How Infant Health Shapes Maternal Earnings *

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Abstract

This paper examines how infant health complications at birth measured by low birth weight and prematurity affect maternal labor market outcomes over the first seven years of a child's life. Using Canadian administrative data linking birth certificates to annual tax records for 640,500 first births between 2006 and 2015, I compare mothers of unhealthy infants—those born with low birth weight or preterm—to carefully matched mothers of healthy infants. I find that mothers of unhealthy infants experience a persistent 2.6% penalty in labor force income and an 8.7% increase in the probability of zero earnings, representing approximately 10% of the total motherhood penalty. The penalty is particularly pronounced among low-income mothers, who face earnings losses 3.5 times larger than high-income mothers, and among mothers with higher-parity births, who are twice as likely to exit the labor market entirely compared to first-time mothers. Evidence from disability benefit receipt, maternity leave exhaustion, and partnership stability argues against benefit-induced work reduction, fertility adjustments, and partnership dissolution as primary drivers, pointing instead to ongoing caregiving demands from persistent child health vulnerabilities.

JEL Classification: J13, J16, J22, I14, I18

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1 Introduction

Economic inequality between men and women is strongly shaped by the disruptions associated with parenthood (Kleven et al., 2019a). A large literature quantifies the "child penalty" in women's labor market outcomes (Lundborg et al., 2017; Kleven et al., 2021) and evaluates the role of family policies such as parental leave and subsidized childcare (Baker et al., 2008; Rossin-Slater et al., 2013; Patnaik, 2019). However, less attention has been paid to how heterogeneity in infant health at birth contributes to this penalty. Yet this source of heterogeneity could offer insights into alternative ways to reduce the economic burden of parenthood. For example, a large body of research shows that well-designed prenatal health interventions can improve birth outcomes, including nutrition programs like the U.S. Supplemental Nutrition Program for Women, Infants, and Children (WIC) (Currie and Rajani, 2015) or targeted cash transfers and prenatal care incentives (Almond et al., 2011; Almond and Currie, 2011). If poor infant health imposes persistent caregiving demands that lower maternal earnings, then improving health at birth could be a complementary lever for reducing the long-run economic disruption of parenthood for women.

This paper focuses on whether early-life health endowments—specifically low birth weight (below 2,500 grams) and preterm birth (before 37 weeks)—affect mothers' earnings trajectories and intra-household income composition in the years following childbirth. I exploit administrative data covering the universe of births in Canada from 2006 to 2015, linked to tax records tracking mothers' labor market outcomes for up to seven years after birth. To estimate the effects of low birth weight and preterm birth, I implement a two-step matching approach that produces mothers who are highly comparable in key pre-birth characteristics such as fertility history, pregnancy complications, receipt of health-related benefits, and labor market trajectories. I then compare income trajectories between treated mothers and mothers in the matched comparison group around the child's birth using an event study design.

A direct comparison between mothers with unhealthy infants (low birth weight or preterm) and mothers in the matched control group shows that the former fall behind on all income variables considered. While there is no gap between the two groups in the pre-birth years, mothers of unhealthy infants have on average 2.6% lower labor force earnings, 1.9% lower total income, and 2.2% lower family income in the seven years following birth. Further decomposition shows that this infant health penalty represents an additional 10% decrease in labor force earnings compared to the motherhood penalty that all mothers are facing—meaning that the estimated infant health penalty represents roughly one-tenth of the overall earnings decline experienced by mothers of unhealthy infants relative to matched mothers of healthier infants, beyond the standard earnings decline associated with parenthood.

Both the intensive and extensive margins of labor supply are at play. Mothers of unhealthy infants are 8.7% more likely to have zero earnings. The average infant health penalty is in the same range when restricting the sample to mothers with positive earnings, suggesting that adjustments along the intensive margin also contribute to the observed earnings losses. However, this pattern differs markedly by parity. Among first-time mothers, there is no effect on the probability of zero earnings, suggesting that the penalty operates primarily through the intensive margin (reduced hours or earnings while maintaining employment). In contrast, among mothers with higher-parity births, the extensive margin effect is pronounced and statistically significant. This suggests that managing the care needs of an unhealthy infant alongside existing children creates binding time constraints that push mothers toward complete labor market exit.

The penalty has important consequences for household wellbeing and economic inequality. At the household level, residual family income declines modestly and does not fully offset mothers' earnings losses. Because partner earnings are not directly observed, these estimates should be interpreted with caution. I find limited evidence that mothers' share of family income declines after an infant health shock, with statistically significant differences emerging only in years 5 and 6 after birth. An important limitation is that the analysis relies on an inferred measure of partner earnings constructed as the difference between family income and maternal labor force income, which may include other income sources and assumes constant household composition, limiting precision in decomposing household responses. The penalty also translates into meaningful increases in economic vulnerability: mothers of unhealthy infants are more likely to fall into the bottom quartile of the earnings distribution and experience marginally significant increases in poverty rates, particularly around school entry (year 5 post-birth). Critically, these distributional effects are highly concentrated among low pre-birth income mothers. Mothers in the bottom income quartile experience extensive margin penalties nearly double the sample average—while high-income mothers show essentially zero labor market exit, adjusting exclusively along the intensive margin.

Several pieces of evidence help interpret the persistent penalty. Mothers of unhealthy infants experience large and sustained increases in disability-related transfers: child disability benefits receipt rise by 60%, dependent disability tax credits by 50%, and mothers themselves are 50% more likely to claim their own disability credit. These patterns indicate ongoing health challenges extending beyond infancy, with spillovers to maternal health. However, benefit-induced work reduction is unlikely to fully explain the earnings penalty: the transfers are not means-tested, total family income declines alongside labor earnings, and controlling for benefit receipt does not attenuate the estimated effect. Alternative mechanisms related to fertility adjustments (birth spacing and family size) and partnership dissolution receive less support in the data. Taken together, the evidence is consistent with the interpretation that caregiving demands associated with children's ongoing health needs contribute to the observed earnings penalty.

These findings contribute primarily to the literature that looks at the impact of children's health on parents' outcomes using event study designs (Eriksen et al., 2021; Breivik and Costa-Ramón, 2024; Vaalavuo et al., 2023; Adhvaryu et al., 2023). Most of this evidence examines health shocks that arrive during childhood—such as cancer diagnoses, accidents, or sudden illnesses—rather than conditions present at birth. A smaller set of papers focuses on conditions that coincide with birth, including Down syndrome (Einav et al., 2025), cerebral palsy, and other disabilities (Gunnsteinsson and Steingrimsdottir, 2019; Chen et al., 2023; Cheung et al., 2025). These conditions share two important features that distinguish them from the health endowments studied in this paper. First, they are severe and often screened prenatally, meaning that observed births predominantly reflect mothers who chose not to terminate their pregnancies—a selected sample with potentially different labor supply preferences or family resources. Second, these conditions are relatively rare: for instance, only 0.33% to 0.87% of children were affected in the sample of Gunnsteinsson and Steingrimsdottir (2019), whereas more than 7% of births in Canada each year are low birth weight or preterm, similar to rates across OECD countries.[1] The distinction between severe, screened conditions and the more common health endowments examined here has implications for both external validity and interpretation. My focus on low birth weight and prematurity—conditions typically not detectable through prenatal screening that affect a much larger share of families—provides evidence on health shocks that are less selected and more generalizable.

Moreover, few existing studies benchmark their estimated child health effects against the well-documented child penalties that all mothers experience. This gap in the literature makes it difficult to assess whether the observed earnings declines represent an *additional* penalty due to health complications or simply reflect the standard costs of childrearing. A notable exception is Einav et al. (2025), who find that mothers of children with Down syndrome experience an incremental effect on labor market income but have overall higher total income due to government benefits in Sweden. I contribute by explicitly decomposing the total effect into the child penalty and the incremental infant health penalty, clarifying that infant health imposes costs beyond standard childrearing. In addition, my findings show that mothers of low birth weight or preterm infants continue to fall behind their counterparts even with higher disability transfers—contrasting with the context of Down syndrome, where transfers fully compensate. This suggests that disability programs may only partially offset the economic consequences associated with poorer infant health: the broader spectrum of health complications captured by low birth weight and prematurity may not qualify for the most generous programs designed for severe, diagnosed conditions.

Finally, I contribute to understanding how health shocks interact with economic inequality and their implications for intergenerational transmission. The finding that the infant health penalty is concentrated among low-income families—who experience substantial labor market exit (2.8 percentage points) while high-income families show none—reveals how health shocks can amplify existing economic disparities. This income gradient is consistent with the idea that families differ in their ability to adapt to additional caregiving demands. High-income families may have greater access to flexible work arrangements and specialized childcare, whereas lower-income mothers may face more constraints in adjusting their labor supply. In addition, a large literature shows that low birth weight and preterm birth have persistent negative effects on cognitive development (Figlio et al., 2014), educational attainment (Black et al., 2007), and adult earnings (Currie and Moretti, 2007). My results suggest one channel through which these effects may be generated or amplified: maternal income losses that reduce family resources precisely during critical developmental periods. The suggestive increase in poverty rates around school entry (year 5 post-birth)—when children with early health complications are transitioning to formal education—is noteworthy. This reduction in available resources may represent one channel through which early-life health disadvantages persist over time, particularly if fewer economic resources are available during important developmental periods. If mothers of unhealthy infants experience persistent income losses concentrated among already-disadvantaged families, and if these income shocks affect investments in children (Almond et al., 2018), then health complications at birth may set children on divergent trajectories. The combination of poorer infant health, larger maternal employment responses, and lower baseline resources may contribute to the persistence of inequalities over

[1] Preterm birth rates range from 5-10% across OECD countries, with the United States at 10.2% and most European countries between 5-7% (OECD Health Statistics 2024). Low birth weight rates are typically 6-8% in developed countries.

the life cycle and across generations.

