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

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Abstract

I assess 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-cost income. A simple time-allocation model—where social housing receipt both relaxes the budget constraint and insures parents against earnings uncertainty—rationalizes these large labour supply responses. I show that parental time is a key causal factor behind improvements in child outcomes by isolating exogenous labour supply responses using features of the program. 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
JEL codes: H53, I38, J08, J22, R28

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Housing assistance policies are a primary tool for addressing economic precarity and breaking the intergenerational transmission of poverty. Nearly all advanced economies operate some form of rental assistance programs targeted at low-income families, yet we still lack clarity on how these policies translate into gains for children. Research has emphasized two main channels through which social housing might affect children: an income effect via the housing endowment, and changes in the neighbourhood environment (Chyn 2018; Jacob 2004; Kling et al. 2007; Ludwig et al. 2013). At the same time, debates over public housing have raised persistent concerns: housing subsidies depress adult labour supply (Jacob and Ludwig 2012; Van Dijk 2019). A full welfare assessment of social housing, therefore, requires careful attention to the effects on both children and their parents, which prior work has not provided.

In this paper, I provide the first single-setting evaluation of the effect of low-rent social housing on parents and children. Using parent–child–linked administrative tax records for all filers in Montréal and Toronto from 1997 to 2021, I track parental earnings and employment around program entry and follow children into adulthood to measure earnings, post-secondary enrolment, and social-assistance receipt. Effects on parents are estimated using a matched event study comparing entrants to observationally similar never-treated households. Long-term impacts on children are identified with an exposure design that leverages quasi-random timing of entry, effectively comparing otherwise similar children who enter earlier versus later.

Entry into social housing significantly reduces parental labour-market participation and earnings. In contrast, earlier entry—and thus longer exposure—raises children's adult earnings, increases post-secondary enrolment, and lowers social-assistance receipt. Together, the results suggest that the reduction in parental labour supply is a potential, overlooked mechanism linking social housing to children's long-run outcomes.

The remainder of the paper examines the mechanisms that may drive the observed gains in children's outcomes. The mechanisms that have been considered in the literature fail to explain the observed pattern of results. First, changes in net-of-housing disposable resources are small—the rent relief is largely offset by lower earnings—making a pure income effect an unlikely explanation for the substantial long-term gain for children. Second, moves at entry do not improve measured neighbourhood quality enough to plausibly explain the results. Instead, the previously underinvestigated channel of parental time investments is most consistent with my results. Specifically, program-induced reductions in parental labour supply are sizable and, by reallocating time toward the home, can meaningfully increase parental time with children. To isolate this mechanism from competing explanations, I exploit an institutional feature that generates quasi-exogenous variation in labour-supply reductions, enabling me to distinguish between gains attributable to parental time from those due to income or neighbourhood changes.

The setting for this paper is the rent-geared-to-income (RGI) programs in Toronto and Montréal, Canada. Administered by municipal housing authorities, these programs require tenants to pay a fixed share of gross income toward rent—30% in Toronto and 25% in Montréal—with the government covering the remainder, bringing rents well below market levels. Together, they house more than 80,000 low-income households across the two cities.

I begin the paper by studying the impact of social housing on adults. My analysis exploits the timing at which families moved into social housing in a matched event study framework, pairing each treated individual with a never-treated counterpart of the same sex, age, and family composition, and with similar income over the prior three years. Adults entering social housing experience a sudden drop in labour market earnings of about \$2,400 (\$1,720 USD, or 20%) annually. Those effects are long-lasting. This decline stems from both a six percentage point lower probability of employment and a 7.3% reduction in hours for those who remain in the same job. 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.

On average, recipients' rent burden falls by \$3,500, naive interpretation would suggest a marginal propensity to earn of unearned income of roughly -0.68—a substantially larger behavioural response than documented for other transfers. This highlights that RGI policies have an impact on recipients in ways that extend beyond simple cash transfers. Beyond the standard income effect, rent-geared-to-income housing depresses labour supply through two additional forces. First, a substitution effect: by taxing earnings at the margin—each extra dollar of income raises rent by \$0.25 or \$0.30—the program lowers the effective after-tax wage and thus the opportunity cost of leisure. Second, an insurance effect: because rent adjusts downward when earnings fall, tenants are partially insulated from adverse shocks, reducing the return to maintaining higher hours. A simple labour–leisure model with stochastic realized hours formalizes these forces: agents choose intended hours, actual hours are noisy around that choice, and the rent schedule both flattens the budget set and cushions income risk. I quantify the relative contributions of each channel to the overall labour-supply response and find that the insurance channel accounts for roughly one-half of the observed decline, while the pure income and pure substitution channels explain 28.4% and 21.3%, respectively.

Next, to study the long-term impact on children, I exploit the variation in the age at which children moved into social housing to get variation in exposure time (Chetty et al.

2016; Chaudhry and Eng 2024). 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 similar observable characteristics. 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. Obtaining social housing one year earlier increases yearly labour earnings by $260 (approximately $190 USD), decreases the probability of receiving social assistance by 0.6 percentage point, and boosts post-secondary attendance by 0.3 percentage point.

I explore potential mechanisms through which social housing would impact long-term child outcomes. My results suggest that income effects, differential neighbourhood effects, and increased housing stability have a limited impact on long-term child outcomes. Instead, parents' labour supply reduction appears to be the main driver of the benefits for children.

First, children might benefit from the income effects of the in-kind transfer. Although families earn less, they also pay much less in rent. When they enter social housing, they pay about $3,500 less in yearly rent per adult, which completely offsets 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.

Second, when families relocate to social housing, their children are exposed to a new environment. I study whether the social housing neighbourhoods are significantly better or worse for child development. I follow Chetty and Hendren (2018) to compute causal place effects on children. Entries into social housing result in a move to slightly worse neighbourhoods in this dimension. Hence, place effects are unlikely to explain positive treatment effects on long-term child outcomes. Furthermore, treatment effects on children do not vary significantly by neighbourhood effects nor average income.

Third, families experience greater residential stability after entering social housing: event study estimates of year-to-year move probabilities show a marked decline in relocation. However, heterogeneous-effect analyses indicate that child outcomes are essentially unchanged among families whose parental labour supply does not fall, even though they were exposed to this increased stability. This pattern suggests that residential stability, although real, is not a primary driver of the long-term benefits for children.

Finally, I demonstrate that the reduction in parents' labour supply is a key mechanism explaining the gains for children. Children's gains are concentrated in households where parents reduce labour supply. To provide convincing evidence of this, I exploit a feature of social housing programs: families have minimal control over which social housing building they are assigned to. That means they do not select the neighbourhood to which they will be relocated, and crucially, how far that will be from their previous home and job. Longer, assignment-driven moves increase commuting costs and disrupt job continuity, resulting in larger, plausibly exogenous reductions in parental work. This highlights an involuntary component to parents' reduction in labour supply. Leveraging displacement distance, the analysis shows that larger declines in parent labour supply causally coincide with larger improvements in children's adult outcomes. I show that the distance itself is not driving any of the treatment effect.

From a policy perspective, my results suggest a clear trade-off between maximizing returns to children and parents' labour market participation. Whether the total net benefits of the policy exceed the direct policy cost and the negative fiscal externality arising from lower parental earnings remains an empirical question. Using the Marginal Value of Public Funds (MVPF) framework (Hendren and Sprung-Keyser 2020), I estimate that one additional year of social housing for the average one-parent, one-child family yields \$1.43 in social benefits per dollar of net cost for the government. I show that MVPF is largest for families with many children, as well as for families assigned to buildings in areas farther from the urban core and those in the second quintile of the citywide distributions of average income and causal place effects.

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 effects of public housing on children have relied on exogenous displacement caused by 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. Because both settings involve the provision of housing vouchers to families exiting public housing, these results may not generalize to the effects of receiving social housing relative to no housing assistance.

More recent research leverages entries into social housing, rather than exits, to estimate treatment effects. Jacob et al. (2015) use randomized housing voucher lottery in Chicago to study the long-term impact on children and find small, if any, effects on economic outcomes.

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

A set of papers use plausibly random variation in entry timing to estimate exposure effects, finding positive impacts on test scores (Han and Schwartz 2021), adult earnings, and reduced safety-net participation (Chaudhry and Eng 2024). Pollakowski et al. (2022) show that those results are robust when using a sibling design. Building on this, my paper offers additional estimates on the causal effect of an additional year in social housing on adulthood earnings, social assistance receipts and post-secondary education attendance. I also provide new insights into the mechanisms by which social housing affects children.[2] First, despite lower parental earnings, sharp rent reductions keep net-of-housing disposable resources roughly unchanged, thereby limiting the pure income–expenditure channel. Second, entry moves are, if anything, toward slightly worse measured place effects for children, and treatment effects vary little with neighbourhood characteristics, making classic neighbourhood-quality channels unlikely to explain the gains. Instead, I show that a large part of the benefits operates through parental behaviour: reduced labour supply frees time for household production and child-focused investments. This is consistent with recent evidence that parental reading time is beneficial for child outcomes (Price and Kalil 2019; Cano et al. 2019).

This paper also contributes to the literature on labour supply responses to housing assistance. Jacob and Ludwig (2012) analyzes a housing-voucher wait-list lottery in Chicago and finds that recipients reduce labour-force participation by about four percentage points (6%) and quarterly earnings by 10%. In a different context, Van Dijk (2019) shows that average moves into Amsterdam's public housing reduce labour market outcomes, worsen neighbourhood quality proxies, and increase social assistance receipt. My estimates align with these findings, documenting substantial declines in earnings and employment. I extend this literature by using linked employer–employee data to study job transitions, job quality, and commuting, and by tracing how rent and net-of-housing-cost income adjust at entry. My analysis clarifies how rent-geared-to-income pricing simultaneously creates work disincentives for adults while providing risk insurance that facilitates greater leisure and parental investment in children—mechanisms that differ markedly from those induced by standard cash or in-kind transfers. I suggest that the value of social housing to recipients extends beyond its monetary value, also encompassing an insurance value (Gadenne et al. 2024). Under a set of assumptions, I show that the insurance effects account for about half of the total labour supply response.

Ultimately, I contribute to the debate about where we should build public housing. Much of that debate has been occupied by issues relating to the impact of social housing on neigh-

[2] In this sense, my paper also relates to Fuenzalida et al. (2024) and Ribeiro and Leite-Mariante (2025), who study home ownership programs in Latin America and explore how treatment effects on children are related to treatment effects on parents.

bourhoods and segregation (Almagro et al. 2024)[3] rather than the heterogeneous effects on recipients. Although primarily descriptive, prior work documents substantial spatial heterogeneity in the impacts of social housing on both adults and children. For adults, Van Dijk (2019) shows that average moves into social housing reduce labour supply, yet moves into high-income neighbourhoods yield positive labour market effects. For children, Chaudhry and Eng (2024) estimate building-level treatment effects and find variation across projects, with larger gains in areas where home ownership shares and median incomes are higher. A key limitation of these studies is that they analyze only one margin at a time—either adult or child outcomes—implicitly holding the other fixed. My results indicate that places that minimize parents' labour supply reduction will deliver lower returns for children. Accounting for this joint relationship, I estimate how both margins vary with neighbourhood characteristics, providing a unified picture of spatial heterogeneity. The quasi-random assignment of households to locations in my context offers a credible setting to explore spatial heterogeneity in both treatment effect dimensions. I find that the total net benefits are largest in areas farther from the urban core and those in the second quintile of the citywide distributions of average income and causal place effects.

Finally, this paper connects to a broader literature on how other transfer programs shape parenting behaviours and children's long-run outcomes. A large body of work shows that cash transfers that raise disposable income—such as child benefits and expansions of the EITC—can improve children's test scores, educational attainment, and later earnings, often alongside measurable changes in parental behaviour (Milligan and Stabile 2011; Duncan et al. 2011; Dahl and Lochner 2012; Bastian and Michelmore 2018). My findings complement this evidence in two ways. First, rent-geared-to-income subsidies deliver resources in kind while simultaneously changing the price of parental time via work disincentives, whereas many cash programs (e.g., the EITC) tend to increase labour market participation. In my setting, parents reduce labour supply, yet children still gain, pointing to time reallocation toward home production and child-focused activities as a central mechanism rather than income alone. The RGI schedule flattens the net-of-housing budget set, encouraging fewer hours; the positive child impacts I document despite lower earnings therefore help reconcile mixed results across transfer types by highlighting the role of parental time. Recent work on unconditional or broadly targeted transfers reaches similar conclusions about the importance of parenting inputs and reduced stress as transmission channels (Krause et al. 2025). Taken together, these comparisons suggest that program design—cash versus in-kind, and

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

the implied incentives for parental labour supply—critically mediates how transfers translate into human-capital investments and intergenerational mobility.

The rest of the paper proceeds as follows. Section 2 presents the social housing programs in Toronto and Montréal, as well as the conceptual framework. Section 3, describes the various data sources and the analysis sample. Section 4 presents the impact of social housing on adults, while Section 5 discusses the long-term effects of social housing on children. Section 6 shows the relationships between effects on adults and effects on children. Section 7 discusses the cost-effectiveness of the policy. Finally, Section 8 concludes.

2 Institutional Setting

2.1 Background: Social Housing in Toronto and Montréal

I study Rent-Geared-to-Income (RGI) programs in Canada's two largest cities, Toronto and Montréal. These programs are administered by municipal corporations—the *Toronto Community Housing Corporation* (TCHC) and the *Office municipal d'habitation de Montréal* (OMHM).[4] The two systems are nearly identical. Tenants contribute a fixed share of gross income toward rent—30% under Toronto's system and 25% under Montréal's—with governments covering the residual. Together, these programs house more than 170,000 residents across over 80,000 units.

Canadian social housing is dispersed across cities: while many developments are in central areas, units adjacent to suburban, middle-class streets are also common. This contrasts with historical models in the United States (central-city slum clearance and redevelopment) and Australia (low-density peripheral estates) (Suttor 2016). The Canadian pattern generates substantial variation in the neighbourhood conditions experienced by social housing residents—well documented for Toronto by Oreopoulos (2003)—and thus provides a rich setting to study how social housing interacts with local environments.

I study entries into social housing from 1993 onward for children and from 2001 to 2017 for adults. Over those years, the stock of social housing has remained relatively stable due to limited funding for expansion. Although housing is constitutionally a provincial responsibility, the federal government historically financed a large share of capital costs. Following the 1990–93 recession and subsequent fiscal consolidation, federal support for new social-housing construction fell sharply (Suttor 2016). Provincial funds largely maintained the

[4] In practice, my analysis also includes the municipal corporations that manage public housing in the Montréal suburbs; these programs operate under the same rules and selection criteria.

existing stock but did little to expand it. As a result, most public housing units in operation today were built between the early 1960s and the early 1990s. During my study period, supply is effectively fixed while demand increases, resulting in persistent oversubscription and long waitlists.

2.2 Application Process and Tenant Selection

Applications are processed through a centralized platform and allocated using a transparent queueing system: (*i*) the application is filed and recorded; (*ii*) eligibility is assessed and a priority score assigned; (*iii*) applicants renew annually and update household information, especially income; and (*iv*) when a suitable unit becomes available, offers are made in queue order. In 2021 dollars, income thresholds for a family of four were roughly \$60,000 (about \$43,600 USD) in Toronto and \$45,000 (about \$32,800 USD) in Montréal. Families typically face multi-year waits before receiving an offer.[5] Because the programs are over-subscribed, only families close to the maximum priority score ever receive offers.

While they are on the waitlist, applicants must update their application files each year to remain eligible. In principle, earnings after move-in should stay below the eligibility thresholds, but enforcement is imperfect. Many newspaper articles document cases of households with incomes above the threshold residing in social housing (CTV News Toronto 2015; La Presse 2025). Those cases are typically individuals who have been in social housing for some time, and have passed the income limit after a couple of years. A potential concern for my study is whether applicants manipulated their earnings to maintain eligibility in the years leading up to entry. Figure A.1 plots the distribution of family income relative to the eligibility limit in the year prior to entry; there is little evidence of bunching just below the threshold, suggesting this is not a first-order concern.

Two features of the allocation mechanism are central for identification. First, the exact timing of entry into social housing is difficult for applicants to manipulate, as wait times are long and all applicants progress through the same queue. Second, families do not choose specific housing projects or neighbourhoods. Although broad geographic preferences can be indicated, OMHM staff report these are rarely exercised because restrictions lengthen wait times; similar behaviour is documented in Toronto by Oreopoulos (2003). Given the immediate value of the subsidy, outright rejections of initial offers are uncommon.

[5] Expedited placements exist (e.g., for individuals fleeing violence or vacating dwellings deemed uninhabitable under municipal by-laws); see OMHM (2025), https://www.omhm.qc.ca/en/submit-application/assessing-applications-and-waiting-lists.

3 Data and Descriptive Statistics

3.1 Canadian Employer-Employee Dynamic Database

This research leverages Canadian administrative employer-employee linked data: the Canadian Employer-Employee Dynamic Database (CEEDD). These data span 1997 to 2021 and provide granular information on the labour market, covering the universe of individual workers and firms.[6] 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 with the National Accounts Longitudinal Micro-data File (NALMF) to provide comprehensive firm characteristics. Earnings information for individuals comes from T4 slips (similar to the W-2 slips in the United States), which report total remuneration, including wages, commissions, and 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 sum their earnings from all jobs, but retain only the primary job information, 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.[7]

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-scale spatial analysis.[8] 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 1992.

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

[6] Information at the job- and firm-level is only available for 2001 onward.

[7] In Appendix E, 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.

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

Starting in 2001, the data provides 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 firms represent the vast majority of firms. This restriction leads to an over-representation 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 a crucial piece of 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 D.1 explains in detail this procedure. Education is a potentially important outcome to consider when examining the long-term effects of individuals who grew up in social housing. I use the information on the 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 E details this approach and shows that this education proxy provides good coverage compared to official enrolment counts, and sensible estimates of return to education.

3.2 Public housing buildings

The locations and characteristics of public housing buildings are retrieved through Freedom of Information requests. The data includes the exact address of each building, 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 public housing residents. 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 composed exclusively of subsidized households, I filter the data to include only postal codes with at least 25 units of social housing, resulting 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 D.2 details this assignment process.

3.3 Sample Description

The analysis focuses on individuals who (i) entered social housing as parents, or (ii) lived in social housing during childhood. I identify entries into social housing when an individual moves from a non-social housing postal code to a social housing postal code with 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 have had children over the event period. I drop individuals who had earnings above the eligibility threshold the year before entering. Over 95% of people who entered social housing had incomes below the eligibility threshold. Figure A.1 also shows that there does not appear to be any bunching just below the eligibility threshold.

