It's About Time: Social Housing, Parental Labour Supply, and Long-term Child Outcomes*

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September 2025

Abstract

This paper studies the effect of highly subsidized social housing on both medium-term parental labour market outcomes and long-term child earnings and educational attainment. Using linked Canadian administrative data, I exploit variation in the timing of entry into social housing to identify the effects of additional exposure during childhood and find that children who enter earlier achieve better adult outcomes. For parents, event-study estimates around entry reveal substantial reductions in labour supply and earnings, with nearly no effect on net-of-housing income. A simple time-allocation model—where social housing receipt both relaxes the budget constraint and insures parents against earnings uncertainty—rationalizes these large responses. Declines in parental labour supply are a key factor behind improvements in child outcomes: gains for children are largest when parents reduce their labour supply the most. This channel is highly robust and appears to be causal, as suggested by an analysis isolating exogenous labour supply responses using displacement distance. These results highlight a critical trade-off between maximizing the return for children and the labour market participation of parents.

Keywords: social housing, neighbourhood effect, income inequality, labour, intergenerational mobility, Canada
JEL codes: H53, I38, J08, R28

*I am indebted to David Green, Raffaele Saggio, Samuel Norris, Thomas Lemieux, and Victor Couture for their generous guidance and support. I thank Claudio Ferraz, Nicole Fortin, Sam Gyetvay, Kevin Milligan, Pascuel Plotkin, Marit Rehavi, Paul Schrimpf, Katherine Wagner [...] for valuable suggestions and comments. This research was made possible thanks to funding from the Fonds de recherche du Québec – Société et culture (FRQSC) and UBC's Stone Centre on Wealth and Income Inequality. I would also like to thank Loïc Courtemanche, Wendy Kei, and Cheryl Fu for their support at the UBC and UQAM Canadian Research Data Centres. Part of this research was conducted at the Québec Interuniversity Centre for Social Statistics (QICSS) and at the UBC RDC, part of the Canadian Research Data Centre Network (CRDCN). This service is provided through the support of QICSS' Member Universities, the province of Quebec, the Canadian Foundation for Innovation, the Canadian Institutes of Health Research, the Social Science and Humanities Research Council, the Fonds de Recherche du Québec, and Statistics Canada. All views expressed in this work and remaining errors are my own.

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Housing assistance policies are a primary tool for addressing economic precarity and breaking intergenerational transmission of poverty. Nearly all advanced economies operate some form of housing assistance programs targeted at low-income families, yet we still lack clarity on how these policies translate into gains for children. Canonical mechanisms emphasize both direct uses of income – spending on child-centred goods (food, clothing, books, etc.) – and indirect channels such as lower parental stress, improved family functioning, and more stable routines. At the same time, debates over public housing raise two persistent concerns: the concentration of poverty and possible harms to children (Chyn 2018; Jacob 2004; Kling et al. 2007; Ludwig et al. 2013), and the potential for subsidies to depress adult labour supply (Jacob and Ludwig 2012; Van Dijk 2019).

This paper demonstrates that parental labour supply responses are a key, often overlooked mechanism linking social housing to children's long-run outcomes. To do so, I provide the first comprehensive evaluation, in a single setting, of the effects of social housing on both children and their parents. Studying programs in Canada's two largest cities, I link parents and children in administrative tax data covering all filers from 1997 to 2021. I track parental earnings before and after entry into social housing and follow children into young adulthood to measure their earnings and post-secondary enrolment. By exploiting the timing of entry, I demonstrate that longer exposure to social housing increases children's adult earnings, raises post-secondary enrolment, and reduces social assistance receipts. My identification relies on variation in years of exposure among individual who lived in social housing at some point during their childhood.

To probe mechanisms and fiscal implications, I conduct a series of event studies on parents' earnings and labour market participation around entry. Consistent with a sizable income effect, entry into social housing reduces parental labour market participation and their earnings. I leverage an institutional feature of those two programs to isolate the role of parental labour supply response in shaping long-term outcomes of children: assignment is based chiefly on household characteristics, and families cannot specify project preference. I exploit this variation in displacement distance in the assignment, which provides heterogeneity in parents' labour market response. Longer moves induce a more pronounced decline in earnings and employment, indicating an involuntary component to the reaction. I then trace how this distance-induced labour supply adjustment influences the treatment effect on children's adult outcomes, allowing me to distinguish gains arising from increased parental time from those due to neighbourhood quality, housing stability, or income alone.

I start the paper by studying the long-term impact on children. Exploit the variation in the age at which children moved into social housing to get variation in exposure time. Given the extensive waiting lists for social housing, the precise timing of entry, conditional on ever receiving it, is plausibly exogenous. This strategy does not require the moving decision to be random, but rather that the timing of the moves is orthogonal to a child's potential outcome among families with the exact origin location. Under this assumption, I estimate that moving into social housing one year earlier has a causal effect on adult outcomes, including labour market outcomes and post-secondary attendance. Getting social housing one year earlier increases labour earnings by about $265 (200 USD) , decreases the amount of social assistance receipts by xx, and boosts post-secondary attendance by 0.35 percentage points.

Next, to study the impact of social housing on adults, I exploit the timing at which families moved into social housing in a matched event-study framework. Adults entering social housing experience a sudden drop in labour market earnings of about $2,400 (1,750 USD) annually. Those effects are long-lasting. This decline stems from both a four percentage point lower probability of employment and a 12% reduction in hours for those who remain employed. While treated individuals are more likely to change employers, this does not result in transitions to lower-paying firms, as measured by their AKM effects.

Although families earn less, they also pay much less in rent. When they enter social housing, they pay about 350$ less in monthly rent per adult, which completely compensates their drop in income, keeping their net-of-housing disposable income constant in the long term. This suggests that expenses for non-housing goods are not significantly affected.

I then explore how the parents' labour market supply reduction relates to treatment effects on children. I find that children whose parents reduced their labour supply are those who drive most of the treatment effects. To provide convincing evidence of this, I exploit displacement distance—the distance families move at entry—to get exogenous variation in the parental labour market response, allowing me to identify the causal relationship between treatment effects on parents and those on children. The estimates confirm that children's gains are largest precisely where parents reduce labour supply more, implying a clear siting trade-off between maximizing the return for children and the labour market participation of parents.

Finally, I compute the net present value of one year of social housing for the average family across places. I find that the total net benefits are maximized in neighbourhood xx. **[Include more details here.]**

This paper contributes to several existing bodies of literature. First, it relates to numerous studies on the impact of public housing on children. Typically, studies on the effect of public housing on children have relied on exogenous displacement due to building demolitions.[1] For instance, Jacob (2004) finds that, because displaced families usually relocated to similarly disadvantaged neighbourhoods, the demolition had no impact on children. Relying on the now-recognized idea that exposure length matters when studying contextual effects, Chyn (2018) compares children displaced at different ages. He finds that displaced children had better outcomes, especially if they moved out at an earlier age. In those two cases, families that left social housing received housing vouchers; hence, we should interpret these results with caution.

More recent research leverages entries into social housing, rather than exits, to estimate exposure effects. By exploiting plausibly random variation in the precise timing of entry, studies have revealed positive impacts on test scores (Han and Schwartz 2021), adulthood earnings, and reduced safety net program participation (Chaudhry and Eng 2024).[2] Building on this, my paper offers additional estimates on the causal effect of an additional year in social housing on adulthood earnings and post-secondary education attendance. I also offer new insights into the mechanisms by which social housing affects children. Part of the benefit comes from the reduced labour supply of the parents, allowing for increased housework and time investment in the children. In this sense, my paper also relates to Fuenzalida et al. (2024) and [Cite Bernardo], who study home ownership programs in Latin America and explore how treatment effects on children are related to treatment effects on parents.

This paper also contributes to research on labour supply responses to housing assistance. Jacob and Ludwig (2012) study a housing voucher wait-list lottery in Chicago and find that recipients reduce labour force participation by around 4ppt (6%) and quarterly earnings by $329 (10%). In a different setting, Van Dijk (2019) finds that the average move into Amsterdam's public housing system negatively affects labour market outcomes and proxies for neighbourhood quality, and increases public assistance receipt. My findings align with these studies, showing comparable adverse effects of social housing on earnings and employment probability. I extend this analysis by leveraging employee-employer linked data to explore job transitions, job quality, and commuting patterns. I also study how rent paid and net-of-housing-cost income change when people enter social housing.

Ultimately, I contribute to the debate about "Where should we build public housing?" Much of that debate has been occupied by issues relating to the impact of social housing

[1] Another notable branch of this literature studied families leaving public housing through the Moving Opportunity (MTO) experiment. Numerous papers have studied the impact of this experiment on children and found limited effects on schooling and later economic outcomes (Kling et al. 2007; 2005; Ludwig et al. 2013), though Chetty et al. (2016) found positive impacts for children whose families moved to less disadvantaged areas.

[2] Pollakowski et al. (2022) also found that an additional year of public housing increased earnings by 6% and reduced incarceration rates at age 26, using a sibling design.

on neighbourhoods and segregation (Almagro et al. 2024)[3]. Although not causal, there are documented spatial heterogeneities in the effect of social housing on adults and children. For instance, Van Dijk (2019) shows that while average moves into social housing led to negative labour supply responses, moves into social housing in high-income neighbourhoods generated positive labour market outcomes. Chaudhry and Eng (2024) estimate development-level treatment effects on children and find heterogeneity across developments. Those development-level effects are positively correlated with the share of homeowners and the median neighbourhood income. The quasi-random assignment of households to locations in my context provides a credible setting to explore spatial heterogeneity in both treatment effect dimensions. This allows me to describe how the impacts of social housing on adults and children are spatially related.

The rest of the paper proceeds as follows. Section 2, presented the Social Housing programs in Toronto and Montréal. Section 4, describes the various data sources and the analysis sample. Section 5 presents the effects of social housing on children, while 6 discusses the impact of social housing on adults. Section 7 shows the relationships between effects on adults and effects on children. Section 8 discusses the cost-effectiveness of the policy. Finally, 9 concludes.

2 Social Housing Program

While housing falls under provincial jurisdiction in Canada, the federal government has historically played a significant role in funding public housing investments. However, the recession of 1990-93 and its subsequent fiscal repercussions led the federal government to substantial cuts in social programs. As a result, federal funding for social housing development was nearly eliminated (Suttor 2016). Although provincial funding helped sustain existing social housing programs, it did little to foster further expansion. Consequently, most of the public housing available today was built between the early 1960s and the early 1990s.

The Rent-Geared-to-Income housing programs are similar in many cities. It houses low-income individuals and families in units they can afford. Tenants pay 25% to 30% of their gross income in rent, and the government covers the remainder of the expenses.

I study the public housing system in the two largest Canadian cities, Toronto and

[3] Many papers study the effects of affordable housing programs that are not social housing (Baum-Snow and Marion 2009; Cook et al. 2023; Diamond and McQuade 2019; Ellen et al. 2016; Freedman and McGavock 2015).

Montréal. Municipal corporations manage the programs: the *Toronto Community Housing Corporation* (TCHC) and the *Office municipal d'habitation de Montréal* (OMHM).[4] The two programs function virtually the same way. The only notable difference is that tenants pay 30% of their gross income in the TCHC program, whereas they pay 25% in the OMHM program. Together, those programs house over 170,000 individuals in more than 80,000 units.

The unit allocation is simple. A social housing unit is allocated according to the following assignment procedure: (*i*) The application is received and recorded in a centralized digital platform, (*ii*) the application is evaluated using a scoring system and then placed on a waiting list, (*iii*) the application is renewed annually, and the file is updated, particularly regarding income, and (*iv*) when a suitable housing unit becomes available, the first people on the list are called, and so on. The family income threshold for a family of 4 is around 60,000$ (43,600 USD) in Toronto and 45,000$ (32,800 USD) in Montréal in 2021 dollars.

The allocation system has two important implications for my research designs. First, the exact timing at which families enter social housing is not easily manipulable. The wait time is long, and all applicants face the same procedure.[5] Second, families cannot specify which project they want to be assigned. In theory, applicants can specify wide geographical preferences. Still, based on discussions with OMHM workers, those preferences are rarely expressed because the fewer regions a family is willing to live in, the longer it will wait for an assignment. Oreopoulos (2003) suggests that this is also true for Toronto. In Canada, social housing buildings are distributed across all parts of cities. While much social housing is to be built in central cities, social housing apartments adjacent to suburban middle-class streets are also common. This approach starkly contrasts with the dominant models in the United States (central-city slum redevelopment) and Australia (low-density peripheral communities) (). This provides an ideal setting for the researcher, as the type of neighbourhood in which social housing residents live varies considerably. The variation in neighbourhood quality is well-documented by Oreopoulos (2003) in the Toronto context.

[4]In practice, my analysis also includes corporation that manages public housing programs in the suburbs of Montréal, but their programs work in the same way, with identical selection criteria.

[5]Rare exceptions of expedited assignment exist for individuals fleeing violence or leaving an apartment deemed unfit for habitation by city by-law (OMHM 2025, https://www.omhm.qc.ca/en/submit-application/assessing-applications-and-waiting-lists

3 Conceptual Framework

This section introduces a model of labour supply and parental investment in children that provides predictions on how parents would respond to getting social housing.

Environment

Time endowment is 1. Leisure $\ell \in [0,1]$, time invested in children $t \geq 0$, hours of work $h = 1 - \ell - t$. Agents also make a monetary investment in their child $m \geq 0$ at price p_m. There is a required good (housing) H with the constraint

$$H \geq 1 \quad \Rightarrow \quad U \in \mathbb{R}, \qquad H = 0 \quad \Rightarrow \quad U = -\infty.$$

The agent consumes a composite of J goods, denoted C, with the composite price normalized to 1. Wage is $w > 0$. Market rent $R \geq 0$ is random; realizations occur after the agent chooses time allocation ex ante. As an outside option, the agent may choose (for free) to live in social housing, in which case rent is a fixed fraction $r \in (0,1)$ of earnings, i.e. $r\,w(1-\ell)$, and there is no rent uncertainty.

Preferences are additively separable and strictly concave:

$$U = u(C) + v(\ell) + \gamma\, g(K), \qquad K = K(t,m), \quad u', v', g' > 0,\ u'', v'', g'' \leq 0.$$

Market housing (uncertain rent)

If the agent rents in the market, after paying R, the composite consumption is

$$C_M(R,\ell,t,m) = w(1-\ell-t) - R - p_m m$$

which is feasible whenever $w(1-\ell-t) \geq R$. The ex-ante problem is:

$$\max_{\ell,t,m\geq 0}\ \mathbb{E}\big[u(C_M)\big] + v(\ell) + \gamma\, g\big(K(t,m)\big).$$

For an interior solution, the first-order conditions (M-FOCs) are:

$$v'(\ell) = w\,\mathbb{E}[u'(C_M)] \tag{M-FOC.1}$$

$$\gamma\, g_K K_t(t,m) = w\,\mathbb{E}[u'(C_M)] \tag{M-FOC.2}$$

$$\gamma\, g_K K_m(t,m) = p_m\,\mathbb{E}[u'(C_M)] \tag{M-FOC.3}$$

Social housing (insured, proportional rent)

If the agent selects social housing, consumption is certain and equals

$$C_S(\ell, t, m) = (1 - r)w(1 - \ell - t) - p_m m.$$

The problem is

$$\max_{\ell,t,m\geq 0}\; u(C_S) + v(\ell) + \gamma\, g\big(K(t, m)\big).$$

with interior first-order conditions (S-FOCs):

$$v'(\ell) = (1 - r)w\, u'(C_S) \tag{S-FOC.1}$$

$$\gamma\, g_K K_t(t, m) = (1 - r)w\, u'(C_S) \tag{S-FOC.2}$$

$$\gamma\, g_K K_m(t, m) = p_m\, u'(C_S) \tag{S-FOC.3}$$

Regime choice

The household chooses the regime with the higher objective; equivalently, compare the certainty-equivalent value in market housing with the certain value in social housing. Social housing changes (i) the net wage for time choices from w to $(1 - r)w$, (ii) removes rent risk (so $u'(C_S)$ vs. $u'(\mathbb{E}[C_M])$ vs. $\mathbb{E}[u'(C_M)]$), and (iii) replaces a lump-sum rent R by a proportional payment $rw(1 - \ell - t)$.

Four channels through which social housing increases leisure and parental time

1. **Budget shape (mechanical substitution effect)**
   Market housing behaves like a lump-sum deduction R from the budget; social housing is a proportional wedge $(1 - r)$ on earnings. This lowers the net price of time (both ℓ and t) from w to $(1 - r)w$, pushing toward more leisure and more time investment t (substitution effect), holding marginal utility fixed.

2. **Mean rent level (pure income effect)**
   If expected market rent exceeds the expected social-housing payment, there is a positive income effect under social housing that raises normal goods (ℓ, t, C, and m) proportionately to their income elasticities.

3. **Risk insurance**
   With $u'' < 0$ (and prudence), $\mathbb{E}[u'(C_M)] \geq u'(\mathbb{E}[C_M])$. High marginal utility in bad rent states makes the RHS in (M-FOCs) larger than under certainty, inducing less ℓ

and less t in the risky regime. Social housing (insurance) thus relaxes the precautionary motive and increases ℓ and t.

4. **Substitution within child investment (t vs m)**
   The proportional rent implies a marginal tax r on earnings, explicitly showing up as $(1-r)w$ in (S-FOCs). This reduces the opportunity cost of time, shifting resources from market work toward ℓ and t. It also tilts the mix between t and m: since t is paid in time (price $(1-r)w$) and m in dollars (price p_m), a lower net wage makes t relatively cheaper than m, ceteris paribus (stronger if $K_{tm} \geq 0$).

Consumption and monetary parental investment
While ℓ and t go up (all four channels go in that direction), the total effects on m and C are ambiguous. For monetary investment m: channels (1), (3), and (4) push down; only the income effect in (2) pushes up. If the social-housing rent relief is large enough to dominate, m can potentially rise; otherwise, m would fall as households substitute toward time spent with the child. For Consumption, channels (1) and (4) push C down; (2) pushes it up; (3) is ambiguous. If social-housing payments are substantially below the expected market rent, the income effect can dominate, and C can potentially rise; otherwise, C would fall as households substitute toward time and earn less.

4 Data and Descriptive Statistics

4.1 Canadian Employer-Employee Dynamic Database

This research leverages Canadian administrative employer-employee links data: the Canadian Employer-Employee Dynamic Database (CEEDD). This data spans 1997 to 2021 and provides granular information on the labour market, covering the universe of individual workers and firms (since 2001). This rich dataset leverages individual tax returns (T1) data, which provide information on the worker and their income. It is merged with records of employment income from businesses (T4). On the firm side, the dataset combines corporate tax (T2) data and the National Accounts Longitudinal Micro-data File (NALMF) to provide comprehensive characteristics of firms. Earnings information about individuals comes from the T4 employment slips (similar to W2 slips in the United States) that report total remuneration that consists of wages, commissions, or bonuses received from an employer in a given year. For workers who received T4 slips from multiple employers in the same year due to job changes or holding multiple jobs, I retain only the primary job, defined as the one with the highest earnings for that year. I deflate all monetary amounts to 2021 Canadian dollars using the all-items Consumer Price Index from Statistics Canada (Statistics Canada 2023). Sociodemographic characteristics are limited to age, gender, marital status, and immigrant status.[6]

One unique advantage of Canadian administrative tax data is that it is partly extracted from the T1 Family File (T1FF). This resource combines individual tax records, employment information, and the Canada Child Tax Benefit data to identify and link spouses and children. Furthermore, the CEEDD provides precise geographic details at the postal code level, enabling fine spatial analysis[7]. This granular location data is crucial for this paper and constitutes a significant comparative advantage over most employee-employer datasets available elsewhere. Although the income information only goes back to 1997, I have location information starting in 1989.