The remainder of the paper proceeds as follows. Section 2 describes the Canadian institutional context and data sources. Section 3 presents the empirical framework, including the matching procedure and event study specification. Section 4 reports results on the infant health penalty. Section 5 investigates mechanisms. Section 6 presents robustness checks. Section 7 concludes.

2 Context and Data

2.1 Institutional setting

Canada offers generous family support relative to other OECD countries, with programs varying by province. All provinces provide maternity leave (15 weeks at 55% wage replacement) and parental leave (additional 10 weeks shared), though Quebec introduced enhanced benefits through the Quebec Parental Insurance Plan (QPIP) in 2006, including higher replacement rates and dedicated paternity leave (Patnaik, 2019). Quebec also implemented universal subsidized childcare in 2000, reducing costs to $5 per day for children aged 0-5, which increased maternal labor force participation and reduced child penalties for users (Haeck et al., 2015; Karademir et al., 2026). However, whether these comprehensive supports mitigate the infant health penalty is unclear, as children with medical needs may require specialized care not available through standard programs.

Canada's disability support system operates through two main programs relevant to this study. The Disability Tax Credit (DTC) is a non-refundable tax credit available to individuals with severe and prolonged impairments lasting at least 12 months, certified by a medical practitioner. Qualifying conditions include marked restrictions in walking, dressing, feeding, mental functions, hearing, speaking, vision, or bowel/bladder functions. Critically, the DTC is *not* means-tested—eligibility depends solely on medical criteria, not income or employment status. The Child Disability Benefit (CDB) is a monthly payment for families with children under 18 who qualify for the DTC. Eligible conditions include autism spectrum disorder, cerebral palsy, Down syndrome, severe asthma, Type 1 diabetes, epilepsy, severe learning disabilities, and significant developmental delays. While the CDB is income-tested (integrated with the Canada Child Benefit and phasing out at high incomes), the underlying DTC eligibility remains purely medical. I use DTC receipt (for mothers) and CDB receipt (for children) as proxies for health shocks. Because the adult DTC is not means-tested, changes in mothers' DTC receipt over time likely reflect genuine health deteriorations rather than strategic behavior. For the CDB, the means-testing could theoretically create work disincentives, but this concern is mitigated because: (1) the child must already qualify for the DTC based solely on medical criteria, and (2) the phase-out affects primarily high-income families less likely to adjust labor supply for modest benefits. I therefore interpret CDB receipt primarily as an indicator of severe childhood disability.

2.2 Data sources and sample construction

Data Sources. This study uses a Statistics Canada data integration project that links births between 2006 and 2015 to mothers' administrative records, including annual tax files and postsecondary education records, through maternal and child identifiers.[2]

The data include birth certificates for children born in Canada between 2006 and 2015. These files contain information on birth outcomes, including birth weight in grams and gestational age in weeks, as well as basic parental characteristics such as marital status (married, single, or common-law), country of birth, age, number of previous live births, and previous stillbirths. Unlike U.S. studies that use restricted birth certificates and include maternal education (e.g., Currie and Moretti, 2007), maternal completed education is not directly available here. Instead, the project includes postsecondary records for mothers in the sample. A key caveat is that mothers not observed in these records may either have no postsecondary education or have postsecondary education that is not captured in the linkage, for example because they studied outside Canada or attended private Canadian institutions not included in the administrative data.[3]

I define the treatment variable based on infant health at birth. An infant is classified as *unhealthy* if born with low birth weight (below 2,500 grams) or preterm (before 37 weeks of gestation). These thresholds follow the World Health Organization's International Classification of Diseases (ICD-10) definitions (World Health Organization, 2004). Low birth weight and prematurity often co-occur and share similar caregiving demands, as both are associated with increased risk of respiratory problems, feeding difficulties, developmental delays, and long-term health complications requiring ongoing medical attention and parental care (Saigal and Doyle, 2008).

Through mothers' identifiers, I link births to mothers' annual tax files, which provide comprehensive income variables collected by the Canada Revenue Agency (CRA). These tax records are organized through the T1 Family File (T1FF), in which Statistics Canada defines a family unit as an individual or couple—married or common-law—living together with their children (if applicable). Common-law partnerships are recognized for tax purposes after 12 consecutive months of cohabitation or upon having a child together. These variables include individual labor market income, defined as wages, salaries, and self-employment income; the mother's total income, defined as income from all sources (e.g., labor earnings, employment insurance, social assistance, disability benefits, investment income, and other taxable income); and family income, defined as the sum of total income across all members of the tax family.[4] Because the linkage is at the mother level, partners' income is not identified separately. I therefore construct an inferred partner-income measure as family income minus the mother's total income. Although partners are not directly observed, the CRA indicates whether the tax family is classified as a dual-earner family (ie, two partners). I use this information in the household analysis (in Section 4.3) by restricting the sample to dual-earner families (or couple family) with family size not exceeding four. This restriction is intended to focus on typical nuclear families.

Information on maternity leave take-up and duration comes from the federal Employment Insurance Benefit System (EISB), which manages parental leave benefits for all provinces except Quebec.[5] The EISB

[2] The project is called *Impact of Preterm Birth on Socioeconomic and Educational Outcomes (IPB)*. This data integration project also includes linkages for mothers of an older cohort (births 1983–1996) as well as long-term follow-up of the children. For this study, I focus on the 2006–2015 cohort, which offers superior linkage accuracy. The project is available on request from a Statistics Canada Research Data Center.

[3] However, a large proportion of mothers could not be linked to the postsecondary data for unknown reasons. I take this into account when constructing my comparison group.

[4] Information on occupation is not available in this version of the tax file.

[5] Quebec's parental leave is managed separately under the Quebec Parental Insurance Plan (QPIP) and is not included in the EISB data. The analysis of maternity leave patterns therefore excludes Quebec mothers.

data include weeks of leave taken and benefit amounts received, allowing me to observe both the intensive margin (length of leave) and whether mothers exhausted their full entitlement (60 weeks).

Sample construction. From nearly 3,800,000 births to 2,500,000 different mothers in Canada between 2006 and 2015 with non-missing birth outcomes, I restrict the sample to mothers aged 20–45. This age restriction excludes very young mothers, who are more likely to be students or to have transitory income. Almost 2,400,000 mothers meet this criterion. I then restrict the sample to mothers with only one observed birth during 2006–2015 and whose child survives until 2018, so that the analysis more closely isolates the influence of the focal birth observed during the study period. From nearly 1,400,000 mother-child pairs meeting these restrictions, I finally limit the sample to mothers who have a tax record in each year from 2002 to 2018. This restriction focuses the analysis on mothers with consistent tax-filing behavior during the study period. Because tax filing may be less complete among mothers with no earnings (Robson and Schwartz, 2020; Genest-Grégoire et al., 2024), the estimates should be interpreted as applying to mothers with stronger attachment to the tax system and may understate labor market exit among mothers who stop filing taxes. The final sample contains approximately 640,400 mothers, of which 8% have given birth to a low-birth-weight or preterm infant.[6]

Sample characteristics. Table 1 reports summary statistics for all mothers by infant health status. We observe 53,900 births with poor infant health (LBW or preterm) and 586,500 births with healthy infants. The table shows that mothers who give birth to unhealthy infants differ from mothers of healthy infants along several dimensions. Mothers of unhealthy infants are slightly older (32.5 vs. 32.1 years) and more likely to have had a child before (birth order 0.80 vs. 0.73). Panel B shows additional demographic differences: mothers of unhealthy infants are more likely to be married (87% vs. 83%). Panel C examines pre-birth labor market characteristics in the year before birth (k=-1). Mothers of unhealthy infants have lower pre-birth earnings: labor force income is \$1,030 lower and family income is \$5,120 lower relative to mothers of healthy infants. They also have lower rates of dual tax filers (85% vs. 82%)[7] and slightly higher unemployment benefit receipt. Moreover, the joint F-test indicates that these differences are jointly significant. These differences indicate that mothers of unhealthy infants are somewhat negatively selected on both labor market attachment and earnings prior to the birth. These differences between mothers of healthy and unhealthy infants might confound a naive comparison of post-birth outcomes. Section 3 presents how I construct a counterfactual comparison group using coarsened exact matching on key pre-birth characteristics, improving comparability between treated and control mothers on observed characteristics.

[6] To meet Statistics Canada data confidentiality policy, the numbers of observations are rounded to the nearest thousand.

[7] The difference between the proportion married/common-law (87% vs. 83%) and dual tax filers (85% vs. 82%) reflects Canadian tax filing requirements: individuals are considered common-law partners for tax purposes if they have lived together for at least 12 consecutive months or have a child together, regardless of formal marital status. This explains the approximate 2 percentage point gap between these measures.