Table 1 presents summary statistics for the treated adults and the general population. As expected, individuals who have ever entered social housing are negatively selected. In the years prior to entering social housing, only 51% of them had any labour earnings, relative to 78% in the general population. They received more than 8 times as much social assistance ($4,896 vs $583). They are more likely to be women, single parents, or immigrants.

For the children analysis, I include individuals born between 1982 and 1995 whose parents entered social housing in 1993 or later while they were 25 years old or younger. 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.

4 The Effects of Social Housing on Adults

4.1 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 not manipulable, provides quasi-experimental variation. However, comparisons between social-housing entrants and the general population are confounded by systematic differences in characteristics and behaviours—for example, baseline income and employment instability, prior housing insecurity, social assistance take-up, family structure, and neighbourhood of residence. 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) lives in the same city, (3) has the same year of birth, (4) has the same marital status, and (5) has the same number of children (top coded at 4). I use caliper 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 that treated and control units are on similar trajectories, it may raise concerns about mean regression post-treatment. In the results, I provide estimates for periods beyond the targeted lags and show that those are also balanced.

Table 1 presents summary statistics for the treated individuals, the matched sample, and the general population. The match rate is about 65%, but the matching does not systematically change the composition of the treated sample.

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

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

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 $k > 0$ represents the causal effect of social housing k years after entry. Those coefficients are normalized to period $k-1$. Standard errors are clustered at the individual level.

4.2 Results

Figure 1 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 1 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 employment and lower income among those who keep their jobs. Social housing tenants are about 6 percentage points less likely to receive any labour earnings in a given year. Figure A.2 shows that earnings for individuals who still work at their $t-1$ employer earn about $2,500 less than the control group average of $34,172. This is equivalent to a 7.3% reduction in hours worked, assuming no change in the hourly wage.

For individuals who stay employed, they are more likely to change employers. Based on a sample of individuals with stable employment[9], 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.[10]

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 exceeds the reduction in rental costs, social housing tenants end up with lower net disposable income. If the rent reduction is larger, even with the lower income, they might have higher disposable income. Figure 3 shows event 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, leaving disposable income unaffected in

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

[10]From the model introduced in Appendix C, one could expect workers to choose to switch to jobs with higher pay, but higher hours variance. This result indicates this is not the case.

the medium term.

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.3 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.4 shows DiD coefficient over characteristics of destination neighbourhood. When families move to high-income neighbourhoods, reductions in parental labour supply 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). A potential explanation is that those moves coincide with an important improvement in access to quality jobs or better transit accessibility (reducing commuting costs). We observe a similar pattern, albeit on a different scale, when examining heterogeneity in neighbourhood effects of destination neighbourhoods. Appendix H details the estimations of those neighbourhood effects.

Why are the labour responses so large relative to the rent relief? The size of the labour supply response, relative to the value of the transfer, is much larger than other cash or in-kind transfers. I argue that this is because Rent-geared-to-income (RGI) housing changes families' budget sets in three ways that differ from cash transfers.

In Appendix C, I introduce a simple time-allocation model where a parent has one unit of time and chooses intended work hours h, plus time and monetary investment in its child (t, m). Because jobs are unstable, actual hours are noisy: $\tilde{h} = h + \mu$ with $\mathbb{E}[\mu] = 0$. Consumption is derived from earnings and is valued alongside leisure/home time; child human capital increases with t and m. Housing H is required each period.

Agents are either under a market-rent regime or a social housing regime. The regimes differ only in how rent is paid. With fixed market rent R,

$$C_M = w\tilde{h} - R - p_m m,$$

so an extra intended hour pays the full wage w. Under the RGI regime, rent is proportional to income, $r \in (0,1)$,

$$C_S = (1-r)\, w\tilde{h} - p_m m,$$

so an extra intended hour pays the lower "net wage" $(1-r)w$.

These two budget rules generate three intuitive forces on h. *(i)* **Income Effect** Compared to paying R, proportional rent lowers the average rent. Because leisure and consumption are normal goods, some parents optimally "buy" more time at home by reducing their working hours. *(ii)* **Substitution Effect** The proportional rent charge flattens the budget constraint: because $(1-r)w < w$, the relative price of leisure/home time falls under RGI, nudging parents toward fewer hours. *(iii)* **Insurance Effect** For workers with volatile hours and unstable jobs, RGI mitigates the consumption hit from low-earnings/hours states because rents adjust downward when earnings fall. With insurance, maintaining high intended hours is less valuable "just in case," further reducing average hours and employment.

Implications While all three channels reduce labour supply, the total effects on consumption/spending are ambiguous and depend on whether rent savings exceed earnings declines. Channels *(ii)* and *(iii)* push consumption down; channel *(i)* pushes it up. If social-housing payments are substantially below the expected market rent, the income effect can dominate, and consumption can potentially rise; otherwise, it would fall as households substitute toward time and earn less.

To interpret the \$2,400 post-entry earnings decline, I decompose it into income, substitution, and insurance channels. Appendix C.1 details the following decomposition. Benchmarking the income effect with "pure income" MPE $\in [-0.3, -0.1]$ estimates (Imbens et al. 2001; Cesarini et al. 2017; Bengtsson 2012) implies that the \$3,500 rent relief would lower earnings by only \$385-\$1,050—far short of the total drop. Using a conservative Frisch elasticity of $\varepsilon = 0.15$ (consistent with micro evidence (Chetty 2012) and with the evidence in the housing voucher context (Jacob and Ludwig 2012)), the RGI implicit tax $r \in [0.25, 0.30]$ yields a substitution component of roughly \$480-\$576. The residual—about \$774 to \$1,535—maps to the insurance channel in which proportional rent lowers the payoff to maintaining high intended hours under hours risk. Using midpoints for illustration (MPE = -0.20, $r = 0.275$, $\varepsilon = 0.15$), the income and substitution channels account for 28.4% and 21.3%, respectively, with the insurance channel explaining the remaining half (50.3%).

5 The Effect of Social Housing on Long-term Child Outcomes

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 variation in exposure time to social housing arising from families entering social housing at different times, when their children are at 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).

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

$$y_i = \beta + \sum_{a=0}^{24} \delta_a \times \mathbf{1}(a_i = a) + \mathbf{X_i}\Gamma + \epsilon_i \tag{2}$$

where y_i is the adulthood outcome of child i (e.g. labour earnings, post-secondary attendance), $\mathbf{1}(a_i = a)$ is an indicator equal to 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 and destination neighbourhood). δ_a are the coefficients of interest and are normalized to δ_{25}. In practice, I estimate equation 2 in 2-year age bins to improve precision.

Figure 4 shows that the effects of social housing are approximately linear in the year of exposure. This is consistent with previous research using exposure designs (Laliberté 2021; Aloni and Avivi 2024; Chetty and Hendren 2018; Chaudhry and Eng 2024). 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 the sample of children whose parents moved into social housing when they were aged 0-18 and estimate this slope coefficient with the slightly more parametric function:

$$y_i = \beta + \delta(18 - a_i) + \mathbf{X_i}\Gamma + \epsilon_i \tag{3}$$

where, under the linearity assumption, 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 is orthogonal to the children's potential outcomes among observationally similar families—including those with the exact same 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 essentially not 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 2 replacing the dependent variables with pre-event family characteristics. Figure A.6 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 extent of selection that exists. I try to measure the level of selection by running a modified version of equation 2 that includes children whose parents never entered social housing. Appendix F shows that children of parents who entered social housing while the children were adults are negatively selected, relative to other disadvantaged children. 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 4 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.5 displays the corresponding coefficients for total income and the likelihood of receiving any social assistance. Table 2 reports the slope coefficient estimates—the parameter δ from equation 3—for all outcomes.

Labour Earnings and Total Income Panel A of Table 2 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 $260 (185 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 $1,300 more annually, a 4.7% gain. The effect on total income is somewhat smaller, reflecting reductions in non-labour income. One additional year of exposure raises total income by $221 (160 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 2 presents the estimated δ from Equation 3 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, relative to the baseline average of 54%. I find that the effect on the number of years of enrolment from age 18 to 25 increases by 0.025 years for each additional year of exposure to social housing. This represents a 1.3% 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, 0.125 more years of post-secondary enrolment. Panel B of Table B.1 shows that the effects of social housing on education are larger for immigrants.

Social Assistance Benefits Finally, Panel C of Table 2 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 $73 (approximately USD $52) per additional year, a 2.2% 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 $365 (11.1%) 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.7 plots treatment effects over destination neighbourhood characteristics. The effects of one additional year of social housing are not related to the destination's average income, and, surprisingly, not to 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 environment (Chyn and Katz 2021). [11]

6 Parents' Labour Supply and Child Outcomes

In this section, I analyze the relationship between parental labour supply responses and the treatment effects on children. As documented in Section 4.1, 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.

A first potential channel is a pure income effect. However, as shown in Section 4.2, the drop in labour earnings at entry is largely offset by the rent reduction, leaving net-of-housing disposable income roughly unchanged in the medium run. This makes increased pecuniary investment in children an unlikely primary driver of the observed gains.

Another candidate mechanism is improved neighbourhood quality. After entering social housing, children are exposed to a better environment, which can benefit their development. However, Figure 5 shows that, upon entry, families relocate—if anything—to slightly worse neighbourhoods as measured by the neighbourhood-effect index. The first-year decline is statistically significant but trivially small—about 0.5 percentile points—leaving little scope for neighbourhood quality to explain the observed gains. In addition, Figure A.7 shows that the treatment effects on children do not vary by neighbourhood income, nor neighbourhood effects.

In Section 6.1, I begin by examining the heterogeneous impact on children and the labour

[11] When calculating the neighbourhood effects, social housing children are excluded. Appendix H details the estimation procedure.

responses of their parents. Then, in Section 6.2, I use the distance of move to induce variation in parents' labour supply responses, providing additional evidence on the relationship between parents' labour supply reductions and the benefits children receive from social housing.

6.1 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 A and B of Table 3 stratify children by how their parents responded to social-housing entry. In Panel A, 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 B, 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 C of Table 3 shows that the estimated effect on children's adult earnings is statistically indistinguishable from zero for families in which the parent was a non-worker at baseline; the positive average effect is concentrated among children of baseline earners. This heterogeneity is consistent with reductions in parental labour supply being the primary channel through which social housing improves children's long-run outcomes.[12]

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.

6.2 Spatial Displacement

Relocating to social housing can disrupt commute patterns and potentially lower adults' working hours and employment attachment. If households that are reassigned farther from

[12] An alternative explanation is that non-working adults are poor role models, and the children are less likely to benefit from better living arrangements in that situation. However, this is inconsistent with the lower treatment effect for children of working parents who did not reduce their labour supply.

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 the social housing programs in Toronto and Montréal is the very limited room for selecting the location of residence. Hence, households can not 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.8 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.9 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 exploit distance-driven variation in parents' labour supply at entry to trace how parental responses map into children's long-run outcomes. Figure 6 plots binned treatment effects by move distance, showing a clear, approximately linear pattern: cohorts reassigned farther from their origin experience larger declines in parental hours and participation, and correspondingly larger gains for children. Quantitatively, the slope implies that each additional one percentage point reduction in parents' working hours at entry raises children's annual earnings in adulthood by about \$26.3. Figure A.10 shows the same monotone relationship for other child outcomes: earlier post-secondary entry and lower safety-net use alongside the earnings gains.

To interpret this as a causal relationship, we have to assume that the distance of the 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, controlling for destination fixed effects, thereby comparing families who land in the same destination tract but differ in how far they had to move. Conditioning on destination census-tract fixed effects—thus comparing families who land in the same tract but travel different distances to get there—yields the same relationship (Figure A.11).

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 A.12 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.

A second concern is that long moves might sever local ties in ways that directly benefit children (e.g., by distancing them from negative peer or family influences). I probe this by splitting moves into short and long distances and interacting distance with parents' labour-supply response. As shown in Figure A.12, children whose parents *do not* reduce labour supply show no significant gains—whether the move is short or long—whereas children whose parents *do* reduce labour supply exhibit similar, positive treatment effects under both short- and long-distance moves. This pattern is hard to reconcile with a direct "social ties" channel of distance and instead points to parental time reallocation as the operative mechanism.

Figure A.12 is informative about two complier groups. The first consists of families that are cash/time constrained but willing to devote more time to children; when the rent subsidy relaxes their budget constraint, these parents voluntarily reallocate time from market work to home production—evidenced by reduced hours without a forced break in the employment relationship. The second group reduces labour supply involuntarily—plausibly because longer assignment-driven moves disrupt job continuity—an effect that is more prevalent among households displaced farther at entry. *A priori*, one might expect voluntary reallocations to be more productive for children than involuntary reductions. The estimates do not support that view: gains are comparable across both groups, suggesting that additional parental time is similarly effective regardless of whether it is chosen or induced.

My analyses indicate that reductions in parental work time are a major driver of children's treatment gains, consistent with parents substituting market work for home production and child-focused time. Although the administrative data cannot reveal whether fewer hours worked translate into more (or higher-quality) parenting, auxiliary evidence from the Time Use Survey helps gauge magnitudes. In Appendix I, I show that—after controlling for observables—employed parents spend roughly 10 fewer hours per week on childcare than otherwise similar non-employed parents; on the extensive margin, part-time workers devote more than 5 additional hours per week to childcare than full-time workers. These gradients imply that the social-housing-induced reductions in labour supply are plausibly large enough to generate meaningful increases in parental time with children. While the broader literature on parental time and child outcomes is mixed (Guryan et al. 2008), recent work points to clear benefits of specific activities—particularly reading—on cognitive functioning and test scores (Cano et al. 2019; Price and Kalil 2019).

7 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 savings 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.

7.1 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 average child entering social housing is 9 years old, and they benefit from the increased income from age 18 to 65. I use 3% rate of return to compute present values. 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 \$2,687.[13]

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

[13]This assumes that parents are not altruistic; they do not benefit directly from their child's higher consumption. In Appendix C, I incorporate altruistic motives into the labour supply model. One could multiply the child's benefit by a factor $\phi \in [1, 2]$ to account for the parents' willingness to pay for better child outcomes. The resulting MVPF would be higher.

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.

7.2 Direct Cost

The annual gross resource cost of supplying one occupied unit for a year in the Toronto program is \$6,218. 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.

7.3 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 \$2,400 during the treatment year, which lowers tax revenue by \$480 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. 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. Summing the parent and child components, the baseline fiscal externality used in the MVPF denominator is -\$1,903; that is, the government gains \$1,903 in tax revenues.

In the absence of estimates specific to the studied housing program, some fiscal externalities are discussed but excluded from consideration. Reductions in crime and criminal-justice involvement plausibly follow from improved stability and neighbourhood environments (Kling et al. 2007; Ludwig et al. 2013; Chyn 2018; Chaudhry and Eng 2024); 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.

7.4 Calculating the MVPF

Given the one-year direct cost of \$6,218 and a fiscal externality of (-\$1,903), the government's net cost is \$4,315. The willingness to pay (WTP) combines the \$3,500 rent saving for parents with the \$2,687 present-value gain for children, for a total of \$6,187. Relative to the \$4,315 net cost, the implied MVPF is (1.43). This calculation is for a one-parent, one-child family in Toronto's program. In Appendix J, I report alternative MVPFs using Montréal's parameters and varying family compositions. Because the lifetime earnings gains of children dominate the WTP, the MVPF scales roughly with the number of children.

A useful robustness check is to compute "partial" MVPFs that isolate each side of the intergenerational ledger. When I include only children's willingness to pay and their induced fiscal externality, the MVPF is 0.70; when I include only parents' willingness to pay and the fiscal externality from their labour-supply response, the MVPF is 0.52. Taken in isolation, either calculation would incorrectly suggest that the policy is undesirable because the willingness to pay falls short of its net cost. By contrast, the complete accounting—aggregating both generations' benefits and fiscal effects—yields an MVPF of 1.43. The comparison highlights that evaluating social housing one margin at a time is misleading: the program's value stems from the joint incidence of benefits on children and parents together, along with offsetting fiscal effects that only become apparent in a comprehensive calculation.

I also compute MVPFs across neighbourhood characteristics using the corresponding treatment effects for children and parents. Accounting for both treatment dimensions is crucial: places that most benefit children often impose larger labour-supply reductions on parents. For example, if policymakers sought to minimize parental labour-supply responses by prioritizing high-income areas (see Figure A.4), children's gains would be smaller (Figure A.7), producing MVPFs below 1 and, thus, undesirable on net. In sum, I find that the

MVPF is highest in areas farther from the urban core and in the lower half of the citywide income distribution. Figure A.16 displays MVPFs by neighbourhood characteristics.

8 Conclusion

In this paper, I provide new evidence that social housing has intergenerational returns that operate, in large part, through parents' time. Two empirical facts anchor the results. First, children who enter social housing earlier earn more as adults, receive less social assistance, and are more likely to enrol in post-secondary education, consistent with exposure effects. In the preferred specification, advancing entry by one year raises adult earnings by roughly $260 and modestly increases post-secondary attendance, while also reducing future safety-net use. Second, parents experience sizable and persistent reductions in labour market activity when they enter social housing—about $2,400 less in annual earnings on average—yet their net-of-housing disposable income remains essentially unchanged once considering the reduction in rent.

I show that the reduction in parents' labour supply is a key mechanism in explaining the gain for children. When relocated farther, parents reduce their labour supply more. This coincides with larger benefits for children. I provide evidence that the distance itself does not directly impact children in ways unrelated to parents' labour supply response. I rule out the alternative mechanisms typically emphasized in the literature.

The gains on child long-term outcomes are hence directly related to the depressed adult labour supply resulting from the housing subsidy. This raises questions about whether the total net benefits are positive. A computation of the Marginal Value of Public Funds suggests that the Willingness-to-pay for the policy is 43% higher than the net cost for the government. Scaling by family size raises willingness-to-pay through both children's gains, and increases the positive fiscal externality for the government, leading to higher MVPF.

This has important policy implications: child-centred returns are real and sizable, but they are tied to parental labour-supply responses. This creates a salient trade-off for program design: interventions that maximize parents' work in the short run may blunt some of the child gains that appear to flow through time reallocation at home. Conversely, designs that permit or even encourage reductions in work hours seem to deliver larger improvements for children.

Several limitations suggest productive directions for future work. First, beyond earnings and schooling, tracking effects on health, fertility, and crime would clarify the full bundle of benefits and externalities. Second, the time-reallocation channel is supported by strong reduced-form evidence; future work could seek direct measures of parental time use and child inputs to disentangle home-production tasks that are most consequential (e.g., supervision, routines, homework help). Third, external validity likely hinges on context: Canadian public-housing neighbourhoods are, on average, reasonably "good." Testing whether similar child benefits arise where baseline neighbourhood conditions are markedly worse (e.g., higher crime, weaker schools, thinner transit) is essential.