With over three decades of coverage, the CEEDD allows me to track individuals from their early years (via their parents), revealing where they lived then and connecting it to their labour market outcomes in their late twenties and early thirties. This extended timeline unveils crucial insights into how early life experiences shape later career paths.

Starting in 2001, I have had the identity and the location of all firms where an individual works. One limitation is that Canadian tax files include firm identifiers, not establishment identifiers. When studying commuting distance, I restrict to workers who work at single-establishment firms. Single establishment first represents the vast majority of firms. This restriction leads to an overrepresentation of smaller firms.

Two shortcomings of the data are that, as tax files usually do, it misses information on rent paid and on education. Rent paid is crucial information for studying the effects of social housing on household finances. I impute rent paid based on the postal code of residence for non-social housing tenants, and use the subsidized housing formula to determine the rent amount for social housing tenants. Appendix C.2 explains in detail this procedure. Education is a potentially important outcome to look for when studying long-term outcomes of individuals who grew up in social housing. I use the information on federal tax credit for tuition paid to infer post-secondary enrolment. I compute the number of years an individual claimed the tax credit between the ages of 18 and 25, and use this as the number of years of post-secondary enrolment. Appendix D details this approach and shows that this education proxy provides a good coverage when compared to official enrolment counts, and sensible

[6] In Appendix D, I build a proxy for post-secondary schooling based on tax credit from paid tuition fees. I then explore how it relates to neighbourhood effects.

[7] The postal code variable can be converted to various geographical units (e.g. census tracts) using Statistics Canada's Postal Code Conversion File.

estimates of return to education.

4.2 Public housing buildings

The locations and characteristics of public housing buildings are retrieved through Freedom of Information (FOI) requests. The data includes the exact address of each project, as well as some building characteristics (e.g., building year, number of units, number of floors, etc.).

I leverage the CEEDD's postal code to precisely identify residents of public housing. Canadian six-digit postal codes are very granular in urban settings, often corresponding to a single building or one side of a street block. This level of geographic precision enables me to focus on individuals living in what I refer to as "social housing postal codes".

Although 75% of public housing units are located in complexes with unique postal codes—even in cases where smaller developments, such as rows of townhouses, share a single code due to their construction design (Oreopoulos 2003)—relying solely on postal codes can result in some misclassification. To ensure that my public housing resident sample is exclusively composed of subsidized households, I filter the data to include only postal codes with at least 25 units of social housing, which results in an almost unequivocal match with verified official listings of public housing developments.

Accordingly, I assume that individuals residing in a social housing postal code are public housing tenants, following the approach of Oreopoulos (2003). For the analysis involving children, I assign their place of residence based on their parents' location each year. Section C.1 details this assignment process.

4.3 Sample Description

The analysis focuses on individuals who (i) entered social housing as parents, or (ii) had parents who lived in social housing during childhood. I identify entries into social housing when an individuals move from a non-social housing postal code to a social housing postal code that has at least 25 social housing units.

For the adults sample, I focus on entries into social housing that occurred between 2001 and 2017. I restrict to individuals who are aged 25 to 55 at entry into social housing, and who had children over the event period. I drop individuals who had earnings above the eligibility threshold the year before entering. Figure XXX shows that above 95% of people who entered social housing had incomes that were below the eligibility threshold.

Table 2 presents summary statistics for the treated adults and the general population.

As expected, individuals who have ever entered social housing are negatively selected. The years prior to entering social housing, 51% of them have any labour earnings, relative to 78% in the general population. They received more than 8 times more social assistance benefits (4,896$ vs 583$). They are more likely to be women, single parents, or an immigrant.

For the children analysis, I include individuals born between 1982 and 1995 whose parents entered social housing while they were 25 years old or younger, 1993 onward. I measure their post-secondary enrolment based on whether they claim the tuition tax credit between the ages of 18 and 25. Their income and social assistance benefits are measured as their average between the ages of 27 and 31.

5 Long-term Outcomes of Social Housing Children

In this section, I discuss the impact of exposure to social housing on children's economic outcomes in early adulthood. Section 5.1 presents the research design, Section 5.3 presents the main results.

5.1 Research Design

Again, an ideal experiment would involve randomly assigning (or not) families to social housing and looking at the adulthood outcomes of children based on their treatment status. In the absence of such an experiment, I instead exploit the variation in exposure time to social housing that arises from families entering social housing while their children are of different ages. This follows the new wave of papers studying the impact of childhood environment and highlights that exposure length is critical in treatment dosage (Chetty and Hendren 2018; Chyn 2018).

I start by estimating a semi-parametric model where a set of dummies for age at the time of entry into social housing explains adulthood outcomes. Crucially, entries into social housing are defined by parents' location rather than the child's place of residence. This allows a pseudo-placebo test for children whose parents moved into social housing while they were adults. I estimate the following equation:

$$y_i = \beta + \sum_{a=0}^{24} \delta_a \times \mathbf{1}(a_i = a) + \mathbf{X_i}\Gamma + \epsilon_i \tag{1}$$

where y_i is the adulthood outcome of the child (e.g. labour earnings, post-secondary attendance), $\mathbf{1}(a_i = a)$ is an indicator equal one if i's parents moved into social housing at age a, and $\mathbf{X_i}$ is a vector of control (e.g. year of birth, gender, origin neighbourhood). δ_a are the coefficients of interest and are normalized to δ_{25}. In practice, I estimate equation 1 in 2-year age bins to improve precision.

Figure 1 shows that on the earnings and educational outcome front, the effects of social housing are approximately linear in the year of exposure. This is consistent with previous research using exposure designs (Aloni and Avivi 2024; Chaudhry and Eng 2024; Chetty and Hendren 2018; Laliberté 2021). This implies that we can summarize the effects of social housing with the slope coefficient, that is, the effect of spending one more year in social housing on y_i.

I now restrict to children whose parents moved into social housing when they were aged 0-18 and estimate this slope coefficient with the slightly more parametric function:

$$y_i = \beta + \delta(18 - a_i) + \mathbf{X_i}\Gamma + \epsilon_i \tag{2}$$

where δ now provides the causal effect of spending one more year in social housing.

5.2 Identification

My identification strategy does not require the moving decision to be random. Instead, it requires that the timing of the moves be orthogonal to the children's potential outcome among observationally similar families – including with the exact origin location. As noted by Chaudhry and Eng (2024) for New York City's public housing program, Toronto and Montréal's social housing programs have features that justify this assumption. Given the extensive waiting lists for social housing, the precise timing of entry, conditional on ever receiving it, is hardly manipulable.

We may still be concerned that children entering social housing at different ages are from families with distinct characteristics. To explore this possibility, I estimate equation 1 replacing the dependent variables with pre-event family characteristics. Figure A.1 plots the coefficients of interest on family income, neighbourhood-level average earnings, family composition, and parents' employment – characteristics that we'd believe are influential to children's outcomes. Age at entry into social housing is not correlated to any of those family characteristics.

My estimates measure treatment effects on the treated, as they are conditional on ever entering social housing. Although the identification assumption underlying my analysis is that the selection effect does not vary with the child's age at move a, it is still interesting to measure the amount of selection there is. I try to measure the level of selection by running a modified version of equation 1 that includes children whose parents never entered social housing. Appendix E shows that children of parents who entered social housing while the children were adults are negatively selected. They earn 2,443$ less in labour earnings, are 5.8 ppt less likely to attend post-secondary, and are 7.1 ppt more likely to receive social assistance.

5.3 Results

Figure 1 presents estimates of δ_a for four outcomes: labour earnings, social assistance benefits received, years of post-secondary enrolment, and the probability of ever enrolling in post-secondary education. Figure A.2 displays the corresponding coefficients for total income and the likelihood of receiving any social assistance. Table 3 reports the slope coefficient estimates—the parameter δ from equation 2—for all outcomes.

Labour earnings and Total Income Panel A of Table 3 shows the estimated δ for annual labour earnings and total income. Each additional year spent in social housing increases children's annual labour earnings by \$242 (180 USD), equivalent to 0.9% of the average \$27,563 earned by individuals whose parents entered social housing when they were age 25. For instance, an individual who entered at age 12 rather than 17 would earn roughly \$1,210 more annually, a 4.4% gain. The effect on total income is somewhat smaller, reflecting reductions in non-labour income. One additional year of exposure raises total income by \$205 (150 USD), or 0.6% of the average total income of \$34,218. In panel A of Table B.1, I estimate treatment effects on men and women separately and find that most of the treatment effect on earnings and income is driven by the impact on women. This is in line with Chaudhry and Eng (2024), who found no effect on men's employment, but positive effects on women's employment.

Post-Secondary Enrolment Panel B of Table 3 presents the estimated δ from equation 2 for the number of years an individual is enrolled in post-secondary education and whether they ever enrolled. I find that each year spent in social housing increases the probability of ever enrolling in post-secondary education by 0.3 percentage points from the baseline average of 54%. I find that the effect on the number of years of enrolment from age 18 to 25 increases by 0.02 years for each additional year of exposure to social housing. This represents a 1.1% increase from the average 1.9 years of post-secondary enrolment for those whose parents moved into social housing when they were 25 years old. An individual who moved into social housing at 12 instead of 17 is 1.5 percentage points more likely to attend post-secondary education — or 2.8% — and has, on average, 0.1 more years of post-secondary enrolment. Panel B Table B.1 shows that the effects of social housing on education are larger for immigrants.

Social Assistance Benefits Finally, Panel C of Table 3 shows the effects on social assistance receipt. Each extra year in social housing reduces the probability of receiving any social assistance in young adulthood by 0.6 percentage points (a 1.5% decline). This lower participation translates into smaller amounts received: benefits decrease by $67 (approximately USD $48) per additional year, a 2% decrease relative to the baseline of $3,283. A child who entered at age 12 rather than 17 is three percentage points (7.5%) less likely to receive social assistance and, on average, receives $335 (10.2%) less in benefits. Panel B Table B.1 shows that the reduction in social assistance receipts is concentrated among natives rather than immigrants.

Spatial Heterogeneity Figure A.13 plots treatment effects over destination neighbourhood characteristics. The effects of one additional year of social housing are not related to the destination average income, and surprisingly, not the neighbourhood effects either. One possible explanation is that neighbourhood effects operate at a hyperlocal level, and the environment within a public housing project is not particularly related to the broader surrounding neighbourhood environments[8] (Chyn and Katz 2021).

6 The Effects of Social Housing on Adults

This section discusses the impact of entering social housing on adults in a family. Section 6.1 presents the research design, and Section 6.2 presents the main results.

6.1 Research Design

The ideal experiment to estimate the causal effect of participation in the social housing program would involve the random selection of recipients. However, there's no such random aspect to this housing program that can be leveraged. Instead, I exploit the timing at which families entered social housing. The exact timing of entry, which is hardly manipulable, provides a quasi-experimental variation. However, comparing individuals who enter social housing to the rest of the population introduces many confounding factors due to differences

[8] When calculating the neighbourhood effects, social housing children are excluded. Appendix C.3 details the estimation procedure.

in the characteristics and behaviours. To address this, I construct a balanced control group by matching treated individuals to observationally similar untreated individuals.

For each treated individual, a potential control is: (1) the same sex, (2) live in the same city, (3) has the same year of birth, (4) has the same marital status and (5) the same number of children (top coded at 4). I use caliper[9] matching on lagged total income, labour earnings, and family income $[t-3, t-1]$. Potential controls are randomly matched to treated units without replacement if they are +/- 5,000$ in each lagged individual-earnings matching variable and +/- 10,000$ in family earnings.

While matching on lagged outcome variables ensures treated and control units are on similar trends, it may raise concerns regarding mean regression post-treatment. In the results, I provide estimates for periods beyond the targeted lags and show that those are also balanced.

Table 2 presents summary statistics for the treated individuals, the matched sample, and the general population. As expected, individuals that ever entered social housing are negatively selected. The years prior to entering social housing, 51% of them have any labour earnings, relative to 78% in the general population. They received more than 8 times more social assistance benefits (4,896$ vs 583$). They are more likely to be women, single parents, or an immigrant.

To estimate the effect of entering social housing on parents, I estimate the following event-study model the matched sample:

$$y_{it} = \beta_0 + \sum_{k=-5}^{7} \beta_k \mathbf{1}\{t = t_i^* + k\} \times Treated_i + \sum_{k=-5}^{7} \theta_k \mathbf{1}\{t = t_i^* + k\} + \mathbf{X}_{it}\Theta + \gamma_i + \mu_{it} \quad (3)$$

where y_{it} is the outcome of interest (e.g. labour earnings, job transition) for individual i at time t; $\mathbf{1}\{t = t_i^* + k\}$ is an indicator variable that references time relative to treatment date t_i^*, γ_i is an individual fixed effect, and $\mathbf{X}_{it}$ is a set of time variant and invariant controls (including year FEs, polynomial in age, single indicator). Under the parallel trend assumption, the coefficient β_k for $k > 0$ gives the causal effects of social housing, k years after entering. Those coefficients are normalized to period $k-1$. Standard errors are clustered at the individual level.

[9] I use the *calipmatch* package from Stepner and Garland (2017)

6.2 Results

Figure A.4 shows the retention in social housing. After 5 years, about 70% of individuals still live in social housing. The reader should consider this when interpreting the results in this section. All event studies are based on people entering social housing, regardless of their duration of stay.

Labour market Figure 2 presents the estimated coefficients β_k from equation 3 for a set of labour market outcomes. When entering social housing, parents suffer a sudden drop in labour earnings. For the first full year that they spend in social housing, they earn 2,073$ less than the control group's average of 12,805 (a 16% reduction). The effect is long-lasting, as they still earn about 2,400$ less six years after entering social housing.

The decrease in labour earnings is driven by both a lower probability of being employed and lower income for those who keep their job. Social housing tenants are about 6 percentage points less likely to receive any labour earnings in a given year. Figure A.5 shows that earnings for individuals who still work at their $t-1$ employer earn about 3,000$ less than the control group average of 34,172$. This is the equivalent of an 8.8% reduction in hours worked, assuming no shift in hourly wage.

For individuals who stay employed, they are more likely to change employers. Based on a sample of individuals with stable employment[10], panel C of Figure 2 shows that they were five percentage points less likely to still work at their original employer. Although they are more likely to change jobs, they do not transition to a lower-paying firm, as measured by firm fixed effects.

Income and Rent Even after the significant decreases in labour market income, it is unclear whether social housing tenants have more or less net-of-housing-cost income. If the income decline is larger than the rental cost reduction, social housing tenants end up with lower net rent disposable income. If the rent reduction is larger, even with the lower income, they might have higher disposable income. Figure 3 shows even study estimates for total income, rent, and net-of-rent income.

Consistent with the substantial decline in labour market earnings, parents who enter social housing experience a significant drop in total income. A couple of years after their entry, they earn around 3,000$ less, a 15% drop relative to observationally similar individuals. The drop in imputed rent paid is slightly larger, so when looking at income net of rent, the sudden increase is quickly offset by the lower rent, resulting in disposable income remaining unaffected in the medium term.

[10]Defined as working at the same firm for two years prior to the event.

Mobility and Commuting Another way social housing can impact adults is through its displacement effect. Moving into social housing might change the distance to current and prospective jobs. Figure A.6 shows that after entry, housing tenants don't see a significant change in their commuting distance. This can be attributed to the typical location of social housing, which is often situated near the urban core. When restricting to individuals who did not change employers, commuting increases slightly, but the change is not statistically significant.

Social housing tenants have higher housing stability. They are about 5 to 6 percent less likely to move from year to year. This represents a considerable decrease compared to the 13.5% of the control group that move every year.

Spatial Heterogeneity Figure A.15 shows DiD coefficient over characteristics of destination neighbourhood. When families move to high-income neighbourhoods, parental labour supply reductions are lower. In fact, when they move to the top quintal neighbourhood, their reduction in labour market income and labour market participation is not significantly different from zero. This finding is consistent with previous research by Van Dijk (2019). We observe a similar pattern, albeit on a different scale, when examining heterogeneity in neighbourhood effects by destination neighbourhoods. Appendix G details the estimations of those neighbourhood effects.

7 Relationship Between Parents' Labour Supply Responses and Treatment on Children

In this section, I analyse the relationship between parental labour supply responses and the treatment effects on children. As documented in Section 6.1, entry into social housing is associated with a reduction in parental labour supply. The resulting effect on children's benefits from social housing is *a priori* ambiguous: lower labour supply may depress household income and weaken labour-market role-model effects, yet if time withdrawn from paid work is reallocated to child-rearing and household production, children's human capital may improve. I assess this trade-off empirically below.

In Section 6.2, I show that the combined effect of lower labour market income and lower rent results in a net-of-housing-cost disposable income that remains unaffected in the medium term. This suggests that the pecuniary investment in children is not a primary channel through which they benefit from social housing.

In Section 7.1, I begin by examining the heterogeneous impact on children and the labour responses of their parents. Then, in Section , I use the distance of move to induce variation in parents' labour supply responses, providing additional evidence of the relationship between parents' labour supply reduction and the benefits children receive from social housing.

7.1 Treatment Effects on Children by Parents' Responses

I first estimate exposure effects separately by parental labour-supply response, restricting to families in which parents worked at baseline. Panels B and C of Table 4 stratify children by how their parents responded to social-housing entry. In Panel B, children whose parents stopped working entirely exhibit the largest gains from social housing. Children whose parents reduced (but did not cease) work experience smaller—yet still statistically significant—gains. By contrast, children whose parents did not reduce labour supply show no statistically significant benefits. This pattern is most pronounced for adult labour earnings and also holds for post-secondary attendance. Panel C, which pools zero-earnings reductions with partial reductions, yields the same qualitative ordering.

Because parental labour responses are endogenous, I also compare children whose parents had positive labour earnings at baseline with those whose parents did not. Parents without baseline earnings cannot reduce labour supply, providing a cleaner contrast. Panel A of Table 4 shows that the effect of social housing on adult annual earnings is indistinguishable from zero for children whose parents were non-workers at baseline; the overall treatment effect is driven by children of baseline earners. This pattern suggests that the capacity to reduce parental labour supply is the main driver of how children benefit from social housing.

Ideally, one would isolate exogenous variation in parental labour responses. In the next section, I leverage move distance as a source of plausibly exogenous variation to provide additional evidence that parental labour-supply adjustments are a key mechanism behind the observed child benefits.