Table 1: Sample Characteristics: Mothers of Healthy vs. Unhealthy Infants

	Healthy Infants (1)	**Unhealthy Infants (2)**	**Difference (2)-(1) (3)**	**SE (4)**
Panel A: Child's characteristics				
Male	0.51	0.52	0.01***	0.000
Birth month	6.43	6.40	−0.03**	0.020
Age in 2018	8.56	8.26	−0.30***	0.010
Birth order	0.80	0.73	−0.07***	0.010
Panel B: Mother and family characteristics				
Mother's age (years)	32.54	32.11	−0.43***	0.020
Married or common law	0.87	0.83	−0.04***	0.000
Native mother	0.80	0.78	−0.02***	0.000
Stillbirth before	0.60	0.68	0.08**	0.030
Partner born in Canada	0.74	0.70	−0.04***	0.000
Partner's age (missing coded as 99)	70.61	87.70	17.09***	0.840
Partner age missing	0.04	0.05	0.01***	0.000
No postsecondary education	0.71	0.72	0.01***	0.000
University degree	0.06	0.06	0.00	0.000
Panel C: Pre-birth labor market (k=-1)				
Labor force income (2015 CAD)	39,830	38,800	−1,030***	143
Total income (2015 CAD)	43,450	42,520	−930***	142
Family income (2015 CAD)	99,700	94,580	−5,120***	307
Two tax filers	0.85	0.82	−0.03***	0.000
Any labor income	0.92	0.91	−0.01***	0.000
Unemployment benefit receipt	0.18	0.16	−0.02***	0.000
Disability tax credit (own)	0.00	0.01	0.01***	0.000
Disability tax credit (dependent)	0.01	0.01	0.00	0.000
Panel D: Geography				
Quebec	0.51	0.52	0.01**	0.005
Sample sizes				
N (mothers)	586,500	53,900		
Joint F-test[a]			F = 101.68 (p < 0.001)	

Notes: This table presents mean characteristics for mothers of healthy infants (column 1) versus mothers of unhealthy infants—defined as low birth weight (<2,500g) or preterm (<37 weeks)—(column 2). Column (3) shows the difference, estimated as the coefficient from a regression of each characteristic on an unhealthy infant indicator, with standard errors in column (4). Pre-birth labor market variables measured at k=-1 (year before birth). Sample includes focal births between 2006 and 2015 in Canada. *** p<0.01, ** p<0.05, * p<0.10. Joint F-test from a regression of the unhealthy infant indicator on all pre-birth characteristics simultaneously.

3 Empirical framework

3.1 Comparison group construction

I implement a two-step matching procedure designed to identify comparison mothers who are observably similar to treated mothers across dimensions that prior research has established as key determinants of both infant health outcomes and maternal labor supply.[8]

Step 1: Exact matching on key demographics. In the first step, I perform exact matching on variables where precise balance is essential: child parity, missing postsecondary education (or below postsecondary), prior stillbirth, birth month, birth year, province of birth, and receipt of Child Disability Benefit and own disability tax credit. This step successfully generates one or more controls for over 90% of treated mothers.

The choice of exact matching variables improves comparability on dimensions where even small differences could confound the estimated effects. Child parity captures both biological risk factors and mothers' lifecycle stage (Shah et al., 2011; Price, 2008). First births carry higher medical risks, while later-born children face different parental time and resource allocations. Mothers' labor supply responses also vary systematically by parity (Angelov et al., 2016), making exact matching on this dimension critical. Birth year and month control for macroeconomic conditions, policy environments, and seasonal variation in health outcomes (Currie and Schwandt, 2013).[9] Province of birth ensures that treated and control mothers face identical institutional contexts, including healthcare quality, access to neonatal intensive care units (Lasswell et al., 2010), childcare subsidies, and parental leave generosity (Baker et al., 2008).

The missing postsecondary education indicator addresses measurement error in the education variable, which does not capture foreign or private institution attendance (approximately 5% of enrollments). By exactly matching on this missingness pattern, I ensure that treated and control mothers have comparable education measurement quality. Prior stillbirth and receipt of disability benefits (both own and child) capture health history and exposure to prior health shocks. Mothers who have experienced stillbirth or who have received disability benefits may have different health behaviors during pregnancy and different labor market trajectories (Persson and Rossin-Slater, 2018). These health history measures are rarely available in administrative data and help reduce the concern that the estimated infant health penalty reflects pre-existing health vulnerabilities rather than differences associated with the current child's health.

[8] Exact matching is often assumed to be the matching procedure most likely to ensure that balance in both observed covariates and unobserved factors is achieved between treated and control groups. However, it is unlikely to successfully find an exact match for each treated mother along all dimensions due to the so-called *curse of dimensionality*. For this reason, I use a matching procedure that combines exact and propensity score matching, similar to what is suggested by Rubin and Thomas (2000) and implemented by Stepner (2019), for example.

[9] Matching on birth month is also important for interpreting outcomes in year $t = 0$ (the birth year). Births occurring in later months of the year imply that most of the pregnancy occurred during that same calendar year. If mothers of low birth weight or preterm infants are more likely to give birth late in the year, year $t = 0$ labor outcomes may reflect pregnancy difficulties or work-related stress during pregnancy rather than post-birth caregiving demands. For instance, if low birth weight and preterm births are driven by stress related to working more intensively during pregnancy, we would expect to observe a sharp increase in labor market outcomes for treated mothers in year $t = 0$ relative to controls. By exactly matching on birth month, I ensure that treated and control mothers have the same distribution of pregnancy timing within the calendar year, allowing for a cleaner interpretation of the birth-year effects.

Step 2: Propensity score matching on remaining dimensions. For the remaining sample where exact matching alone does not yield controls, I estimate the propensity score—the probability of having a preterm or low birth weight infant—conditional on: labor income and family total income measured one and two years pre-birth, maternal and paternal age, immigration status, exact education level (university vs. college), infant sex, and marital status at birth.

The inclusion of pre-birth income at specific lags (one and two years before birth) warrants particular justification. Differences in the timing of childbearing decisions can reflect both underlying health risks and economic circumstances that independently influence post-birth labor outcomes. For instance, mothers who delay childbearing due to career investment may face different age-related fertility risks (Cnattingius et al., 1992) while also having steeper earnings trajectories. Conversely, mothers experiencing health problems may accelerate childbearing, resulting in both worse birth outcomes and lower pre-birth earnings. By conditioning on income at one and two years before birth—rather than all pre-birth years—I strike a balance between controlling for earnings differences that may confound the infant health effect and preserving the ability to flexibly test the parallel trends assumption. Conditioning on all pre-birth years would mechanically impose parallel trends by construction, whereas conditioning on only these two specific lags allows the data to reveal whether treated and control mothers had similar earnings trajectories in earlier years (three and four years pre-birth), thereby providing a credible test of the identifying assumption (Kleven et al., 2019a).

Maternal and paternal age are included as continuous variables to capture non-linear relationships with both fertility risks and earning potential (Fraser et al., 1995; Bertrand, 2010). Immigration status captures the "healthy immigrant effect" (Urquia et al., 2010) and distinct labor market opportunities (Picot and Sweetman, 2005). Exact education level (university vs. college) refines the first-step matching, addressing the fact that mothers attending foreign or private institutions are incorrectly classified as having no postsecondary education. More educated mothers have healthier birth outcomes due to better prenatal care utilization and health knowledge (Currie and Moretti, 2003), as well as higher opportunity costs of time spent on childcare (Lundborg et al., 2017). Infant sex controls for sex-specific health risks and parental preferences that may affect time allocation (Lundberg, 2005). Marital status captures household resources and time allocation constraints, which influence both health investments during pregnancy (Shah et al., 2011) and labor supply responses to childbirth (Waldfogel, 1998). For each treated mother, I retain one control mother based on nearest-neighbor propensity score matching.[10] This two-step procedure substantially improves comparability between treated and control mothers in terms of lifecycle stage (via parity), institutional environment (via province), pre-birth economic outcomes (via income and employment history), health history (via disability benefits and stillbirth), and demographic characteristics (via age, education, immigration status, and family structure). Remaining differences in post-birth earnings are therefore interpreted as the estimated infant health penalty under the identifying assumption that treated and matched control mothers would otherwise have followed similar trajectories.

In a sensitivity analysis (Section 6), I compare the main results with those obtained using a more parsimonious matching approach.

[10]Even not included in this manuscript, a comparison of the distribution of the propensity scores across group shows a substantial overlap.