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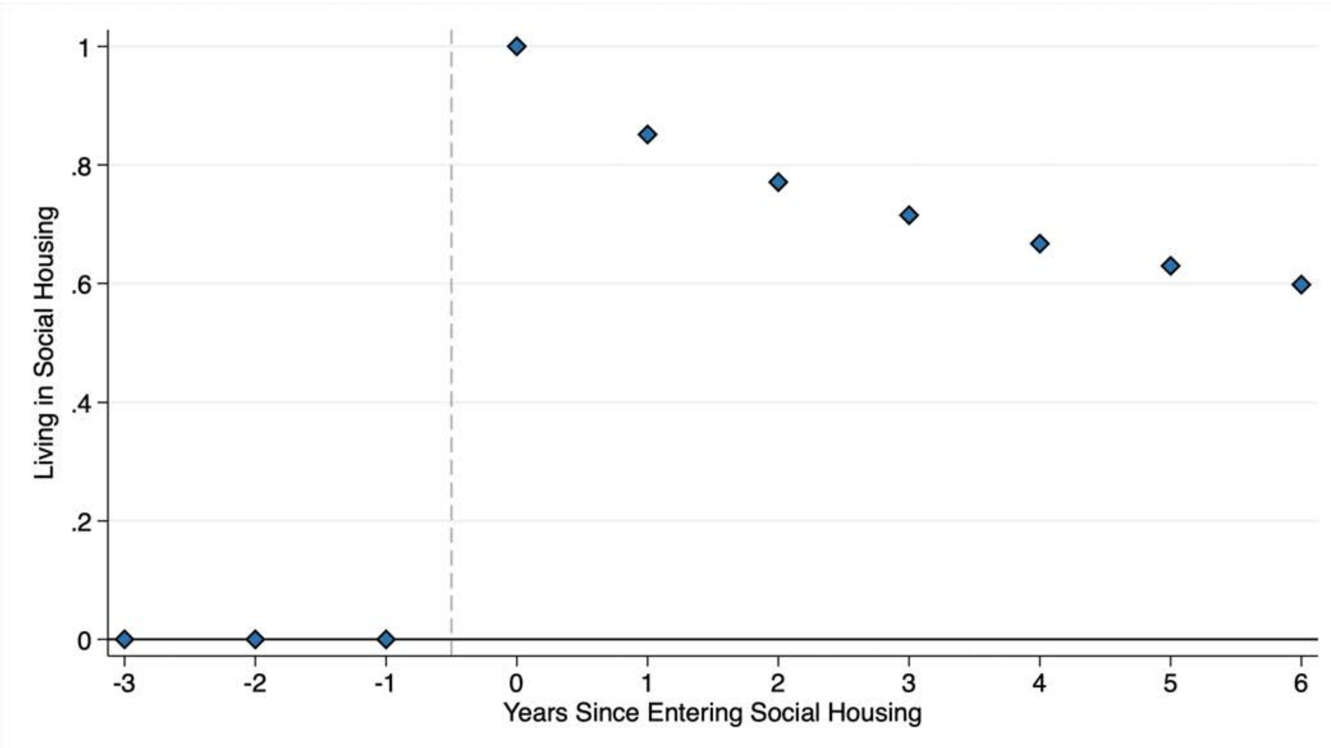
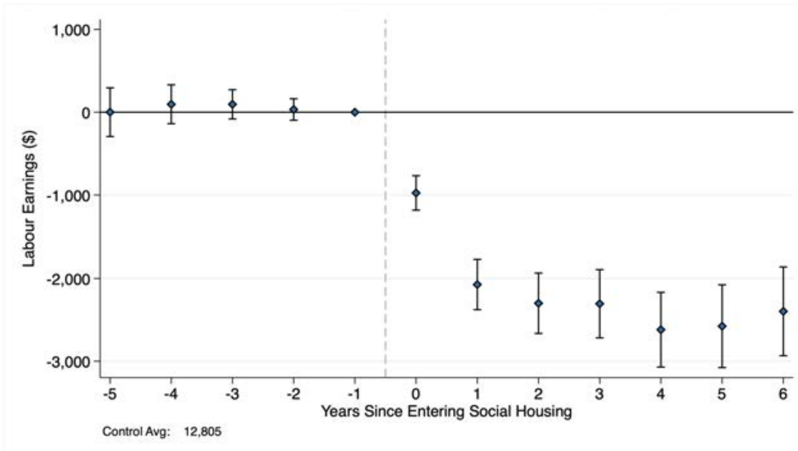
9 Figures

Figure 1: Entry into Social Housing, Retention

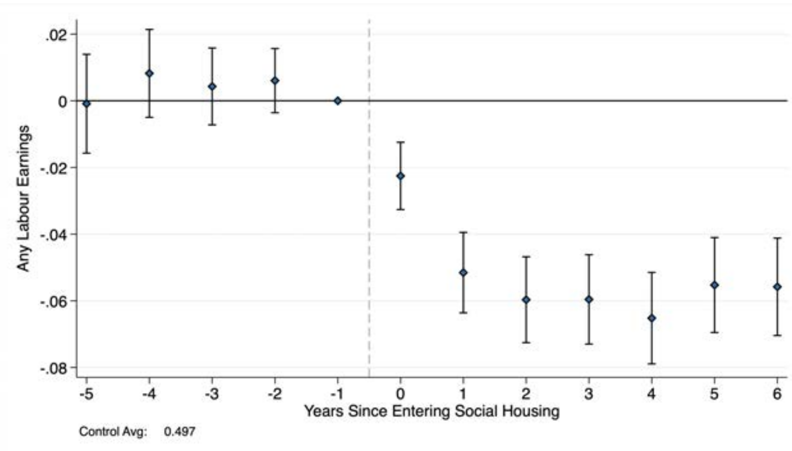


Notes: This figure reports the event study estimates from Equation 1 on a dummy equal to one if the person lives in social housing, and zero otherwise. Treated workers are those who moved from a non-social-housing postal code to a social-housing postal code. Controls individuals are matched using a caliper matching approach described in Section 4.1. The regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children, and a single dummy.

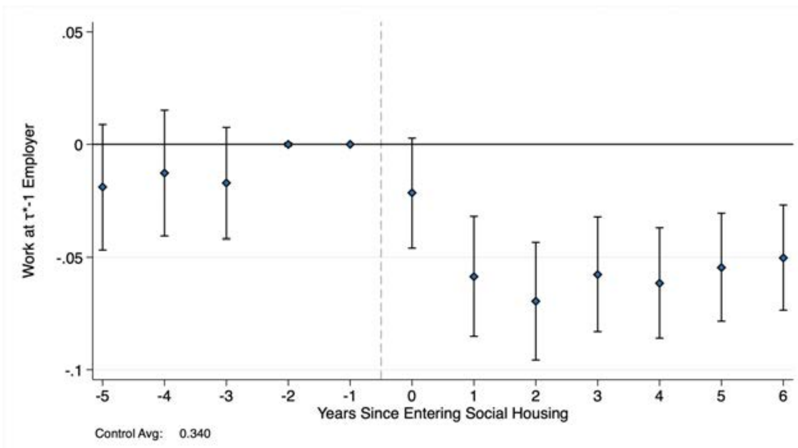
Figure 2: The Effect of Social Housing on Adults, Labour Response



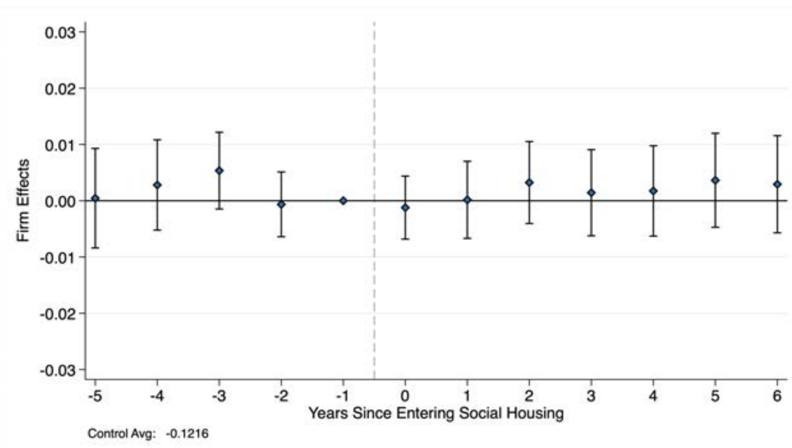
(a) Labour Earnings



(b) Receive Any Labour Earnings



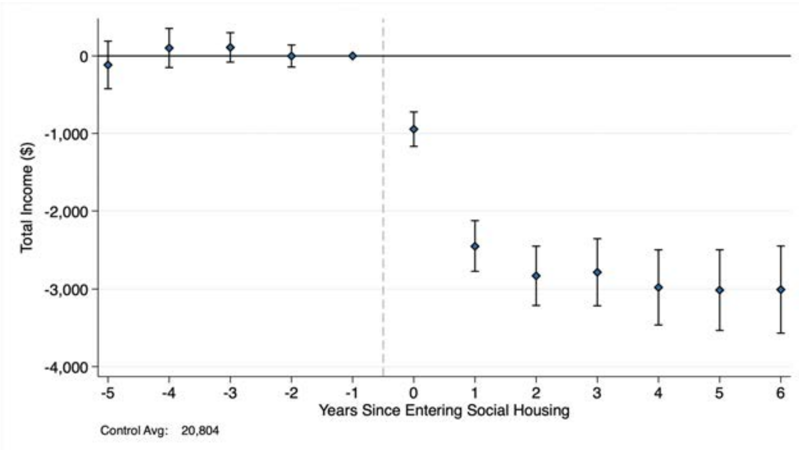
(c) Work at $t-1$ Employer



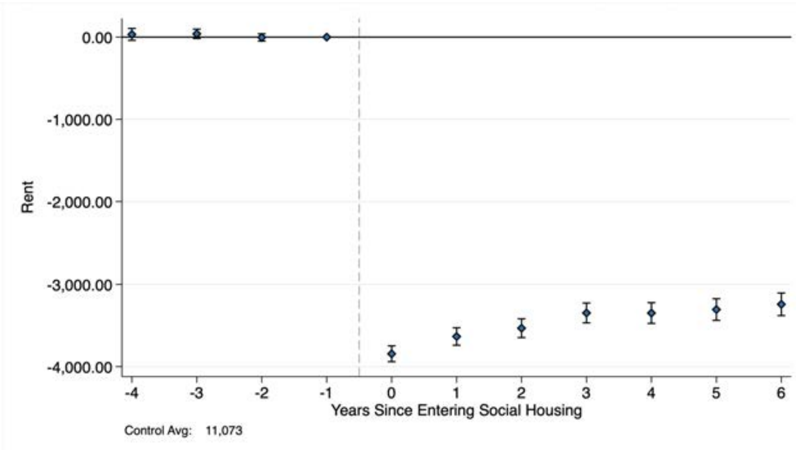
(d) Firm Effects

Notes: Each panel reports the event study estimates from Equation 1 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 4.1. 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 with stable employment, defined as 2 years of tenure at the firm. In panel (d), Firm Effects are computed by estimating a two-way fixed effect model described in Appendix G. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the individual level.

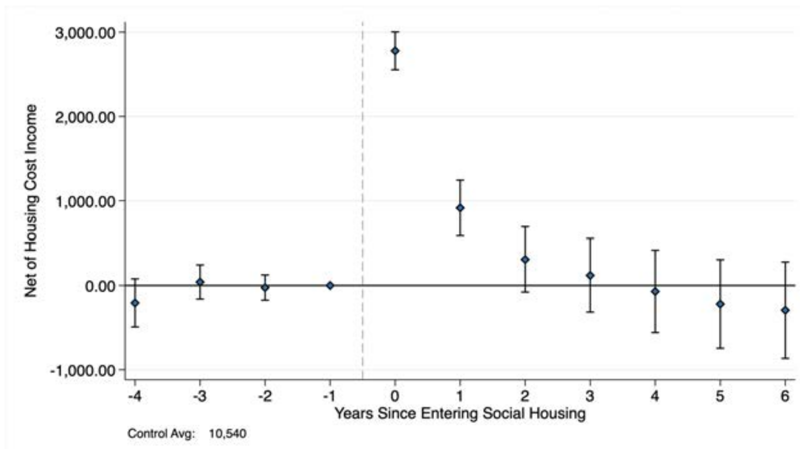
Figure 3: The Effect of Social Housing on Adults, Income



(a) Total Income



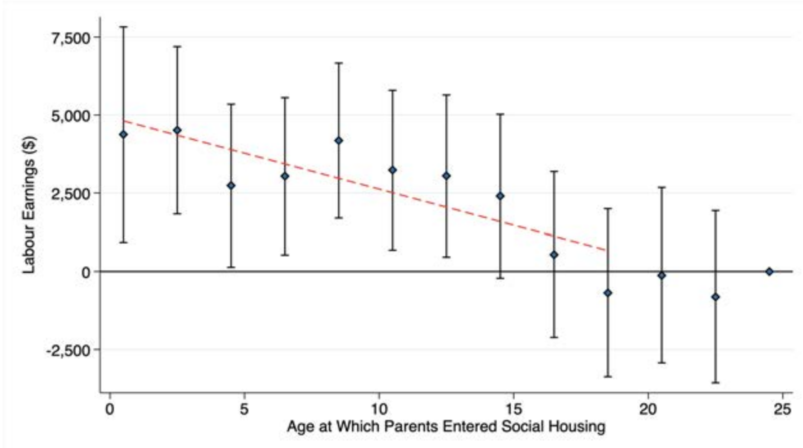
(b) Rent



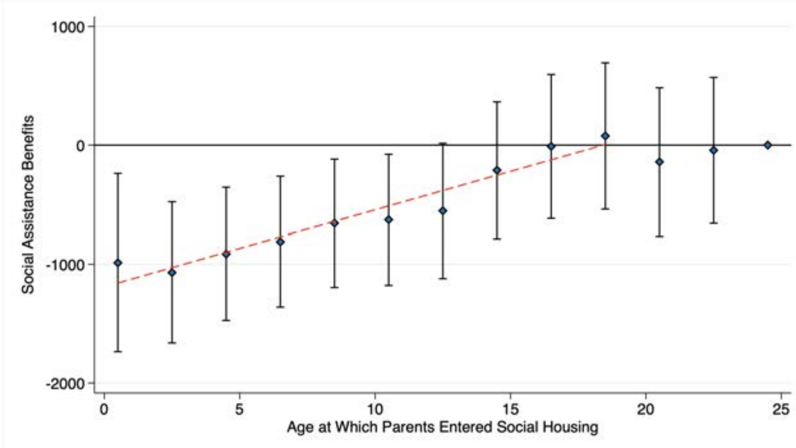
(c) Total Income Net of Rent

Notes: Each panel reports the event study estimates from Equation 1 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 4.1. 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 D.1. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the individual level.

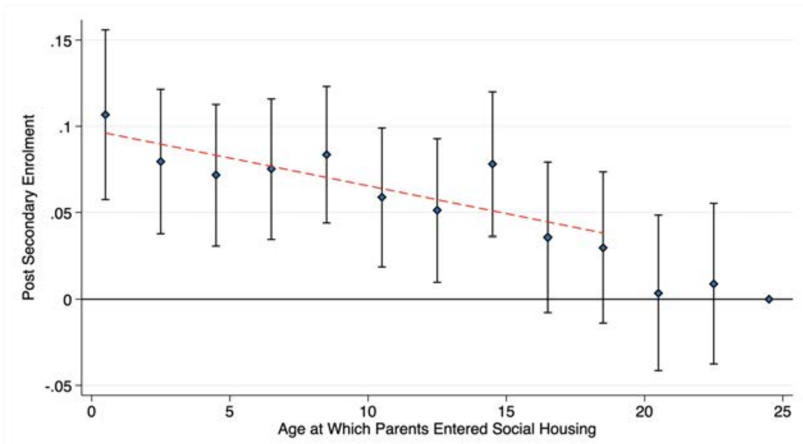
Figure 4: The Effect of Social Housing on Children, by Age of Entry