7.2 Spatial Displacement

One potential reason adults earn and work less is that relocating to social housing often requires moving far from their initial home and potentially their workplace. If individuals who had to relocate farther have larger treatment effects, this suggests that part of the labour supply reduction is involuntary.

A useful feature of the social housing programs in Toronto and Montréal is that there is very limited room for selection of the location of residence. Hence, households can hardly select their distance of displacement. I can then use the distance of displacement to get variation in parents' labour supply responses, which is not correlated with the potential outcome. Figure A.9 confirms that the distance moved is not associated with pre-event employment, labour earnings, and family composition, nor family characteristics.

I split entries into social housing moves based on the distance of the move, categorized into five quintiles. Figure A.10 shows that moves into social housing that resulted in longer distance moves lead parents to reduce their labour supply by larger margins. This analysis is limited to individuals who had positive earnings prior to entering social housing. Those who moved less than a kilometre had their labour income decreased by 3,862$ (17.8%), and reduced their probability of having a job by 4.2 percentage points. Those who made long-distance moves (16 to 40km) reduced their earnings by 5,916$ (27%) and their probability of working by 10.1 ppt.

I use the distance of moving to induce variation in parents' labour supply responses to estimate the relationship between parents' labour market responses and the treatment effect on children's long-term outcomes.

The exclusion restriction is that the distance of move does not directly affect children's outcomes, but instead does so only through a reduction in parents' labour supply. This assumption would be violated if the distance of the move is associated with moves to the urban fringe, an area with a lower crime rate and higher-performing schools. To avoid this, my main specification includes destination census tract fixed effects.

Relocating to social housing can disrupt commute patterns and potentially lower adults' working hours and employment attachment. If households that are reassigned farther from their original home (and, by implication, their workplace) exhibit larger post-move labour-supply declines, this points to an involuntary component of the response.

A key institutional feature of Toronto and Montréal social housing programs is the limited scope for applicants to choose the location of their unit. Households have little influence over their eventual location, so the displacement distance is essentially not of their own choosing. I therefore use displacement distance as a source of variation in parents' labour-supply responses that is plausibly orthogonal to both parents' and children's potential outcomes. Consistent with this, Figure A.9 shows that distance is unrelated to pre-event employment, earnings, family composition, and other baseline characteristics.

I partition moves into quintiles of displacement distance. Among individuals who had positive labour earnings at baseline, Figure A.10 shows that moves into social housing that resulted in longer distance moves lead parents to reduce their labour supply by larger margins. For moves under 1 km, parents' annual labour income falls by \$3,862 (17.8%) and employment declines by 4.2 percentage points. For long-distance moves (16–40 km), earnings fall by $5,916 (27%) and employment declines by 10.1 p.p.

I use this distance-induced variation in parental labour supply to estimate how parental responses relate to the effects of social housing on children's long-run outcomes. Figure 4 plots the impact of social housing on parents and children by bins of distance of moves. Figure A.11 presents the same figure with other child outcomes.

To interpret this as a causal relationship, we have to assume that the distance of move does not directly affect children's outcomes, but instead does so only through a reduction in parents' labour supply. There are two potential violations of this assumption. First, if longer moves systematically relocate families to the urban fringe—areas that may differ in crime, school quality, or other child-relevant amenities. To address this concern, I estimate the treatment effects on children, including destination fixed effects, thereby comparing families who land in the same destination tract but differ in how far they had to move. Figure A.14 presents those estimates. The second potential violation would arise if long-distance moves were more likely to break social ties that are negative to the child's development. To explore this possibility, I separate long and short distance moves by whether the parents reduced or increased their labour earnings. Figure **??** shows that children whose parents did not reduce their labour supply do not receive a significantly positive treatment effect, regardless of whether they moved a short distance or a long distance. On the contrary, children whose parents reduce their labour supply receive comparable treatment effects whether they make long or short-distance moves.

8 Marginal Value of Public Funds

This section evaluates the welfare impact of providing *one year* of social housing to a family using the Marginal Value of Public Funds (MVPF) framework of Hendren and Sprung-Keyser (2020). The MVPF is defined as the ratio of beneficiaries' willingness to pay for the policy to the government's net cost of providing it. I implement this in the context of the Toronto program, treating the policy as a one-year treatment. All direct program costs and the parents' rent saving are annual flows realized during the treatment year, while children's effects and their associated fiscal externalities are present values induced by one additional year of exposure.

8.1 Willingness to Pay

The willingness to pay combines the value to parents of the rent reduction during the treatment year and the present value of gains accruing to children in adulthood from that additional year of exposure. First, the program lowers the family's housing payment by **3,500$** during the treated year. I treat this rent saving as the parents' transfer value; that is, I assume they value the rent reduction dollar for dollar.

Second, the children's component pools the present value of higher adult earnings and any change in transfers caused by the marginal year of exposure. The after-tax present value of the earnings gain per child is **$4,109**, and the present value of reduced annual transfers is **$1,422**. Combining these yields a children's consumption-equivalent gain of **$3,714**.

My analysis omits potentially important benefits for which I don't have credible estimates. Excluding them provides a conservative baseline calculation. First, there is an insurance value: by stabilizing housing costs and partially insuring labour-income risk, the program increases certainty-equivalent consumption for risk-averse households. Quantifying this requires assumptions about risk aversion and earnings volatility; it would raise the willingness to pay. Second, there are housing stability and amenity gains: more predictable housing, fewer forced moves, mental health improvements, and other non-pecuniary benefits for parents and children, beyond what is captured in measured earnings and transfer changes. Montpetit et al. (2025) suggests that omitting those non-pecuniary benefits can lead to a significant underestimation of the welfare gains. I consider my MVPF estimate as a lower bound.

Putting pieces together, the baseline willingness to pay for one additional year consists of the **$3,500** rent saving to parents during the treatment year plus the children's present-value gain of **$2,687**.

8.2 Direct Cost

The annual gross resource cost of supplying one occupied unit for a year in the Toronto program is **$4,182**. I obtained this number by subtracting autonomous revenues (e.g., rent paid by tenants) from the total expenses of the TCHC and then dividing by the social housing unit stock managed by the TCHC. Hence, this accounts for direct material costs (including utilities, operating, and maintenance expenses), as well as municipal taxes, depreciation of capital, and interest paid on debt. Because we evaluate a one-year treatment, this figure is used directly as the program's direct cost in the MVPF denominator; no discounting is required.

8.3 Fiscal Externality

Fiscal externalities refer to the changes in public finances resulting from a policy, net of the direct program costs. I use an effective tax rate of twenty percent when translating earnings changes into tax revenues.

For parents, the marginal year of social housing reduces labour income by **$2,500** during the treatment year, which lowers tax revenue by **$500** at the stated tax rate.

For children, the relevant objects are present values induced by the one-year treatment. The government collects additional taxes on the children's higher lifetime earnings equal to twenty percent of **$5,136**, which is **$1,027.20**. In addition, the program lowers the present value of children's future transfers by **$1,422**, which is a one-for-one fiscal saving. Combining these two yields a children's fiscal externality of **$2,449.20**. Summing parent and child components, the baseline fiscal externality used in the MVPF denominator is therefore **$1,949.20**.

In the absence of estimates specific to the studied housing program, some fiscal externalities are discussed but excluded. Reductions in crime and criminal-justice involvement plausibly follow from improved stability and neighbourhood environments (Chyn 2018; Kling et al. 2007; Ludwig et al. 2013); these would raise the fiscal savings and thus lower the net cost in the denominator. Other potential items that could be included are changes in emergency shelter costs and education outlays linked to post-secondary attendance.

8.4 Calculating the MVPF

Given the one-year direct cost of **$4,182** and the fiscal externality of **$2,949.20**, the net cost to the government is **$1,232.80**. Using this denominator, three versions of the MVPF are informative.

The WTP sums the **$3,500** rent saving and the **$3,714** child gain, for a total of **$7,214**. Relative to the **$1,732.80** net cost, the combined MVPF is approximately **4.16**.

If policymakers omitted the long-term benefits for children when considering an additional dollar investment in social housing, they would underestimate the social return. When considering only parents' WTP and the fiscal loss due to income reduction, the MVPF is **xxx**.

cost:

Average subsidy: Montréal 2009: (234,493,138-79,085,039)/20,382 = 7,772$ (7,103 in 2004$)

Table 1: MVPF calculation

MVPF components	Values
Willingness-to-pay	
Transfer value	3,500 $
NPV of gains on children	4,109 $
Taxes and reduced transfers	- 1,422 $
Cost	
Direct cost	4,182 $
Fiscal Externalities	- 1,949 $
Total	2,233 $
MVPF	
Baseline	**XXX**
Parents benefits only	**XXX**

Notes: All amounts in 2021 CAD represent the effect of one year of social housing. Parents' WTP equals the contemporaneous rent saving. Children's willingness to pay (WTP) is the present net value (PV) of lifetime earnings gains net of transfer reductions. Fiscal externalities use a 20% effective tax rate for translating earnings changes into tax revenue.

source: https://www.omhm.qc.ca/sites/default/files/publications/Rapport_annuel2009.pdf (number of units: Annexe E; cost: p26)

Toronto 2004: (528,664,000 - 261,308,000)/58,500 = 4,570$
https://torontohousing.ca/sites/default/files/2023-03/toronto_community_housing_annual_review_2004.pdf
If 25% of RGI: 5,314

In 2021 dollars: (total expenditure - autonomous revenue)/units 246,392,000/58,908 = 4,182 https://torontohousing.ca/sites/default/files/2023-05/tchc_annual_report_2021.pdf#page=125.10

Ontario tax rate: https://www.taxtips.ca/taxrates/on.htm

9 Conclusion

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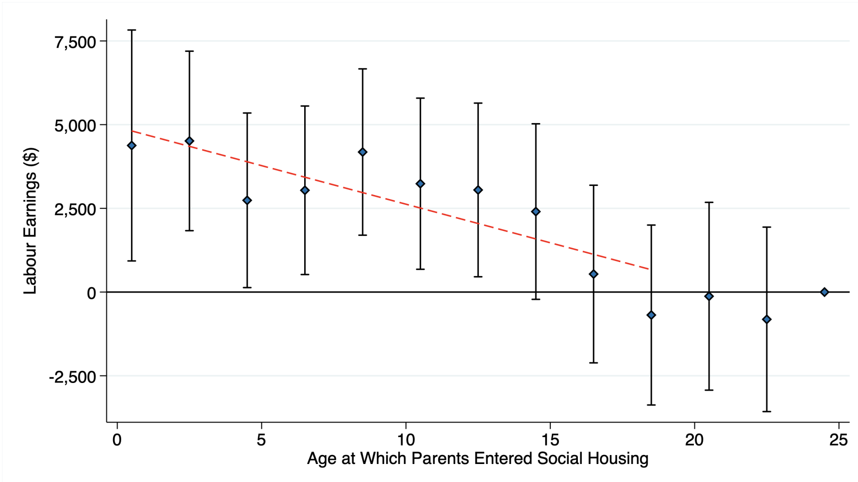
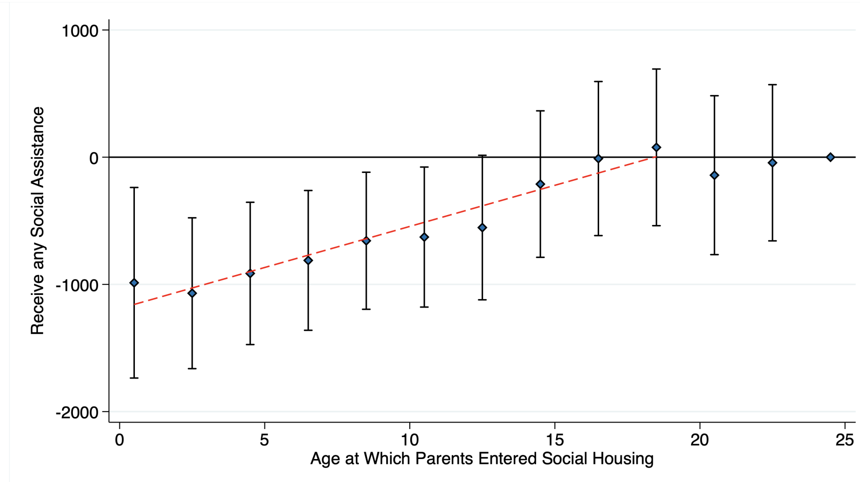
10 Figures

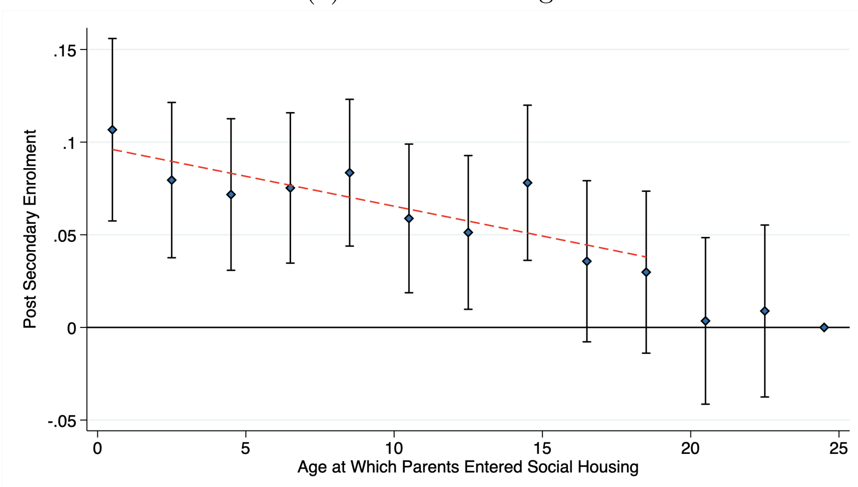
Figure 1: The Effect of Social Housing on Children, by Age of Entry



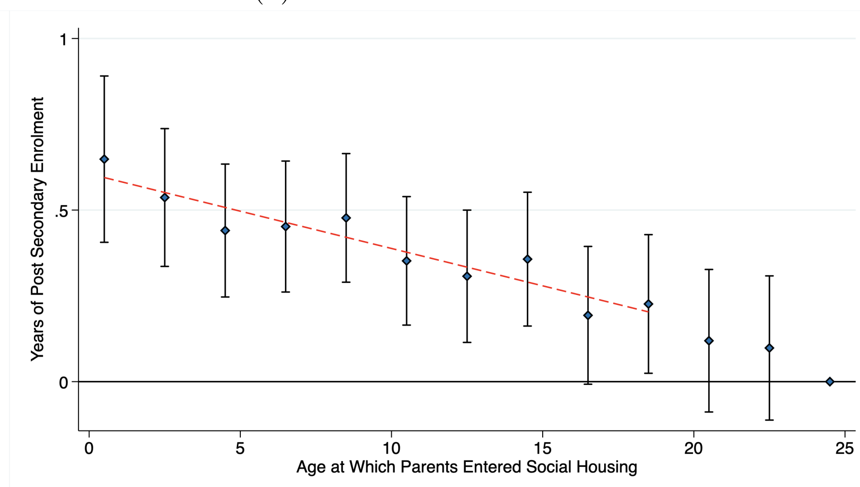
(a) Labour Earnings



(b) Social Assistance Benefits



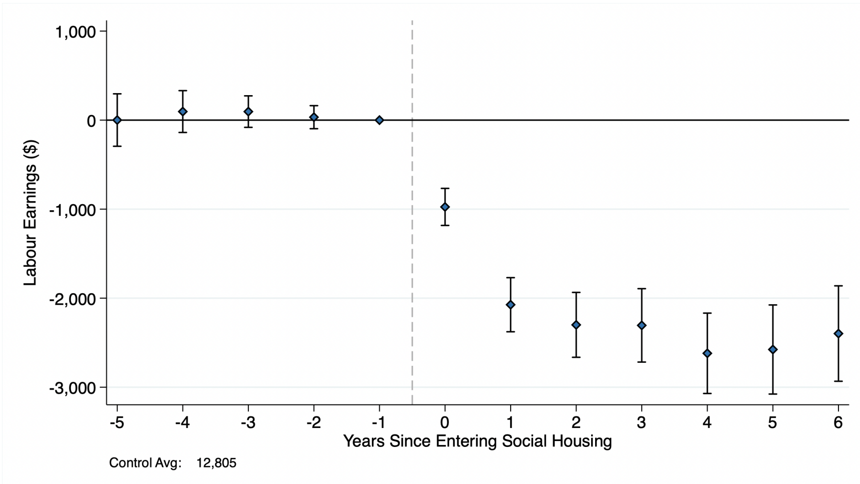
(c) Some Post-secondary Enrolment



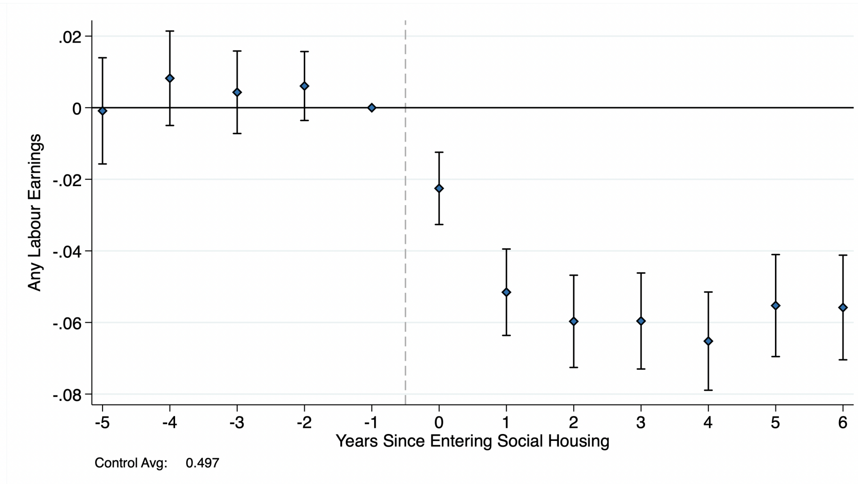
(d) Year of Post-secondary Enrolment

Notes: Each panel plots the estimate for the coefficients δ_a from equation 1 a specified outcome. Each coefficient is the effect of moving into social housing at age a, relative to having parents who moved into social housing at age 25. Age at entry is grouped in 2-year intervals and his based on the timing of when parents moved into social housing, regardless of whether the children still live with their parents. Each regression includes cohort, sex, and origin Census Tract fixed effects. The red dashed line is a linear fit for the coefficient points from 0 to 18. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the family level.