3.2 Event study specification

I use an event study design that directly compares how outcomes evolve for mothers whose child is born unhealthy (low birth weight or preterm) relative to mothers whose child is born healthy (neither low birth weight nor preterm), focusing on an event window spanning from 4 years before birth (k=-4) to 7 years after birth (k=7).[11] The baseline specification takes the following form:

$$y_{ik} = \alpha_i + \sum_{\substack{k=-4 \\ k\neq -1}}^{7} \beta_k \cdot \mathbb{1}[t=k] + \sum_{\substack{k=-4 \\ k\neq -1}}^{7} \gamma_k \cdot \mathbb{1}[t=k] \times \text{Unhealthy}_i + \epsilon_{ik}. \tag{1}$$

In each period k relative to the birth, this equation estimates the penalty for mother i of giving birth to a low-birth-weight and/or a preterm baby on the outcome y (e.g., labor force income, family total income). α_i is an individual fixed effect that allows each mother to have a different level of outcome y due to characteristics not included in the model. β_k estimates secular changes in the mean outcome y among mothers in the comparison group. The event time $k = -1$ is omitted so that the changes are relative to the year preceding the child's birth. The parameters of interest are γ_k, which evaluate the gap in the outcomes between mothers whose child is born unhealthy and mothers whose child is born healthier. The dependent variables in the regression (1) are in levels instead of logarithms to keep zeros due to non-participation in the dataset.[12] Therefore, γ_k captures the estimated differential effect in levels. To have a percentage measure of the infant health penalty, I construct a measure in the same spirit of recent estimates of the child penalty (e.g., Kleven et al. 2021; Artmann et al. 2022; Andresen and Nix 2022):

$$ATT_k = \frac{\gamma_k}{E[\widetilde{y_k} \mid k]}, \tag{2}$$

where $\widetilde{y_k}$ is the predicted counterfactual outcome in year k since the child's birth.[13] Therefore, ATT_k should be interpreted as the percentage change in the outcome at relative year k of mothers of poor health children compared to what they would have obtained if their child had been born healthy. I compute robust standard errors by randomly resampling the matching pairs with replacement and re-estimating the event study regressions using bootstrapped samples with 250 replications. To account for within pair correlation, I cluster the standard error at that level.[14]

The identification assumption is that within the matched sample, giving birth to low-birth-weight or preterm child is *as good as random*. Consequently, there would not have been a differential impact of childbirth on income in the absence of child health problems. For this assumption to be more likely to hold, the parameters ATT_k should not be statistically significant for $k < 0$ (i.e., before the event). This provides support for interpreting post-birth differences as related to the health issues of the newborn rather than to

[11] Similar matched event study designs are used in Sarsons (2017), Stepner (2019), and Jäger and Heining (2022). I drop all observations outside the k=-4 to k=7 window to maintain a balanced panel around the birth event.

[12] Given that the data include negative values for earnings, I further test the robustness of my findings by restricting the sample to observations with non-negative earnings and applying a log transformation to the dependent variable. The results remain consistent with this approach.

[13] I obtain these predictions by estimating the following regression: $\widetilde{y_{ik}} = \alpha_i + \sum_{-4,k\neq-1}^{7} \beta_k$.

[14] De Chaisemartin and Ramirez-Cuellar (2024) show this level of clustering ensures robust inference in match pair design. However, the results are robust to clustering at the individual level.

pre-existing trends or disparities between mothers. In Section 6, I discuss how this could be violated even if the parallel trend is verified.

Although equation (1) cleanly identifies the differential effect of infant health (γ_k), it does not reveal what this gap represents relative to the broader trajectory around childbirth. Specifically, γ_k could indicate an *additional loss* on top of an existing child penalty (if $\beta_k < 0$), the *reversal of a gain* (if $\beta_k > 0$), or the *sole driver* of earnings changes (if $\beta_k \approx 0$). I then follow a classical empirical approach that estimates how parental outcomes evolve following a child's birth (see Kleven et al. (2019a)) using the comparison mothers (mothers with healthier infant either in the matched sample or in original sample) to contextualize the incremental effect of the child's low birth weight or preterm status estimated by γ_k. I estimate the following event study:

$$y_{ikt} = \alpha_i + \sum_{\substack{k=-4 \\ k \neq -1}}^{7} \delta_k \cdot \mathbb{1}[t = k] + \sum_{a} \lambda_a \cdot \mathbb{1}[\text{Age}_{it} = a] + \sum_{t} \theta_t \cdot \mathbb{1}[\text{Year}_t = t] + \epsilon_{ikt}, \tag{3}$$

where $\mathbb{1}[\text{Age}_{it} = a]$ controls non-parametrically for life-cycle trends and $\mathbb{1}[\text{Year}_t = t]$ controls for calendar year effects. As discussed in Kleven et al. (2019a), the parameters of interest δ_k compares realized outcomes to the potential outcome absent the birth of the child.

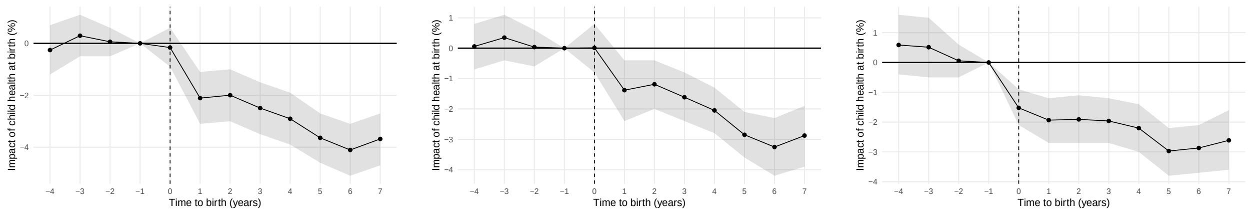
When presenting the main estimates, I benchmark the gap (γ_k from equation (1)) between mothers in the treatment group and mothers in the matched control group to the average change in the outcomes of interest in the years following the focal child's birth using equation (3).

4 Results

4.1 Main estimates: The Infant Health Penalty

I begin by presenting how labor force income and total income of mothers of unhealthy children evolve relative to mothers of healthy children using specification (1), and then compare this gap to the changes in outcomes due to motherhood (the child penalty) using specification (3).

Direct comparison. Figure 1 presents the event study estimates for labor force income (Panel (a)), individual total income (Panel (b)), and family total income (Panel (c)). The figure plots the differential effect ATT_k from equation (2)—the gap in percentage between mothers of unhealthy infants and matched mothers of healthy infants at each event time k relative to birth. First, the pre-birth coefficients are small and statistically indistinguishable from zero, supporting the event-study interpretation of post-birth differences. The average pre-birth effect is 0.03% for labor force income (standard error = 0.28%), 0.15% for total income (standard error= 0.18%), and 0.39% for family income (standard error= 0.29%). Figure 1 shows that mothers of unhealthy infants start to fall behind their counterpart mothers of healthy children from the first year to the end of the observation period (year 7) across all income variables considered. There is no evidence of recovery or catch-up over time. Averaging over the seven post-birth years (years 1 through 7), the effect appears to be the largest on labor force income, which captures earnings from employment and self-employment and the lowest on total income, which includes income from all sources. Specifically, mothers of unhealthy infants experience a penalty of 2.6% in labor force income (standard error = 1.3%), 1.9% in total income (standard error = 1.1%), and 2.2% in family income (standard error = 0.5%) relative to matched mothers of healthy infants. All effects are statistically significant at conventional levels ($p < 0.05$ for labor and total income; $p < 0.001$ for family income). The size of the effect on family income compared to mothers' total income suggests that infant health may affect other components of family income. I investigate this further when examining the consequences for the household.



(a) Labor Force Income (b) Total Income (Mother) (c) Family Total Income

Figure 1: Effect of infant health on earnings and total income

Notes: This figure plots the event study coefficients ATT_k from equation (2), showing the differential effect of poor infant health (low birth weight or preterm) on income at each year k relative to birth. Panel (a) shows labor force income (earnings from employment and self-employment). Panel (b) shows total income for the mother (labor income plus transfers and investment income). Panel (c) shows total family income (aggregating all household members). Estimates measured as percentage change relative to the counterfactual (what treated mothers would have earned if their child had been born healthy). Shaded areas show 95% confidence intervals. Standard errors are clustered at the matched-pair level using 250 bootstrap replications. The reference period is $k = -1$ (year before birth). Sample consists of approximately 636,000 observations (53,000 unique matched mother-pairs observed over 12 years, from $k = -4$ to $k = 7$). Pre-birth coefficients are jointly insignificant for all outcomes, supporting parallel trends.

Benchmarking to the child penalty. To provide context for the magnitude of the infant health penalty presented above, I compare it to the traditional motherhood penalty. Following Kleven et al. (2019a), I estimate the changes in outcomes due to motherhood using equation (3), which compares the outcomes of mothers in my sample to their potential outcomes absent the birth of the focal child. Table 2 presents the results for mothers with healthy infants from the original population (column 1), mothers in the matched comparison group with healthy infants (column 2), and the gap in outcomes between matched comparison mothers and mothers of unhealthy infants, estimated using equation (1) (column 3). All of the results are the average post-birth (year 1 through 7) changes. Consistent with recent estimates of the child penalty (e.g., Kleven et al. 2019b; Connolly et al. 2023), mothers in both the original sample and the matched comparison group experienced substantial decreases in all three income variables due to the birth of the focal child. Table 2 shows that the penalty amounts to -\$10,949 (standard error = 193) for the original sample and -\$10,675 (standard error = 614) for the matched comparison group on average in the first seven years after the child's birth, representing approximately 28-29% of pre-birth earnings. On top of these changes due to motherhood, treated mothers lose an additional \$1,154 (standard error = 198) due to their child's health at birth, causing them to face a total penalty of \$11,829 (-\$10,675 + (-\$1,154)) in labor force income, representing approximately 31% of pre-birth earnings. While the infant health penalty is smaller in absolute magnitude than the child penalty, it represents an economically meaningful 9.8% of the total earnings loss.

The same pattern applies to maternal total income and family total income, though with important differences in magnitudes. For maternal total income, the child penalty is -\$6,832 (16.5% of pre-birth income) and the infant health penalty is -\$991 (2.4%), yielding a total effect of -\$7,823 (18.9%). The smaller magnitude of the child penalty for total income compared to labor force income suggests that government transfers and other non-labor income sources partially offset labor market losses, consistent with the role of Canada's income support programs in cushioning the impact of childbirth on family finances.

