The Effect of Reducing Welfare Access on Employment, Health, and Children's Long-Run Outcomes

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Abstract

Welfare caseloads in North America halved following reforms in the 1990s and 2000s. We study how this shift affected families by linking Canadian welfare records to tax returns, medical care, educational attainment, and crime data. We find substantial and heterogeneous employment responses that increased average income despite reduced transfers. We find zero effects on aggregate healthcare costs, but mothers saw reduced preventative care and increased mental health treatment, consistent with the transition to employment elevating time pressure and stress. We find no effect on teenagers' education or criminal charges as young adults but find some evidence of intergenerational welfare transmission.

JEL Codes: H23, H31, I14, I24, I38, J62

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Prior to major reforms in the 1990s and early 2000s, welfare systems across North America prioritized families with young children. That focus was justified as helping the "deserving poor", including children born into poverty, and investing in future generations (Ziliak, 2015). It was reflected in the almost exclusive restriction of benefits to lone-parent families in the US welfare system, whose name – Aid to Families with Dependent Children (AFDC) – says it all. It was also a focus of the Canadian welfare system. The reforms represented a dramatic shift in this perspective. They cut welfare caseloads in general but especially reduced receipt by families with children, based on a belief that existing systems created intergenerational dependency (Page, 2004).

The impact of these reforms was sizeable. In the US, welfare caseloads dropped by 50% between 1995 and 2000 (Hoynes, 2009). Equally large declines occurred in Canada's welfare system known as Income Assistance, or IA (Kneebone and White, 2014). In British Columbia (BC), the province we study, caseloads dropped by 50% between 1996 and 2004.

The predicted consequences of the reforms ran between two extremes. In the most optimistic outlook, the reforms would improve parental health, child health, and child educational attainment by raising family income, improving feelings of self-efficacy stemming from employment, and providing positive role modeling for kids. Governments touted this as a 'tough love' approach.[1] The most pessimistic critics argued that the reforms would reduce income, increase stress, and, if work was found, reduce parental time with children – all of which would worsen child health and education. One prominent policy commentary on the BC reforms stated: "We are deeply concerned that the new welfare rules are a social catastrophe in the making... with profound social and health consequences"(Klein and Long, 2003). We investigate which view is accurate or whether the truth lies somewhere in between.

A major innovation of this paper is our novel data. We link individual-level panel administrative data from numerous government ministries, including all outpatient and hospital care, most pharmaceutical spending, education attainment, IA receipt, tax data, and criminal charges. This allows us to examine the holistic impacts of welfare reform alongside the income and employment effects that are the usual focus (see Ziliak (2015); Chan and Moffitt (2018) for reviews).

[1] A BC government press release wrote: "These income assistance reforms are designed to help break intergenerational dependency on welfare... this legislation will give the ministry the tools it needs to provide assistance, create opportunity and help people achieve independence."(Ministry of Human Resources, 2002) A Wall Street Journal article described the US reforms as "the greatest advance for America's poor since the rise of capitalism" (Deparle, 2004)

Our setting is British Columbia's 2002 reform that majorly cut welfare access, especially for mothers. Our identification strategy leverages a key aspect of the reform: a reduction in the age of the youngest child (from 7 to 3) at which a mother in receipt of IA was required to search for work. In a difference in differences setup, we compare families newly subject to these requirements with families already subject to them (a control group with children aged 8 to 11).[2]

After the reform, IA receipt dropped by 7.2 percentage points in the treated group relative to the control group. Among those 7.2 p.p., annual benefit loss was $10,687 (equal to 12 months of benefits). By comparison, a minimum wage job earned $14,000 (35 hours $\times$ 50 weeks $\times$ $8 = $14,000). Employment income offset these benefit declines: 63% of the complier group moved from zero to positive earnings and *average* family after-tax income rose. However, a small minority of families saw income drop, and evidence suggests that increased childcare costs partly offset the mean income gains. Because most mothers gradually exit IA as their children age, we view the complier group as mostly employable mothers for whom the reform accelerated the return to work.

For health, we find precise zero impacts on total universally insured healthcare costs for mothers. For every dollar saved by the government in reduced IA payments, health costs increased by 0.35 cents, with 90% confidence bounds of -2.2 and 2.9 cents. However, underlying the zero overall effect are impacts in some health services with plausible mechanisms linking them to welfare reform. We find suggestive evidence that the reform reduced preventative care and increased mental health treatment. These findings are consistent with mothers' shift to employment elevating stress and reducing time available for non-urgent care. In contrast to mothers, we do not find evidence of changes in any health outcomes for children (aged 8 to 15).

Unlike hospital and outpatient care, pharmaceuticals are not universally insured in Canada. IA recipients receive full coverage, but low-income non-recipients do not. Among mothers in the complier group, we estimate that losing IA increased the percent of drug costs paid privately by 84 percentage points. Related to this, we observe reductions in most medication use by mothers, including chemical contraceptives, but not by children.

If the transition to employment reduced parent-child time, we might expect impacts on child

[2]Work-search requirements (and exemptions for mothers with young children) are embedded in welfare programs elsewhere such as TANF in the US (Congressional Budget Office, 2022). The imposition and enforcement of search requirements reduced welfare receipt in Australia (Chan et al., 2024) and the Netherlands (van den Berg, van der Klaauw and van Ours, 2004). Cook and East (2024) find that work search requirements for SNAP recipients with children reduce benefit receipt but have no impact on employment.

education outcomes. However, we find that IA receipt from age 8 to 15 had zero impact on grade 10 test scores and high school graduation. Nor do we find a detectable impact on the likelihood of being charged with a crime as a young adult (age 20 to 21). However, we do find that IA receipt during adolescence increases the likelihood of receiving IA when they reach young adulthood (age 20 to 21). Given the lack of educational attainment effects, parental role modeling and information transmission are plausible mechanisms, which are emphasized by Dahl, Kostol and Mogstad (2014) and Hartley, Lamarche and Ziliak (2022).

Our overall conclusion is that neither extreme view of the reform's potential impacts was correct. The transition to employment increased average income but likely also increased maternal stress and reduced time available for preventative health care. Despite these changes, adolescents' health and education attainment were not significantly affected. This apparent resilience may reflect mothers sacrificing time for themselves to shield their children.[3] It may also reflect Canada's public health and education systems providing services regardless of families' income level and source, and to a lesser extent, the government's partial and means-tested childcare subsidy that may have modestly smoothed mothers' return to work.

A large literature has concluded that welfare reform increased employment among lone mothers (Ziliak, 2015), but evidence of effects on other margins is more limited. The few existing studies of health typically rely on a narrow set of self-reported measures: both Kaestner and Tarlov (2006) and Basu et al. (2016) find limited effects on maternal self-reported mental and physical health, but Basu et al. (2016) find that welfare reform increased smoking and drinking. Among children, Gennetian et al. (2010) find small effects of the US welfare-to-work experiments on parent-reported child health.[4] For education outcomes, Duncan, Morris and Rodrigues (2011) show that *income* gains from US welfare-to-work experiments increased test scores and Miller and Zhang (2009) find that US welfare reform raised test scores. These studies measure test scores at younger ages than we do, which may explain the minor discrepancy with our results[5], although Løken, Lom-

[3] Bitler et al. (2023) show related evidence of mothers acting as buffers when families lose nutrition benefits.

[4] An exception to the use of self-reported outcomes is Leonard and Mas (2008) who study how TANF time limits impact infant mortality. The US safety net literature contains three additional exceptions: Almond, Hoynes and Schanzenbach (2011) studies the effect of the roll-out of food stamps on birth weight and Hoynes, Miller and Simon (2015) and Evans and Garthwaite (2014) study the impact of the EITC on birth weight and the prevalence of risky levels of biomarkers in mothers. The broader literature on the EITC's health impacts has also relied more heavily on self-reported measures (Averett and Wang, 2018; Boyd-Swan et al., 2016; Braga, Blavin and Gangopadhyaya, 2020).