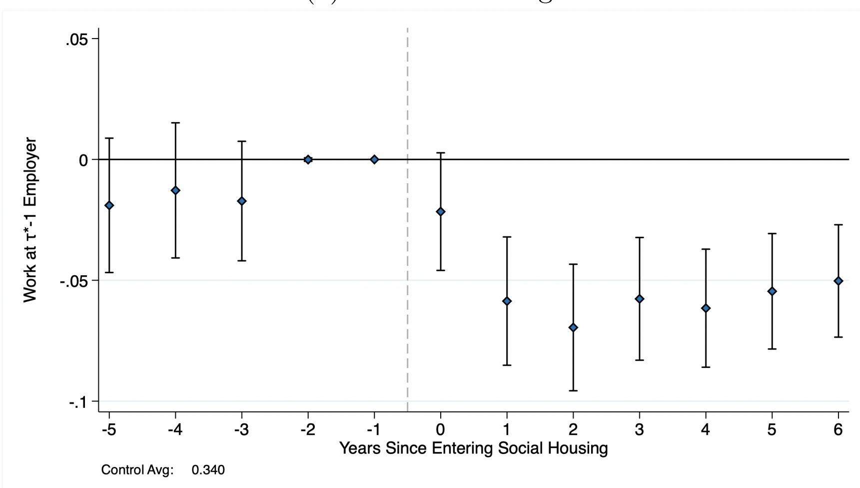
Figure 2: The Effect of Social Housing on Adults, Labour Response



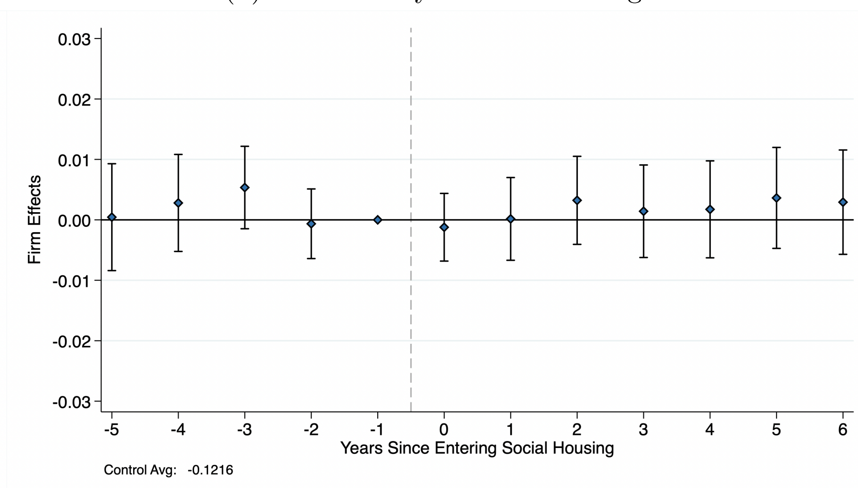
(a) Labour Earnings



(b) Receive Any Labour Earnings



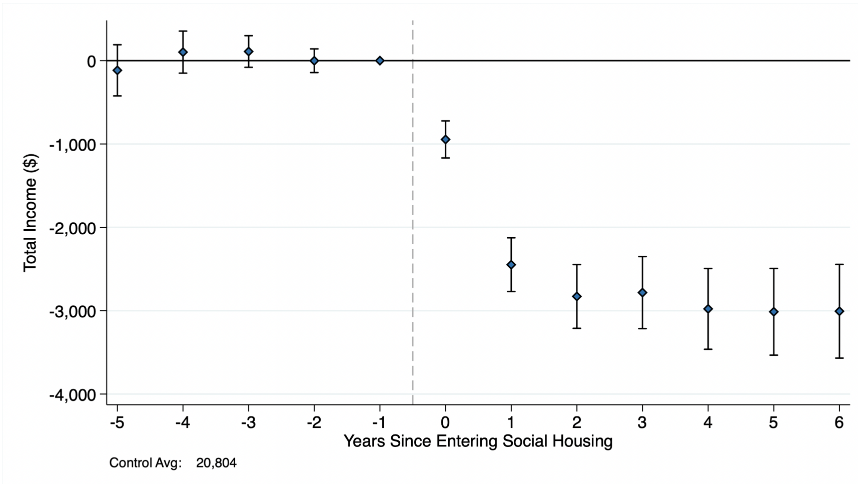
(c) Work at $t-1$ Employer



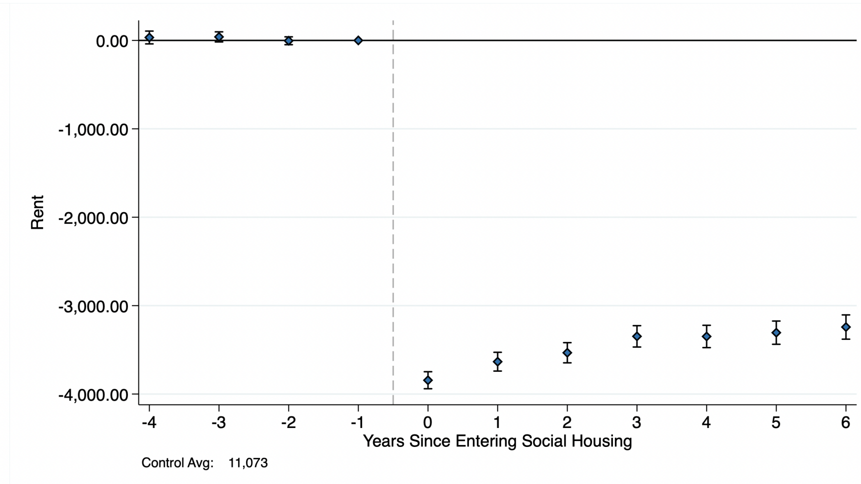
(d) Firm Effects

Notes: Each panel reports the event study estimates from equation 3 on a specified outcome. Treated workers are those who moved from a non-social housing postal code to a social housing postal code. Controls individuals are matched using a caliper matching approach described in Section 6.1. Each regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children and a single dummy. In panel (c), the sample is restricted to those who had stable employment, defined as a 2-year tenure at the firm. In panel (d), Firm Effects are computed by estimating a two-way fixed effect model described in Appendix F. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the individual level.

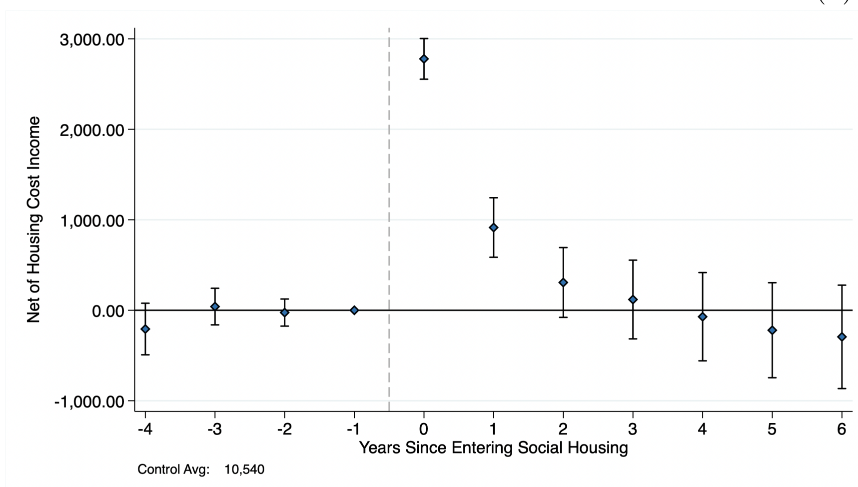
Figure 3: The Effect of Social Housing on Adults, Income



(a) Total Income



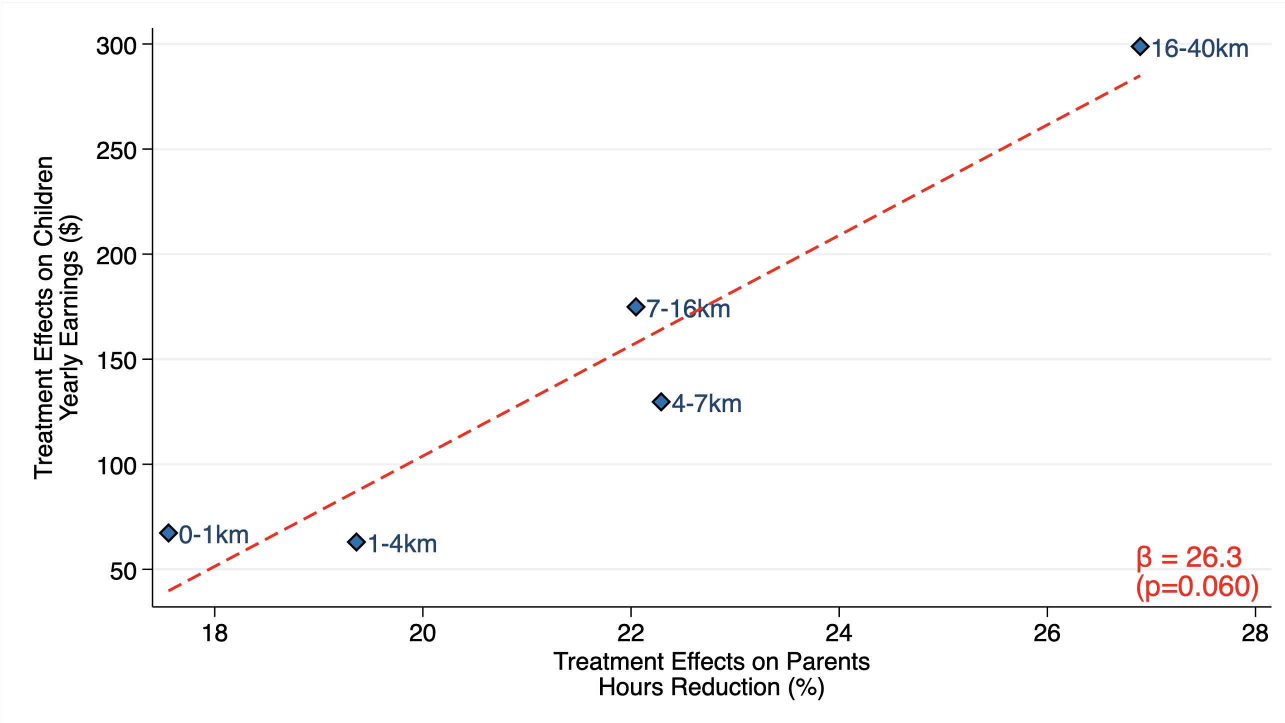
(b) Rent



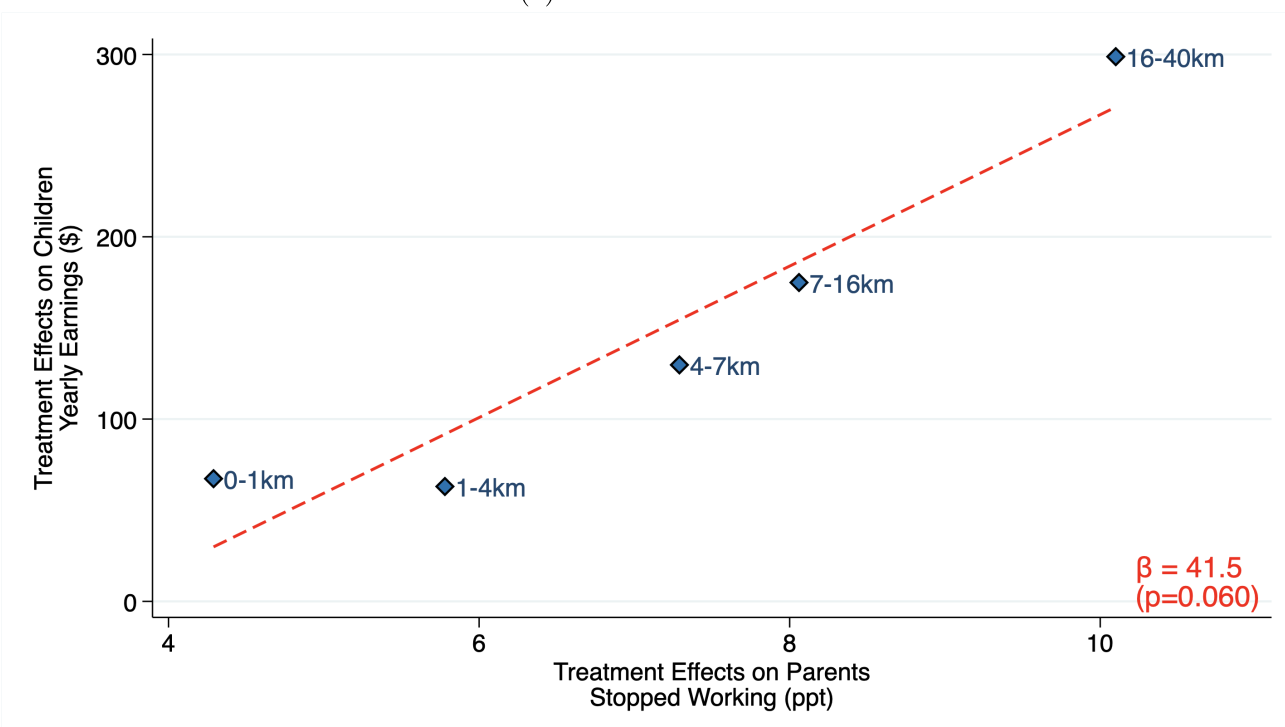
(c) Total Income Net of Rent

Notes: Each panel reports the event study estimates from equation 3 on a specified outcome. Treated workers are those who moved from a non-social housing postal code to a social housing postal code. Controls individuals are matched using a calliper matching approach described in Section 6.1. Each regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children and a single dummy. In panel (d), the Rent amount is calculated from the yearly census-tract median for non-social housing tenants, or using the relevant formula for social housing tenants, described in Appendix C.2. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the individual level.

Figure 4: The Effect of Social Housing by Distance of Move



(a) Hours Reduction



(b) Participation Reduction

Notes: Each panel reports the DiD coefficient on parents' outcome when entering social housing and the treatment effects on children's adult labour earnings. The red line is an OLS estimate of the relationship between treatment effects on adults and on children. The coefficient estimates and the p-value are printed in the bottom right corner. The p-values are obtained through a bootstrap test with 1,499 bootstrap samples.

11 Tables

Table 2: Summary Statistics for Social Housing Tenants and Control individuals

	(1) Matched Treated	(2) Matched Control	(3) All Treated	(4) All Individuals
Share Female	0.58	0.58	0.56	0.51
Couple Parent at $t-1$	0.23	0.23	0.27	0.54
Single Parent at $t-1$	0.24	0.24	0.25	0.09
Number of kids at $t-1$	1.0	0.9	0.9	1.1
Share Immigrant	0.58	0.49	0.60	0.37
Age	39.3	39.3	38.5	40.0
	(9.0)	(9.0)	(8.8)	(8.8)
Year entered social housing	2006.5	2006.5	2006.5	—
Any Labour Earnings	0.43	0.43	0.51	0.78
Labour earnings	9,032	9,284	10,976	48,019
	(14,518)	(14,668)	(15,332)	(108,083)
Total Income	16,737	16,809	18,688	59,118
	(12,381)	(12,432)	(13,522)	(171,311)
Social Assistance	5,795	4,169	4,896	583
	(6,989)	(6,328)	(6,887)	(2,686)
Number of individuals	38,220	38,220	59,090	99,990,150

Notes: Column 1 presents the summary statistics for individuals entering social housing for whom I can find a matched control individual. Characteristics are calculated using the year before entry. Column 2 shows the characteristics of matched control individuals. Potential matched controls are individuals who never lived in a social housing postal code. Matched controls must be of the same sex, year of birth, marital status, and have the same number of kids (top coded at 4). Additionally, a calliper matching based on years lagged labour earnings, total income, and family income is conducted to assign exactly one matched control individual to each treated individual. Column 3 reports the summary statistics for all individuals who moved from a non-social housing postal code to a social housing postal code for the first time. Column 4 reports the summary statistics for all individuals in the Toronto and Montréal Census Metropolitan areas. Dollar amounts are expressed in real terms (2021 CPI). Standard deviations are reported in parentheses.

Table 3: Effects of Social Housing on Children

	(1)	(2)	(3)
Panel A. Income			
Labour Earnings	77.8	183.3***	260.2***
	(48.3)	(54.8)	(62.6)
Total Income	183.0***	150.4***	221.3***
	(45.6)	(51.8)	(58.9)
N	14,520	14,520	14,520
Panel B. Post-Secondary enrolment			
Any Post-Secondary	0.006***	0.004***	0.003***
	(0.001)	(0.001)	(0.001)
Years enrolled in Post-Secondary	0.044***	0.026***	0.025***
	(0.004)	(0.004)	(0.005)
N	17,765	17,765	17,765
Panel C. Social Assistance			
Any Social Assistance Benefits	-0.008***	-0.006***	-0.006***
	(0.001)	(0.001)	(0.001)
Amounts of Social Assistance Benefits	-38.15***	-72.70***	-72.65***
	(11.2)	(13.3)	(14.0)
N	14,520	14,520	14,520
Cohorts, Sex and Immigrant FEs		X	X
Origin and Destination Census Tract FEs			X

Notes: Each column and row is a different estimate of δ in equation 2. Column (1) reports coefficients from univariate regressions, column (2) includes cohort and sex fixed effects, and column (3) additionally includes origin Census Tract fixed effects. Each regression includes children whose parents moved into social housing between the ages of 0 and 18. Dollar amounts are expressed in real terms (2021 CPI). Standard deviations clustered at the family level are reported in parentheses. Level of significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

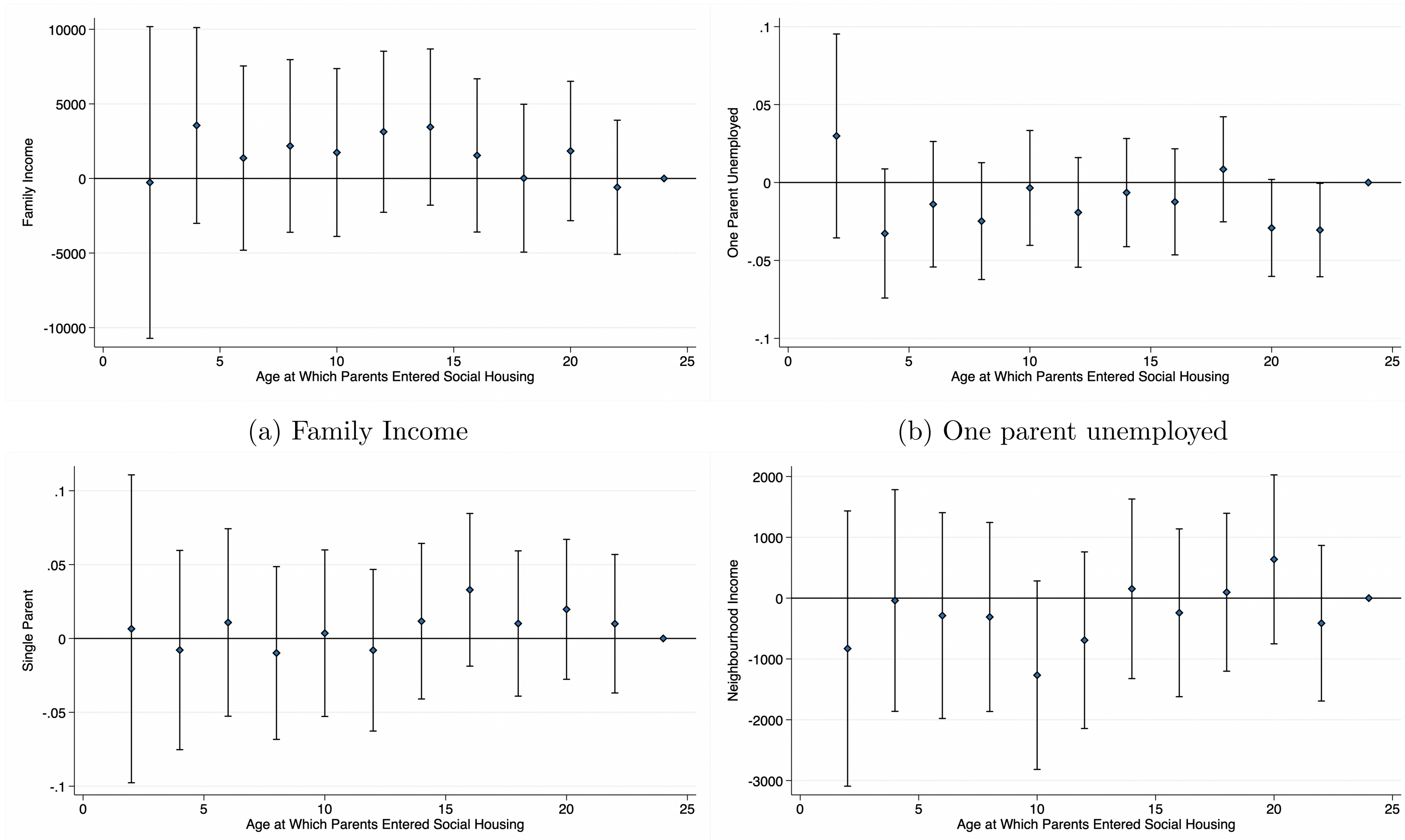
Table 4: Effects of Social Housing on Children, by parent responses