However, an interesting pattern emerges for family total income. The child penalty at the family level is substantially smaller—only -\$4,363, or approximately 4.5% of pre-birth family income—suggesting that families receive some compensation through two channels. First, government transfers provide additional support at the family level. Second, as documented by the literature on the child penalty (e.g., Kleven et al. 2019b; Connolly et al. 2023), partners may experience smaller losses than mothers, and continued labor market attachment by other household members may partially cushion the family-level impact.

Despite the smaller baseline child penalty at the family level, the incremental effect of infant health is noteworthy: treated families lose an additional \$2,357 (standard error = 426) due to poor infant health. This infant health penalty represents more than half the magnitude of the child penalty itself (-\$2,357 vs. -\$4,363), accounting for 35.1% of the total family income loss. This disproportionately large share at the family level—compared to 9.8% for labor force income and 12.7% for maternal total income—indicates that the infant health shock affects not only the mother's earnings but also other household income sources, consistent with mother's partner income decreasing, which I explore further in the subsequent section. The total family income penalty for treated families amounts to -\$6,720, or approximately 7.0% of pre-birth family income.

In summary, although the infant health penalty appears small in magnitude it represents an incremental effect of an existing motherhood penalty. I further test the economic significance of this penalty below.

Table 2: Average Infant Health Effects compared to Average Birth Effects

Outcome	**Untreated Original**	**Matched Untreated**	**Infant Health Penalty**	**Mean at k=-1 (Treated)**
Labor Force Income				
Level (2015 CAD)	−10,949***	−10,675***	−1,154***	38,250
	(193)	(614)	(198)	(30,470)
% change	−28.9	−27.9	−3.0	
N	3,300,000	318,000	636,000	
Total Income (Mother)				
Level (2015 CAD)	−7,429***	−6,832***	−991***	40,505
	(190)	(603)	(201)	(29,766)
% change	−18.3	−16.5	−2.4	
N	3,300,000	318,000	636,000	
Family Total Income				
Level (2015 CAD)	−3,302***	−4,363***	−2,357***	91,492
	(410)	(1,270)	(426)	(65,116)
% change	−3.6	−4.5	−2.4	
N	3,300,000	318,000	636,000	

Notes: This table decomposes the total effect into: (1) child penalty (equation 3); (2) infant health penalty (equation 1). "Untreated Original" = average change (k=-1 to years 1-7) for all healthy-infant mothers. "Matched Untreated" = matched controls. "Infant Health Penalty" = additional penalty for treated mothers. Standard errors in parentheses, clustered at mother level (columns 1-2) and matched-pair level (column 3). Pre-birth means at k=-1 for treated mothers. Total effect = Matched Untreated + Infant Health Penalty. *** p<0.01.

4.2 Intensive and extensive margins

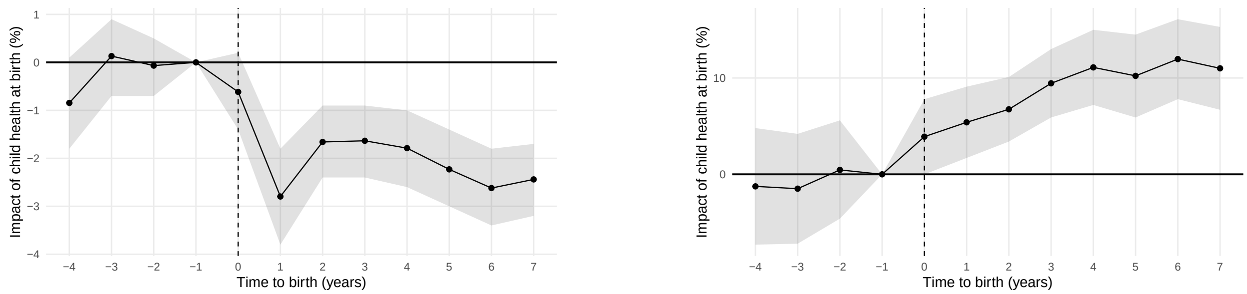
To isolate the intensive margin, I restrict the sample to mothers with non-zero market earnings in each year.[15] To examine the extensive margin, I estimate the effect on the probability of having zero or negative market earnings.[16] Figure 2 presents event study estimates for both margins. Panel (a) shows the intensive margin effect. There are no statistically significant differences in the prenatal period (pre-treatment ATT: 0.031%, standard error = 0.278), supporting parallel trends. The effect emerges immediately at birth, and the penalty persists throughout the seven-year observation window. The average post-birth ATT across years 1-7 is -1.973% (standard error = 0.704), statistically significant at the 1% level. Panel (b) shows the extensive margin effect. Again, there is no pre-treatment difference (pre-treatment ATT: -0.761%, standard error = 1.063). The probability of having zero or negative earnings increases sharply from birth and grows over time, reaching approximately 12% by year 7. The average post-birth ATT across years 1-7 is 8.723%

[15] Approximately 0.03% of earnings observations are negative, likely reflecting self-employment losses or repayment of excess unemployment insurance benefits. Negative values are recoded to zero. Results remain substantively unchanged when negative earnings are retained.

[16] The decision to remain employed is clearly endogenous. The intensive margin estimates should therefore be interpreted as conditional on labor force participation.

(standard error = 2.981), statistically significant at the 1% level.

These results reveal that the infant health penalty operates through both margins, with the extensive margin effect being substantially larger in magnitude. The extensive margin penalty is approximately 4.4 times larger than the intensive margin penalty, indicating that labor market exit appears to be an important channel through which the infant health shock affects maternal earnings.



(a) Earnings penalty among employed

(b) Penalty on labor force participation

Figure 2: Intensive and extensive margins of the infant health penalty

Notes: This figure shows the infant health penalty (ATT_k from equation 2) for each year relative to birth. Panel (a) reports the earnings gap for mothers with positive labor market income (N = 423,100 mother-year observations). Average post-birth ATT: -1.973% (SE = 0.704); pre-treatment ATT: 0.031% (SE = 0.278). Panel (b) shows the penalty on the probability of having zero or negative income, a proxy for labor force non-participation (N = 636,000 mother-year observations). Average post-birth ATT: 8.723% (SE = 2.981); pre-treatment ATT: -0.761 pp (SE = 1.063). Standard errors are clustered at the matched-pair level and computed using 250 bootstrap replications. The reference period is $k = -1$ (year before birth). 95% confidence intervals shown.

4.3 Consequences for the household

As mentioned in Section 2, partners' outcomes are not directly observed in the administrative linkage. To examine household-level implications, I construct a proxy for partner earnings by taking the difference between total family income and the mother's labor force income.[17] While imperfect—this measure may include other household income sources and assumes constant household composition—it provides a suggestive proxy for partner earnings given the data constraints.

Figure 3 presents the event study estimates for three outcomes: mothers' labor force income (Panel a), inferred partner income (Panel b), and the share of mother's labor force income in total family income (Panel c). Panel (a) confirms that the penalty on labor force income for mothers in dual-earner couples is similar to the main estimates, with an average post-birth penalty of -2.556% (SE = 0.990, $p < 0.01$).

Turning to inferred partner income, Panel (b) suggests that residual family income net of the mother's income declines following the birth of an unhealthy infant, though the magnitude is smaller than the maternal penalty. The effect never exceeds 2% in any post-birth year, and on average, inferred partner earnings decline by -1.384% (SE = 0.294, $p < 0.01$).[18] This pattern suggests that the infant health shock may affect household finances beyond the direct maternal earnings channel. Because partner earnings are inferred rather than

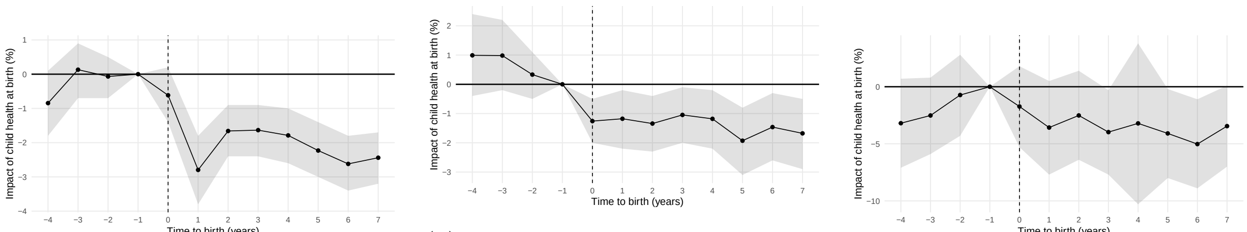
[17] I restrict the analysis to mothers in couple families whose household size does not exceed the number of children at focal birth plus two. This yields N = 467,400 mother-year observations.

[18] The partner income decline is statistically significant but substantially smaller than the maternal penalty, consistent with findings in Kleven et al. (2019a) that partners experience minimal child penalties compared to mothers.

directly observed, this evidence should be interpreted cautiously.

