parental Income: 4th quintile		-2.26***		-2.22***
		(0.74)		(0.73)
Omitted variable: 5th quintile (top)		-		-
Married - Parents		5.90***		5.31***
		(1.57)		(1.57)
Upper and upper middle class		5.98***		5.58***
		(1.73)		(1.71)
Lower middle class, officials		4.82***		3.91**
		(1.41)		(1.42)
Lower middle class, entrepreneurs		-1.21		-1.19
		(1.75)		(1.70)
Working class, skilled workers		-1.13		-1.18
		(2.23)		(2.16)
Omitted variable: Working class, unskilled workers		-		-
Years of Education (Head of Household)		0.17		0.13
		(0.16)		(0.20)
Move age 7-10*NQ			-0.58***	-1.13***
			(0.00)	(0.01)
Move age 10-14*NQ			0.40***	0.24***
			(0.00)	(0.05)
Move age 14-18*NQ			-0.04***	0.29***
			(0.00)	(0.06)
Observations	1,663	1,345	1,663	1,345
R-squared	0.01	0.10	0.04	0.12
Origin FE	YES	YES	YES	YES
Age interval FE	NO	NO	YES	YES
Controls	NO	YES	NO	YES

Notes: The table shows the results when only including moves to and from the most dominant municipality, Stockholm municipality, in our main analysis. Each column represents one regression: Model 1 shows the regression results from Equation (1), Model 2 from Equation (3), Model 3 from Equation (2) and Model 4 from Equation (4), with the alteration of including only moves to and from Stockholm municipality. The patterns are similar to the main analysis. Robust standard errors are presented in parentheses, and clustered on origin. * p<0.10, ** p<0.05, *** p<0.01.

Table 26: Number of moves to and from Stockholm municipality, by moving age interval and destination municipality

	Moving status				
Neighbourhood quality	Moved once, age 0-7	Moved once, age 7-10	Moved once, age 10-14	Moved once, age 14-18	Total
59.03 (Danderyd)	70	23	9	3	105
56.80 (Salem)	3	2	3	14	22
54.53 (Solna)	98	20	12	22	152
54.27 (Sollentuna)	76	18	2	5	101
54.16 (Täby)	85	21	9	7	122
52.93 (Lidingö)	102	25	8	8	143
52.85 (Huddinge)	77	35	26	38	176
50.96 (Tyresö)	31	32	21	17	101
50.16 (Stockholm)	338	25	22	24	409
50.08 (Sundbyberg)	26	5	4	4	39
49.55 (Österåker)	9	5	3	9	26
49.38 (Haninge)	30	21	16	22	89
49.36 (Nacka)	70	25	16	24	135
48.90 (Upplands Väsby)	5	5	3	2	15
48.74 (Järfälla)	59	47	35	20	161
48.24 (Sigtuna)	5	11	1	2	19
44.86 (Vallentuna)	5	3	4	3	15
42.76 (Botkyrka)	20	8	4	5	37
Total	1,109	331	198	229	1,867

Notes: The table displays the distribution of movers per destination municipality and moving age interval, when only including moves to and from the most dominant municipality, Stockholm municipality, in our main analysis. We note relatively few observations among the highest quality municipalities for the moving age interval 10-14 years old, perhaps driving the positive coefficient we observe in that age interval in Table 25.