[5] Herbst (2016) and Washbrook et al. (2011) study how work search exemptions based on the age of the youngest child

merud and Reiso (2018) find that Norwegian welfare reform reduced teenage test scores. Finally, our estimate of intergenerational transmission of welfare is consistent in sign with estimates from elsewhere (Pepper, 2000; Dahl, Kostol and Mogstad, 2014; Hartley, Lamarche and Ziliak, 2022).

Our contributions to the literature are threefold:

First, the data that we assemble provides advantages relative to past studies. For instance, while most existing studies focus on self-reported general health, we access hospital records, outpatient billings, and pharmaceutical prescriptions alongside diagnostic codes. This allows us to partly disentangle mechanisms underlying the effects of welfare reform and to calculate government fiscal spillovers. The administrative data is also less prone to measurement error, which is often severe in surveys of program participation (Meyer and Mittag, 2019). For instance, we observe intergenerational welfare transmission using caseload data and administrative linkages of all mothers and children, in contrast to research that relies on small samples of self-reported welfare receipt.

More generally, we study effects across outcomes and generations using a consistent methodology. For example, by linking children's welfare, health, and education records, we can examine impacts in all three realms using the same identifying variation. This allows us to evaluate three channels through which welfare reform may affect children's future reliance on transfers. Similarly, by studying mothers and children simultaneously, we can better understand how the adaptive behavior of mothers mediates effects on their children. In contrast, most existing literature examines a few outcomes at a time with varying methods and data.

Second, because health insurance is near-universal in Canada, we study welfare reform holding insurance coverage mostly constant. In contrast, US welfare reform reduced Medicaid coverage (Garrett and Holahan, 2000; Kaestner and Kaushal, 2003; Bitler, Gelbach and Hoynes, 2005; Cawley, Schroeder and Simon, 2006; Meyer and Sullivan, 2008), making it hard to tell whether effects on health reflect changes to health insurance or welfare access. Indeed, Medicaid access increases health care use and improves subjective health (*e.g.*, Aizer (2007); Finkelstein et al. (2012)).

Third, welfare reforms in the US, the UK (Gregg, Harkness and Smith, 2009), and Norway (Mogstad and Pronzato, 2012) coincided with expansions of in-work benefits, which complicates the interpretation of welfare reform's effects. For example, isolating the effects of welfare reform and EITC expansion has proven challenging (Grogger, 2003; Fang and Keane, 2004; Chan, 2013;

(in TANF) affect infant development. Most states cap the exemption period at age 1, much earlier than Canada.

Schanzenbach and Strain, 2021; Bastian and Jones, 2021; Kleven, 2024; Kuka and Shenhav, 2024). Similarly, US reform coincided with shifts to other safety net programs (Schmidt and Sevak, 2004), further complicating interpretation. Our setting does not feature these complications, which allows cleaner identification of the effect of welfare access.

The paper is structured as follows. Section 1 outlines the history of BC IA reform. Section 2 overviews the data. Section 3 presents the identification strategy. Section 4 presents the labour market effects. Section 5 presents effects on health outcomes. Section 6 shows long-run effects on educational attainment, criminal charges, and IA receipt as young adults. Section 7 concludes.

1 Institutional Background

1.1 Income and Disability Assistance

BC's Income Assistance (IA) and Disability Assistance (DA) provide cash transfers and support for re-entering employment to households with income and assets below thresholds deemed minimally necessary for survival. IA benefits have no time limit, decline rapidly with earnings, and require recipients to search for work unless young children are present. DA benefits are higher and without job search requirements but require a demonstrable medical barrier to work — extremely few mothers receive DA and it was not the focus of the 2002 reform. The most analogous US counterparts are AFDC/TANF and SSI, respectively.

The caseload increased markedly in the early 1990s (see panel (a) of Figure 1), coinciding with a recession, reforms that reduced access to unemployment insurance, and policy changes that broadened access to IA. By 1995, over 12% of the population of BC received IA each month. Lone-parent households made up the largest share of recipients, followed by childless adults.

Prompted by rising caseloads, the federal government ceased matching provincial spending on social programs in 1996, opting to provide block grants instead. This shift incentivized provinces to lower caseloads. BC responded with legislative reforms in 1996 and 2002 and ongoing regulatory adjustments.[6] The 2002 reform was dramatic. It included requiring new applicants to demonstrate two years of financial independence prior to application (intended to prevent young people from accessing IA), a three-week waiting period after application, and a reduction of the age of the youngest child below which a mother was exempt from job search requirements (from age 7 to

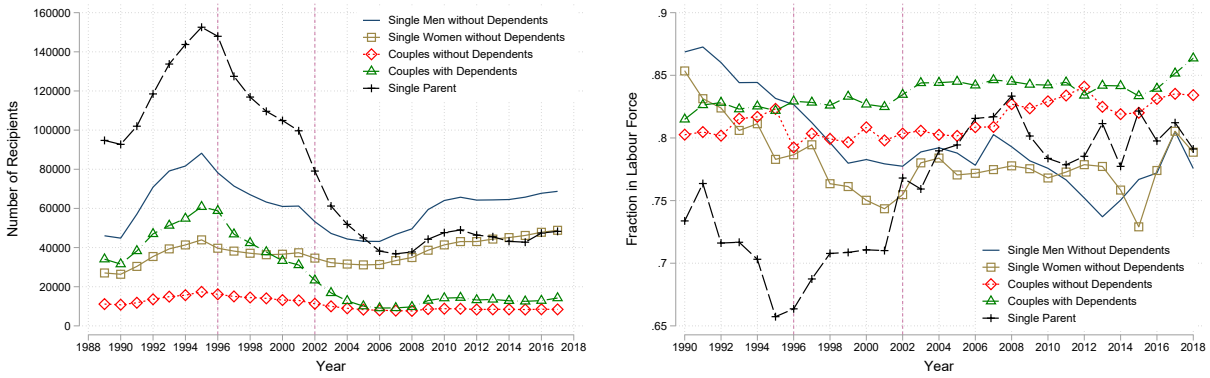
[6]Regulatory changes included closing 40% of the offices where people applied for benefits (Hicks, 2024).

3).[7][8] These changes in work search requirements will guide our identification strategy.

After the reforms, the caseload fell from 350,000 in 1995 to under 150,000 in 2006. Lone and two-parent family recipients (including children) decreased from 220,000 to 50,000. In contrast, the childless adult caseload dropped from 150,000 to 80,000. The system transformed from a widely accessed safety net with an emphasis on families to a much smaller system with a greater emphasis on childless adults and people with greater health problems (Green et al., 2021*b*).

Figure 1: **Welfare and Labour Market Trends in British Columbia**

(a) **Number of IA Recipients** (b) **Labour Force Participation Rates**



Note: Panel (a) plots the number of recipients (IA and DA combined) as of January 1st in each year. Panel (b) plots labour force participation rates among persons aged 20-59 with less than a university education. Trends are shown for five mutually exhaustive household types.

In contrast to the changes in access rules, the benefit levels for recipient families only fell by 6% (see Figure III.1). As a result, our study of the 2002 change in access rules is not complicated by changes in benefit rates. A single parent with one child and a couple with two children received monthly benefits of $896 and $1,051 in 2001, respectively. By comparison, the poverty line was $1,972 per month for a family of two and $2,943 for a family of four.[9]

The reforms are evident in the trajectory of labour force participation shown in panel (b) of Figure 1. Participation rates among lone-parent families (without a university degree) inversely track caseloads, with sizeable increases following the 1996 and 2002 reforms. By 2004, the long-standing gap in participation rates between single mothers and women without dependents had

[7]Other changes in 2002 included the elimination of the $200 monthly earnings exemption, an increase in the tax back rate on other income from 75% to 100%, and the elimination of a 'transition-to-work' benefit of $150 per month.
[8]TANF's age-of-youngest exemption for work search is 12 months or less in most states (Rowe and Murphy, 2008).
[9]Statistics Canada's before-tax low-income cut-offs for cities with over 500,000 inhabitants in 2001 (see Table 11-10-0241-01, Low-income cut-offs (LICOs) before and after-tax by community size and family size, current dollars).

closed completely, as had occurred in the US following welfare reform in 1996 (Kleven, 2024). In contrast, the participation rate for couples without dependents was constant, consistent with this group's low welfare use. Participation rate trends for couples with children were also quite flat, reflecting a stable labour market during this period. This provides an easier environment for identifying policy impacts than the late 1990s US which saw a coinciding economic boom.