Panel (c) examines the implications for intra-household income distribution by plotting the share of mother's labor force income in total family income. The results provide limited evidence that the infant health shock decreases mothers' economic position within the household. The mother's income share declines in the post-birth years, though the effect exhibits some year-to-year variation. The decline becomes statistically significant at the 5% level in years 5 and 6 after birth, where mothers' income share decreases by approximately 4-5% compared to mothers in the matched comparison group. This reduction in income share should be interpreted cautiously because partner earnings are inferred and the estimates vary across post-birth years.

In summary, the household-level analysis suggests that the infant health shock is associated with declines in family income that are not fully explained by mothers' own earnings losses. However, because partner income is inferred and mothers' income-share estimates are imprecise, this evidence should be interpreted as suggestive rather than definitive.



(a) Mother's labor force income

(b) Inferred partner's labor force income

(c) Mother's share of family income

Figure 3: Household-level consequences of infant health shocks

Notes: This figure shows the household-level effects of infant health shocks (ATT_k from equation 2) for each year relative to birth. The sample is restricted to mothers in couple families whose household size does not exceed the number of children at focal birth plus two (N = 467,400 mother-year observations). Panel (a) shows the mother's labor force income in the restricted sample. Average post-birth ATT: -2.556% (SE = 0.990). Panel (b) shows inferred partner labor force income (family income minus mother's earnings). Average post-birth ATT: -1.384% (SE = 0.294). Panel (c) shows the mother's share of family income. Average post-birth ATT: -3.447 pp (SE = 1.004). Standard errors are clustered at the matched-pair level and computed using 250 bootstrap replications. The reference period is $k = -1$ (year before birth). 95% confidence intervals shown.

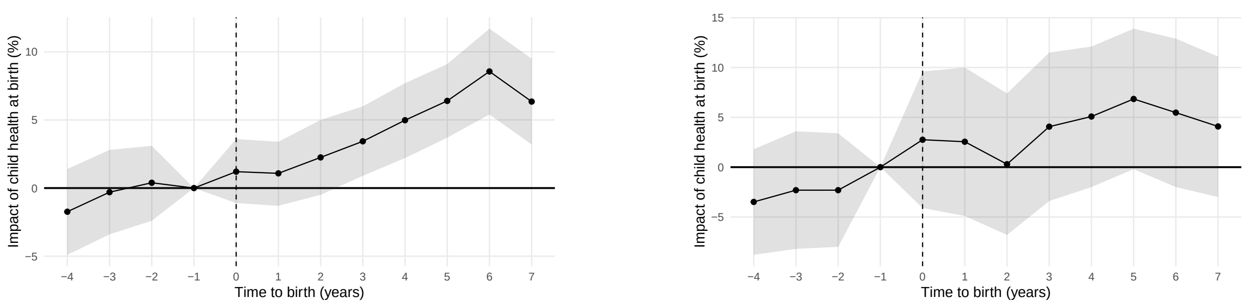
4.4 Consequences for earnings inequality and poverty

To test the economic significance of the earnings effects documented above, I examine distributional outcomes that capture mothers' position in the earnings distribution and their likelihood of experiencing poverty. I analyze two measures: (1) the probability of falling in the bottom quartile of all first-time mothers' earnings distribution (approximately 11 million observations), and (2) Statistics Canada's poverty definition, which classifies families as poor if their income falls below 50% of the median family income, adjusted for family size.

Figure 4 presents the results. First, there is no evidence of a gap in either outcome in the pre-birth years, strengthening the interpretation that even for distributional outcomes, mothers in the treatment and matched control groups were indistinguishable prior to the birth. Panel (a) shows that the infant health shock increases the probability of being in the lowest earnings quartile beginning in the third year after birth and persisting through the end of the observation window. The effect grows over time, reaching over 6% in years 6-7 post-birth. While the average effect exhibits some imprecision, the consistent pattern of increasing vulnerability over time—with statistically significant effects emerging by year 3 and growing thereafter—indicates that the infant health shock pushes a substantial fraction of mothers toward the bottom of the earnings distribution, where they face the most precarious labor market positions.

Panel (b) examines the effect on poverty status. The average post-birth ATT is 3.893% (SE = 2.025), marginally significant at the 10% level. However, the effect exhibits an important temporal pattern: it becomes statistically significant at the 10% level by year 5, reaching nearly 7%—coinciding precisely with the typical age of school entry for the focal child. This timing suggests that the transition to formal schooling may exacerbate economic vulnerability, possibly due to reduced access to subsidized childcare (which is more widely available for younger children in Canada) or increased accommodation needs as children with health complications navigate the educational system and require additional support services.

These findings suggest that the modest percentage effects on earnings documented above may translate into increases in economic vulnerability and downward mobility in the income distribution. Even a 2-3% earnings penalty may push some families below critical thresholds—the bottom quartile of the earnings distribution or the poverty line—with potentially cascading consequences for child wellbeing, maternal health, and long-term economic security. The fact that these effects are particularly pronounced around school entry highlights a critical period when families with medically complex children face compounded challenges in managing both health care needs and educational transitions.



(a) Lowest income quartile

(b) Poverty status

Figure 4: Effects on earnings inequality and poverty status

Notes: This figure shows the distributional effects of infant health shocks (ATT_k from equation 2) for each year relative to birth (N = 636,000 mother-year observations). Panel (a) plots the event study for the probability that mother's labor force income falls in the lowest quartile of the earnings distribution of all first-time mothers in the original sample (approximately 11 million observations) in each calendar year. Average post-birth ATT: 4.281% (SE = 2.728); pre-treatment ATT: -0.549% (SE = 1.084). Panel (b) shows the probability of being in poverty. Average post-birth ATT: 3.893% (SE = 2.028); pre-treatment ATT: -2.704% (SE = 0.681). Standard errors are clustered at the matched-pair level and computed using 250 bootstrap replications. The reference period is $k = -1$ (year before birth). 95% confidence intervals shown.

4.5 Heterogeneous average effects

To investigate whether the infant health penalty varies across key dimensions, I estimate heterogeneous treatment effects by: geographic location (Quebec vs. rest of Canada), pre-birth income (bottom vs. top quartile), parity (first child vs. higher-order births), household earnings structure (egalitarian vs. non-egalitarian), and infant sex. Appendix Figure C plots the average post-birth ATT (years 1-7) for each subgroup across four outcomes: family income, maternal labor force income, maternal total income, and zero earnings probability.

The figure shows that most confidence bands across groups overlap and are similar in magnitude to the main estimates, suggesting no statistically significant differences across most subgroups. However, two dimensions reveal noteworthy heterogeneity. First, by pre-birth income, low-income mothers (Q1) experience substantially larger penalties than high-income mothers (Q4) across all outcomes. For family income, low-income mothers face penalties of approximately \$3,500 compared to roughly \$1,000 for high-income mothers—a 3.5:1 ratio. Most notably, for zero earnings probability, low-income mothers show a 2.8 percentage point increase while high-income mothers show essentially zero labor market exit. This suggests high-income mothers adjust along the intensive margin (reduced hours, flexible positions) while maintaining employment, whereas low-income mothers face rigid job constraints that force complete labor market withdrawal.

Second, by parity, first-time mothers show no statistically significant increase in zero earnings probability, indicating they adjust primarily along the intensive margin. In contrast, mothers with higher-order births experience larger extensive margin effects, suggesting that managing an unhealthy infant alongside existing children creates binding time constraints that push mothers toward labor market exit. The penalties are similar across Quebec versus rest of Canada (despite Quebec's comprehensive family policies),[19] household structure (egalitarian vs. non-egalitarian), and infant sex, indicating that these dimensions do not substantially moderate the maternal labor market response to infant health shocks.

5 Evidence on mechanisms

To explore the mechanisms underlying the infant health penalty, I focus on three potential channels through which poor infant health may persistently affect maternal labor market outcomes: partnership patterns, child health vulnerabilities during childhood, and maternal health deterioration.

5.1 Partnership Stability

Since tax files contain marital status information, I test whether partnership dissolution can explain the findings.[20]

Figure 5 shows the average post-birth ATT across years 1-7 is -3.179% (SE = 1.400), statistically significant at the 5% level, indicating mothers of children with health complications are slightly less likely to experience partnership dissolution. However, the year-by-year coefficients are not individually statistically

[19] Appendix Figure F shows that the effect on total income is not significant at the 5% level in the first three years post-birth for Quebec mothers, probably suggesting a compensatory role of Quebec benefit programs in the immediate aftermath of the infant health shock. However, the penalty becomes significant in later years as ongoing caregiving demands persist.

[20] The Canada Revenue Agency recognizes both legal marriages and common-law partnerships (conjugal relationships of at least 12 consecutive months, or with a child in common). The outcome captures both divorce (dissolution of legal marriage) and separation (dissolution of common-law partnership), providing a comprehensive measure of partnership dissolution.

significant, suggesting the average effect is driven by consistent small negative effects rather than by specific years with large effects. The pre-treatment ATT is small and insignificant (1.477%, SE = 2.323), supporting parallel trends.

This finding is consistent with Eriksen et al. (2021) and Breivik and Costa-Ramón (2024), who find no evidence of increased relationship dissolution following child health shocks. Couples—both married and common-law—may stay together to pool resources and share caregiving responsibilities. While this stability could reflect relationship strengthening, it may also indicate that mothers feel constrained from leaving partnerships due to economic vulnerability or the need for support in managing their child's health needs. Importantly, this partnership stability does not prevent the substantial economic losses documented in previous sections.