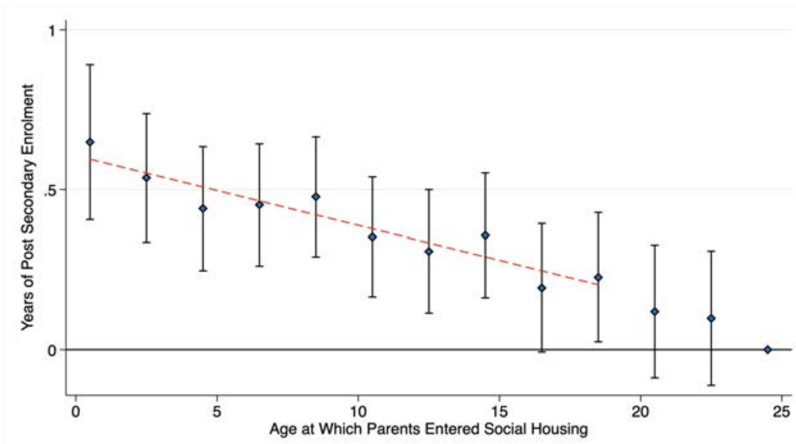
(a) Labour Earnings



(b) Social Assistance Benefits



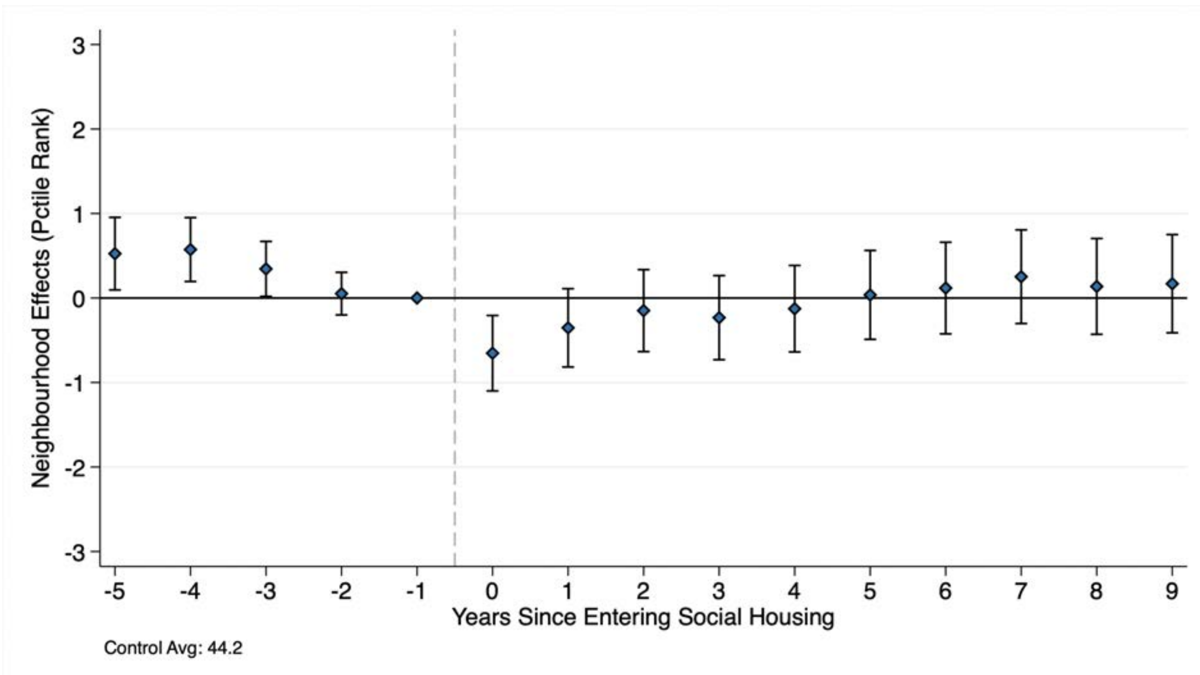
(c) Some Post-secondary Enrolment



(d) Year of Post-secondary Enrolment

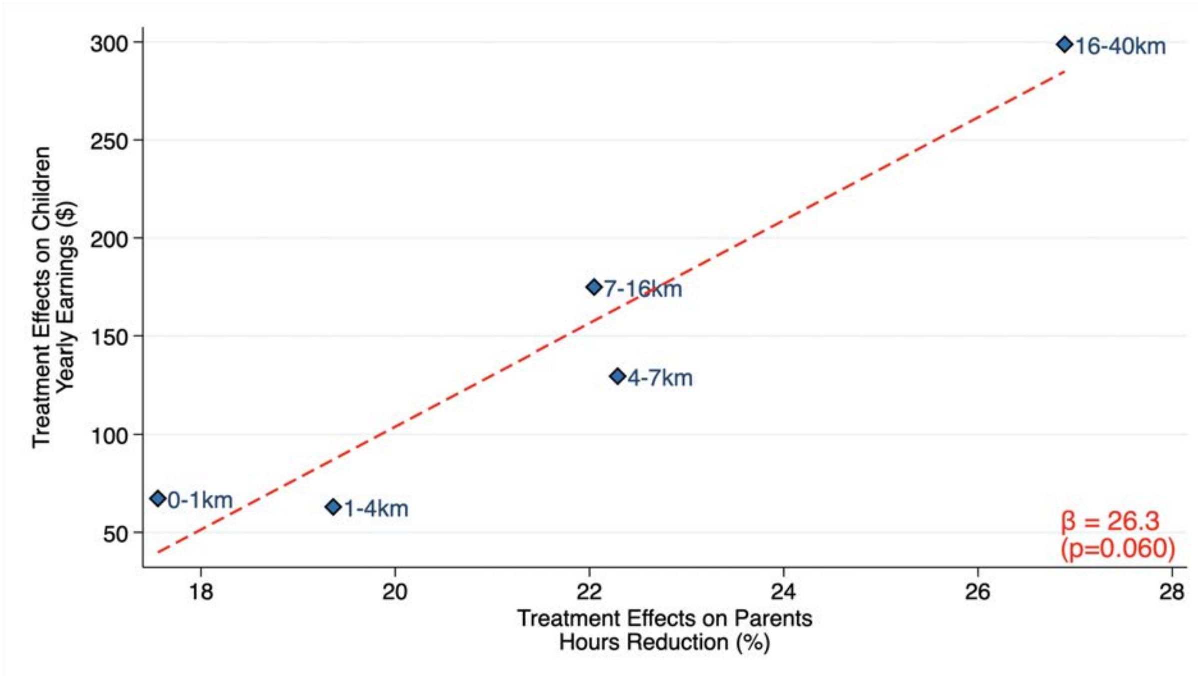
Notes: Each panel plots the estimate for the coefficients δ_a from Equation 2 on a specified outcome. Each coefficient is the effect of moving into social housing at age a, relative to having parents who moved into social housing at age 25. Age at entry is grouped in 2-year intervals and his based on 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 and destination 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 5: Neighbourhood Effects

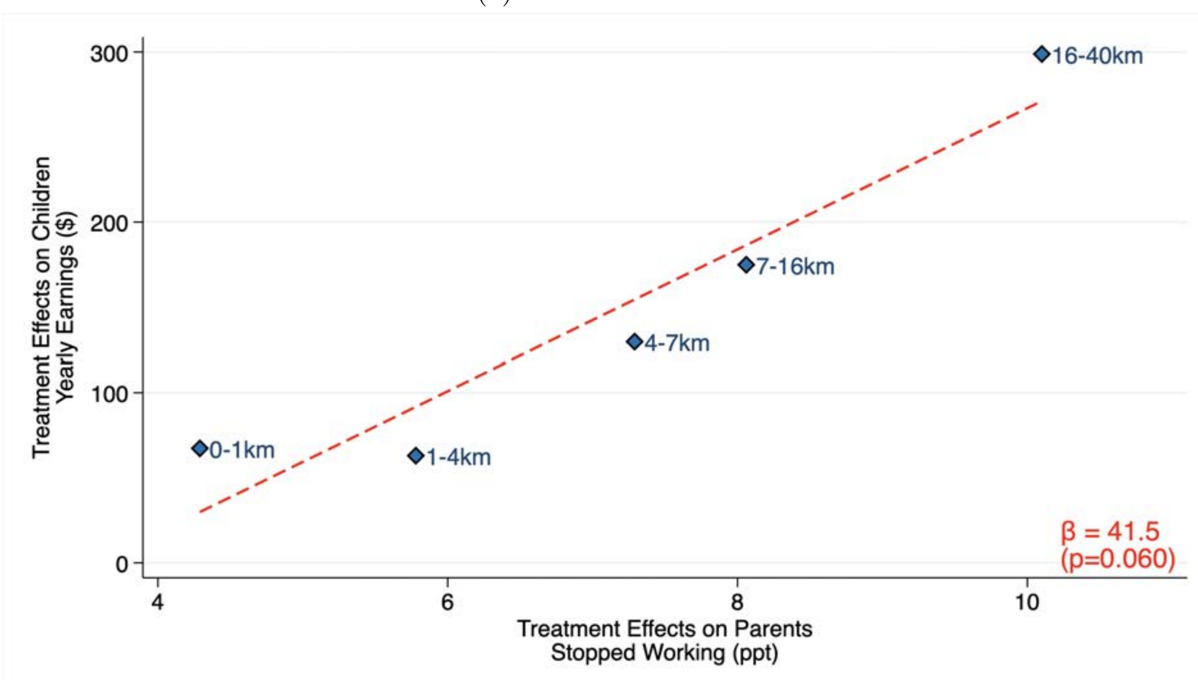


Notes: This figure shows the neighbourhood effect percentile for individuals who enter social housing. Treated workers are those who moved from a non-social-housing postal code to a social-housing postal code. Controls individuals are matched using a caliper matching approach described in Section 4.1. The regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children, and a single dummy. The neighbourhood effects estimation is detailed in Appendix H and are transformed into percentile ranks. Each point reports 95% confidence intervals clustered at the family level.

Figure 6: 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 p-values are printed in the bottom-right corner. The p-values are obtained through a bootstrap test with 1,499 bootstrap samples.

10 Tables

Table 1: 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 caliper 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 2: 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 3. Column 1 reports coefficients from univariate regressions, column 2 includes cohort and sex fixed effects, and column 3 additionally includes origin and destination 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 3: Effects of Social Housing on Children, by parent responses

	Labour Earnings (1)	Any Post-Secondary (2)
Panel A. 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)
Panel B. 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 C. 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)

Notes: Each column of each panel is a different estimate of the heterogeneous exposure effect. Controls include cohort, sex, immigrant status, and origin and destination Census Tract fixed effects. Each regression includes children whose parents moved into social housing between the ages of 0 and 18. Panels A and B 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

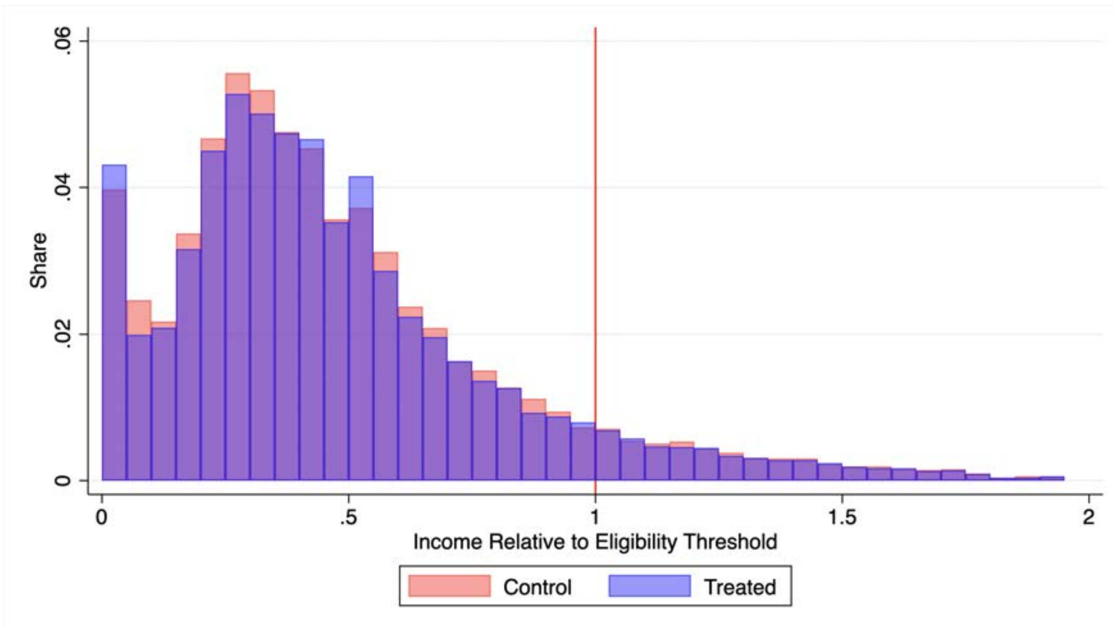
Table 4: MVPF calculation

MVPF components	Values
Willingness-to-pay	
Transfer value	3,500 $
NPV of gains on children	4,109 $
Taxes and reduced transfers	- 1,422 $
Total	6,187 $
Cost	
Direct cost	6,218 $
Fiscal Externalities	- 1,903 $
Total	4,315 $
MVPF	1.43

Notes: Dollar amounts are expressed in real terms (2021 CPI) and 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 (NPV) of lifetime earnings gains net of transfer reductions. Fiscal externalities include changes into tax revenue and transfers. I use a 20% effective tax rate to translate earnings changes into tax revenue. I use a 3% discount rate when calculating present values.

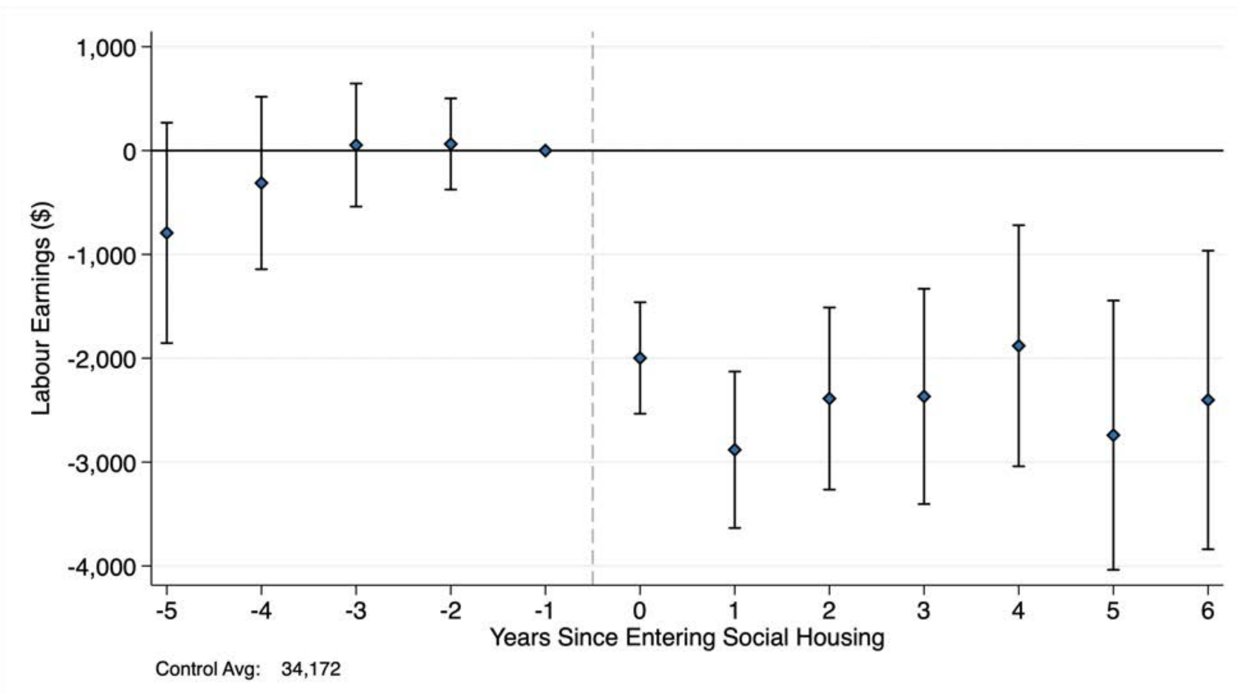
A Appendix Figures

Figure A.1: Income Distribution Around Eligibility Threshold



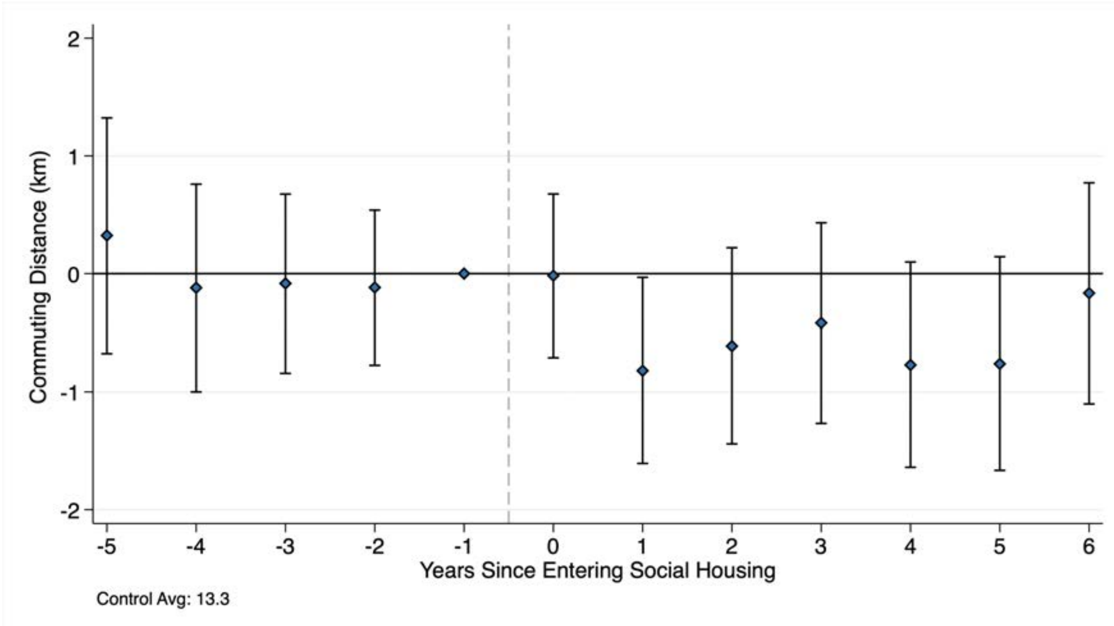
Notes: This figure shows the family income distribution relative to the eligibility threshold, one year before entry into social housing. Only individuals with strictly positive income are included.

Figure A.2: Entry into Social Housing, Labour earnings at initial employer

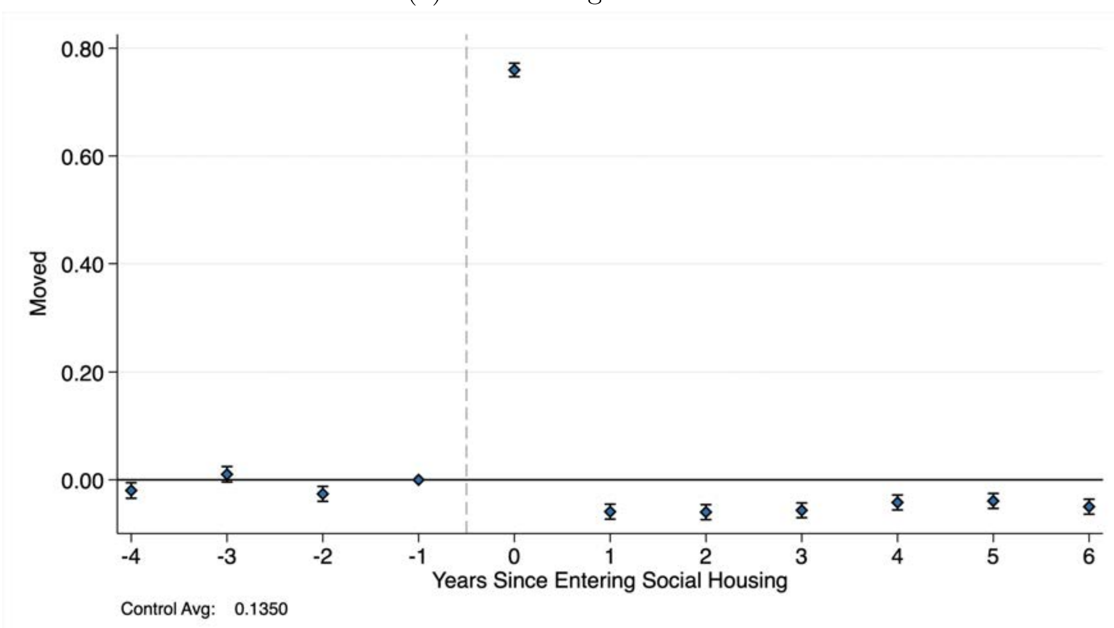


Notes: This figure reports the event study estimates from Equation 1 on labour earnings for individuals who work at their $t-1$ employer. Treated workers are those who moved from a non-social housing postal code to a social housing postal code. Controls individuals are matched using a caliper matching approach described in Section 4.1. The regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children and a single dummy.