	Labour Earnings (1)	Any Post-Secondary (2)
Panel A. Employment at Baseline		
Exposure × Parent Worked	333.20***	0.005***
	(126.50)	(0.002)
Exposure × Parent Didn't work	185.70	0.003*
	(153)	(0.002)
Panel B. 3-Way Response		
Exposure × Stopped Working	527.20***	0.010***
	(155.30)	(0.002)
Exposure × Reduced Labour Supply	306.10**	0.006***
	(140.20)	(0.002)
Exposure × Increased Labour Supply	168.6	0.005**
	(137.00)	(0.002)
Panel C. 2-Way Response		
Exposure × Reduced Labour Supply	379.50***	0.007***
	(135.50)	(0.002)
Exposure × Increased Labour Supply	180	0.005***
	(136.90)	(0.002)

Notes: Each column of each panel is a different estimate of the heterogeneous exposure effect. Controls include cohort, sex, immigrant status, and origin Census Tract fixed effects. Each regression includes children whose parents moved into social housing between the ages of 0 and 18. Panels B and C are restricted to children whose parents worked before entering social housing. Dollar amounts are expressed in real terms (2021 CPI). Standard deviations clustered at the family level are reported in parentheses. Level of significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Appendix Figures

Figure A.1: Family Characteristics by Children's Age at Move



(a) Family Income

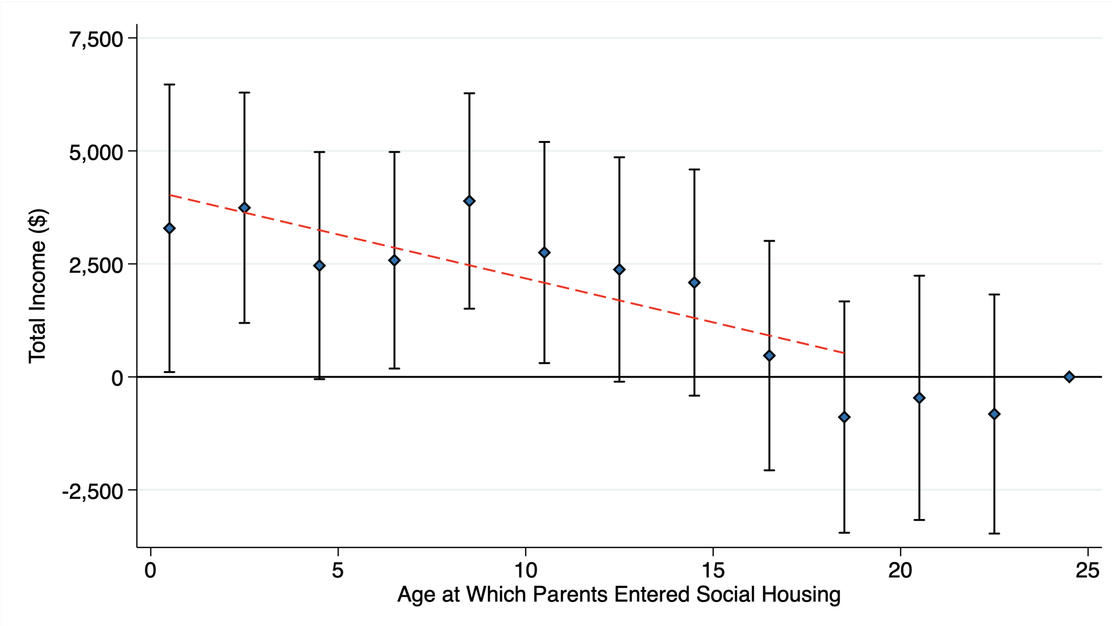
(b) One parent unemployed

(c) Single Parent Family

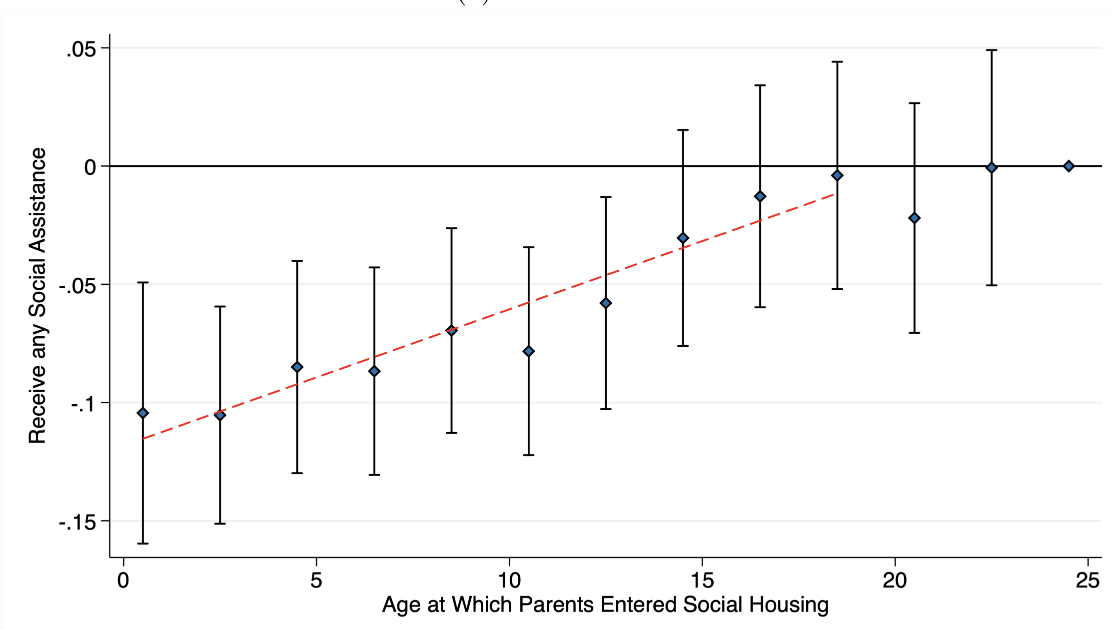
(d) Origin Census Tract Average Earnings

Notes: This figure shows family characteristics over children's age when entering social housing. Each panel plots the estimate for the coefficients δ_a from equation 1 for a specified pre-event family characteristic. Age at move is based on the age of the child when parents entered social housing, regardless of whether the child still lives with their parents. Each regression includes cohort and sex fixed effects, and panel (a), (c), and (d) also include origin Census Tract fixed effects. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the family level.

Figure A.2: The Effect of Social Housing on Kids, by Age of Entry



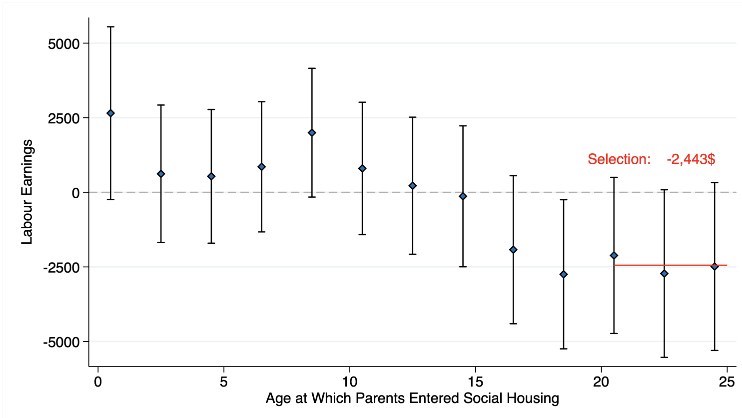
(a) Total Income



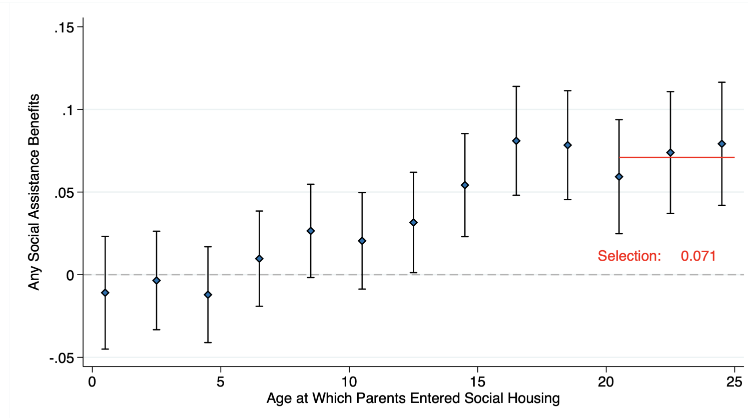
(b) Social Assistance Benefits

Notes: Each panel plots the estimate for the coefficients δ_a from equation 1 a specified outcome. Each coefficient is the effect of moving into social housing at age a, relative to having parents who moved into social housing at age 25. Age at entry is grouped in 2-year intervals and is based on the timing of when parents moved into social housing, regardless of whether the children still live with their parents. Each regression includes cohort, sex, and origin Census Tract fixed effects. The red dashed line is a linear fit for the coefficient points from 0 to 18. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the family level.

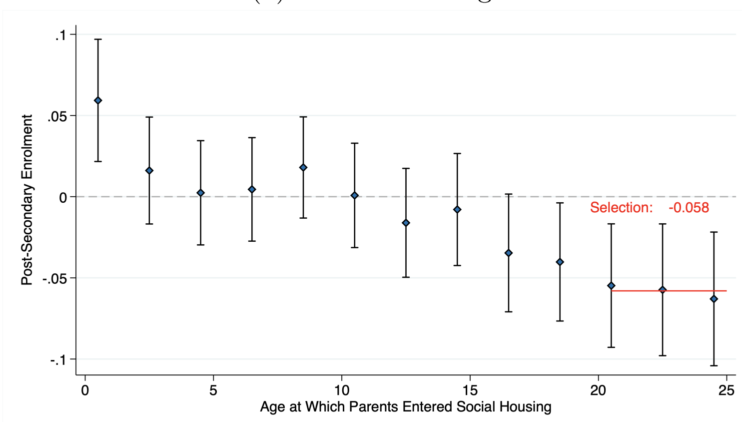
Figure A.3: Selection into Social Housing



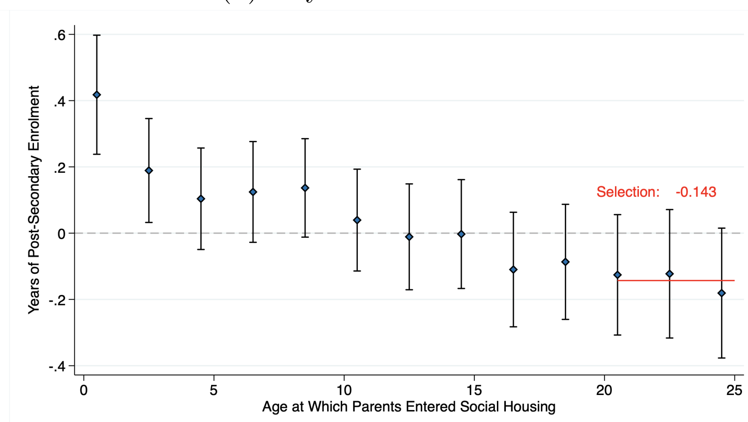
(a) Labour earnings



(b) Any Social Assistance



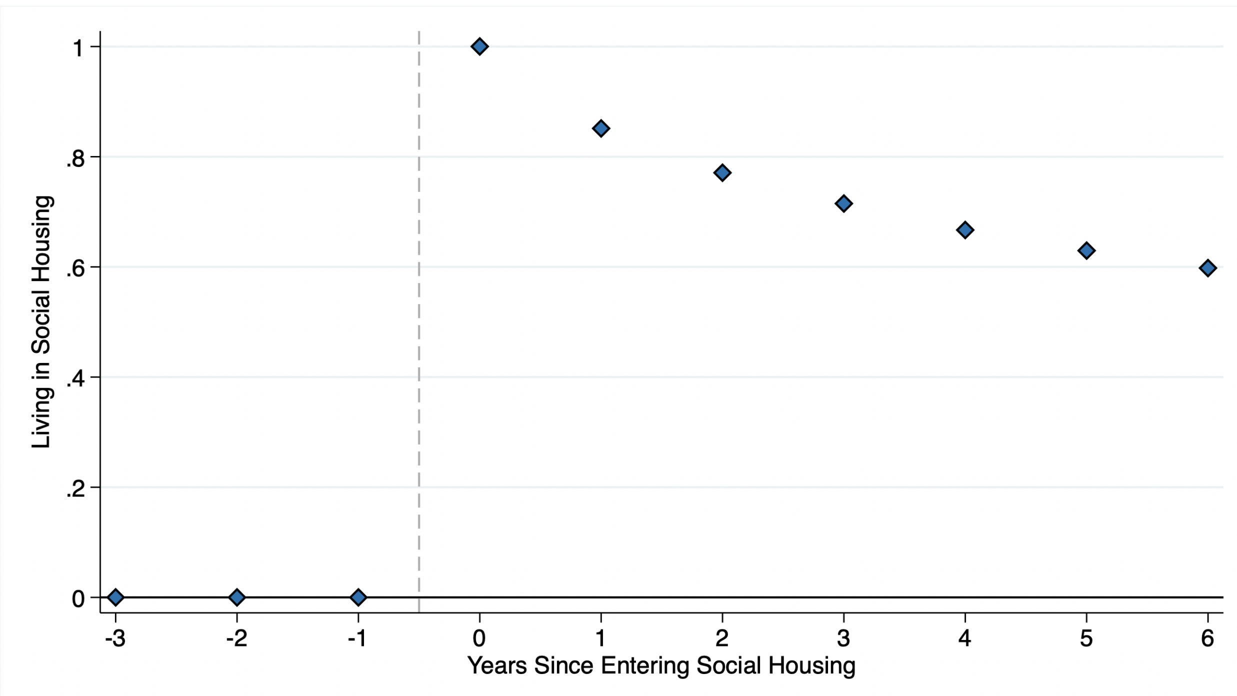
(c) Any Post-Secondary Enrolment



(d) Years of Post-Secondary Enrolment

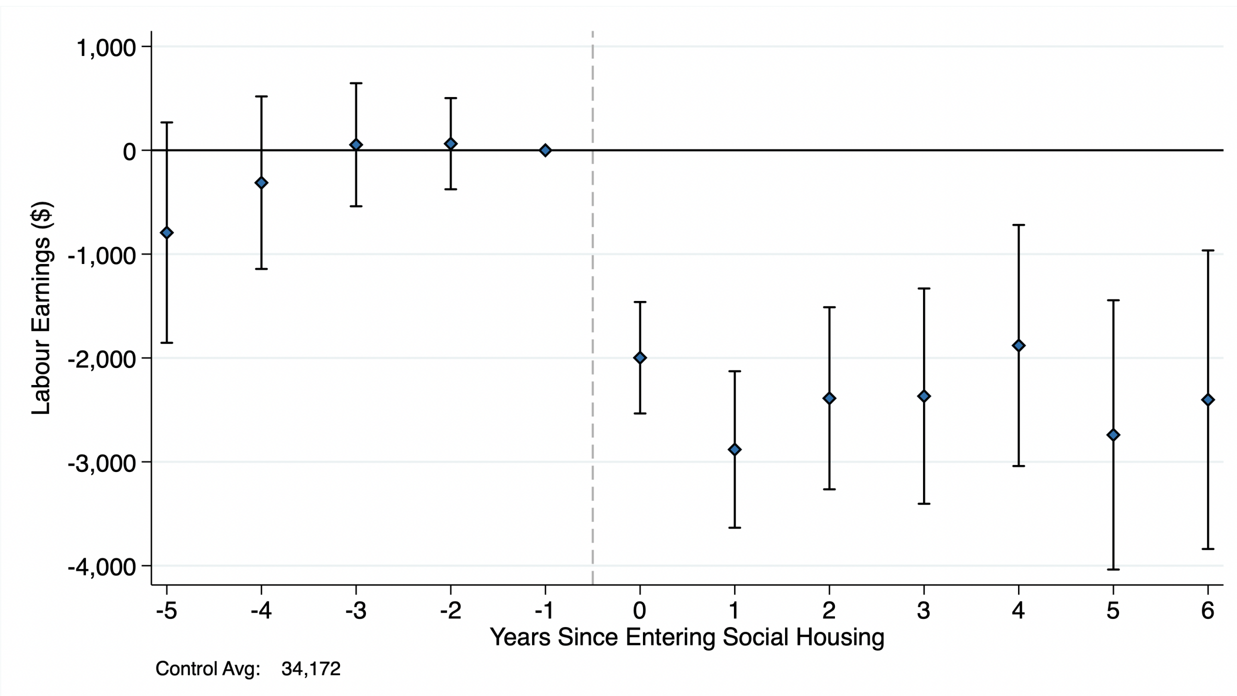
Notes: Each panel reports the coefficients δ_a from equation E.1 on a specific outcome. Treated children are those whose parents moved into social housing while they were aged 0-25. Control children are those of parents who are matched to treated parents in section 6.1. Labour earnings and social assistance are the average between the ages of 27 and 31. The selection measure is defined as the mean value of the δ_a estimates for $a > 20$; this represents a selection effect because parents' move that occur when the children are adults should not affect their outcomes. Each regression includes cohort, sex fixed effects, and origin Census Tract fixed effects. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the family level.