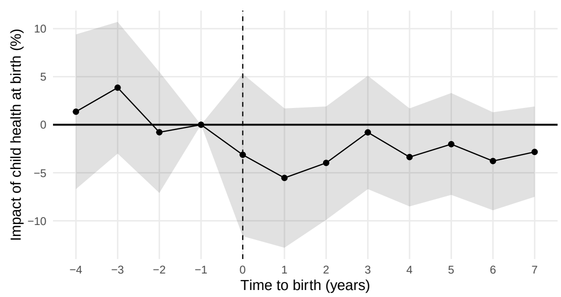


Figure 5: Partnership instability following infant health shocks

Notes: Probability of divorce or separation. N = 636,000 mother-year observations. Average post-birth ATT (years 1-7): -3.179% (SE = 1.400, t = -2.27, $p < 0.05$); pre-treatment ATT: 1.477% (SE = 2.323, p = 0.52). Year-by-year coefficients are not individually statistically significant. Standard errors clustered at matched-pair level, 250 bootstrap replications.

5.2 Health vulnerabilities during childhood

Existing medical and economics studies emphasize the role of prematurity and low birth weight in determining future health trajectories.[21] This relationship could explain the persistence of the infant health penalty. To test this mechanism, I examine whether children born with health complications continue to experience health problems requiring ongoing parental care and medical attention.

I focus on two outcomes: the probability of receiving a tax credit for a dependent disability and the probability of receiving a child disability benefit.[22] While these outcomes may not capture all day-to-day care needs, they serve as strong indicators of significant ongoing health challenges.

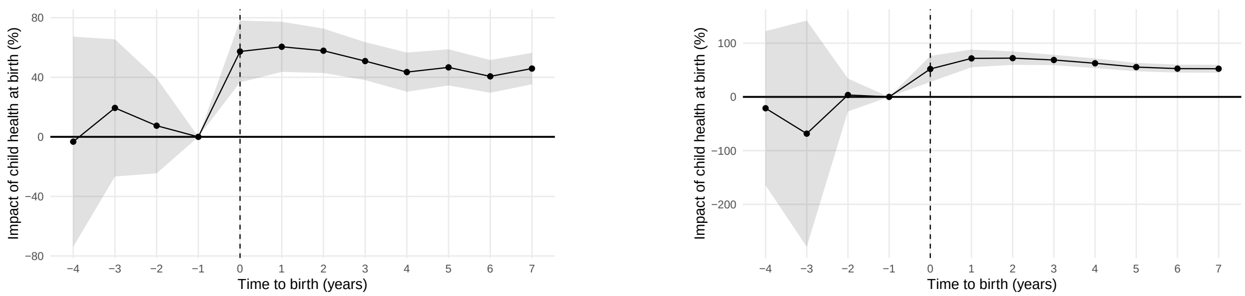
Figure 6 presents notable results. Panel (a) shows the effect on dependent disability tax credit receipt. The average post-birth ATT across years 1-7 is 50.394% (SE = 7.402), statistically significant at the 1% level. Panel (b) shows an even larger effect on child disability benefit receipt: the average post-birth ATT is 60.952% (SE = 8.914), statistically significant at the 1% level. The effects grow over time as children age

[21]See, for example, Almond et al. (2005), Black et al. (2007), Figlio et al. (2014), and Bharadwaj et al. (2017) for evidence on educational and cognitive outcomes; Hack et al. (2002) and Saigal and Doyle (2008) for evidence on health trajectories.

[22]These are distinct benefits. The tax credit provides tax relief for families with disabled dependents. The child disability benefit provides monthly payments to families and is available even without taxable income. As discussed in Section 2.1, Canadian families with a child under 18 with severe or prolonged impairment of physical or mental functions are eligible. The benefits are not means-tested.

and conditions become diagnosed. Pre-treatment ATTs are insignificant (7.884% for dependent disability, SE = 11.339; -28.655% for child disability benefit, SE = 36.572), supporting parallel trends and the absence of pre-existing differences in health outcomes of previous children.

These near-doublings in disability benefit receipt indicate that poor health at birth strongly predicts persistent health challenges requiring ongoing parental care, specialized medical attention, and potentially educational accommodations. This result is consistent with childhood health conditions being a primary driver of the persistent earnings gap between mothers with unhealthy and healthy infants.[23]



(a) Tax credit for dependent disability

(b) Child disability benefit

Figure 6: Impact of health at birth on disability trajectory in childhood

Notes: N = 636,000 mother-year observations. Panel (a): Probability of receiving dependent disability tax credit. Average post-birth ATT (years 1-7): 50.394% (SE = 7.402, t = 6.81, $p < 0.01$); pre-treatment ATT: 7.884% (SE = 11.339, p = 0.49). Panel (b): Probability of receiving child disability benefit. Average post-birth ATT (years 1-7): 60.952% (SE = 8.914, t = 6.84, $p < 0.01$); pre-treatment ATT: -28.655% (SE = 36.572, p = 0.43). Effects grow over time as children age and conditions are diagnosed. Standard errors clustered at matched-pair level, 250 bootstrap replications.

5.3 Maternal health deterioration

Poor pregnancy outcomes and ongoing caregiving demands may give rise to long-lasting maternal health shocks that constrain labor supply. To investigate this, I examine the likelihood of receiving a disability tax credit for the mother's own health limitations. This credit is granted when medical practitioners certify prolonged limitations in mental functions, mobility, hearing, or similar constraints. While not capturing every health challenge, it serves as a broad indicator of significant impairments such as fatigue, burnout, or mental health challenges.

Figure 7 shows the average post-birth ATT across years 1-7 is 49.933% (SE = 14.902), statistically significant at the 1% level, representing a near-doubling in maternal disability benefit receipt. The effect grows over time, mirroring the trajectory of child disability. The pre-treatment ATT is negative and large in magnitude (-25.453%, SE = 16.505), though not statistically significant, supporting the robustness of parallel trends.

This dramatic increase likely reflects multiple concurrent mechanisms. First, genuine health deterioration from caregiving strain—consistent with Breivik and Costa-Ramón (2024), who find adverse child health

[23] Online Appendix Table E.5 provides a non-exhaustive list of health conditions eligible for the Child Disability Benefit, along with references to studies showing their association with low birth weight and prematurity.

events are associated with 55% increases in maternal mental health visits within one year.[24] Second, if some children's conditions have a genetic component, the child's diagnosis may raise the mother's awareness of her own health issues, leading to discovery of pre-existing conditions she previously dismissed or normalized. This is consistent with treated and control mothers having similar pre-birth labor market outcomes, as mothers might participate normally while unaware of conditions for which they could claim benefits. Third, having navigated the complex disability benefit system for her child, the mother becomes more knowledgeable about eligibility and procedures, reducing the transaction costs of applying for herself.

The observed sharp rise likely represents the combined effect of: (1) genuine health deterioration from caregiving stress, (2) increased awareness and diagnosis of pre-existing conditions, and (3) reduced barriers to navigating the benefit system. While the data do not allow decomposition of relative contributions, all three channels plausibly operate simultaneously. Regardless of the specific mix, the result indicates that having a child with health complications substantially increases mothers' disability benefit receipt, directly constraining labor market participation.[25]

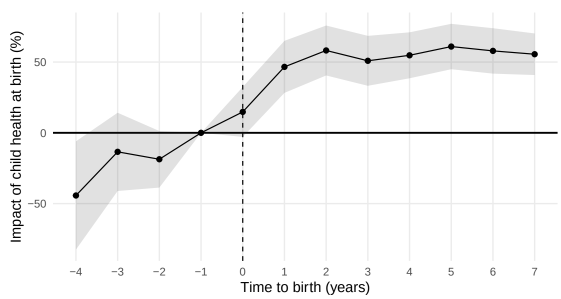


Figure 7: Impact of child's health status at birth on maternal work limitations

Notes: Probability of receiving disability tax credit for own health limitations. N = 636,000 mother-year observations. Average post-birth ATT (years 1-7): 49.933% (SE = 14.902, t = 3.35, $p < 0.01$); pre-treatment ATT: -25.453% (SE = 16.505, t = -1.54, p = 0.12). Effect grows over time, mirroring child disability trajectory. Standard errors clustered at matched-pair level, 250 bootstrap replications.

6 Robustness Checks

The main results rely on the identifying assumption that, conditional on the matching variables, the timing and severity of infant health complications are as good as random. While the parallel pre-trends provide strong support for this assumption, I conduct several additional tests to assess the robustness of the findings.

Placebo test. To validate that the observed penalties are not driven by spurious correlations rather than genuine infant health effects, I conduct a placebo test by randomly reassigning treatment status within matched pairs. I repeat this procedure 1,000 times, generating a distribution of placebo treatment effects under the null hypothesis of no true effect. The distribution of placebo estimates is tightly centered around zero for all outcomes, with no discernible pattern across event time. Crucially, in all 1,000 replications,

[24] Additional evidence comes from Burton, Lethbridge and Phipps (2008), showing Canadian mothers of disabled children rate their health status as generally poor.

[25] A relevant question is what happens to partners. Unfortunately, the available data do not allow us to answer this.

none of the randomly assigned placebo effects approach the magnitude of the true effect, supporting the interpretation that the observed infant health penalty is not attributable to chance or to unobserved factors correlated with the matching procedure. The placebo distributions are not included in the manuscript due to Statistics Canada confidentiality rules but are available upon request.