Figure A.3: The Effect of Social Housing on Adults, Mobility and Commuting



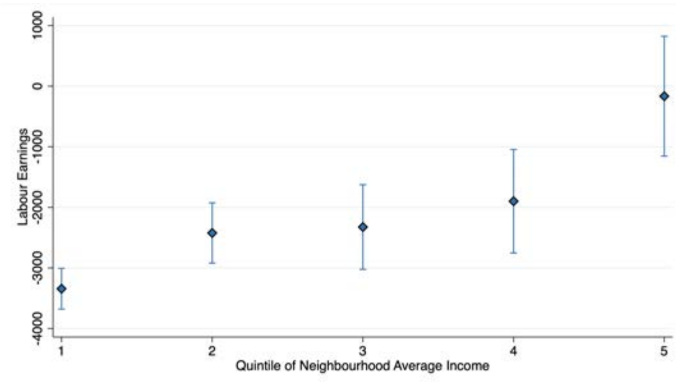
(a) Commuting Distance



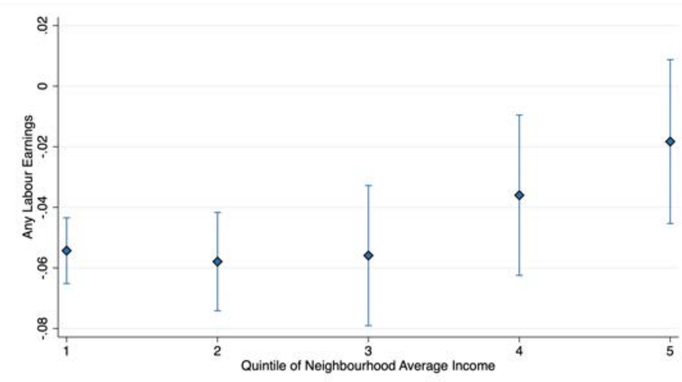
(b) Probability of Moving

Notes: Each panel reports the event study estimates from Equation 1 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 4.1. 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.

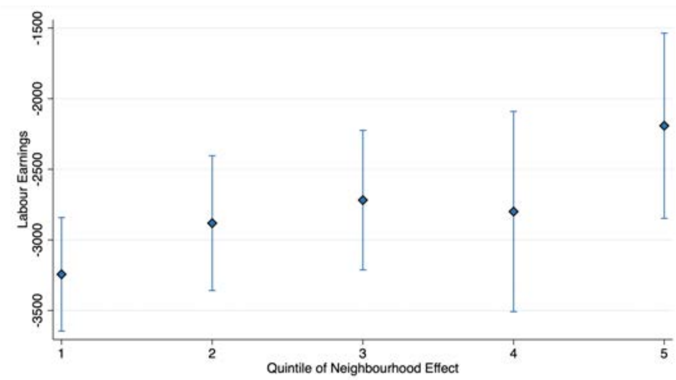
Figure A.4: Parents' Labour Response by Neighbourhood Characteristics



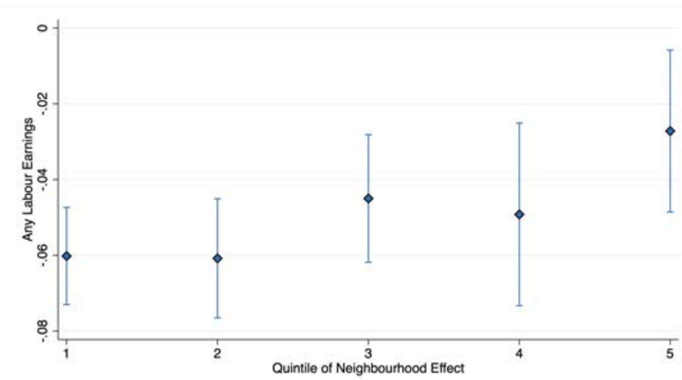
(a) Labour Earnings by
CT Average Income



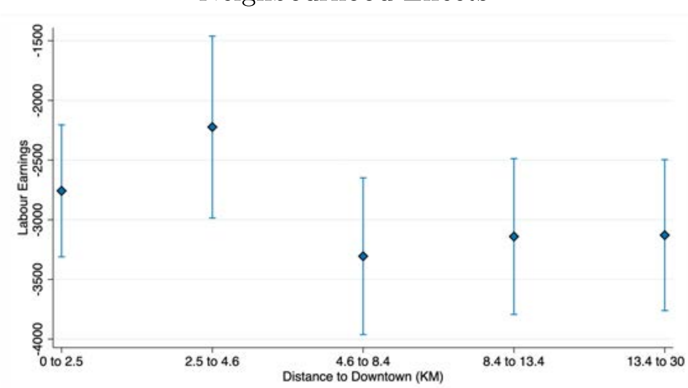
(b) Any Labour Earnings by
CT Average Income



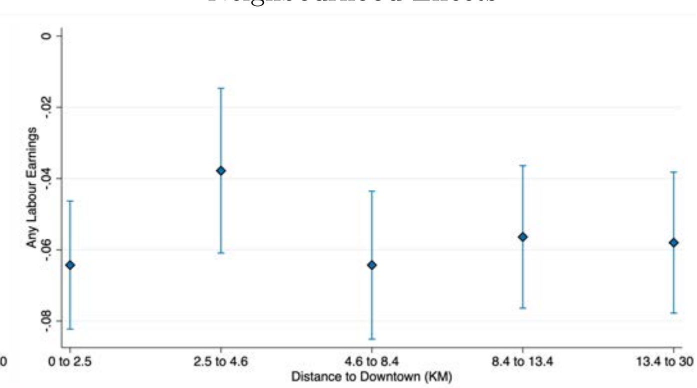
(c) Labour Earnings by
Neighbourhood Effects



(d) Any Labour Earnings by
Neighbourhood Effects



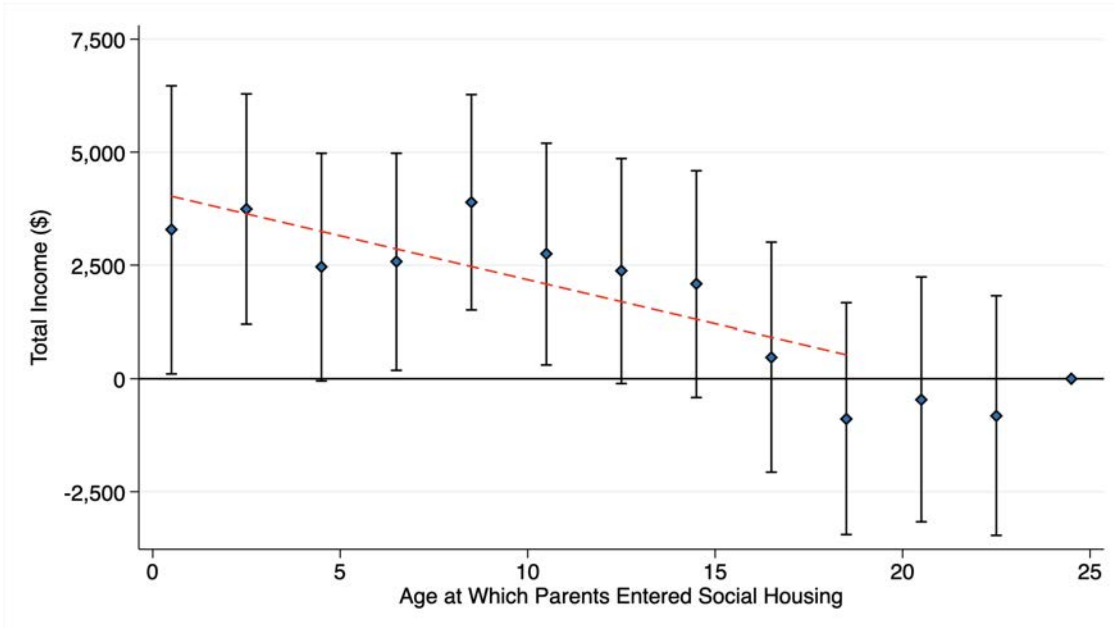
(e) Labour Earnings by
Distance to Downtown



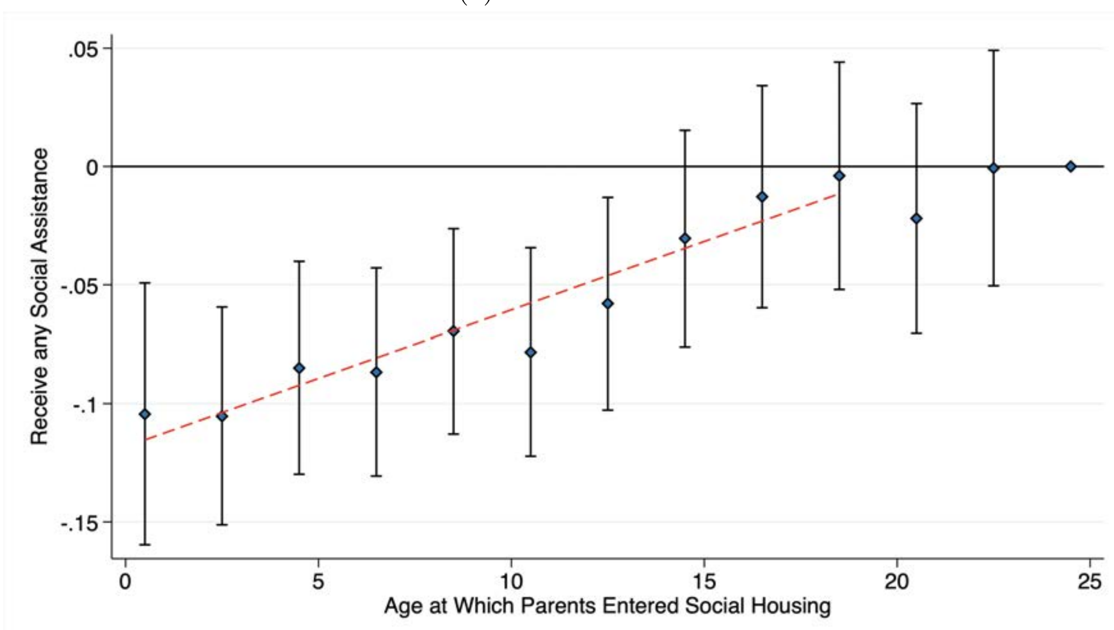
(f) Any Labour Earnings by
Distance to Downtown

Notes: Each panel reports the DiD coefficient on parents' outcome when entering social housing by a given neighbourhood characteristic. 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 4.1. Each regression includes worker and city-year fixed effects, a cubic polynomial in age, the number of children and a single dummy. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the individual level.

Figure A.5: The Effect of Social Housing on Kids, by Age of Entry



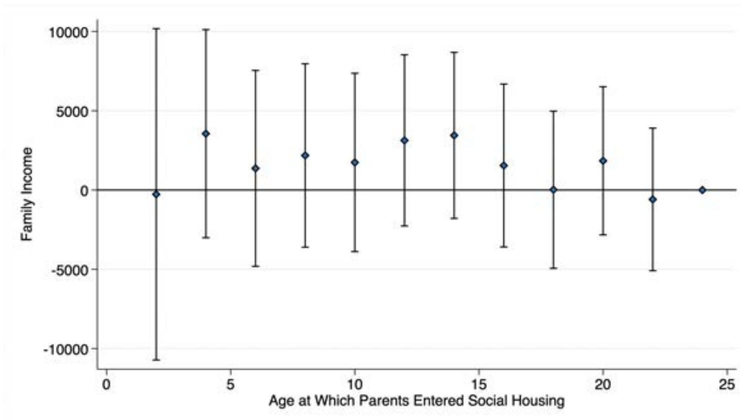
(a) Total Income



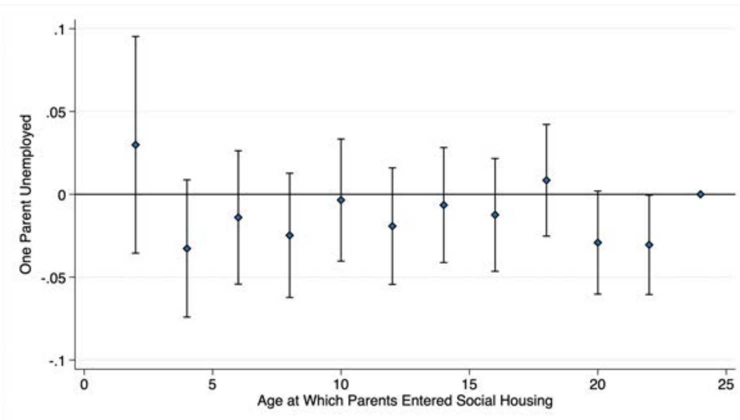
(b) Any Social Assistance Benefits

Notes: Each panel plots the estimate for the coefficients δ_a from Equation 2 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 when parents moved into social housing, regardless of whether the children still live with their parents. Each regression includes cohort, sex, and origin and destination 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.

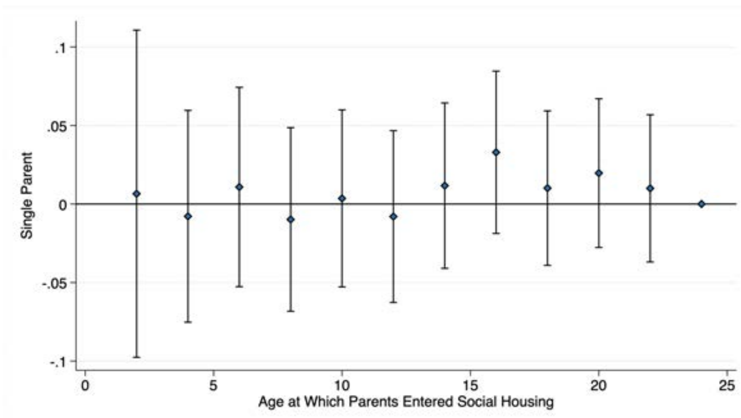
Figure A.6: Family Characteristics by Children's Age at Move



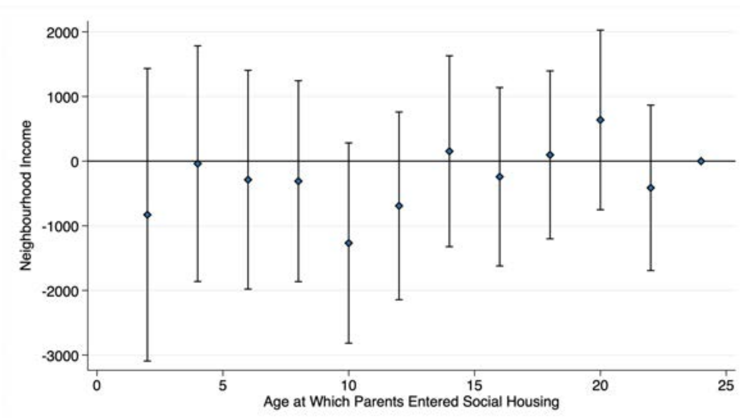
(a) Family Income



(b) One parent unemployed



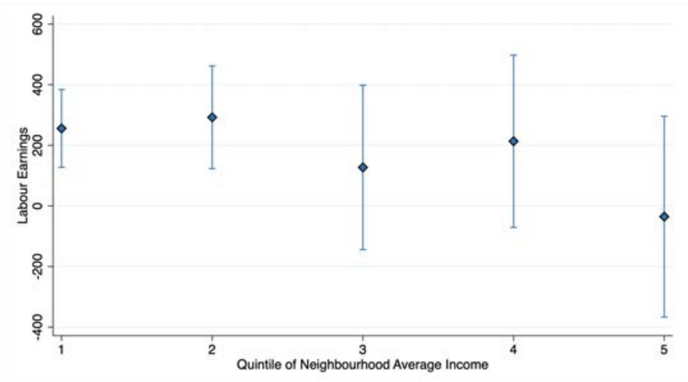
(c) Single Parent Family



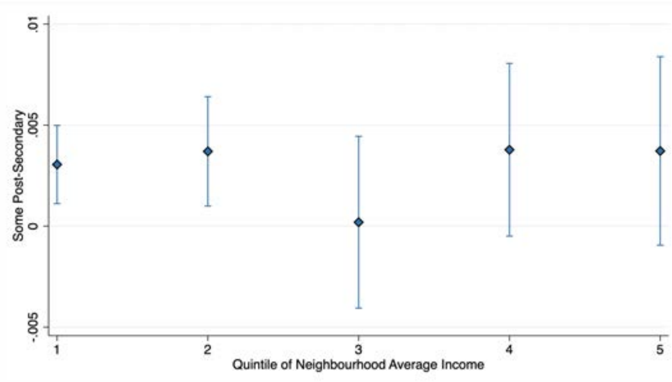
(d) Origin Census Tract Average Earnings

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 2 for a specified pre-event family characteristic. The age at move is based on the child's age when their parents entered social housing, regardless of whether the child still lives with them. Each regression includes cohort and sex fixed effects, and panel (a), (c), and (d) also include origin and destination Census Tract fixed effects. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the family level.