1.2 Health and Education Institutions

All hospital and medically necessary outpatient care is universally insured in Canada. Pharmaceuticals, dental care,[10] vision and some medical devices are not. IA recipients receive 100% subsidization for pharmaceuticals, while non-recipients receive partial means-tested coverage.[11]

About 93% of children attended public schools in 1998, and 7%, mainly from high-income households, attended private schools. Our education data includes private and public schools.

2 Data and Sample Selection

2.1 Data Sources

We utilize a rich linkage of government administrative panel data. We first describe each individual administrative dataset and then outline the linkage between the data sets in the following section. A summary of the data sources and variable definitions is included in Appendix I.4.

Income Assistance: We access the universe of monthly IA/DA records (1989-2019) that indicate the amount and type of benefits received. All household members are recorded, including children.

Employment, Income, and Taxes: We access tax records of BC residents from 1998 to 2018. We obtain after-tax income — the sum of all income sources, including transfers and alimony, minus tax liabilities — from the main tax form (the T1). Mothers have strong incentives to file returns to receive child tax benefits and IA recipients are nominally required to file. An estimated 92 to 96% of mothers appear in our tax data (see Appendix III.3 for details). We obtain employment income from employer-issued T4 slips (the analog to US W-2s), which we observe regardless of whether

[10] Children from low-income households were all eligible for the same dental subsidy regardless of IA status. Among adults, only those with disabilities or "persistent multiple barriers to work" received dental coverage.

[11] Before 2003, only IA recipients and seniors received drug subsidies, except in catastrophic circumstances. In 2003, a 70% subsidy was introduced for non-senior, non-IA recipients, for drug costs above a deductible equal to 0%, 2%, and 4% of gross family income, for families with income less than $15,000, between $15,000 and $30,000, and greater than $30,000, respectively. Hanley et al. (2008) find that this subsidy expansion had very little effect on the private expenses of non-IA households, perhaps due to most pharmaceutical needs not exceeding the deductible. Drugs delivered in a hospital inpatient setting are universally insured.

the person files a T1.

Hospital and Outpatient Care: The universe of hospital inpatient visits and hospital-based day surgeries is reported in the Discharge Abstract Database (DAD) (see Appendix I.2 for more details). Outpatient services are recorded in the Medical Services Program (MSP) data. The MSP is the government's universal insurance program that covers all medically necessary care not directly provided by hospitals. We access the universe of MSP procedure-level billings (1991 to 2020), including the cost of each procedure and the associated ICD9 diagnosis code. The combined hospital and outpatient data cover most healthcare services, except for non-medically-necessary dental and vision services and allied health professionals.[12] About 70% of emergency room visits are identifiable in the combined DAD and MSP data during our time period (Peterson et al., 2021).

Pharmaceuticals: We access PharmaNet (1995 to 2020), which tracks all prescriptions filled in community pharmacies regardless of insurance coverage, the total cost of the prescription, and the fraction of the cost paid by government insurance. We observe American Hospital Formulary Service (AHFS) Pharmacologic-Therapeutic Classification codes for each prescription, allowing us to examine sub-categories of pharmaceutical use, specifically those related to mental health. PharmaNet excludes prescription drugs dispensed in a hospital or mental health center.

Education Records: We observe all primary and secondary school enrollments, whether in public or private schools, along with high school graduation records, from 1991 to 2020. We also observe grade 10 test scores in Mathematics and Languages (see Section 6 for further details).

Criminal Charge Records: We observe all criminal charges issued in the province from 2001 to 2019, regardless of whether the charge led to a conviction. With this data, we construct an outcome variable indicating whether children were charged with a crime at age 20 or 21.

2.2 Data Linkage and Sample Selection

Linking Datasets: Tax and IA data are linked at one access location, while health and education files are linked to IA caseloads at another. The tax data is not linked directly to the health and education data. Due to a government restriction imposed on the linkage between tax and IA records, we restrict the tax sample to all adults who received IA at some point between 1989 and 2001 (the year before the reform), and all children associated with those adults. We refer to this as the *re-*

[12] Allied health services include acupuncture, massage therapy, physiotherapy, non-surgical podiatry, naturopathic and chiropractic services, and (often) psychologists.

stricted sample throughout. The linkage of health and education records to IA records contains the universe of residents, which we call the *full sample*. To maintain a consistent sample between the tax and health analyses, we impose the restricted sample criteria on the health data in our baseline analyses but show robustness to the full sample. When examining children's long-run outcomes in Section 6, we also show results from both the restricted and full samples.

Linking Mothers to Children: We use BC birth records to identify all biological mother-child pairs for children born in BC. We identify (i) biological pairs for children born *outside* BC and (ii) non-biological mother-child pairs using the MSP health insurance registry and the welfare records. Appendix I.1 has details. Our approach successfully attributes a mother to over 97% of children.

Sample Selection and Variable Definition: We exclude fathers because it is more difficult to link men to children and the links that can be made may be affected by IA receipt. As part of the identification strategy described in Section 3 for contemporary outcomes, we restrict the sample to mothers (age 20 to 60) whose youngest child is between ages 4 and 11. This means we capture teen births, as a 20-year-old mother with a 4-year-old child would have given birth at age 16.

We drop the very few mothers who received DA before the reform. Throughout the paper, we define "IA receipt" as receiving either IA or DA, in case some mothers switch to the disability program after the welfare reform, but in practice, no such switch was detectable.

2.3 Descriptive Statistics and Patterns of Income Assistance Use

Table 1 shows descriptive statistics from 1998-2001 for mothers and children in the full sample, the restricted sample, and among IA recipients. Illustrating the widespread nature of IA in the 1990s, 34% ($\frac{246297}{729158}$) of the full sample received IA at least once between 1989 and 2001. Single parents make up 52% of the restricted sample and 71% of IA recipients.

Mothers in the restricted sample are less healthy than those in the full sample, and IA recipients are the least healthy. A more muted gradient is evident in children. Children generally interact with the healthcare system less: healthcare spending per child is one-third of the adult amount.

Unsurprisingly, IA recipients have substantially lower labour market attachment. Their average employment income is $3,300 compared to $9,600 for the restricted sample. Figure III.2 shows that IA benefits constitute 48.9% of total annual income among IA recipients. The next largest source is child tax benefits (23.8%), followed by market income (21.4%).[13]

[13]Some mothers move between IA and employment during the year, causing both market income and IA receipt to