Existing child health burden. A potential concern is that mothers of unhealthy newborns may be more likely to have previously unhealthy children, introducing upward bias. To address this, I restrict the sample to first-time mothers, eliminating confounding from prior births. The results remain substantively unchanged for all outcomes including mechanisms (child disability benefits, maternal disability claims, and maternity leave exhaustion; see Online Appendix Figure I), confirming that the penalty reflects the focal child's health rather than accumulated fertility burdens.

Threshold sensitivity. To assess whether minimal health differences near the low birth weight threshold drive the results, I incrementally exclude observations in bins of 100g, 200g, 300g, 400g, and 500g around 2,500g. As the exclusion window widens, the penalty becomes more pronounced, supporting that more severe health consequences lead to larger penalties (results available upon request). This gradient is consistent with a causal interpretation rather than measurement error or threshold artifacts.

Alternative matching approaches. The results are robust to different matching approaches including: (1) exact matching on birth year and province only; (2) exact matching on birth year, province, parity, and own disability history; (3) one-step propensity score matching with all variables simultaneously. Across all specifications, an increasing penalty emerges post-birth with very similar magnitudes, confirming that the two-step coarsened exact matching followed by propensity score matching used in the main analysis is not driving the results (see Appendix G).

7 Discussion and Conclusion

This paper examines how infant health complications—specifically low birth weight and preterm birth—affect maternal labor market outcomes in the years following childbirth.The central finding is that mothers of unhealthy infants experience a persistent 2.6% decline in labor force income averaged over seven years. While this percentage may seem modest in absolute terms, benchmarking against the child penalty reveals its economic significance: it represents an additional $1,150 per year in lost income, or roughly 10% of the total earnings loss attributable to having a child—an economically meaningful increment on top of an already-substantial motherhood penalty. The magnitude of this penalty is consistent with estimates from the broader literature on childhood health shocks, which documents maternal earnings losses ranging from 3% following a child's cancer diagnosis (Adhvaryu et al., 2023) to 4–5% following a diagnosis of type 1 diabetes (Eriksen et al., 2021) and 4.6–4.7% following a child's hospitalization (Breivik and Costa-Ramón, 2024). Studies examining more severe conditions such as childhood disability document larger effects, reaching 12% in Taiwan (Cheung et al., 2025). That the penalty documented here falls at the lower end of this range is expected, as low birth weight and prematurity affect a larger share of families and are on average less severe than the conditions examined in those studies.

The persistence of the penalty over seven years, rather than fading as children age, points to the main interpretation: low birth weight and prematurity are not transient shocks but initial manifestations of ongoing health vulnerabilities generating sustained caregiving demands. Evidence for this interpretation comes from several sources. First, mothers of unhealthy infants take longer maternity leave in the immediate post-birth period, suggesting heightened care needs from the outset. Using data from all provinces except Quebec,[26] I find that giving birth to a low birth weight or preterm child is associated with an additional 0.23 (sample mean: 48.77 weeks) weeks of maternity leave (Online Appendix Table H). The effect is more pronounced when considering the probability of taking the maximum entitled leave: mothers with unhealthy infants are 14% more likely to take the full 60 weeks of available maternity leave, indicating that initial care demands are substantial enough to exhaust available benefits. Second, disability benefit receipt rises sharply and persistently—child disability benefits by 60%, dependent tax credits by 50%, and maternal disability claims by 50%—indicating that children's health challenges extend well beyond infancy with spillovers to maternal health. The income gradient in labor market responses reveals different binding constraints by resources: high-income mothers adjust along the intensive margin (reducing hours, accessing specialized childcare, negotiating flexibility) while maintaining employment, whereas low-income mothers face rigid jobs incompatible with intensive caregiving demands, forcing complete labor market withdrawal. The parity heterogeneity reinforces this interpretation: first-time mothers adjust primarily along the intensive margin, but when an unhealthy infant arrives alongside existing children, the total caregiving load becomes unsustainable within employment, pushing mothers toward complete withdrawal.

While the evidence points toward ongoing caregiving demands as the main interpretation, this conclusion is inferred from observable patterns rather than directly measured caregiving time. Alternative interpretations deserve consideration. One possibility is benefit-induced work reduction: if disability transfers create work disincentives, mothers might voluntarily reduce labor supply. However, several pieces of evidence argue against this interpretation. Most importantly, Canadian disability transfers are not means-tested—eligibility depends on medical certification, not income or employment status—removing mechanical work disincentives. Additionally, total family income declines alongside labor earnings, indicating transfers do not fully compensate, and controlling for benefit receipt does not attenuate the earnings penalty (see Appendix E). Sensitivity analyses argue against fertility adjustments and partnership dissolution as alternative explanations. Regarding fertility, the matching procedure ensures that treated and control mothers are identical in terms of parity, and controlling for family size does not attenuate the estimated penalty. Regarding partnership dissolution, I find no evidence that infant health shocks increase the probability of separation or divorce.

Several data limitations deserve acknowledgment. First, the sample is restricted to mothers with a tax record in each year between 2002 and 2018. While filing a tax return is expected of all Canadian residents, non-compliance is not uncommon among individuals with very low or zero earnings (Robson and Schwartz, 2020). If the health shock increases the probability of dropping out of the tax system—for instance, because the most affected mothers exit the labor market entirely and cease filing—then the estimated penalty likely represents a lower bound of the true effect. Second, the analysis of household responses relies on

[26] Maternity leave in Quebec is not managed by the federal employment system, which is the source of information on maternity leave.

an inferred measure of partner income constructed as the difference between family and maternal labor force income. This measure may include other household income sources and assumes constant household composition over time, which limits the precision of conclusions regarding partners' labor market responses to the infant health shock. Third, while the evidence is consistent with ongoing caregiving demands as the main interpretation—particularly the sharp and persistent increases in disability tax credit receipt for both children and mothers—the data do not allow direct measurement of caregiving time or a definitive decomposition of the relative contributions of each channel. A large body of evidence documents the long-run consequences of low birth weight and prematurity for children's health trajectories (Hack et al., 1995; Saigal and Doyle, 2008), cognitive development (Figlio et al., 2014), and educational attainment (Black et al., 2007), and the patterns documented here—sustained increases in disability benefit receipt extending well beyond infancy—are consistent with this literature. However, the absence of direct caregiving time measures and detailed health records means that the caregiving interpretation, while well-supported, remains inferential.

Fourth, the administrative data do not contain information on occupation or industry, which prevents the analysis from examining whether mothers of unhealthy infants transition toward more family-friendly jobs as an additional margin of adjustment. Recent evidence suggests that job characteristics may play a limited role in this context—for instance, Breivik and Costa-Ramón (2024) find no effect of childhood hospitalization on occupational sorting—but this channel cannot be directly assessed here. More broadly, occupation and industry represent examples of variables that would ideally have been included in the matching procedure but are unavailable in the administrative data. The comparison group constructed in this paper is therefore conditional on observed pre-birth characteristics, and unobserved dimensions along which treated and control mothers may differ—such as occupational flexibility or sector of employment—could potentially confound the estimated penalty. The ideal identification strategy would require random assignment of infant health, which is of course not feasible. A promising direction for future research would be to exploit quasi-exogenous variation in infant health at birth—such as shocks to prenatal health through environmental exposures or access to prenatal care—to obtain cleaner identification. However, such instruments would need to satisfy an exclusion restriction, which may be difficult to establish if the shock affects maternal outcomes through channels other than infant health alone.

These findings reveal how health shocks interact with economic inequality and contribute to intergenerational transmission. The concentration of penalties among low-income families, combined with suggestive evidence of increased poverty rates around school entry, suggests a mechanism through which early health shocks amplify the well-documented long-run effects of low birth weight and prematurity on children's cognitive development and educational attainment (Figlio et al., 2014; Black et al., 2007): maternal income losses reduce family resources precisely during critical developmental periods, limiting investments when children with learning challenges need them most. From a policy perspective, improving infant health at birth—through prenatal nutrition programs, cash transfers, or enhanced prenatal care—could serve as a complementary lever to traditional family policies for reducing the economic burden of parenthood. The similarity of effects between Quebec and the rest of Canada is notable given Quebec's comprehensive family support policies (universal subsidized childcare and generous parental leave). This suggests that even comprehensive family policies may be insufficient when children require specialized care. This contrasts with findings from more severe and diagnosed conditions—such as Down syndrome in Sweden, where disability transfers fully compensate maternal income losses (Einav et al., 2025)—suggesting that the broader spectrum of complications captured by low birth weight and prematurity may not qualify for the most generous support programs. Standard childcare programs and parental leave, while effective at reducing the general child penalty, do not fully address the unique needs of families with medically complex children. Targeted interventions—specialized childcare equipped for children with medical needs, extended paid leave for ongoing care, or income support for families managing chronic child health conditions—may be necessary to prevent labor market withdrawal among economically vulnerable families. This paper contributes to understanding the economic consequences of infant health complications affecting 8% of births and reveals that addressing health inequalities at birth is not only a matter of child wellbeing but also a lever for reducing gender inequality and breaking cycles of intergenerational disadvantage.

Declarations

Author Contribution

The author is solely responsible for the conceptualization, data analysis, interpretation of results, and writing of this manuscript.

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Competing Interests

The author has no competing interests to declare that are relevant to the content of this article.

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