Figure A.4: Entry into Social Housing, Retention



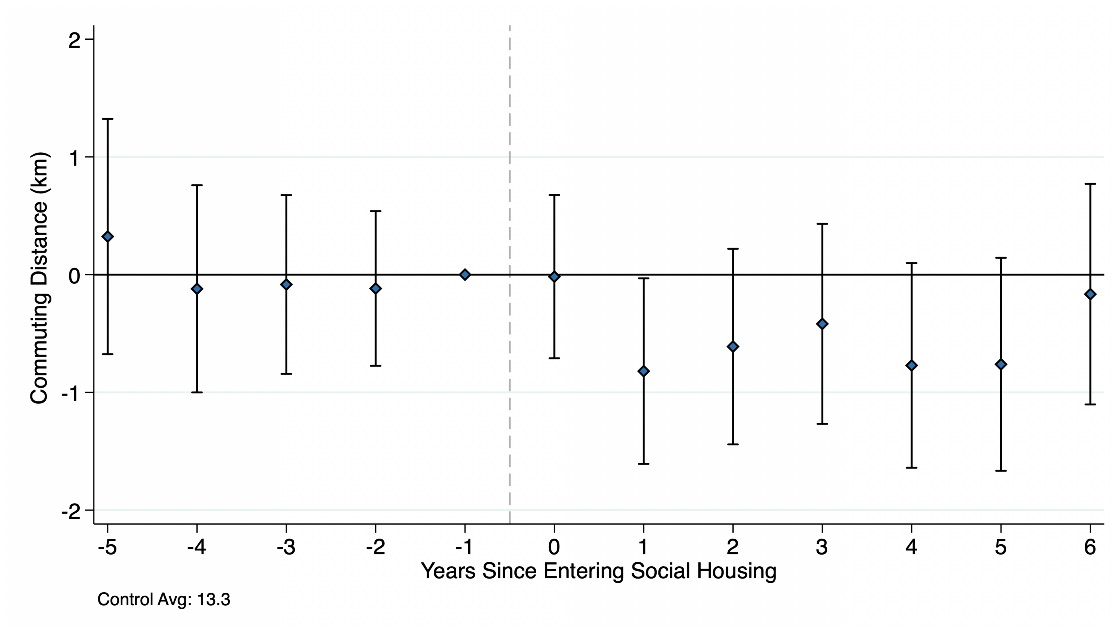
Notes: This figure reports the event study estimates from equation 3 on a dummy equal to one if the person live in social housing, and zero otherwise. Treated workers are those who moved from a non-social housing postal code to a social housing postal code. Controls individuals are matched using a caliper matching approach described in Section 6.1. The regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children and a single dummy.

Figure A.5: Entry into Social Housing, Labour earnings at initial employer

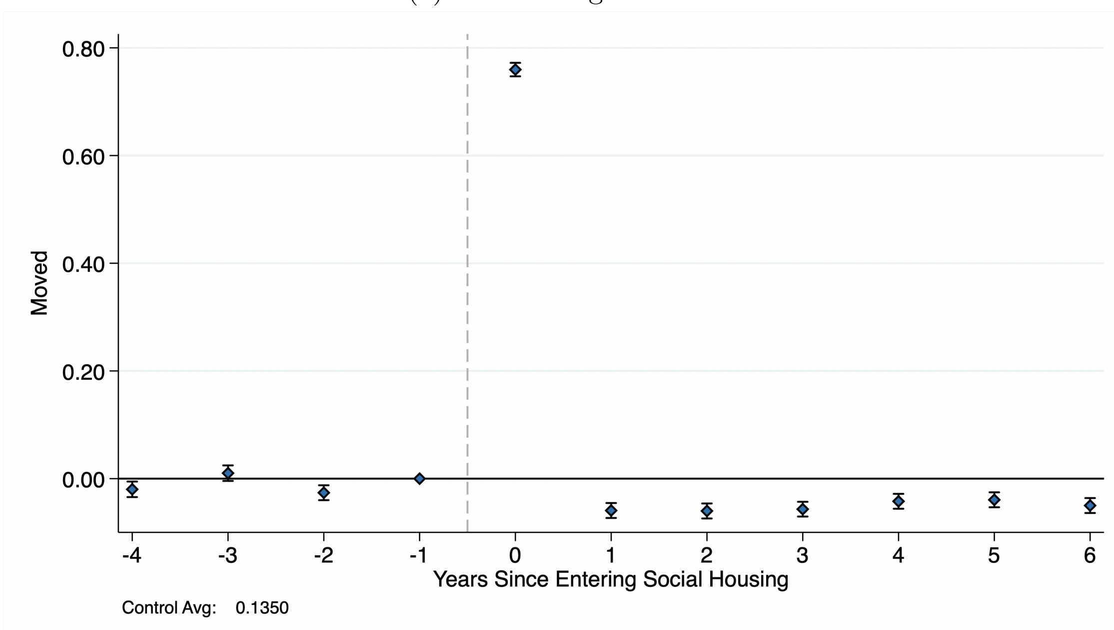


Notes: This figure reports the event study estimates from equation 3 on labour earnings for individuals who work at their $t-1$ employer. Treated workers are those who moved from a non-social housing postal code to a social housing postal code. Controls individuals are matched using a caliper matching approach described in Section 6.1. The regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children and a single dummy.

Figure A.6: The Effect of Social Housing on Adults, Mobility and Commuting



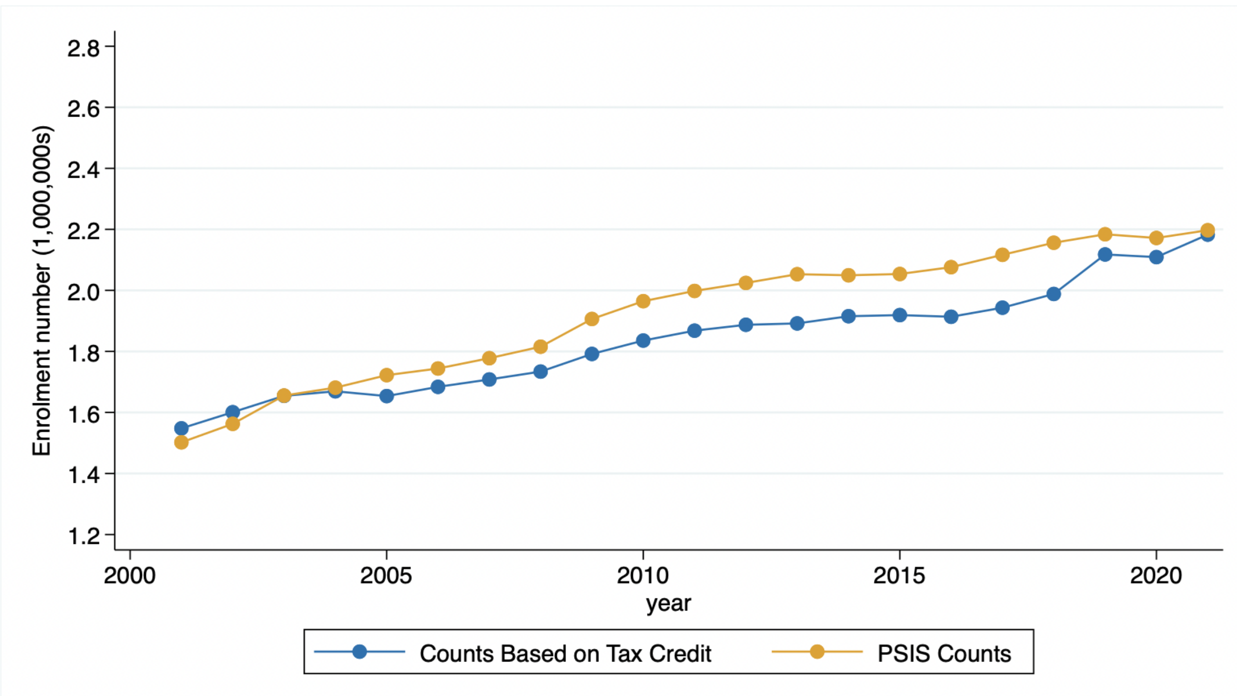
(a) Commuting Distance



(b) Probability of Moving

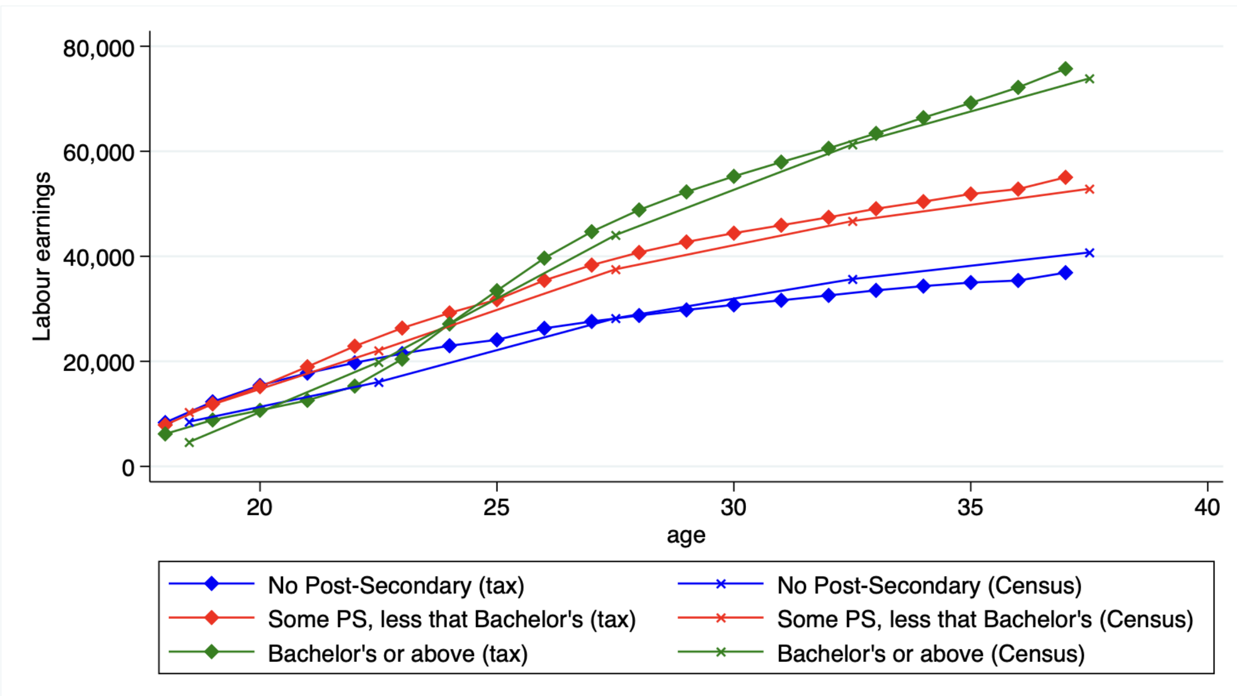
Notes: Each panel reports the event study estimates from equation 3 on a specified outcome. Treated workers are those who moved from a non-social housing postal code to a social housing postal code. Controls individuals are matched using a caliper matching approach described in Section 6.1. Each regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children and a single dummy. In panel (a), the sample is restricted to individuals who are working at a single establishment firm. In panel (b), moving is defined as changing the postal code of residence year-over-year. Each point reports 95% confidence intervals clustered at the individual level.

Figure A.7: Post-Secondary enrolment



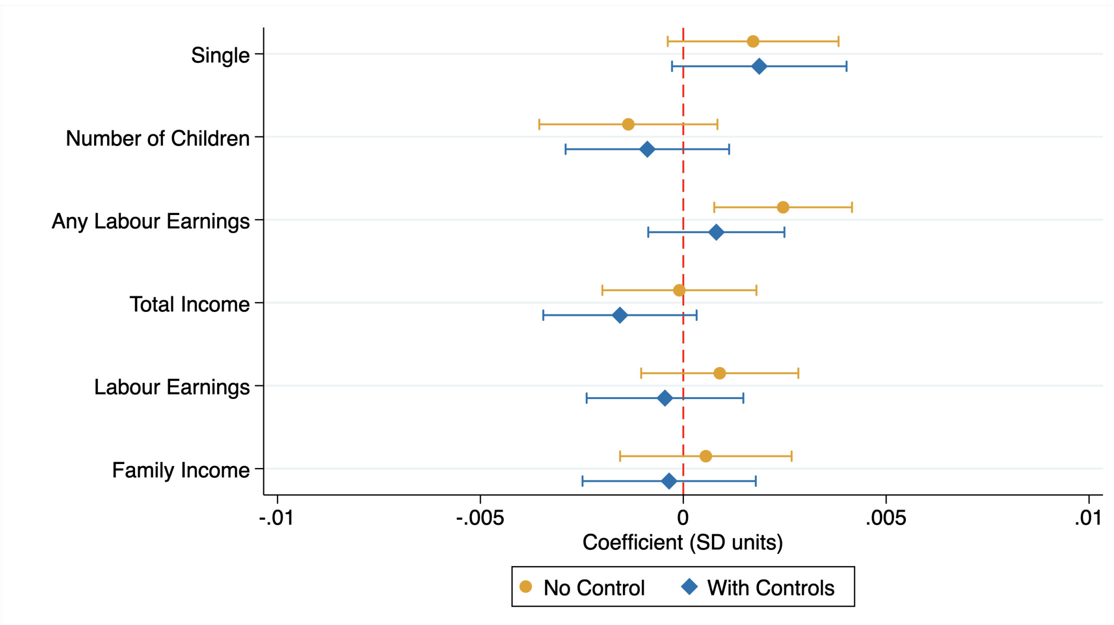
Notes: This figure shows the number of individuals aged 18 to 35 who claimed the post-secondary tuition tax credit (blue line) and PSIS counts (yellow line). Before 2009, the PSIS counts omitted individuals registered in programs related to pre-employment, apprenticeship, basic training, or skills upgrading, second language training, job readiness, or orientation programs. This leads to undercounts before 2009.

Figure A.8: Education Earnings Profiles



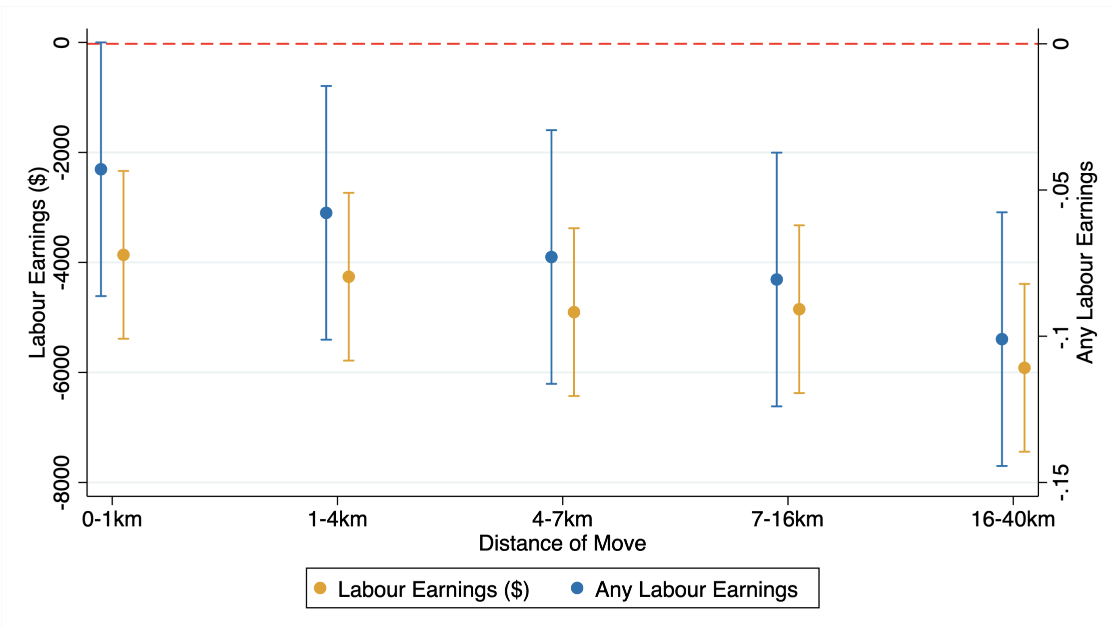
Notes: This figure shows the age earning profile for three education groups. Diamonds are from the tax files, and the groups are based on the education proxy. Xs are from the 2021 Census of Population Public Use Microdata File. Census education categories represent the highest degree completed, whereas the proxy only uses the number of years of enrolment to categorize the education level. Real 2021 dollars.

Figure A.9: Distance of Move: Balance Tests



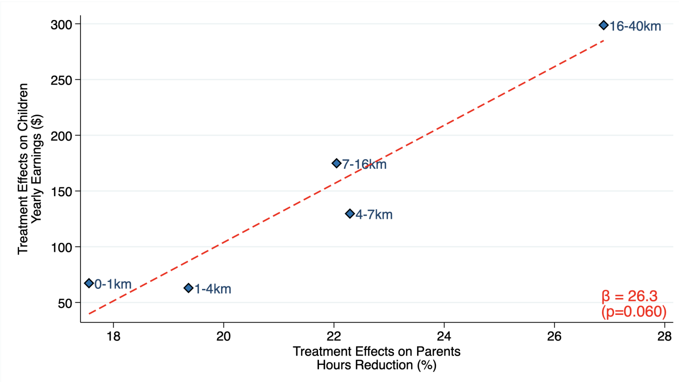
Notes: Each point is an estimate from a regression of a given outcome on the distance of the move. Each point reports 95% confidence intervals clustered at the origin census tract level. Only treated individuals are included. Yellow points represent estimates from univariate regressions, while blue points represent estimates from multivariate regressions that control for an age cubic polynomial, sex, year of birth, and city-year fixed effects.

Figure A.10: Parents' Response by Distance of Move

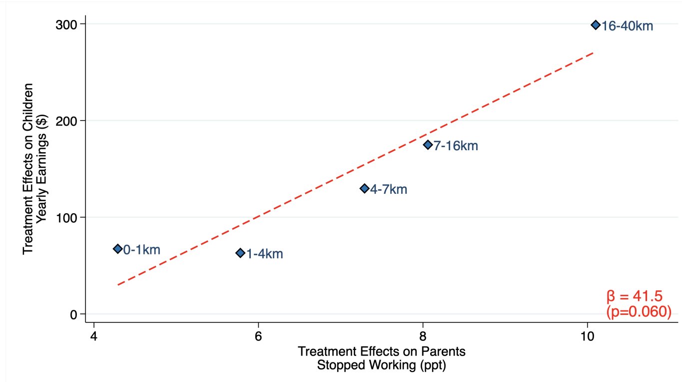


Notes: This Figure shows parents' labour market responses across quintiles of the distance of the move. Controls include an age cubic polynomial, the number of children, individual fixed effects, and city-year fixed effects. Each point reports 95% confidence intervals clustered at the individual level.