Table 1: **Descriptive Statistics, 1998-2001**

Health Data Adults	Full Sample		Restricted Sample		On IA	
	Mean	SD	Mean	SD	Mean	SD
On IA In Year	0.13	0.33	0.38	0.48	1	0
Months IA Received in Year	1.09	3.17	3.22	4.78	8.56	3.88
IA Benefit Amounts in Year	924	2820	2735	4311	7265	4056
Age	37.74	6	34.92	6.28	34	6.55
Number Kids	2.13	1	2.13	1.12	2.06	1.09
# of Emergency Room Visits	0.16	0.65	0.27	0.91	0.36	1.14
Hospital Inpatient Costs	132	554	178	644	204	695
Outpatient Expenditure	382	478	441	524	494	554
# of General Practitioner Visits	5.05	5.83	6.16	7.52	7.43	9.45
Received Mental Health Diagnosis	0.25	0.43	0.32	0.47	0.38	0.49
Received Injury Diagnosis	0.19	0.39	0.23	0.42	0.26	0.44
Has Prescription	0.69	0.46	0.69	0.46	0.75	0.43
Fraction Drug Cost Out of Pocket	0.87	0.30	0.67	0.43	0.27	0.36
Received Pharma Contraceptive	0.10	0.30	0.12	0.33	0.15	0.36
Observations	729158	.	246297	.	92731	.
Health Data Children	Mean	SD	Mean	SD	Mean	SD
Age	10.68	2.06	10.61	2.05	10.55	2.05
# of Emergency Room Visits	0.15	0.48	0.18	0.55	0.20	0.60
Hospital Inpatient Costs	32	205	40	229	43	237
Outpatient Expenditure	144	216	155	228	170	238
# of General Practitioner Visits	2.22	2.62	2.32	2.72	2.63	2.95
Received Mental Health Diagnosis	0.06	0.24	0.09	0.28	0.10	0.30
Received Injury Diagnosis	0.19	0.39	0.21	0.41	0.22	0.42
Has Prescription	0.45	0.50	0.44	0.50	0.50	0.50
Fraction Drug Cost Out of Pocket	0.39	0.48	0.29	0.44	0.13	0.30
Observations	978597	.	334961	.	109680	.
Tax Return Data	Mean	SD	Mean	SD	Mean	SD
Single Parent	.	.	0.52	0.50	0.71	0.45
Employed	.	.	0.60	0.24	0.45	0.25
Individual Employment Income	.	.	9600	12800	3300	5900
Individual After-Tax Income	.	.	17200	10500	15700	7200
Spousal Market Income	.	.	13000	20800	4300	11400
Family After-Tax Income	.	.	29200	18500	20800	11200
Observations	.	.	235092	.	94485	.

Note: Means and standard deviations of key variables from 1998 to 2001 for mothers aged 20-60 whose youngest child is between ages 4-6 or 8-11. The Full Sample contains all such mothers in the administrative files. The Restricted Sample contains mothers who received Income Assistance (IA) at some point from 1989 to 2001 (potentially as children). The On IA sample further restricts to mothers receiving IA in the year of observation. The sample of children are those aged 8 to 15 who are in a household whose youngest child is age 4-6 or 8-11. Dollar amounts are rounded to the nearest $100 due to disclosure rules, shown in 2002 CAD, and winsorized at the 99th percentile.

3 Identification and Estimation for Contemporary Outcomes

Our goal is to estimate the causal impact of welfare access on labour and health outcomes. We adopt an identification strategy based on the 2002 drop in the age of the youngest child at which a mother is required to search for work. Enforcement of the requirements is detailed in Appendix II.

3.1 First Stage: Impacts on IA Use

To illustrate our identifying variation, in panel (a) of Figure 2 we plot the fraction of mothers who receive IA by the age of their youngest child, for each year between 1998 and 2006. The figure shows two patterns. First, receipt declines as the youngest child ages, from 47.5% at age 4 to 36.6% at age 11 in 1998. This decline is also evident in the full sample of mothers (see Figure III.3). It reflects a dynamic in which mothers enter IA around the time they give birth, then slowly exit as their children age (see Figure III.4).[14] The second pattern is the decline in IA receipt after the 2002 reform. Importantly for our identification strategy, that decrease was larger for mothers of younger children, resulting in a flattening of the age gradient. In 1998, a mother with a youngest child of age 4 was 10 percentage points more likely to receive IA than one whose youngest was 11. By 2006, that difference was only 2 percentage points[15] This flattening is consistent with the 2002 introduction of job search requirements for mothers with youngest children aged 3 to 6 years.

We use the flattening of the age gradient to identify the causal effect of IA. We define the treatment group as mothers with a youngest child aged 4 to 6 and the control group as mothers with a youngest child aged 8 to 11. The latter were already subject to the search requirement. We exclude families with a youngest child aged 3 or 7 since they may change treatment status within a given year.[16] We exclude families with a youngest child aged 0 to 2 because they did not become subject to search requirements. We exclude families with youngest children older than 11 because families with older children are, ex-ante, less comparable to the treatment group.

Figure 3 illustrates the construction of the treatment and control groups. As the youngest child ages, the family transitions from treatment to control. This means that starting in 2004, the control

appear on tax forms. Also, before 2002, IA recipients could earn small amounts before benefits were clawed back.

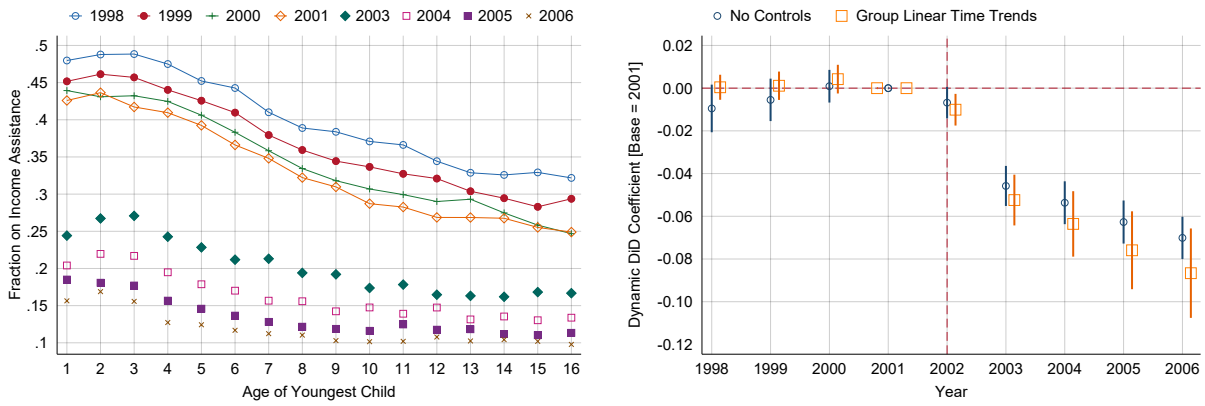
[14] Many low-income mothers do not qualify for maternity benefits due to insufficient qualifying work in the year before birth. Source: https://www150.statcan.gc.ca/n1/daily-quotidien/030321/dq030321b-eng.htm

[15] Kleven (2024) shows a similar gradient and flattening after the 1990s US reform for TANF/AFDC.

[16] We define child age as of December 31st in the calendar year. So, a 7-year-old would likely have been age 6 for some of the calendar year, in which case they effectively belong to the treatment group. We drop households with age-youngest 3 or 7 from the control to avoid this.

Figure 2: **Rotation of the Age-of-Youngest Gradient**

(a) **Age-of-Youngest Gradient** (b) **First Stage: Extensive Margin**



Note: Panel (a) plots the fraction of mothers that received IA for each calendar year and age of youngest child. Panel (b) plots estimates of π from a dynamic version of equation 1 and 95% confidence intervals. The treated group is mothers with youngest child age 4 to 6 and the control group are mothers with youngest child age 8 to 11. The two specifications shown in panel (b) are described in Section 3. Standard errors are clustered at the individual level.

group contains families that were previously subject to the work search requirements imposed in 2002 (the "treatment"). This could contaminate the control group if treatment effects persist over time. In Section 5.4, we present evidence suggesting that this is not a concern.