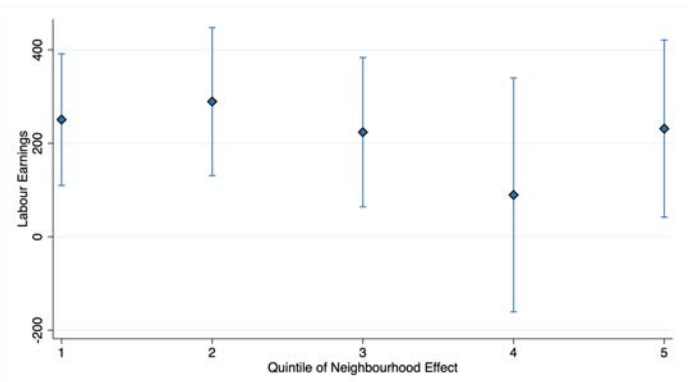
Figure A.7: Treatment Effects on Children by Neighbourhood Characteristics



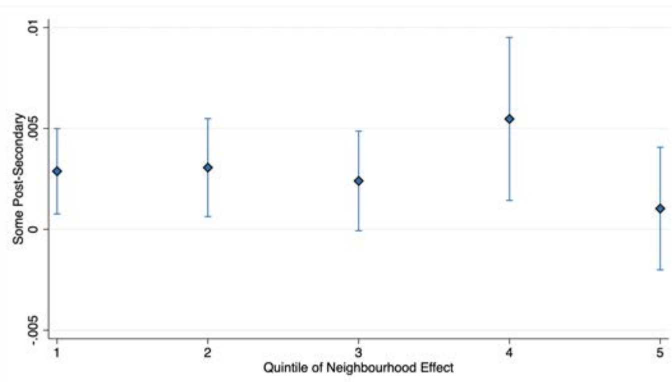
(a) Labour Earnings by CT Average Income



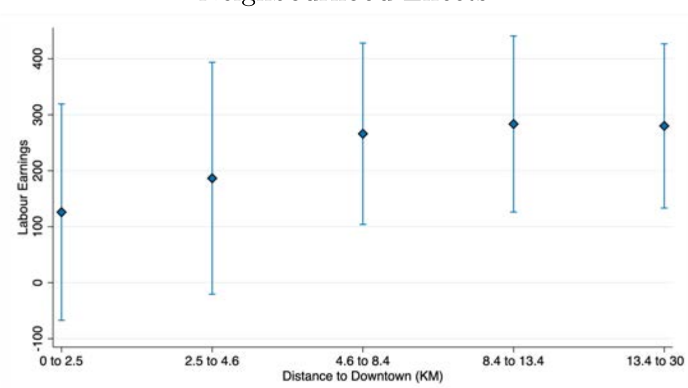
(b) Any Post-Secondary by CT Average Income



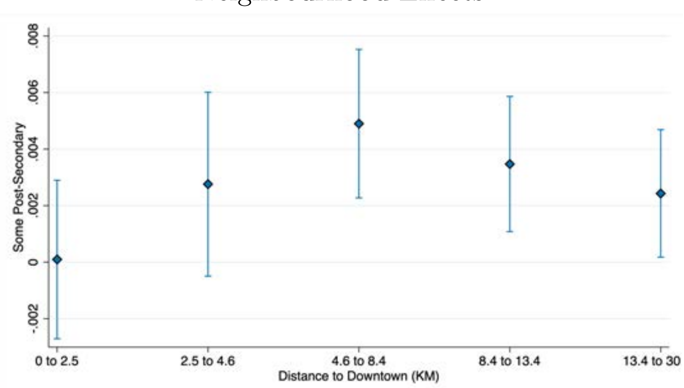
(c) Labour Earnings by Neighbourhood Effects



(d) Any Post-Secondary by Neighbourhood Effects



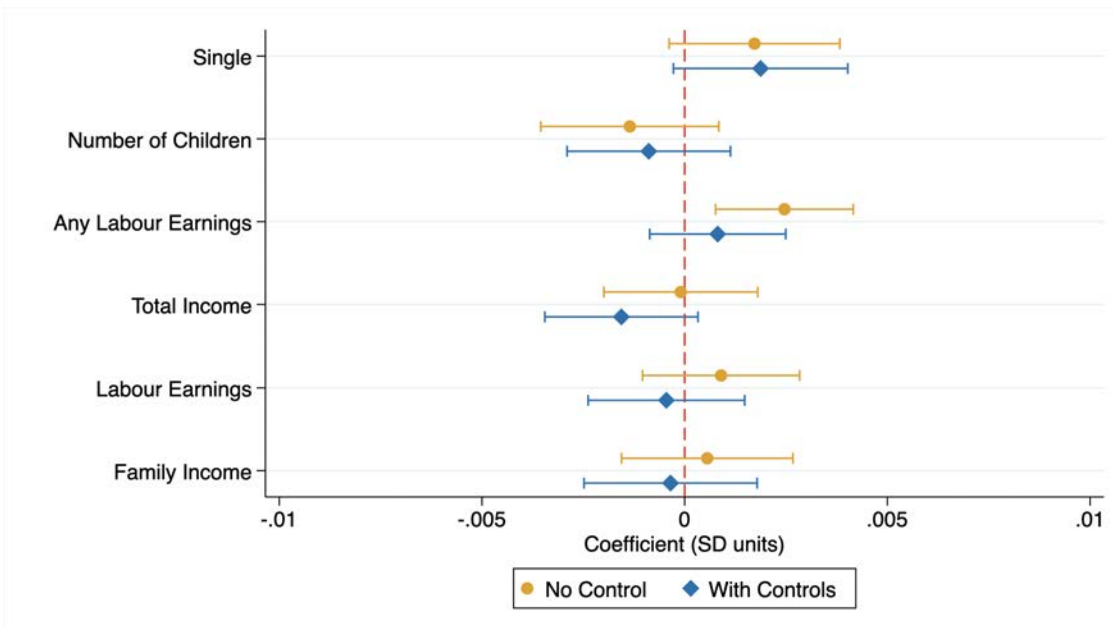
(e) Labour Earnings by Distance to Downtown



(f) Any Post-Secondary by Distance to Downtown

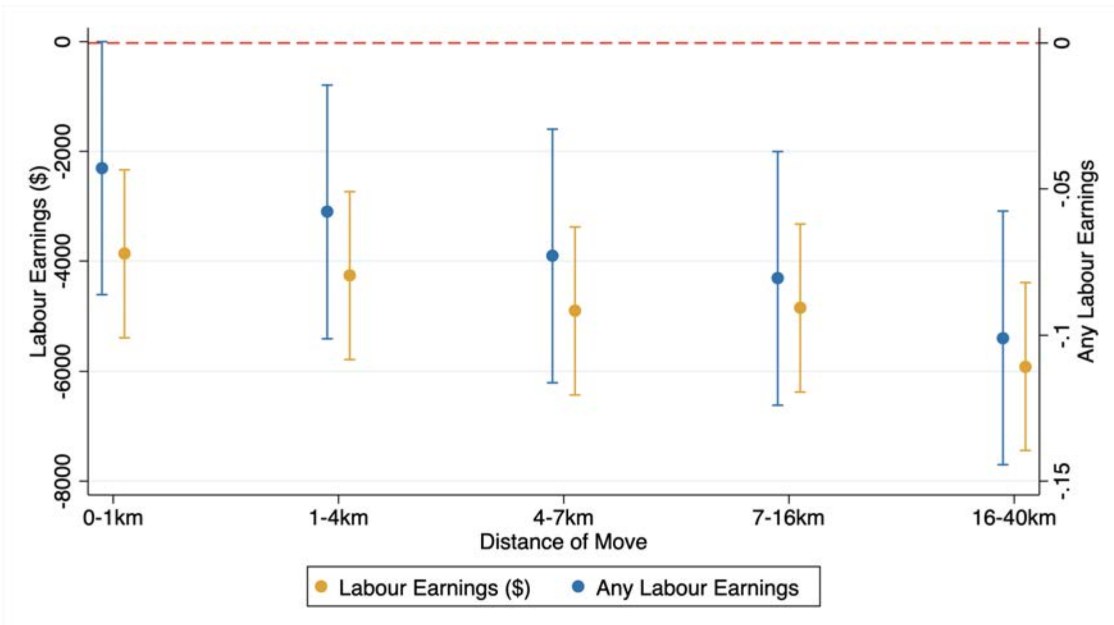
Notes: Each panel reports the slope coefficient on children's outcome by a given neighbourhood characteristic. Each coefficient is the effect of moving into social housing one year earlier. The estimates are from a regression analogous to Equation 3, where years of exposure are interacted with neighbourhood quintile dummies. Each regression includes year of birth, gender and origin and destination neighbourhood. Each point reports 95% confidence intervals clustered at the family level.

Figure A.8: Distance of Move: Balance Tests



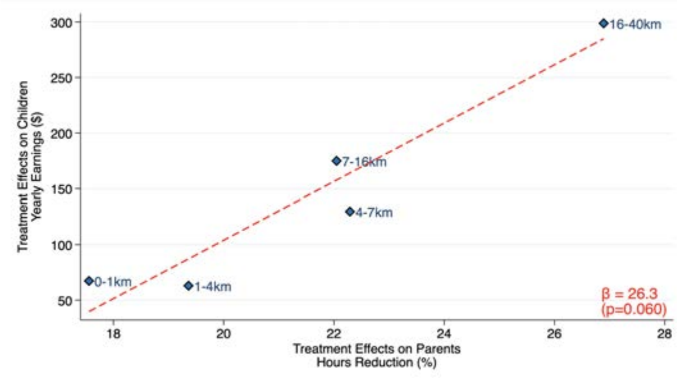
Notes: Each point is an estimate from a regression of a given outcome on the distance of the move, in kilometres. Each point reports 95% confidence intervals clustered at the origin census tract level. Only treated individuals are included. Yellow points represent estimates from univariate regressions, 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.9: Parents' Response by Distance of Move

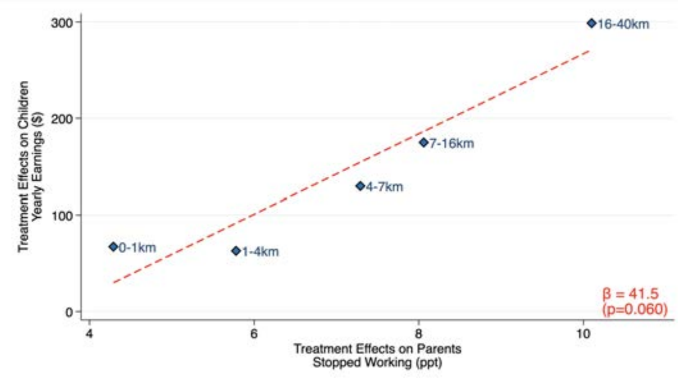


Notes: This Figure shows DiD coefficient on parents' labour market responses across quintiles of the distance of the move. Treated workers are those who moved from a non-social housing postal code to a social housing postal code. Controls individuals are matched using a caliper matching approach described in Section 4.1. 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.

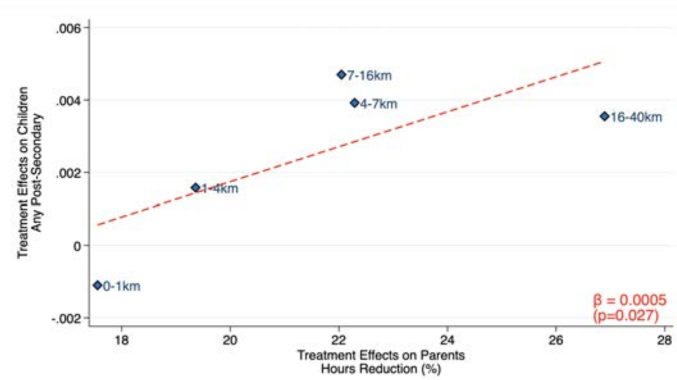
Figure A.10: Adults and Children Treatment over Distance of Move



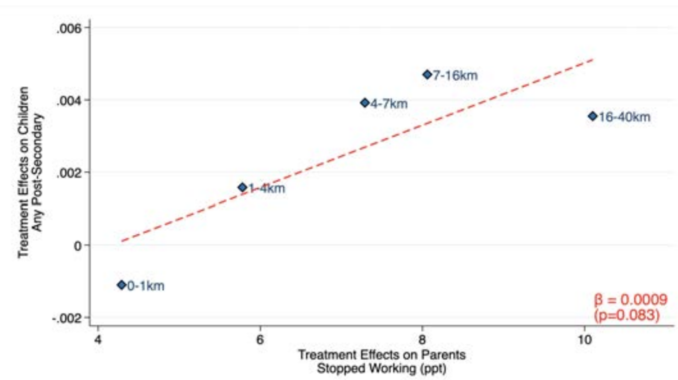
(a) Labour Earnings on
Parents' Hours Reduction



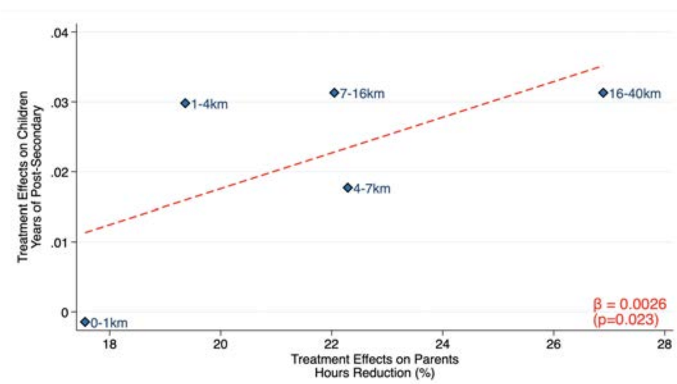
(b) Labour Earnings on
Parents' Participation Reduction



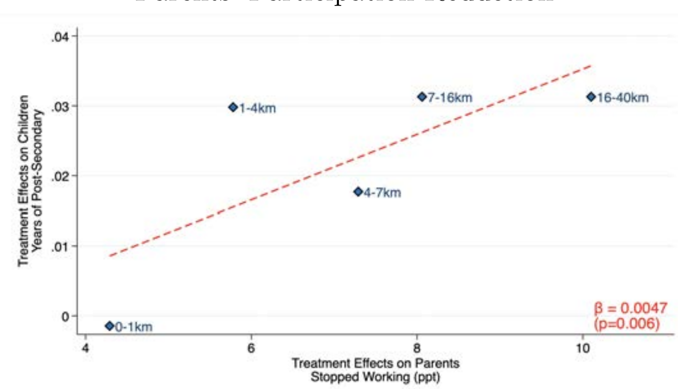
(c) Any Post-Secondary on
Parents' Hours Reduction



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



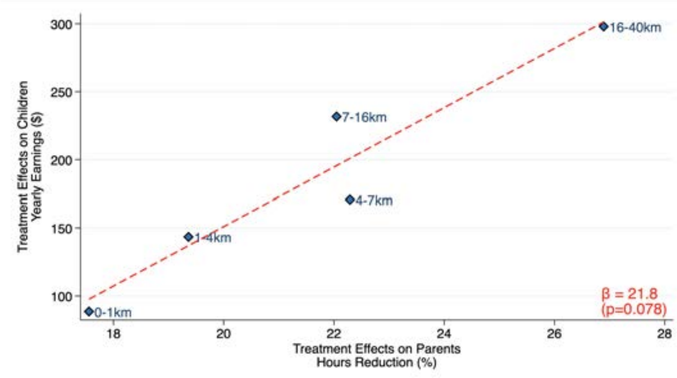
(e) Years of Post-Secondary on
Parents' Hours Reduction



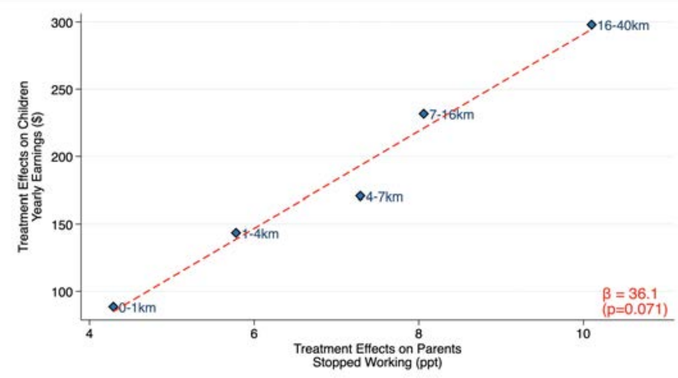
(f) Years of Post-Secondary on
Parents' Participation Reduction

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.

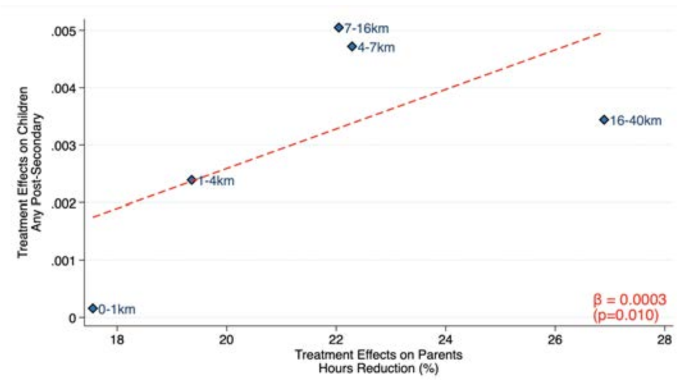
Figure A.11: Adults and Children Treatment over Distance of Move, CT FE



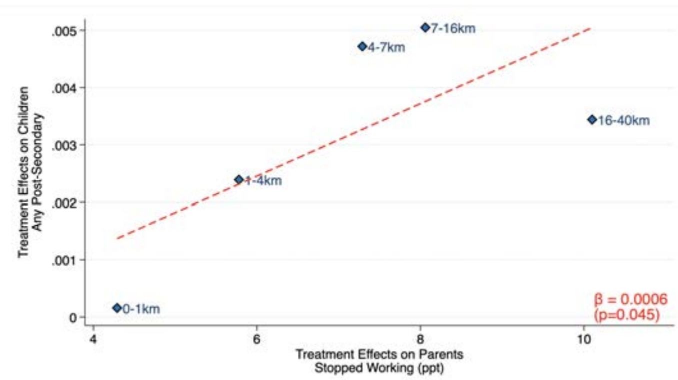
(a) Labour Earnings on Parents' Hours Reduction



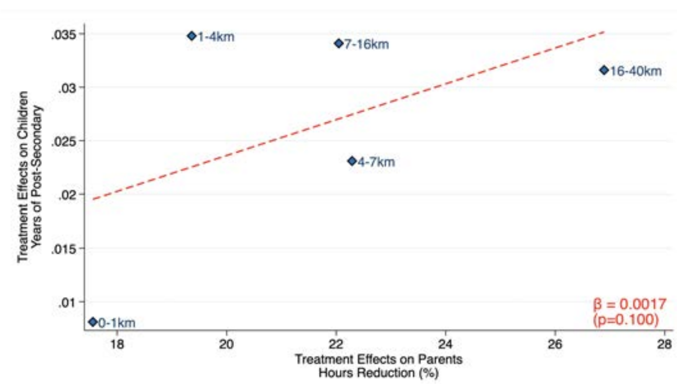
(b) Labour Earnings on Parents' Participation Reduction