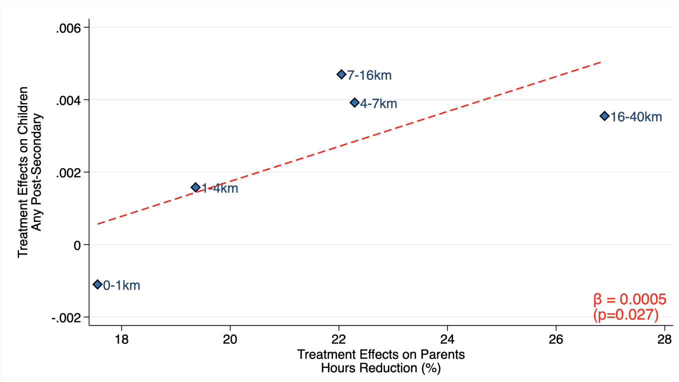
Figure A.11: Adults and Children Treatment over Distance of Move



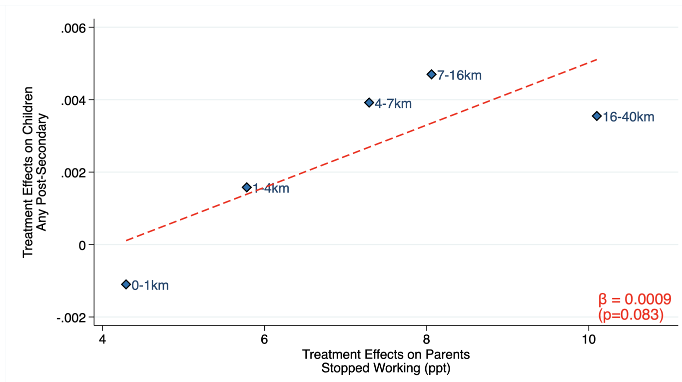
(a) Labour Earnings on
Parents' Hours Reduction



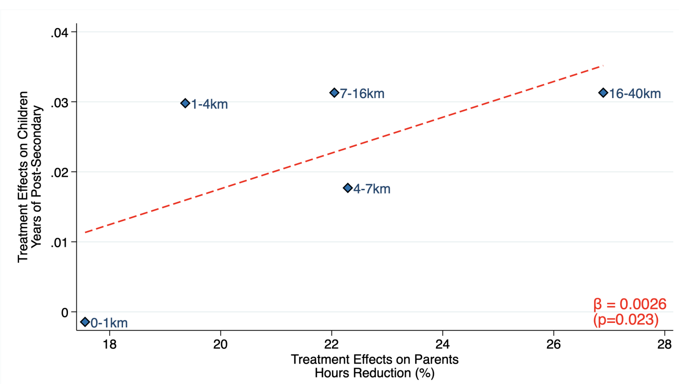
(b) Labour Earnings on
Parents' Participation Reduction



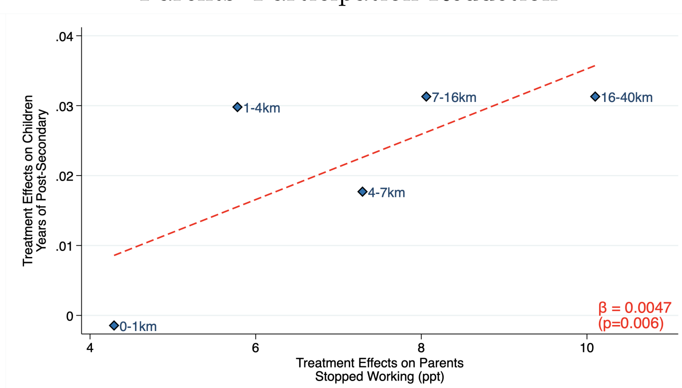
(c) Any Post-Secondary on
Parents' Hours Reduction



(d) Any Post-Secondary on
Parents' Participation Reduction



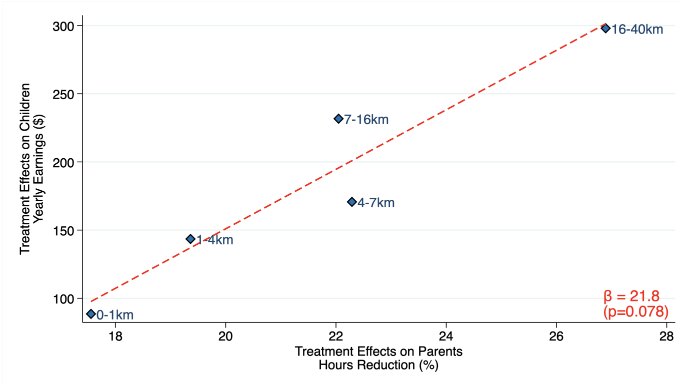
(e) Years of Post-Secondary on
Parents' Hours Reduction



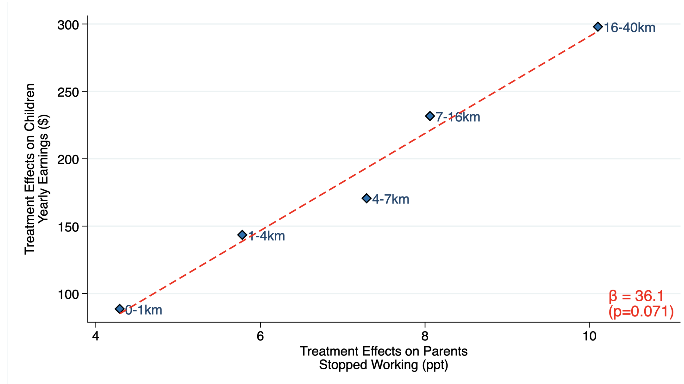
(f) Years of Post-Secondary on
Parents' Participation Reduction

Notes: Each panel reports the DiD coefficient on parents' outcome when entering social housing and the treatment effects on children's long-term outcomes for various combinations of outcomes. The red line is an OLS estimate of the relationship between treatment effects on adults and on children. The coefficient estimates and the p-value are printed in the bottom right corner. The p-values are obtained through a bootstrap test with 1,499 bootstrap samples.

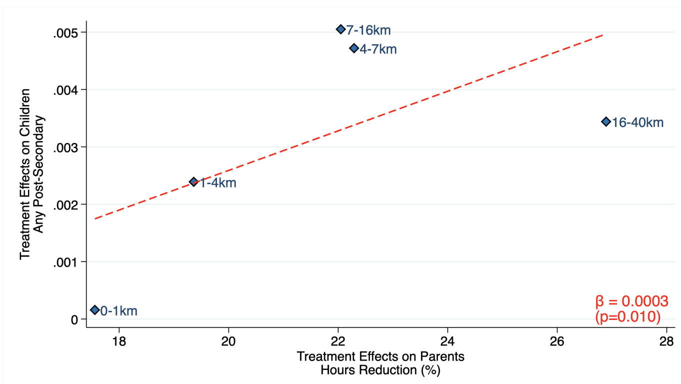
Figure A.12: Adults and Children Treatment over Distance of Move, CT FE



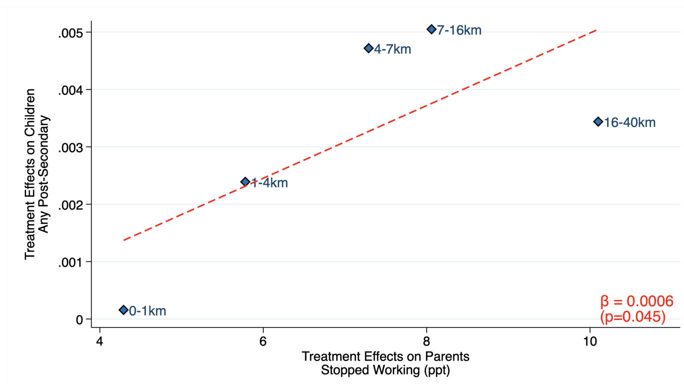
(a) Labour Earnings on Parents' Hours Reduction



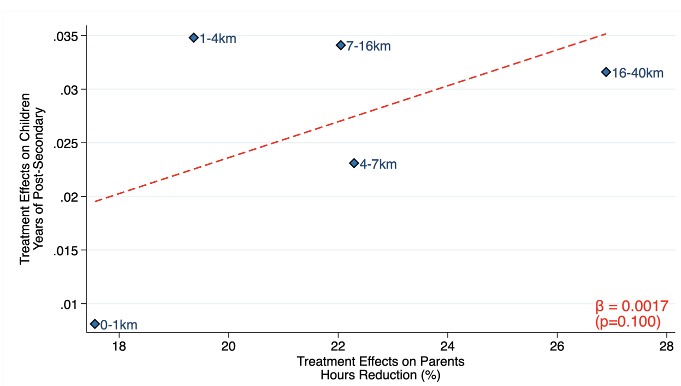
(b) Labour Earnings on Parents' Participation Reduction



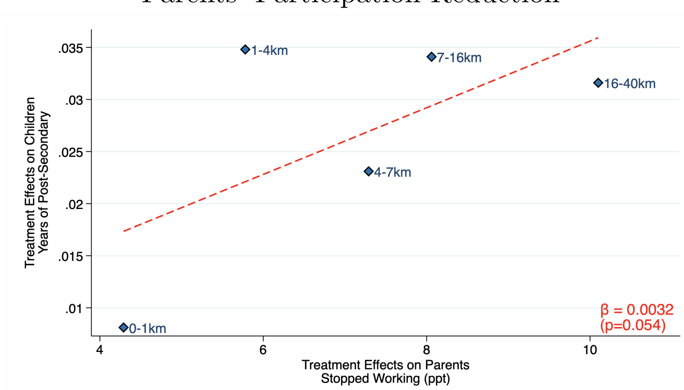
(c) Any Post-Secondary on Parents' Hours Reduction



(d) Any Post-Secondary on Parents' Participation Reduction



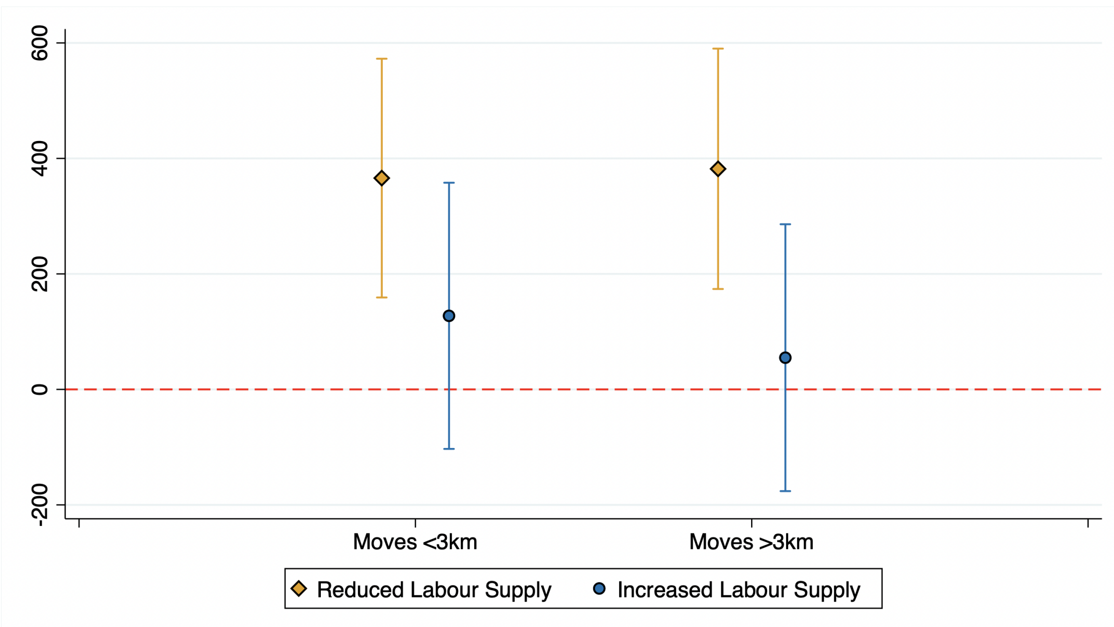
(e) Years of Post-Secondary on Parents' Hours Reduction



(f) Years of Post-Secondary on Parents' Participation Reduction

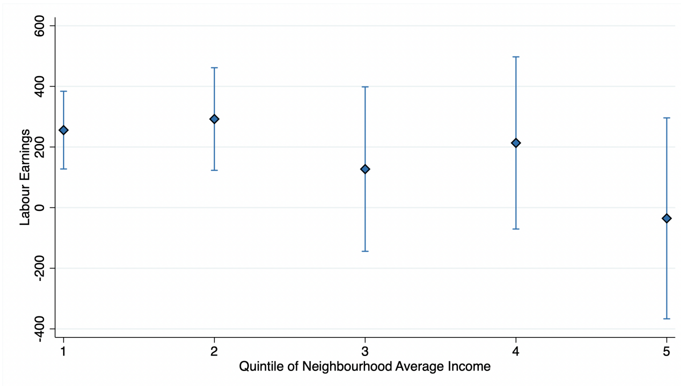
Notes: Each panel reports the DiD coefficient on parents' outcome when entering social housing and the treatment effects on children's long-term outcomes for various combinations of outcomes. The red line is an OLS estimate of the relationship between treatment effects on adults and on children. The regression includes origin and destination census tract fixed effects. The coefficient estimates and the p-value are printed in the bottom right corner. The p-values are obtained through a bootstrap test with 1,499 bootstrap samples.

Figure A.13: Children Treatment Effects, by Parents' Response and Distance of Moves

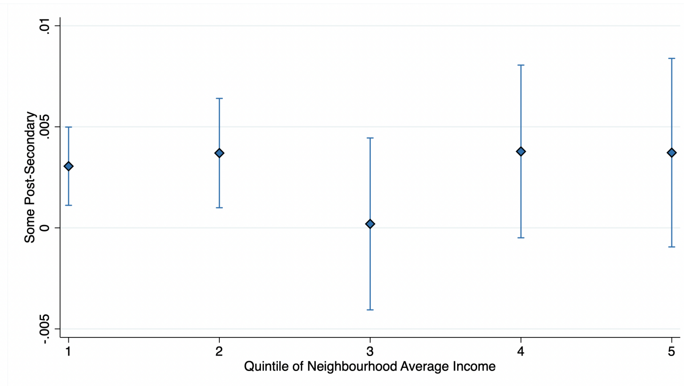


Notes: INCLUDE NOTES HERE.

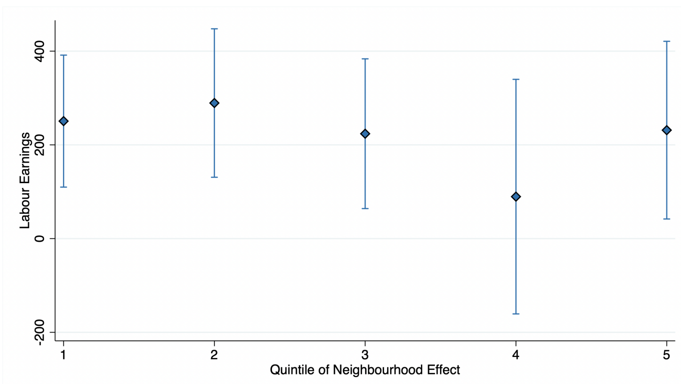
Figure A.14: Treatment Effects on Children by Neighbourhood Characteristics



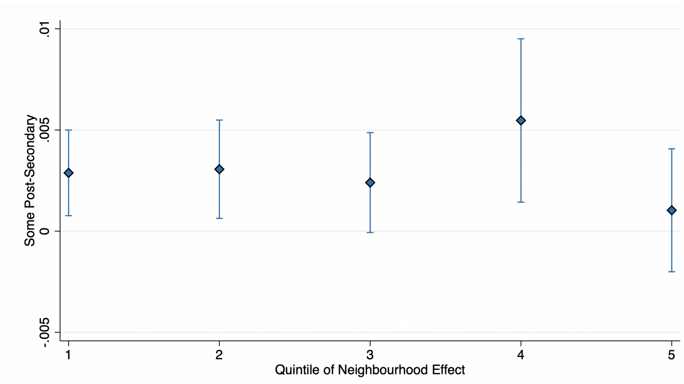
(a) Labour Earnings by CT Average Income



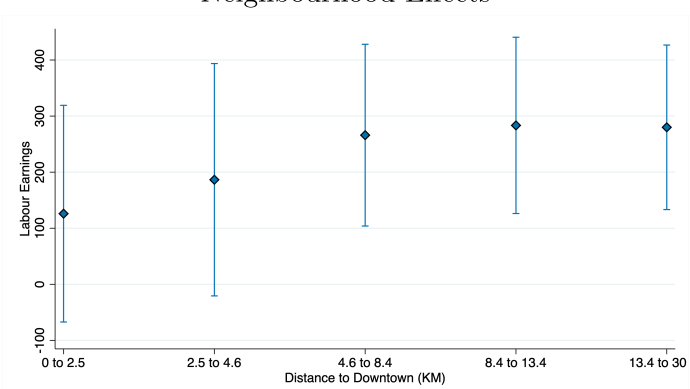
(b) Any Post-Secondary by CT Average Income



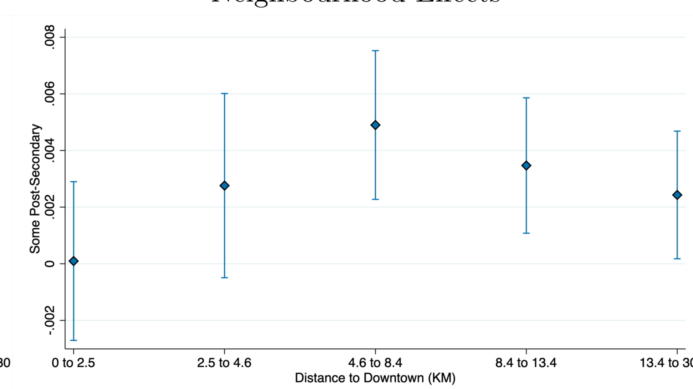
(c) Labour Earnings by Neighbourhood Effects



(d) Any Post-Secondary by Neighbourhood Effects



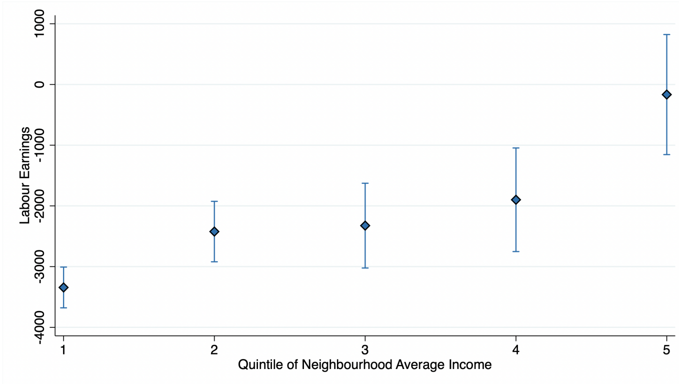
(e) Labour Earnings by Distance to Downtown



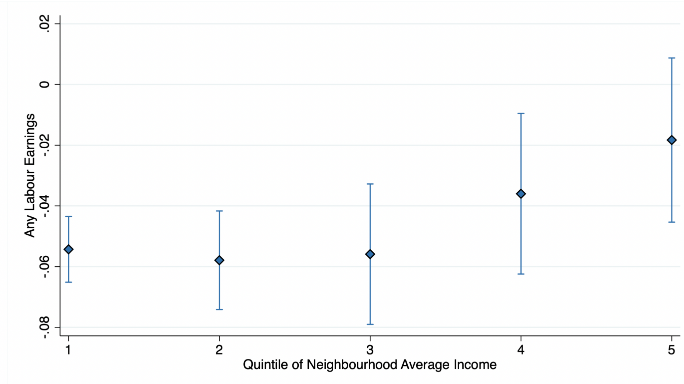
(f) Any Post-Secondary by Distance to Downtown

Notes: Each panel reports the slope coefficient on children's outcome by a given neighbourhood characteristic. Each coefficient is the effect of moving into social housing one year earlier. The estimates are from a regression analogous to equation 2 when the years of exposure are interacted with neighbourhood quintal dummies. Each regression includes year of birth, gender and origin neighbourhood.