This identification strategy assumes that the outcomes of treated families would have evolved parallel to those of control families in the absence of the reform. As we will see, parallel pre-reform trends in outcomes support this assumption. Other contemporary policy changes were small compared to welfare reform, did not hinge on the age of the youngest child, and were likely to affect the treatment and control similarly.[17]

Our first-stage estimating equation for person i in year t is:

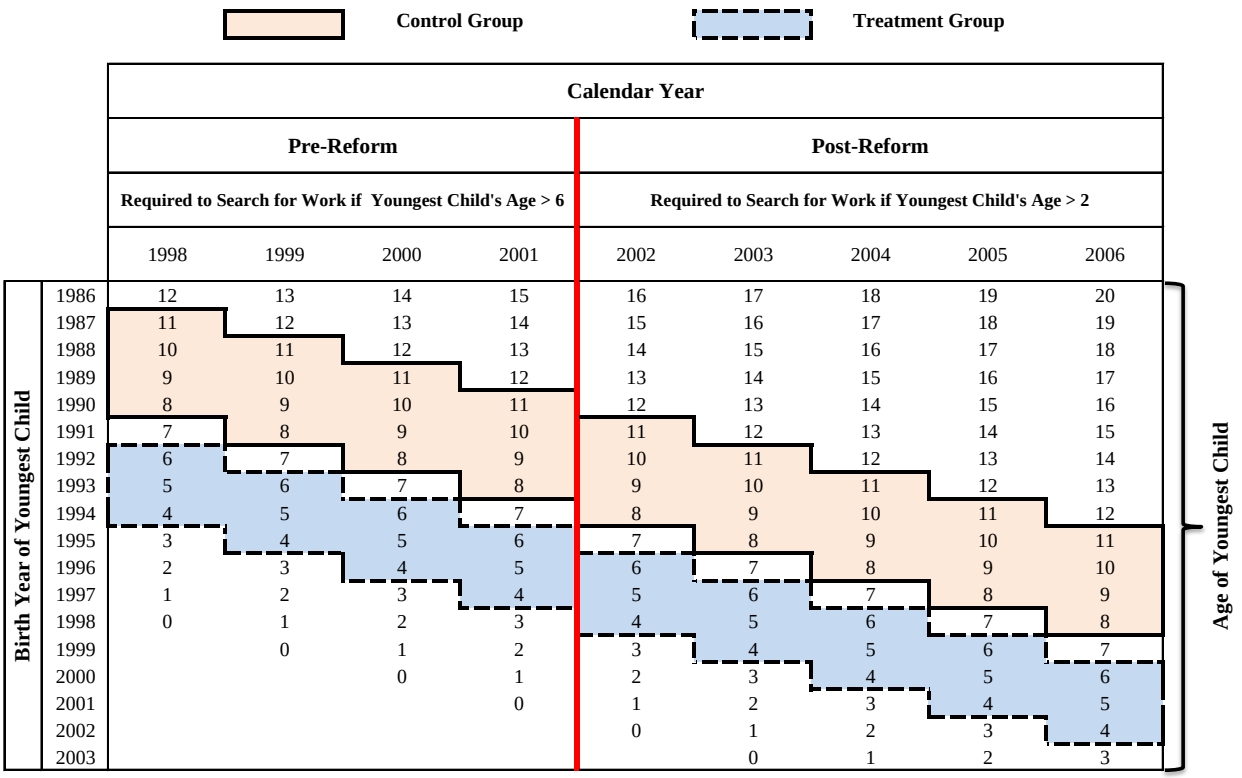
$$IA_{i,t} = \pi_t + \pi_D + \pi D_{i,t} Post_t + v_{i,t} \tag{1}$$

where $IA_{i,t}$ indicates IA receipt, $D_{i,t}$ indicates being in the treatment group, $Post_t$ is an indicator

[17] Minor reductions in provincial marginal tax rates in 2002 were unrelated to children. In the late 1990s, the federal government introduced a new National Child Benefit (NCB), which gave families with children a benefit that was not immediately taxed back with other income (Milligan and Stabile, 2011). BC had its own Family Bonus that operated on the same model and pre-dated the NCB. BC reduced the Family Bonus dollar-for-dollar with NCB income and so, until the NCB exceeded the Family Bonus in 2003, there was effectively no change in income transfer. Starting in 2002, the federal government instituted a series of increases in the NCB (increases of 13%, 4%, and 14% in 2003, 2004, and 2005, respectively). The NCB payments varied by family size but not by the age of the youngest child.

variable for the post-2002 years, and π_t and π_D are fixed effects for year and treatment group. We also show estimates that adjust for potential violations of parallel pre-trends by estimating linear trends of $IA_{i,t}$ separately for treatment and control using 1998-2001 data, projecting those trends to 2002-2006, then estimating equation 1 on the de-trended data.

Figure 3: **Construction of Control and Treatment Groups by Age of Youngest Child**



Note: Cells show the youngest child's age according to year of birth and calendar year. The control group is identified by the solid outline and the treatment group by the dashed outline. Families whose youngest child is neither 8-11 nor 4-6 years old are excluded from the analysis.

In panel (b) of Figure 2, we plot an event study version of equation 1 in which we replace $Post_t$ with a set of dummy variables corresponding to each year from 1998 to 2006. The plotted effects are the π coefficients corresponding to each year, with the effect in 2001 normalized to zero. The trend before the reform is almost flat. We find similarly parallel pre-trends when using the amount of IA benefits or the number of months of benefits as the dependent variables (see Figure III.5). We also show results using the de-trended data. After the reform, the treated group shows a relative decline in IA receipt of approximately 5 percentage points.

Table 2 presents estimates of π from equation 1 using three measures of IA use. The results

including group-specific linear trends in columns (2), (4), and (6) show that extensive margin IA receipt fell by 7.2 percentage points, months of benefit receipt fell by 0.86, and the annual dollar amount by $769.50 (in 2002 dollars). The change in dollar amount is an average over the whole treatment group (including mothers who did not change their IA status) and the extensive margin effect is the proportion who changed IA receipt because of the reform. Hence, the estimate of lost IA income for the complier group is $\frac{769.50}{0.072} = \$10{,}687.50$. This is equivalent to 1,335 hours of work, or 38 weeks of full-time work, at the provincial minimum wage at the time ($8/hour). The same calculation for months of receipt implies that compliers lost $\frac{0.858}{0.072} \approx 12$ months of benefits.

Table 2: **First Stage Estimates of Income Assistance (IA) Receipt**

	Received Any IA		Months of IA		Dollar Amount of IA	
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post	-0.054***	-0.072***	-0.663***	-0.858***	-634.1***	-769.5***
	(0.004)	(0.004)	(0.036)	(0.036)	(31.9)	(31.9)
N	461322	461322	461322	461322	461322	461322
Group Linear Time Trends	No	Yes	No	Yes	No	Yes
F Statistic	227		227		227	
Adjusted SE		0.010		0.100		90.76

Note: This table shows estimates of π from equation 1 for mothers, using three measures of IA use: extensive margin IA receipt, the number of months received, and the annual dollar amount received. The two specifications are described in Section 3. We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level and shown in parentheses. Dollar amounts are in 2002 CAD and winsorized at the 99th percentile.

Table 3: **Pre-Reform Income of Treatment Mothers that Received 12 Months of Benefits**

	Mean	SD	N
Employed	0.226	0.175	23170
IA Amount	11100	2300	23170
Individual Employment Income	740	2000	23170
Individual After-Tax Income	16400	5300	22860
Family After-Tax Income	19200	8000	22860

Note: This table restricts the sample to mothers in the treatment group (youngest child age 4 to 6) in the pre-reform period (1998-2001). It further restricts to those person-year observations in which the mother received 12 months of IA benefits. The table shows the average employment and income among this set of person-year observations. Income variables are in 2002 CAD and rounded to the nearest $100 due to disclosure rules for data access.

Based on the finding that complier mothers would have received IA for 12 months of the year in the absence of reform, we can approximately characterize this group by examining treatment group mothers who received 12 months of IA before the reform. Table 3 shows that among this

group, the average annual benefit amount was \$11,100, similar to the \$10,687 loss of benefits estimated from the first stage. This \$11,100 represented 68% of total after-tax income ($\frac{\$11,100}{\$16,400}$), while the remaining 32% came from child-related tax benefits and employment income. Of these full-time IA recipients, 22.6% received employment income which amounted to a yearly average of \$3,274.33 ($\frac{740}{0.226}$).[18] So, while some mothers supplemented IA income with small amounts of formal work, the majority (77.4%) did not.