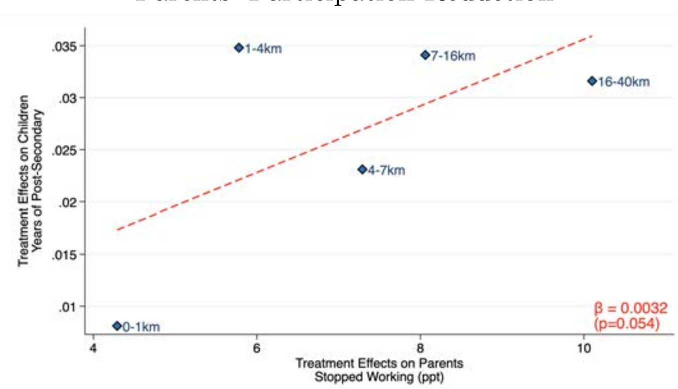
(c) Any Post-Secondary on Parents' Hours Reduction



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



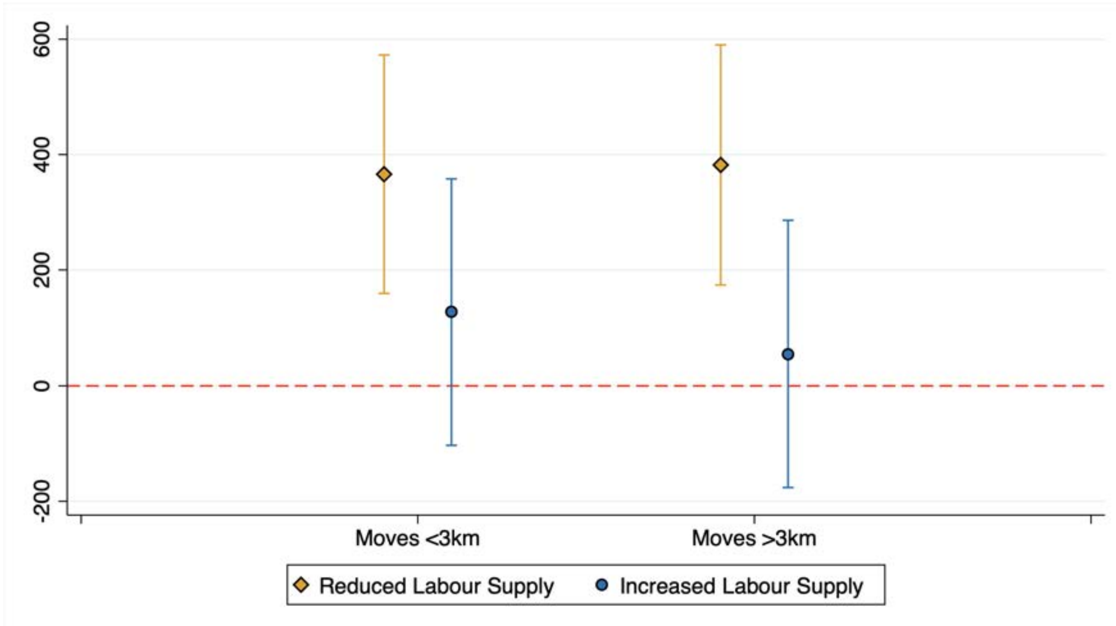
(e) Years of Post-Secondary on Parents' Hours Reduction



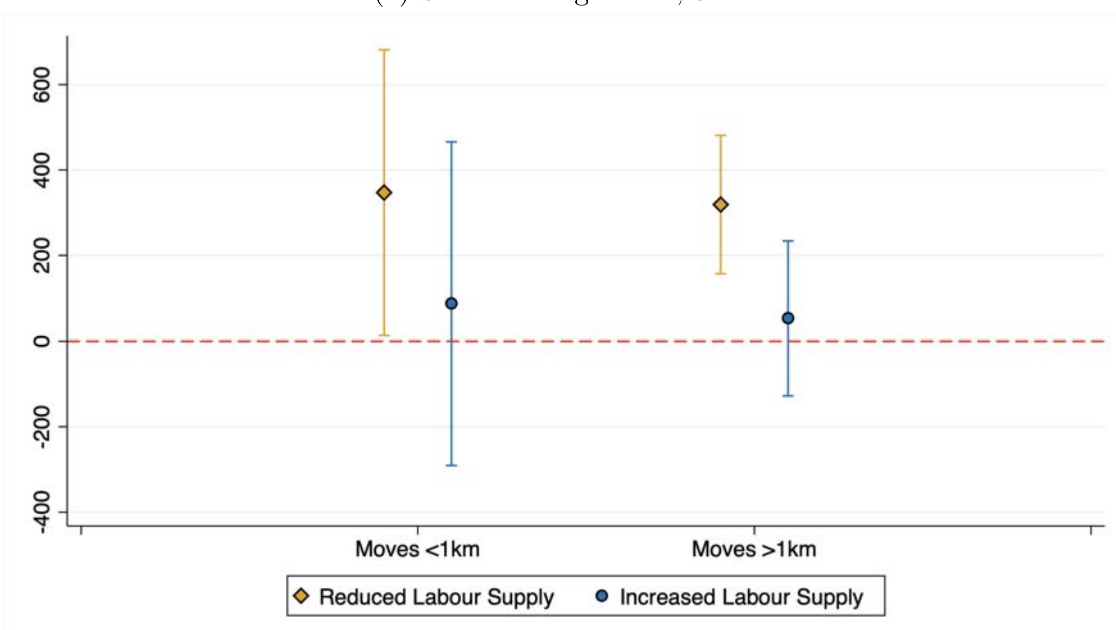
(f) Years of Post-Secondary on Parents' Participation Reduction

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.12: Children Treatment Effects, by Parents' Response and Distance of Moves



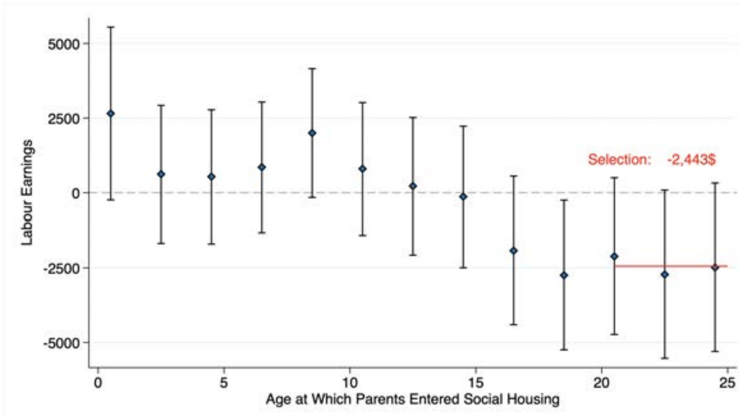
(a) Short vs long moves, 3 km



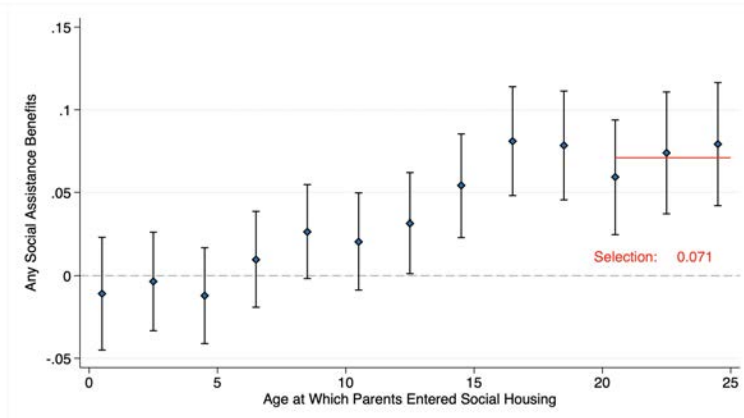
(b) Short vs long moves, 1 km

Notes: This figure plots regression coefficient from Equation 3 for various sample. Each panel reports the treatment effect on children from families who made long or short moves, depending on whether their parents reduced their labour supply. Panel (a) defines long move as > 3km, panel (b) defines it as > 1km.

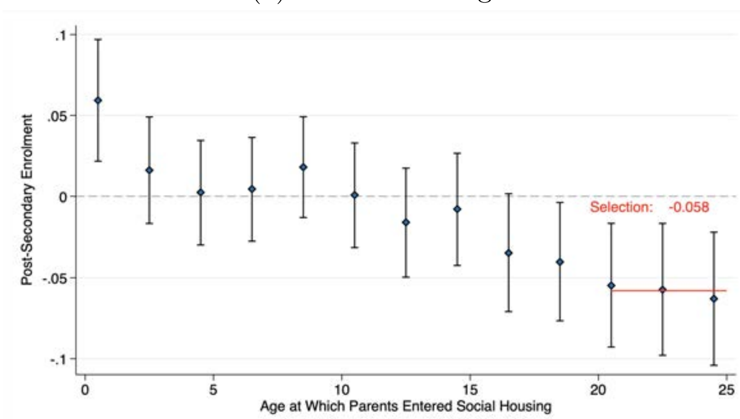
Figure A.13: Selection into Social Housing



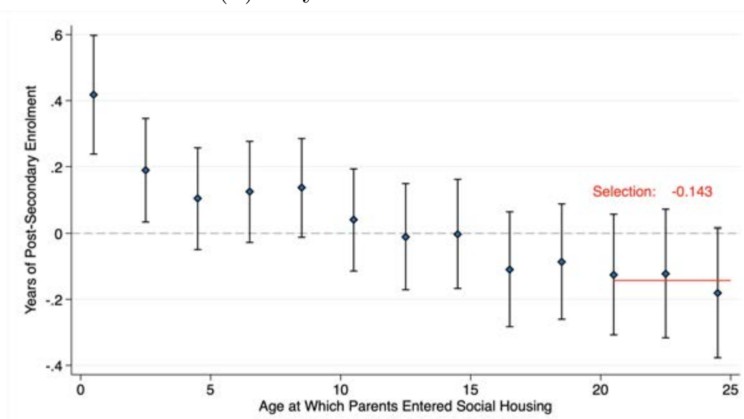
(a) Labour earnings



(b) Any Social Assistance



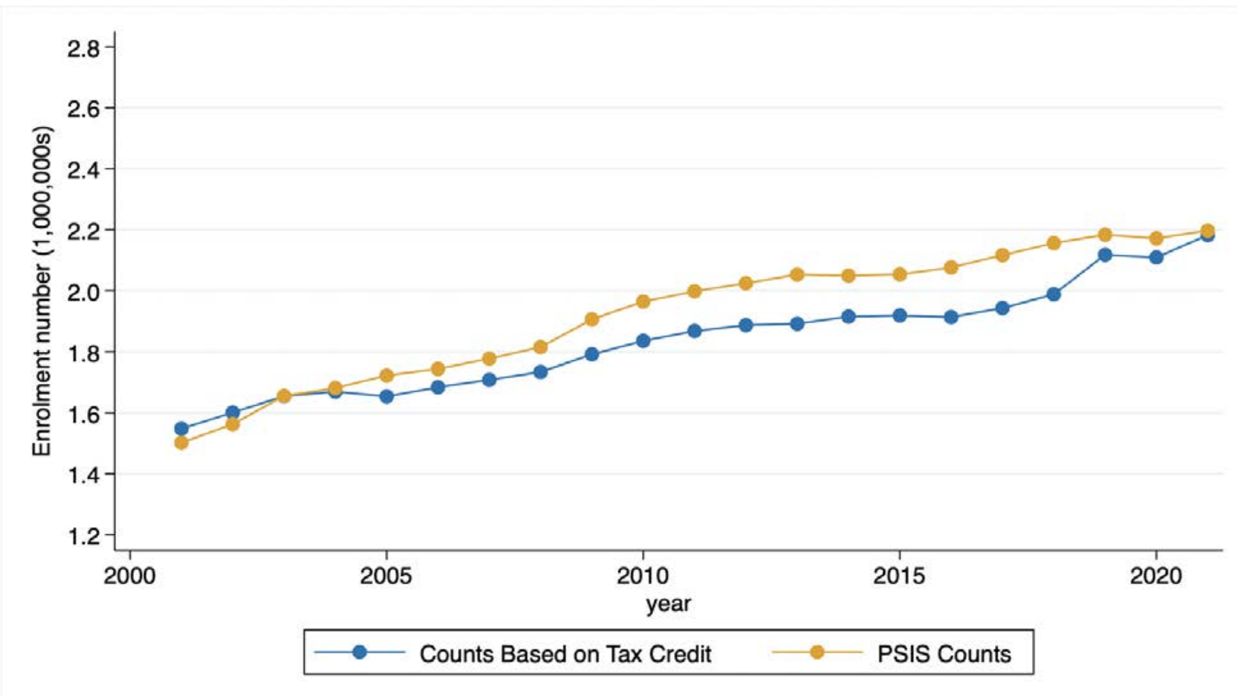
(c) Any Post-Secondary Enrolment



(d) Years of Post-Secondary Enrolment

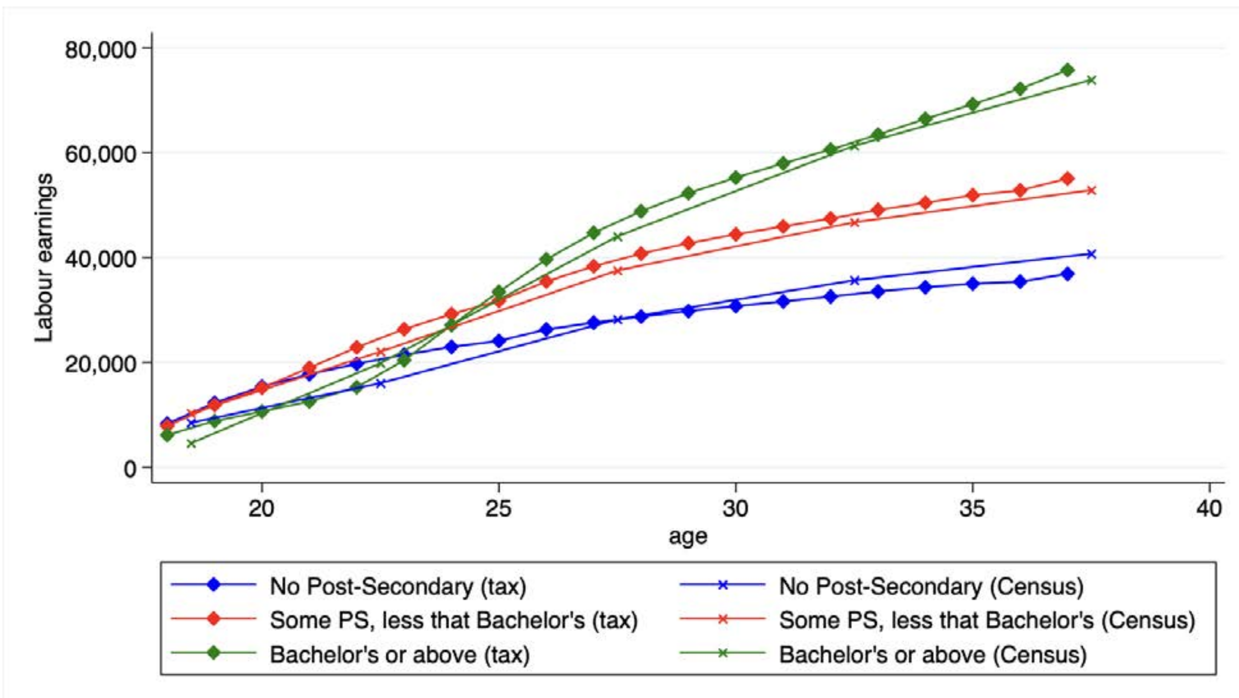
Notes: Each panel reports the coefficients δ_a from Equation F.1 on a specific outcome. Treated children are those whose parents moved into social housing while they were aged 0-25. Control children are those of parents who are matched to treated parents in Section 4.1. Labour earnings and social assistance 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 fixed effects for cohort, sex, and Census Tract of origin. All dollar amounts are in 2021 Canadian dollars. Each point reports 95% confidence intervals clustered at the family level.

Figure A.14: Post-Secondary enrolment



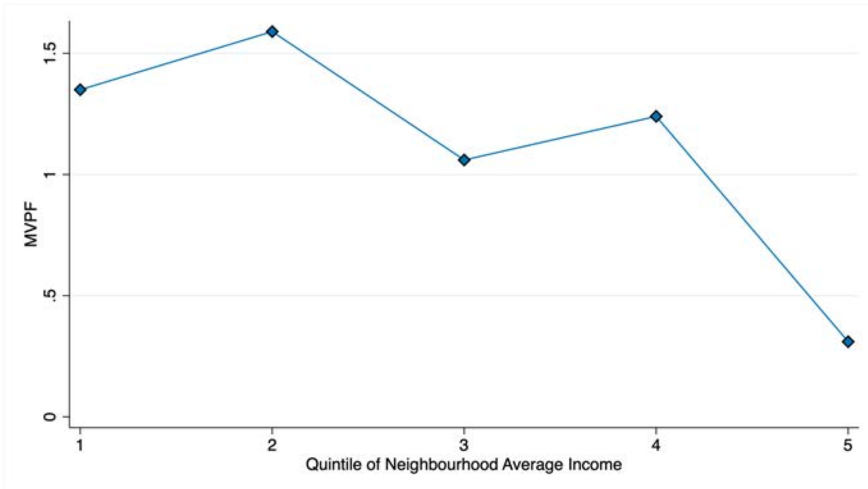
Notes: This figure shows the number of individuals aged 18 to 35 who claimed the post-secondary tuition tax credit (blue line) and PSIS counts (yellow line). Before 2009, the PSIS counts omitted individuals registered in programs related to pre-employment, apprenticeship, basic training or skills upgrading,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

Figure A.15: Education Earnings Profiles

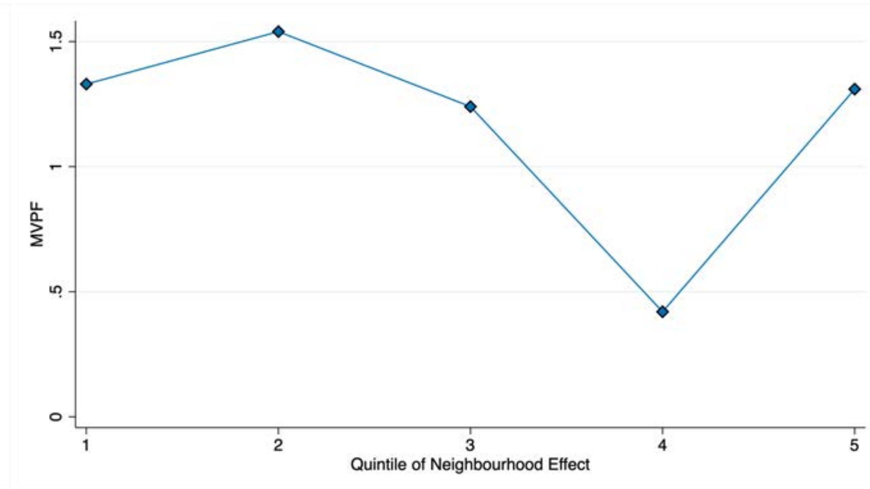


Notes: This figure shows the age-earnings profile for three education groups. Diamonds are from the tax files, and the groups are based on the education proxy. ×s are from the 2021 Census of Population Public Use Microdata File. Census education categories reflect the highest degree completed, whereas the proxy uses only the number of years of enrolment to categorize education level. All dollar amounts are in 2021 Canadian dollars.