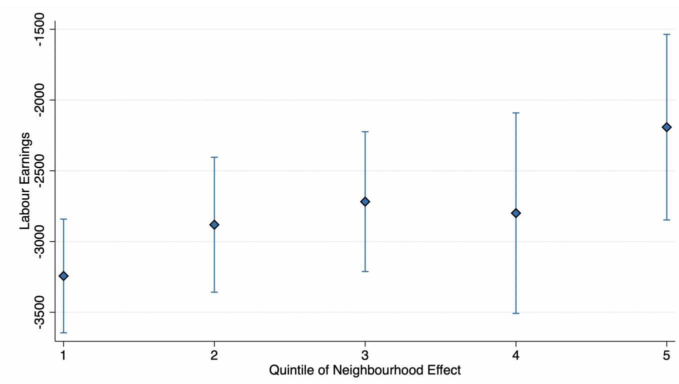
Figure A.15: Parents' Labour Response by Neighbourhood Characteristics



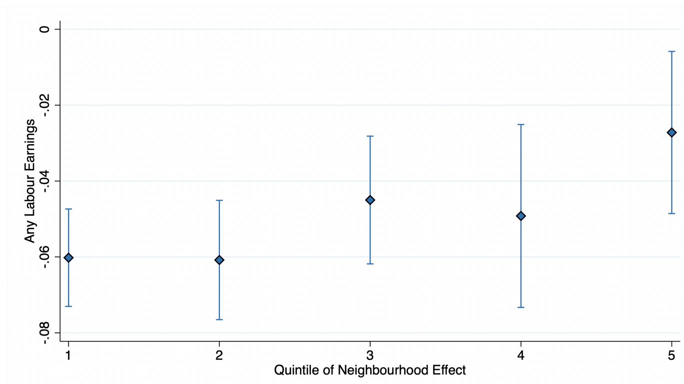
(a) Labour Earnings by
CT Average Income



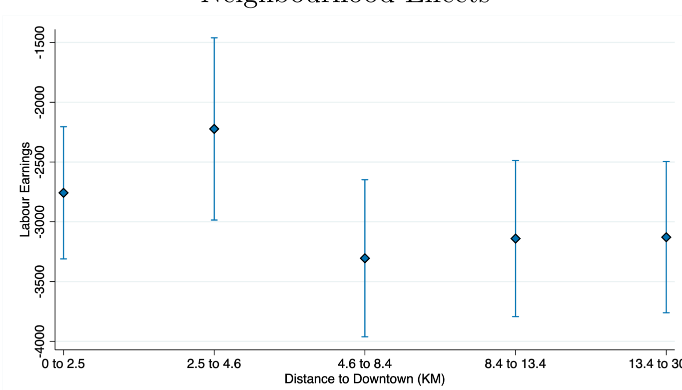
(b) Any Labour Earnings by
CT Average Income



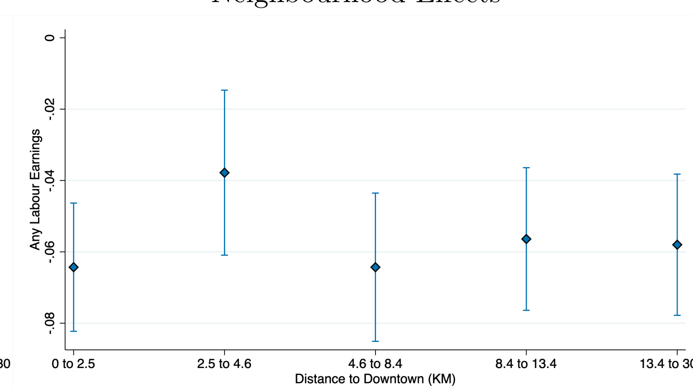
(c) Labour Earnings by
Neighbourhood Effects



(d) Any Labour Earnings by
Neighbourhood Effects



(e) Labour Earnings by
Distance to Downtown



(f) Any Labour Earnings by
Distance to Downtown

Notes: Each panel reports the DiD coefficient on parents' outcome when entering social housing by a given neighbourhood characteristic.

B Appendix Tables

Table B.1: Effects of Social Housing on Children, Heterogeneity

	Labour Earnings	Total Income	Any Post-Secondary	Years of Post-Secondary	Any Social Assistance Ben.	Amount Social Assistance Ben.
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. By Gender						
Exposure	112.50	81.38	0.003 ***	0.015***	-0.006***	-72.76***
	(80.2)	(75.6)	(0.001)	(0.006)	(0.001)	(19.0)
Female	-381.20	-229.10***	0.194***	1.132	0.033**	570.0***
	(992.1)	(933.5)	(0.013)	(0.069)	(0.016)	(204.0)
Exposure × Female	314.90***	298.20***	0.000	0.020***	-0.000	0.639
	(97.6)	(91.9)	(0.001)	(0.007)	(0.002)	(20.7)
N	14,520	14,520	17,765	17,765	14,520	14,520
Panel B. By Immigrant Status						
Exposure	390.70***	285.30***	0.004**	0.009	-0.010***	-153.90***
	(117.0)	(108.2)	(0.002)	(0.008)	(0.002)	(30.2)
Immigrant	4,888.00***	4850.80***	0.225***	1.27***	0.101***	-1,006.6***
	(1255.50)	(1154.90)	(0.018)	(0.087)	(0.022)	(299.1)
Exposure × Immigrant	158.70	77.86	0.001	0.019**	0.004*	98.80***
	(125.0)	(116.0)	(0.002)	(0.009)	(0.002)	(31.2)
N	14,520	14,520	17,765	17,765	14,520	14,520

Notes: Each column and row is a different estimate of δ in equation 2. Column (1) reports coefficients from univariate regressions, column (2) includes cohort and sex fixed effects, and column (3) additionally includes origin Census Tract fixed effects. Each regression includes children whose parents moved into social housing between the ages of 0 and 18. Dollar amounts are expressed in real terms (2021 CPI). Standard deviations clustered at the family level are reported in parentheses. Level of significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.2: Mincer Equations

	Labour earnings		
	(1)	(2)	(3)
Any Post-Secondary	0.395***		
	(0.0004)		
Years of Post-Secondary		0.0797***	
		(0.0001)	
Less than Bachelor's			0.267***
			(0.0005)
Bachelor's or above			0.494***
			(0.0004)
N	30,796,350	30,796,350	30,796,350

Notes: Each column is a different specification of a mincer equation. Each regression includes a cubic polynomial in age, sex, year, and province FEs. Included cohorts are those born between 1984 and 1995, and their earnings are measured at age 25 onward. In columns 1 and 3, the omitted category is no post-secondary education. A bachelor's equivalent is defined as 3 years in Québec and 4 years in the rest of Canada. Years of Post-Secondary is defined as the number of years an individual claimed the tuition tax credit while aged 18 to 25. Each regression is person-year weighted, and robust standard errors are in parentheses. Level of significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

C Data Appendix

C.1 Inferring parents and location during childhood

I link the deidentified Social Insurance Numbers (SINs) of parents and their children using the T1 Family File (T1FF). T1 forms are the main annual tax returns filed by individuals in Canada. While the T1 is filled out individually, identifying information about a spouse or common-law partner has to be provided. The T1FF incorporates that additional information on spouses and common-law partners, combined with Canada Child Tax Benefit data, to construct a family identifier. Through the analysis, I refer to "parents and children", but it must be noted that no biological like can be established from the data. Hence, parents should be considered the household heads.

I assign a primary and a secondary parent to each child. The primary and secondary parents are the first and second individuals identified as parents in the data, respectively. If the two parents are identified simultaneously (the modal case), the mother is assigned as the first parent, and the father is assigned as the second parent.

For each year, I assign a postal code based on the child's most likely place of residence. In a given year, I assign the postal code as follows: (*i*) if the child filled a T1, I assign the child's postal code; (*ii*) if the child did not fill a T1, I assign the primary parent's postal code; (*iii*) if nor the child nor the primary parent filled a T1, I assign the secondary parent's postal code.

For some cohorts, I only start observing children when they are aged 7 (those born in 1984 are 7-8 in 1992). For those born before 1992, I assume that they didn't move before the data starts. That is, I assign the neighbourhood where they lived in 1992 to the neighbourhood for all the years before.

C.2 Inferring paid rent

Longitudinal data on rent paid at the individual level are hard to find, and there is no information on housing costs in tax files. To evaluate the impact of social housing on housing costs, I have to infer the rent paid. I do this in two steps. First, for social housing tenants, I know exactly how much their respective housing corporations charge them: 25% of household total earnings in Montréal and 30% of household total earnings in Toronto. Second, for individuals not living in social housing, I base my imputation on their census tract of residence. I use census profiles for census years 2001 to 2021 and linearly interpolate between census years to get a yearly census tract-level median rent[11]. I assign this median rent to people not living in social housing.

[11] In Canada, censuses occur every 5 years.

C.3 Historical neighbourhood

Census geographic boundaries change over time. The CEEDD database includes location information based on the 2021 census definition. When computing other measures of neighbourhood quality, I use data from the 2001 Census, which I transpose to the 2021 geographic definition using the Canadian Longitudinal Tract Database (Allen and Taylor 2018b).

Allen and Taylor (2018) use a combination of map-matching techniques, dasymetric overlays, and population-weighted areal interpolation to create a set of cross-walk tables that link Census tract identifiers across years. This enables researchers to study more constant geographical units across long periods of time.

D A Proxy for Post-Secondary Education

The primary limitation of using tax records to study human capital accumulation is that they lack direct information on education. However, Canadian post-secondary students can claim tax deductions for tuition paid at post-secondary institutions. Claiming the tuition tax credit is costless, but it does require knowledge of the program. Students declare the amount of tuition paid in a given year on line 32300 of Schedule 11 in the T1 file. Then, 15% of the tuition they paid can be deducted from the income tax they owe. If they don't use the full amount, they can carry it forward to future years or transfer it to a spouse or other eligible family member (typically their parents). Even if they wish to transfer the amounts to a designated individual or carry forward their deductions, individuals who attend post-secondary institutions still must complete a Schedule 11 form.

Frenette (2021) documents the post-secondary tuition credit claim rates among post-secondary students, combining both the T1 tax files and the post-secondary Student Information System (PSIS). The PSIS contains administrative data on enrolment in post-secondary education. Overall, among 19-year-old post-secondary students who filed their taxes in

2017, about 9 in 10 claimed the credits. Figure XXX presents the number of individuals aged 18-35 who claim the tuition tax deduction over the years compared to the number of individuals enrolled in post-secondary education in the PSIS data[12]. Before 2009, the PSIS count excluded students enrolled in programs related to pre-employment, apprenticeship, basic training, or skills upgrading, second language training, job readiness, or orientation programs, which are potentially captured by my proxy. From 2009 to 2018, the coverage of my education proxy has been stable, undercounting the official enrolment counts by 5 to 10%. In 2019, the Federal government introduced the *Canada Training Benefit*, which can be used to cover tuition for post-secondary training for individuals aged 25 to 55. This new program was highly publicized and might have led younger individuals to learn about the other tax credit they are eligible for.

I explore the return to education based on my post-secondary education measure. Figure XXX present the age-earnings profile for three groups: (i) people with zero year of post-secondary education, (ii) people with less than a bachelors degree, (iii) people with the equivalent to a bachelors degree[13]. I benchmark the earnings progression of those two groups, using data from the 2021 Canadian Census public use microdata files (PUMF). My proxy reproduces patterns from the Census data. This highlights the importance of measuring tax credit claims over several years.

Finally, I estimate three Mincer equations. Different specifications are presented in Table B.2. The estimates are in the same order of magnitude as previous research on the return to education in Canada. Specifically, column 3 of Table B.2 shows a premia of 26.7 log points for some PS and 49.4 log points for a bachelor's degree equivalent. Boudarbat et al. (2010) finds that those premia are about 21 log point and 45 log points respectively using 2005 data (See figure 3). Note that those are relative to high-school completion, whereas my numbers are relative to no post-secondary education (regardless of high-school completion).

[12]Statcan table 37-10-0018-01

[13]In the taxfiles, I define someone as having the equivalent of a bachelor degree if they claimed the tax credit for 4 years (3 years in Québec) or more.

Although the number of years a person claims the tax credit resembles the number of years of education, there's no formal information on the level of schooling (college/university, undergraduate/graduate), the field of study, or whether the program was completed. Hence, in the analysis, I limit the interpretation of my education proxy as *some post-secondary schooling* and *number of years enrolled in post-secondary education.*

E Selection into Social Housing

The identification assumption underlying the analysis is that the selection effect does not vary with the child's age at move a. Nevertheless, it is interesting to measure selection into social housing to assess how generalizable the results are.

To quantify selection, I estimate a modified version of equation 1 where I include all children of matched parents in section 6.1. I assign their age at move based on the year their matched treated pair moved into social housing. In this version of the exposure estimation, I interact the treatment dummy with the exposure dummies.

$$y_i = \beta + \sum_{a=0}^{24} \delta_a \times \mathbf{1}(a_i = a) \times T_i + \mathbf{X_i}\Gamma + \epsilon_i \tag{E.1}$$

As before, y_i is an adulthood outcome of the child, $\mathbf{1}(a_i = a)$ is an indicator equal one if i's parents moved into social housing at age a (or placebo age at move for control children), and $\mathbf{X_i}$ is a vector of control (e.g. year of birth, gender, origin neighbourhood). T_i is a treatment dummy that equals one for children whose parents moved into social housing and zero for children of parents who did not actually move into social housing. δ_a are the coefficients of interest. Note that here, no coefficient is normalized to zero.

Figure A.3 exhibits a general shape that is similar to that seen in Figure 1, but without the normalization to zero. For each panel, I include a selection level based on the average of the coefficients for moves (or placebo moves) that occurred after age 20. Children whose parents enter social housing are negatively selected. In the absence of treatment effect, they would earn 2,443 $ less in labour earnings, would be 7.1 ppt more likely to be a social assistance recipient, 5.8 ppt less likely to attend post-secondary, and would have 0.143 less years of post-secondary education.

The fact that the coefficients flatten for moves occurring after the children are aged 20 suggests that selection is fairly constant across age at move.

F Two-way Fixed-Effect model

To retrieve firm pay premia, I estimate a two-way fixed effects model *à la* AKM (Abowd et al. 1999).

When estimating the AKM, I trim the bottom end of the earnings distribution to remove observations with low hours of work, which are not observable in the T4 files. Following Dostie et al. 2023 and Beauregard et al. 2025, I use a "full-time-at-minimum-wage" threshold of about \$19,000 in dollars of 2021[14]. I also restrict the AKM estimation to "prime age" workers, aged 25-59, to reduce the variation in hours linked to part-time work while in school and pre-retirement decline in labour market attachment. I use the full 2001 to 2023 sample to fit the model. Crucially, all workers that I ever observed living in social housing in the data are excluded from the estimation sample.

I estimate the following equation, where a worker effect and a firm effect explain an individual's log earnings (y_{it}):

$$y_{it} = \alpha_i + \psi_{J(i,t)} + \beta X_{it} + \epsilon_{it} \tag{F.1}$$

[14]In Canada, the minimum wage is set at the provincial level. The lowest minimum hourly wage in 2021 was 11.45\$ in Saskatchewan. Based on a 35-hour week, over 48 weeks, the minimum yearly earnings of a full-time worker was 19,236\$.

Where α_i captures the portable component of productivity of worker i; $J(i,t)$ returns the identity of the firm hiring worker i in period t; $\Psi_{J(i,t)}$ captures the earnings premium (or discount) paid by employer j to all its employees; X_{it} represents a set of observable characteristics (e.g. year fixed effects, age effects). The error term ϵ_{it} captures drift in worker productivity, random match effects, and measurement error.

The key identifying assumption of equation (F.1) is that workers do not select their employer based on the unobserved component ϵ_{it}, i.e., moves across employers occur because of "exogenous mobility" factors. Sorting based on the worker effects α_i, firm effects ψ_j, and observables X_{it} does not violate exogenous mobility. Endogenous mobility occurs when workers select their employer based on an idiosyncratic productivity component of the job (i.e., a "match effect") or due to changes in ϵ_{it}, driven, for instance, by employer learning. An examination of the presence of endogenous mobility — using the usual event study of job transitions popularized by (Card et al. 2013) — suggests that it does not constitute a primary concern in my setting.[15]

G Neighbourhood Effects

I estimate the causal effect of spending one year in a given neighbourhood by leveraging the variations in children's exposure time to different places during childhood, arising from families moving while children are at different ages. This strategy does not require the moving decision to be random, but rather that the timing at which the moves occur is orthogonal to a child's potential outcome among the families with the same sequences of location choices. As in Aloni and Avivi (2024), I include children who moved twice during childhood, diverging from the usual strategy of using one-time movers popularized by Chetty

[15]Other researchers reached the same conclusion for the United States (Song et al. 2019); Germany (Card et al. 2013); Italy (Casarico and Lattanzio 2024); Portugal (Card et al. 2016); Canada (using a different sample) (Dostie et al. 2023).

and Hendren (2018).

$$y_{it} = \sum_{n=1}^{N} \eta_n \times e_{in} + \beta \mathbf{X_{it}} + \xi_{od_1d_2} + \epsilon_{it} \tag{G.1}$$

Where η_n is the causal effect on y of spending one additional year in the neighbourhood n, X_{it} is a set of both time-invariant controls (e.g. gender, parental earnings when age 15-19) and time-variant controls (e.g. cohort and year FEs), $\xi_{od_1d_2}$ is a fixed effect for origin-destination sequences, and e_{in} is the number of years an individual i spent in the neighbourhood n, and is defined as:

$$e_{in} = \begin{cases} m_1, & \text{if } n = o(i) \\ m_2 - m_1, & \text{if } n = d_1(i) \\ 18 - m_2, & \text{if } n = d_2(i) \\ 0, & \text{otherwise.} \end{cases} \tag{G.2}$$

Where m_{i1} and m_{i2} are the age at which the child moved the first and second time, respectively, $o(i)$ is the original neighbourhood, $d_1(i)$ is the second neighbourhood, and $d_2(i)$ is the third neighbourhood in which the lived while aged 0 to 18. For one-time movers, $m_2 = 18$.

I estimate equation G.1 using all children who have never lived in social housing in my sample. For each neighbourhood n, I retrieve the neighbourhood effects η_n. Each of those η_n can be interpreted as the causal effect of place on labour earnings; a high η_n implies that a child who spends a year more in place n will earn more than the children who spent that year in the reference neighbourhood.

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