3.2 Second Stage: Impacts on Contemporary Outcomes

Outcome Normalization: For outcomes ($Y_{i,t}$) that are non-negative and include zeros, such as earnings, we normalize $Y_{i,t}$ by the pre-reform average of the group (treatment or control) to which person i belongs in year t. In these cases, the regression outcomes, denoted $\tilde{Y}_{i,t}$, are $\frac{Y_{i,t}}{\bar{Y}_{D=1,2001}}$ and $\frac{Y_{i,t}}{\bar{Y}_{D=0,2001}}$ for treatment and control group observations, respectively. In our difference in differences framework, the treatment effects will be percent changes over the group mean, which approximate treatment effects in exponential models (*e.g.*, Poisson).This normalization follows suggestions by Chen and Roth (2023) and is economically meaningful in our context. To illustrate, consider the example of employment income. Our treatment group is mothers with younger children, which means they have lower employment participation rates than the control group. If wages are trending upwards for both groups, this will cause non-parallel trends in employment income when expressed in *levels* because of different baseline employment rates. But by dividing by $\bar{Y}_{D,2001}$, we are controlling for differential employment rates, thereby removing the non-parallel trends caused by aggregate wage trends. We do not normalize binary variables, so $\tilde{Y}_{i,t} = Y_{i,t}$ for these.

Second Stage: We first present reduced form event study specifications given by:

$$\tilde{Y}_{i,t} = \alpha_t + \alpha_D + \sum_{s \neq 2001} \gamma_s D_{i,t} \mathbb{1}\{t = s\} + \epsilon_{i,t} \tag{2}$$

where α_t and α_D are fixed effects for calendar year and treatment group, respectively. The estimate of γ_s is the difference in the outcome between treated and control groups in year s, relative to the base year 2001. Estimates of γ_s before 2002 serve to test parallel pre-reform trends. Estimates of γ_s from 2002 to 2006 quantify the reform's impact.

In our second step, we use two-stage least squares (2SLS) estimation of the equation:

[18] Before 2002, \$200/month in earnings were exempt. Additional earnings reduced IA benefits by 75 cents per dollar.

$$\tilde{Y}_{i,t} = \beta_t + \beta_D + \beta \, \text{IA}_{i,t} + \eta_{i,t} \tag{3}$$

where we instrument for $\text{IA}_{i,t}$ using $D_{i,t}\text{Post}_t$ in equation 1. Assuming that the control group is a valid counterfactual, β is the causal effect of IA receipt on $\tilde{Y}_{i,t}$ for the compliers. Or, $-\beta$ is the impact of *losing* IA due to the reform.

Exclusion Restriction: For $D_{i,t}\text{Post}_t$ to be a valid instrument for $\text{IA}_{i,t}$, the expansion of work search requirements cannot have affected outcomes of families who continue receiving IA *despite* the policy change (the always-takers). We see two ex-ante plausible violations. First, if search requirements caused some families to switch from 12 to 6 months of IA receipt, this would not be captured by the annual extensive margin ($\text{IA}_{i,t}$) but could nonetheless impact Y. However, results in the previous section suggest that complier families switched from 12 to 0 months (i.e. permanent welfare exits), suggesting this concern is unlikely an issue. Second, search requirements may impose psychological stress or time commitments on always-takers, which could affect Y. We cannot rule this out, but the reduced form estimates (γ_s) are valid regardless.

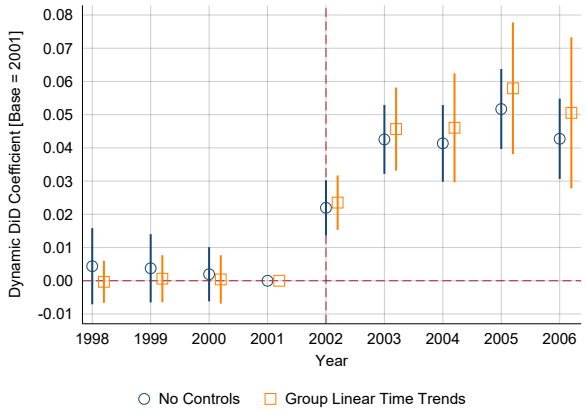
Parallel Trends: Most outcomes exhibit parallel pre-trends. However, for robustness, we also show estimates that adjust for potential violations (in the spirit of Rambachan and Roth (2023)). We estimate linear trends of $IA_{i,t}$ and $\tilde{Y}_{i,t}$ separately for treatment and control using 1998-2001 data, project those trends to 2002-2006, then work with the residuals relative to the trend. This approach is used by Bhuller et al. (2013), Dobkin et al. (2018), Goodman-Bacon (2018), and Kleven, Landais and Søgaard (2019). We adjust the standard errors following Newey and McFadden (1994) to account for noise in the de-trending step. See Appendix IV for details.

4 Labour Market Outcomes

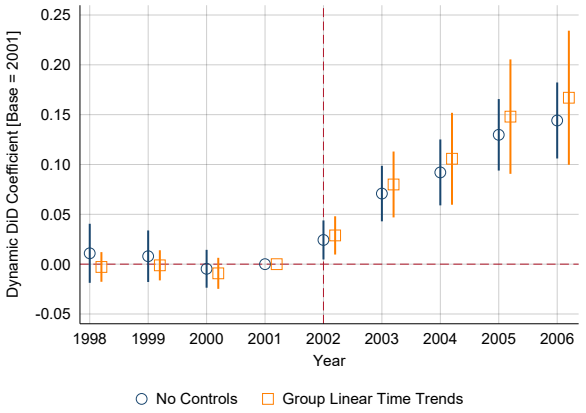
Estimates of γ_s and β are shown in Figure 4 and Table 4, respectively. As shown in panel (a) of Figure 4, there is a sizable increase in the employment rate starting in 2002 among treated mothers. The corresponding estimate of β indicates that 63% of mothers in the complier group entered employment. This large effect is consistent with the introduction of job search requirements for the treated group. Furthermore, mothers tend to leave IA as their children age (see Figure III.4). The reform's effect can be interpreted as hastening the transition to employment that was likely to occur for many mothers rather than denying benefits to those who would have used IA indefinitely.