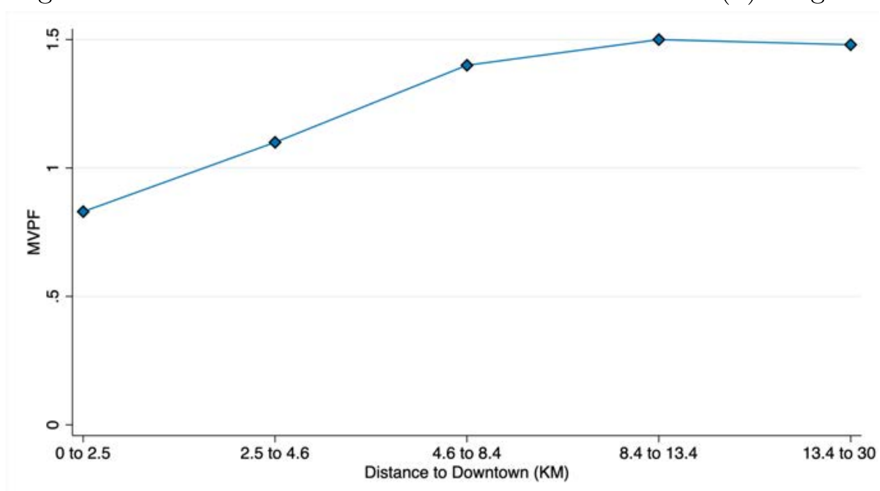
Figure A.16: MVPF by Neighbourhood Characteristics



(a) CT Average Income



(b) Neighbourhood Effects



(c) Distance to Downtown

Notes: Each panel reports MVPF estimates by quintile of neighbourhood characteristics. Each MVPF estimate is calculated using numbers from figures A.7 and A.4.

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 $\times$ 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 $\times$ 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 3. Column 1 reports coefficients from univariate regressions, column 2 includes cohort and sex fixed effects, and column 3 additionally includes origin and destination 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: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3: The effects of work on parental time

	Hours spent on childcare	
	(1)	(2)
Worked	-9.87***	
	(3.02)	
Worked part-time (1-34h)		-5.18
		(4.89)
Worked full-time (35-44h)		-10.67***
		(3.05)
Worked extra hours (45h+)		-13.03***
		(3.29)
N	1,886	1,886

Notes: Each column is a different specification of a regression of hours spent looking after children from own household over labour market participation. Each regression includes controls for age, gender, marital status, gender, immigrant status, education, family income, survey month and a dummy for the presence of a pre-school-aged child. Regressions are weighted using person-level sample weights. Robust standard errors are in parentheses. Level of significance: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4: MVPF by family composition

	Number of adults 1	2
Panel A: linear aggregation		
Number of children		
0	0.52	0.49
1	1.43	1.29
2	4.59	3.68
3	∞	415.96
4	∞	∞
Panel B: square root aggregation		
Number of children		
0	0.52	0.49
1	1.43	1.29
2	2.19	1.92
3	3.17	2.67
4	4.59	3.68
5	6.95	5.14

Notes: Dollar amounts are expressed in real terms (2021 CPI) and represent the effect of one year of social housing. Parents' WTP equals the contemporaneous rent saving. Children's willingness to pay is the net present value of lifetime earnings gains net of transfer reductions, and is computed using a 3% rate of return. Fiscal externalities use a 20% effective tax rate for translating earnings changes into tax revenue.

C Conceptual Framework

This Appendix section introduces a simple time-allocation model in which the agent chooses intended work hours, but actual hours are stochastic around those hours. I compare a market-housing regime with a fixed rent to a social-housing regime with a rent proportional to income. The model highlights that social housing programs of this type differ from other cash or in-kind transfers because multiple channels are at play beyond the simple income transfer. We do not expect the same labour supply responses as those induced by other programs studied in the literature.[14]

Although the model includes uncertainty in realized hours, one could instead include uncertainty in wage or rent (e.g., the probability of being evicted and paying a different rent). Those two alternatives would yield qualitatively similar results, with the uncertain rent case differing slightly because social housing fully insures tenants against rent risk. The focus on hours uncertainty is motivated by evidence suggesting that workers have limited control over their hours worked (Lachowska et al. 2025; Labanca and Pozzoli 2022; Chetty et al. 2011). In practice, this also accounts for the frequent job instability in the population of interest.

Environment and timing. Time endowment is normalized to 1. The parent chooses intended work hours $h \in [0,1]$, but also time investment in the child $t \in [0,1]$, and monetary investment $m \geq 0$. After the choices, the realized hours are

$$\tilde{h} \;=\; h+, \qquad \mathbb{E}[\,|\; h] = 0, \qquad \tilde{h} \in [0,1] \text{ a.s.}$$

Leisure is $\ell = 1-\tilde{h}-t$. Wage $w > 0$. Consumption (C) is the numéraire good. Monetary investment in the child has a price $p_m > 0$. Housing (H) is required: if $H \geq 1$ utility is as below; if $H = 0$ utility is $-\infty$.

Preferences are additively separable and strictly concave:

$$U \;=\; u(C) + v(\ell) + \phi\, s\big(K(t,m)\big),$$

with $u' > 0, u'' < 0,\ v' > 0, v'' < 0,\ s' > 0, s'' \leq 0,\ K_t > 0, K_m > 0,\ K_{tt} \leq 0$, and $K_{mm} \leq 0$. The parameter $\phi > 0$ captures altruism or the weight on child outcomes. Setting $\phi = 0$ leads to a standard labour-leisure model.

[14] For example, the Child Tax Benefits (Milligan and Stabile 2009) and the Universal Child Care Benefit (Schirle 2015) in Canada, and the Food Stamp Program (Hoynes and Schanzenbach 2012), Medicaid (Baicker et al. 2014) and EITC (Eissa and Hoynes 2004) in the US.

Market housing (fixed rent). In the market housing sector, rent is fixed at $R \geq 0$. Consumption is

$$C_M \;=\; w\tilde{h} - R - p_m m \;=\; w(h+\mu) - R - p_m m.$$

The ex-ante problem is

$$\max_{h\in[0,1],\, t\in[0,1-h],\, m\geq 0} \; \mathbb{E}\big[u\big(w(h+\mu) - R - p_m m\big)\big] + v(1-h-t) + \phi\, s\big(K(t,m)\big).$$

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

$$v'(1-h-t) \;=\; w\,\mathbb{E}\big[u'\big(w(h+\mu) - R - p_m m\big)\big]\,, \tag{M-h}$$
$$v'(1-h-t) \;=\; \phi\, s'\big(K(t,m)\big)\, K_t(t,m), \tag{M-t}$$
$$p_m\,\mathbb{E}\big[u'\big(w(h+\mu) - R - p_m m\big)\big] \;=\; \phi\, s'\big(K(t,m)\big)\, K_m(t,m). \tag{M-m}$$

Social housing (proportional rent). Under social housing, rent is a fraction $r \in (0,1)$ of earnings; there is no lump-sum payment. Consumption is

$$C_S \;=\; (1-r)w\tilde{h} - p_m m \;=\; (1-r)w(h+\mu) - p_m m.$$

The ex-ante problem is

$$\max_{h\in[0,1],\, t\in[0,1-h],\, m\geq 0} \; \mathbb{E}\big[u\big((1-r)w(h+\mu) - p_m m\big)\big] + v(1-h-t) + \phi\, s\big(K(t,m)\big),$$

with interior FOCs

$$v'(1-h-t) \;=\; (1-r)w\,\mathbb{E}\big[u'\big((1-r)w(h+\mu) - p_m m\big)\big] \tag{S-h}$$
$$v'(1-h-t) \;=\; \phi\, s'\big(K(t,m)\big)\, K_t(t,m) \tag{S-t}$$
$$p_m\,\mathbb{E}\big[u'\big((1-r)w(h+\mu) - p_m m\big)\big] \;=\; \phi\, s'\big(K(t,m)\big)\, K_m(t,m). \tag{S-m}$$

Comparative statics and mechanisms (with investments). Let $\sigma_\mu^2 = \mathrm{Var}(\mu)$.

1. *Income effect.* At a given (h,t,m),

$$\mathbb{E}[C_S] - \mathbb{E}[C_M] \;=\; (1-r)wh - (wh - R) \;=\; R - rwh.$$

If $R > rwh$ at the relevant choice, social housing raises expected resources, which (by concavity) lowers $\mathbb{E}[u'(\cdot)]$. From (S-m), a lower $\mathbb{E}[u'(\cdot)]$ raises the optimal m. From (S-h) and (S-t) sharing the common $v'(1-h-t)$, the same force tends to increase total non-work time; with interior t and ℓ, this shifts time toward child investment t (and/or leisure).

2. *Substitution effect (inplicit tax on work).* Social housing replaces the fixed rent R by a proportional wedge r, lowering the net return to work hours from w to $(1-r)w$. Comparing (M-h) to (S-h), holding $\mathbb{E}[u'(\cdot)]$ fixed, this pushes toward lower h and thus more time available for ℓ and t.

3. *Insurance effect (risk sharing).* Under market housing, $\mathrm{Var}(C_M) = w^2\sigma_\mu^2$; under social housing, $\mathrm{Var}(C_S) = (1-r)^2 w^2 \sigma_\mu^2 < \mathrm{Var}(C_M)$. With $u'' < 0$, lower consumption risk reduces $\mathbb{E}[u'(\cdot)]$. From (S-m), this increases m. Going from (S-h) to (M-h), the RHS falls, so equilibrium requires a lower $v'(1-h-t)$—achieved by reducing h and/or raising t (since both lower v' via higher ℓ or higher t). The insurance channel, therefore, amplifies shifts toward leisure and child investments when σ_μ^2 is larger. Intuitively, high marginal utility in bad hours states makes the agent choose higher mean hours in case they draft a low μ draw. In the social housing regime, low μ draws are not as detrimental because the agent is partially insured against them.

While ℓ and t go up (all three channels reduce hours worked), the total effect on spending (C and m) is ambiguous. Channels (2) and (3) push C down; channel (1) pushes it up. 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.

C.1 Quantitative Decomposition

To sharpen the interpretation of adults' labour-supply responses, I decompose the total effect into income, substitution, and insurance components. I benchmark the income effect using "pure income" shocks from lottery windfalls and cash-transfer reforms. Estimates of the marginal propensity to earn (MPE) out of unearned income cluster between -0.11 and -0.30 (Imbens et al. 2001; Cesarini et al. 2017; Bengtsson 2012). Applied to the roughly \$3,500 reduction in annual rent payments at entry, this range implies an earnings decline of \$385–\$1,050 (about 3–8 percentage points relative to the pre-entry mean), which is far smaller than the observed total decline of about \$2,400.

To isolate the substitution effect, I use a Frisch elasticity—which captures the response to a change in the net wage holding marginal utility of wealth fixed—of $\mu = 0.15$. This value is a conservative average of micro estimates, excluding larger elasticities identified from top-income tax variation (see Chetty (2012), Table 1, Panels A and B), and aligns with evidence from the housing-voucher context in Jacob and Ludwig (2012). In the rent-geared-to-income (RGI) regime, the after-tax wage falls from w to $(1-r)w$, with $r \in [0.25, 0.30]$. Under a standard intensive-margin approximation,

$$\frac{\Delta h}{h} \approx \varepsilon \cdot \frac{\Delta w}{w} = \varepsilon \cdot (-r),$$

so with $\varepsilon = 0.15$ the substitution-driven reduction is about 3.75 to 4.50 percent. Given the control-group average earnings ($\approx$ \$12,805), this corresponds to an effect of \$480–\$576.

The remaining gap between the total decline and the sum of income and substitution components—roughly \$774 to \$1,535 ($\approx$ 6–12 percentage points)—is naturally attributable to the insurance channel. In the model with stochastic realized hours, proportional rent both flattens the budget set and partially insures against adverse realizations, reducing the payoff to maintaining high intended hours. Using midpoints for illustration ($\varepsilon = 0.15$, $r = 0.275$, and MPE $= -0.20$), the decomposition assigns about \$700 to the pure income effect, \$527 to the pure substitution effect, and \$1,243 to the insurance effect, which together roughly sum to the observed total decline. Under this illustrative split, the insurance channel accounts for half of the total effect (50.3%), while the income and substitution effects account for 28.4% and 21.3%, respectively.

D Data Appendix

D.1 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 gross income in Montréal and 30% 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[15]. I assign this median rent to people not living in social

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

housing.

The CEEDD database includes location information based on the 2021 census definition, whereas Census geographic boundaries change over time. When computing measures of CT-level rent, I transpose the geographic units to the 2021 census 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 consistent geographic units over long periods of time.

D.2 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 link 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 designated 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*) I assign the primary parent's postal code if available; (*ii*) if the primary parent did not fill out 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.

E 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 A.14 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.[16] 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 credits they are eligible for.

I explore the return to education based on my post-secondary education measure. Figure A.15 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[17]. I benchmark the earnings progression of the three groups using data from the 2021 Canadian Census public-use microdata files. My proxy reproduces patterns from the Census data. This highlights the importance of measuring tax credit

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

[17] 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.

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 premium of 26.7 log points for some PS and 49.4 log points for a bachelor's degree equivalent. Boudarbat et al. (2010) find that these premiums are approximately 21 log points and 45 log points, respectively, using 2005 data (see Boudarbat et al. (2010), Figure 3). Note that these are relative to high school completion, whereas my numbers are relative to no post-secondary education (regardless of high school completion), which explains why my estimates are slightly larger.

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*.

F 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 the generalizability of the results.

To quantify selection, I estimate a modified version of Equation 2 that includes all children of matched parents in Section 4.1. 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.

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

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.13 exhibits a general shape that is similar to that seen in Figure 4, but without the normalization to zero. For each panel, I include a selection level based on the average coefficient for moves (or placebo moves) that occurred after age 20. Children whose parents enter social housing are negatively selected, relative to other disadvantaged children. 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 fewer 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.

G Two-way Fixed-Effect model

To retrieve firm pay premiums, 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[18]. I also restrict the AKM estimation to ”prime age” workers, aged 25-59, to reduce variation in hours associated with part-time work during school and the pre-retirement decline in labour market attachment. I use the full 2001-2021 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}):

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

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 G.1 is that workers do not select their em-

[18] 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.

ployer 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.[19]

H 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 various ages. This strategy does not require the moving decision to be random, but rather that the timing of moves is orthogonal to a child's potential outcome across families with the same sequence 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 and Hendren (2018).

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

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:

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

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

[19] 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).

third neighbourhood in which the lived while aged 0 to 18. For one-time movers, $m_2 = 18$.

I estimate equation H.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.

I Time Use Survey

A limitation of my data is that I don't observe whether the reduction in hours worked actually translates into more parental time or better parenting practices. To provide a benchmark on the number of hours spent on childcare by labour market participation status, I use public-use microdata files from the 2022 Canadian Time Use Survey (TUS). The Time Use Survey is part of the General Social Survey program and is conducted every five years. The TUS is the only Statistics Canada survey that measures the time spent on different activities, including unpaid work and care for children. It is also the primary source of data for measuring gender inequalities, specifically regarding the various types of unpaid work.

The TUS PUMF contains 12,336 respondents across all 10 Canadian provinces, of whom 2,177 have at least one child aged 14 or younger. It includes demographic information on age, gender, marital status, gender, immigrant status, education, family income, as well as time use on various topics. Crucially, it provides information on the number of hours spent on looking after children from own household. Most continuous variables are discretized for confidentiality reasons, but the number of hours of childcare is not.

I run a regression of hours on childcare at home over labour market participation measures. I control for an array of demographic variables. B.3 shows regression estimates. Column 1 shows that individuals who work spend, on average, 9.9 hours less on childcare than individuals who are observationally similar to them. Column 2 includes intensive measures of work, categorized into three levels: part-time, full-time, and above full-time. The discretized variables of the PUMF data guide those categories. Individuals who work part-time spend 5.2 hours less on childcare, although this coefficient is not statistically different from zero. Full-time workers and those who work more than 45 hours a week spend 10.7 and 13 hours less with their child, respectively.

J MVPF

J.1 Direct Cost

In Section 7, I compute the Marginal Value of Public Funds for the Toronto program for a family of one adult and one child. I retrieve direct program cost from TCHC's 2004 Annual Review[20]. I use the 2004 report because it is in the middle of my sample period. I then deflate it to 2021 dollars to match my treatment effects units.

J.2 Family structure

In the main text, I provide an MVPF estimate for a family of one parent and one child. However, it is possible to scale the willingness to pay and the fiscal externalities by the number of family members. Table B.4 shows the MVPF by the number of parents and children in the family. The most straightforward way to sum the benefits of children linearly. However, that might not be a realistic way to compute benefits if the treatment effects come from parental time investment. If the marginal productivity of time is decreasing, one could instead scale the benefits by the square root of the number of children. Table B.4 presents MVPF using the two aggregation approaches. When summing children benefit linearly, the MVPF reaches infinity quickly because the policy pays for itself with just 3 or 4 children.

J.3 MVPF for Montréal's program

I estimate the MVPF using parameters of Montréal's program. The two changes are the direct cost and the marginal tax rate. For workers earning less than $50,000 the tax rate is 26% (compared to 20% in Ontario). The program costs are also higher with $7,435 in 2021 dollars[21]. Using this parameter, the MVPF for a one-parent one-child family is 1.09.

J.4 MVPF by Neighbourhood Characteristics

I use the treatment effect estimates from Figures A.7 and A.4 to compute MVPF values for neighbourhood types. Figure A.16 shows MVPF estimates by quintile of CT average income, Neighbourhood effects, and Distance to downtown. The MVPF is maximized farther

[20] *Annual Review 2004*, https://torontohousing.ca/sites/default/files/2023-03/toronto_community_housing_annual_review_2004.pdf

[21] *Rapport Annuel 2009*, https://www.omhm.qc.ca/sites/default/files/publications/Rapport_annuel2009.pdf

from downtown, in a neighbourhood with a second quintile of neighbourhood effects and an average income. A caveat in this analysis is that it assumes the government's direct costs are constant across neighbourhoods.

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