Figure 4: **Reduced Form Effects on Labour Market Outcomes**

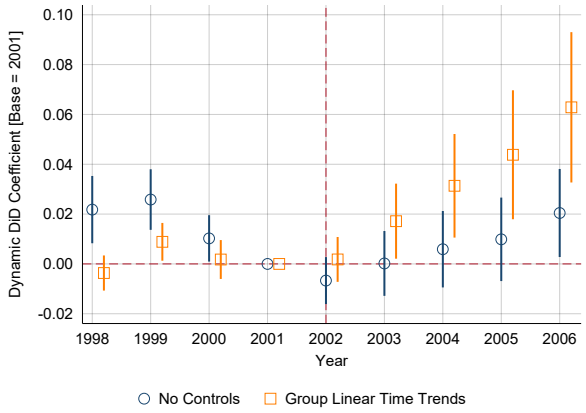
(a) **Extensive Margin Employment**



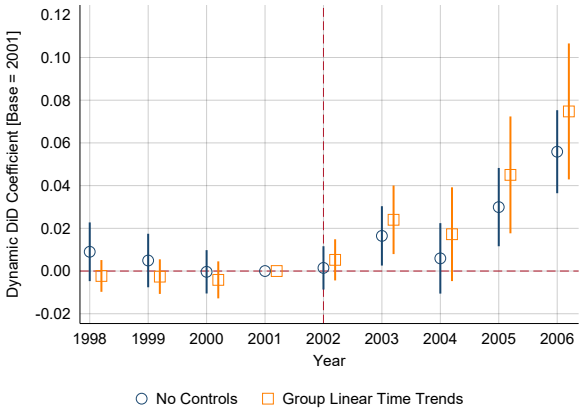
(b) **Individual Employment Income**



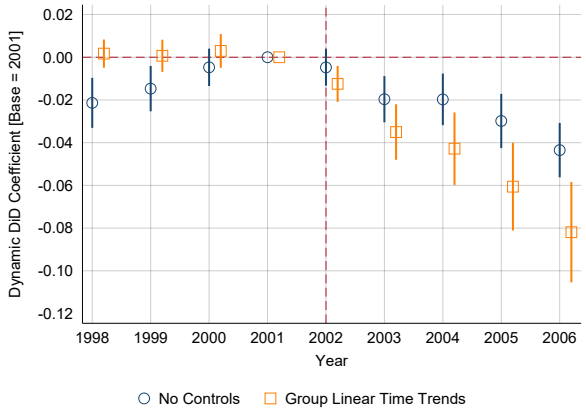
(c) **Individual After-Tax Income**



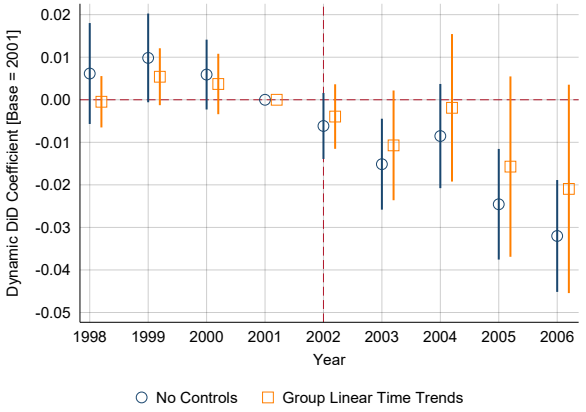
(d) **Family After-Tax Income**



(e) **Family After-Tax Income Below Poverty Line**



(f) **Single (Self-Reported on Tax Returns)**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals for both specifications described in Section 3.2. Standard errors are clustered at the individual level. Dollar-valued outcomes are winsorized at the 99th percentile and normalized as described in Section 3.2.

Panels (b), (c), and (d) show reduced form effects on three measures of (normalized) income: individual pre-tax employment income, individual after-tax income, and family after-tax income. We observe parallel pre-trends for employment income and family after-tax income, while for individual after-tax income, we need to include a pre-trend adjustment.

By 2004, mothers' earnings in the treatment group, relative to their 2001 average, had grown by 10% more than in the control group. To translate the effects on $\frac{Y_{i,t}}{\bar{Y}_{D,2001}}$ into dollars, we multiply the estimated β by $\bar{Y}_{D=1,2001}$ (see Table 4). We estimate that losing IA raises earnings by $13,726. If the $13,726 increase came solely from the 63% of mothers that went from zero to positive earnings, then the implied annual earnings of compliers would be $21,787 ($\frac{\$13,726}{0.63}$). At 35 hours a week for 50 weeks, annual earnings of $21,787 imply an hourly wage of $12.45. In 2002, the minimum wage was $8.00 and the median wage for mothers with no post-secondary education was $12.89.[19] In Appendix III.2, we calculate an implied extensive margin employment elasticity of 1.1, which is on the upper end of McClelland and Mok (2012)'s reported range of 0.3 to 1.2.

The positive effect on individual after-tax income in Figure 4 suggests that the increased earnings, on average, were more than sufficient to offset the loss of transfer benefits.[20] The IV estimates in Table 4 indicate that losing IA caused individual after-tax income to increase by $7,802: mothers lost $10,687 in IA transfers, gained $13,726 in employment earnings and $4,763 ($7,802 +$10,687 - $13,726) in other income (some of which is self-employment income). The effect on *family* after-tax income is substantially larger than the effect on individual after-tax income: $15,635 versus $7,802. Some of this difference comes from increased reporting of a spouse in tax records. Table 4 suggests 18% of complier mothers pushed off IA began reporting a spouse, increasing recorded family income. Distinguishing household formation from reporting changes is difficult because spouses are linked together on tax returns based on self-reporting.[21] It's also possible that male spouses increased labour supply in response to reform (Lichtman-Sadot, 2024). So we view the effect on *individual* after-tax income as a lower bound and the effect on *family* after-tax income as an upper bound on the real change in income of the mother's household.

[19] Calculated from hourly wages reported in the Labour Force Survey (Statistics Canada, 2002).

[20] The very small difference in observations between the employment income and after-tax income regressions is due to tax non-filing. We see after-tax income only if the mother files a T1 tax return, but see employment irrespective of tax filing from employer-issued T4 slips.

[21] A common concern in the government at the time was that mothers on welfare would not report a supporting spouse to retain welfare eligibility. It's possible that when reform pushed mothers off of IA, they increased the reporting of a spouse on their tax returns without any change in actual relationship status.

Table 4: **IV Effects on Labour Market Outcomes and Income**

	B	SE	Treated Mean 2001	Implied Effect	N
Extensive Margin Employment	-0.63	0.14	.	.	444840
Normalized Employment Earnings	-1.60	0.34	8600	-13726	444840
Normalized Individual After-Tax Income	-0.45	0.17	17300	-7802	437110
Normalized Family After-Tax Income	-0.52	0.15	30300	-15635	437110
Single Parent (Self-Reported on Tax Return)	0.18	0.13	.	.	440620
Family After-Tax Income Below Poverty Line	0.70	0.12	.	.	444840

Note: Estimates of β from equation 3 from the specification that allows for differing linear time trends between treated and control (as described in Section 3.2). We exclude 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. Dollar outcomes are winsorized at the 99th percentile and normalized as described in Section 3.2. The implied dollar effect of β is obtained by multiplying β with the treated mean in 2001.

Childcare Costs: Childcare costs may have offset the income gains of working. In the early 2000s, half-day kindergarten in public schools started at age 5, and as a result, some working mothers would require full-day or after-school childcare. Consistent with this, Appendix III.3 shows that complier mothers were 20-40 percentage points more likely to claim tax deductions for childcare costs after the reform. Year-round, full-time center-based care cost $5,928 on average in 2001 (Friendly, Beach and Turiano, 2003). However, a partial and means-tested childcare cost subsidy existed (see Appendix III.3) so offsetting effects on disposable income could be lower.

4.1 Heterogeneity and Robustness

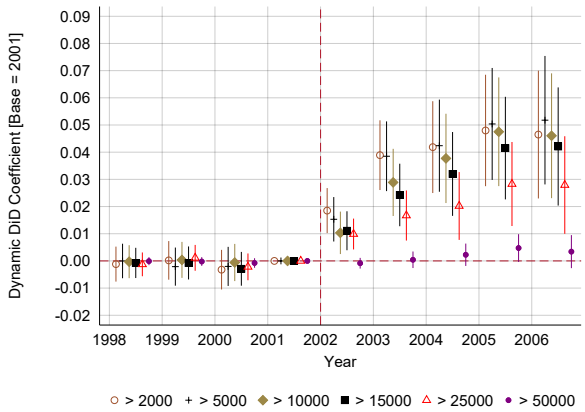
Heterogeneity: To illustrate the heterogeneity underlying the average effects, we estimate equations 2 and 3 while defining the outcome variable $\tilde{Y}_{i,t}$ as an indicator for income exceeding a specific threshold: $\mathbb{1}\{I_{i,t} > \bar{I}\}$. For example, we examine the impact of the reform on the likelihood that a mother has earnings greater than $25,000.[22] In Figure 5, we show reduced form and IV estimates for thresholds ranging from $2,000 to $50,000.

We view the $50,000 threshold ($75,000 in 2022 dollars) as a placebo test for effects on individual earnings because we expect that very few mothers affected by welfare reform would eventually move into jobs with pay this high. Panel (a) confirms that. Conversely, the treatment effects on earnings for $\mathbb{1}\{I_{i,t} > 2000\}$ and $\mathbb{1}\{I_{i,t} > 5000\}$ are nearly identical, indicating that mothers who moved into employment did so for jobs paying more than $5,000 per year. The treatment effects grow smaller as the threshold increases, illustrating the substantial heterogeneity in employment

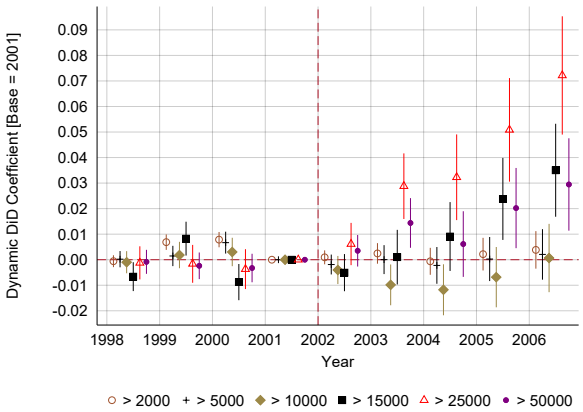
[22] We are grateful to Kory Kroft for suggesting this exercise.

Figure 5: **Distributional Estimates: Effects on Income** $> \bar{I}$

(a) **Individual Employment Income**



(b) **Family After-Tax Income**



(c) **Static IV Estimates**

	Individual Employment Income		Family After-Tax Income	
	B	SE	B	SE
2000	-0.53	0.10	0.01	0.03
5000	-0.56	0.10	0.02	0.04
10000	-0.49	0.09	0.09	0.06
15000	-0.43	0.09	-0.24	0.08
25000	-0.28	0.07	-0.58	0.09
50000	-0.04	0.02	-0.23	0.07

Note: Panels (a) and (b) plot estimates of γ_s from equation 2 and 95% confidence intervals from the specification that allows for differing linear time trends between treated and control (described in Section 3.2). Panel (c) shows the static IV estimates of β from equation 3. Outcome variables are of the form $\mathbb{1}\{I_{i,t} > \bar{I}\}$, where $I_{i,t}$ is income and $\bar{I}$ is some cutoff. Panel (a) plots individual employment income and panel (b) family after-tax income. Panel (c) shows both. Standard errors are clustered at the individual level. Dollar amounts are in 2002 CAD.

outcomes among complier mothers. Panel (b) of Figure 5 shows results from the same exercise for after-tax income. The IV estimates in panel (c) indicate that 9% of compliers saw their reported family after-tax income drop below $10,000 — recall that complier mothers received $10,687 in annual benefits (counter-factually). By contrast, about 24% of compliers saw their family after-tax income go above $15,000 and 58% saw their income move above $25,000.[23] So while many mothers saw income gains due to employment responses, a minority fell deeper into poverty.

Unreported Income: Our analysis will overestimate the income effects of welfare reform if moth-

[23]The larger effect at the $25,000 threshold is consistent with Table 3, which shows that mean family after-tax income among mothers who received 12 months of IA before 2002 was $19,200; i.e. between $15,000 and $25,000.

ers switch from informal to reported earnings.[24] This may occur because IA recipients faced high marginal tax rates on earnings due to aggressive clawbacks. Leaving IA, therefore, reduces their incentive to hide income by reducing their marginal tax rate. But this concern is probably second order. Lemieux, Fortin and Frechette (1994) find that welfare recipients had $1,200 in average *annual* undeclared income, and that number is likely much lower for mothers (Lemieux, Fortin and Frechette, 1994; Green et al., 2021*a*). Based on this, our estimated effect on formal earnings ($13,726) at most only slightly overstates the total earnings effect. Our analysis of after-tax income will also be biased if reform changed tax filing rates. Appendix III.3 shows this is not the case.

Transitions from Treated to Control: Consider a mother who exits IA in 2002 (due to the reform) when her youngest child is age 6. She re-enters employment and a career path with rising wages. When her child turns 8 in 2004, she enters the control group. The rise in her wage rate from 2002 to 2004 implies that her earnings are higher when she enters the control group, which would bias the treatment effect towards zero in 2004-2006. As a robustness check, we consecutively widen the age gap between treatment and control by sequentially dropping mothers whose youngest child is age 7-8, 6-8, and 6-9. When dropping mothers whose youngest is aged 6-9, there is no potential bias, since mothers whose youngest was age 5 in 2002 would not enter the control until 2007 (which is outside of the sample years). As shown in Figure III.7, the estimates of β are stable as the age gap widens, which implies limited confounding due to transitions from treatment to control.

Fertility: We find no detectable effect on fertility (see Figure III.9), indicating that our results are not biased by fertility-driven sample attrition.[25]

4.2 Comparisons to US Literature

Grogger and Karoly (2005) report that welfare-to-work experiments based on mandatory work or related activities had mostly positive and statistically significant impacts on employment, on average raising employment rates by 5.6 percentage points.[26] But benefit losses often offset the earnings gains, leading to negative or null effects on individual income. Experiments based instead

[24] Edin and Lein (1997) interviewed 379 AFDC recipients before welfare reform and found that 32-52% of single mothers had some unreported earnings, and 69%-91% received some cash from a family member or boyfriend.

[25] Ziliak (2015) summarizes the literature on US welfare reform's effect on fertility. The evidence is very mixed and hard to interpret given the different methodologies and the bundle of measures in US reform, including family caps, child support enforcement, and child care. Pre-PWRORA studies are equally mixed (Moffitt, 1998).

[26] This is an average over heterogeneous estimates ranging from -0.9 to 15.1 percentage points. Some authors have noted that local economic conditions may have contributed to this variation (e.g., Hotz, Imbens and Klerman, 2006).

on financial work incentives more positively impacted total income, particularly when combined with work measures (Grogger and Karoly, 2005). The variation in effects across experiments may also reflect heterogeneity: even within a single experiment, Bitler, Gelbach and Hoynes (2006) demonstrate highly heterogeneous effects.

Observational studies of welfare reform find similarly varied impacts on the employment rate of single mothers (Schoeni and Blank, 2000; Grogger, 2003; Kaushal and Kaestner, 2001; Meyer and Rosenbaum, 2001) and on earnings and income (Moffitt, 1999; Schoeni and Blank, 2000; Meyer and Sullivan, 2008). The latter's implied IV effects from state waivers — which we calculate by dividing their reduced form employment effects by their welfare participation effects — say that annual earnings rose by \$34,287.50 ($\frac{274.30}{0.008}$) (Moffitt, 1999) or 585% ($100 \times \frac{0.0503}{0.0086}$) (Schoeni and Blank, 2000) among women who lost welfare. These large and variable estimates may reflect challenges in identifying the pure effect of welfare access using US reforms. Potential confounders include EITC expansion, a macroeconomic boom, and changes in childcare and training programs (Meyer and Rosenbaum, 2001). Our setting does not face these issues.[27] Moreover, these studies use survey data that heavily misreport program participation, and to a lesser extent, earnings (Ziliak, 2015; Meyer and Mittag, 2019). In contrast, we use administrative data on both.

5 Health Outcomes

Welfare reform could affect health through multiple channels. The shift to employment may elevate the risk of injury and illness while on the job, magnify mental stress, or decrease the time available for preventative health care. The net change in disposable income may also directly affect health. Potential mechanisms for children are similar. Increased parental employment may also reduce parental time with and supervision of children, which could increase injuries due to unsupervised play and reduce the time available to take children to health care professionals. Changes in parental stress could also influence children's well-being. We cannot fully separate these mechanisms, but we can examine categories of health care more strongly associated with specific mechanisms, such as treatment for injuries, mental health, primary care visits, and preventive care.

Another channel exists in the US: welfare reform changed health insurance coverage as some mothers transitioned from Medicaid to being uninsured or having employer-sponsored insurance.

[27] US reform also lowered clawback rates for earnings. Grogger (2003) suggests that this increased work.

This is not true in Canada, except for medication (see Section 1.2) which we assess separately.

5.1 Adults

Reduced form event studies are shown in Figures 6 and 7 and 2SLS estimates (of β from equation 3) are shown in panel (a) of Table 5. As in Section 3.2, health measures that are non-negative and non-binary, such as health costs, are normalized as $\frac{Y_{i,t}}{\bar{Y}_{D,2001}}$. In this case, the coefficient β is interpreted as the average percent change relative to the pre-reform treatment group mean. The final column of Table 5 shows $\bar{Y}_{D=1,2001}$ to facilitate interpretation in terms of $Y_{i,t}$.

For universally insured hospital and outpatient spending, which make up 80% of health costs, the estimated effect is -0.06 (or -$38). We can rule out effects greater than 0.37 (or $235) and smaller than -0.49 (or -$312) with 90% confidence. To put that into perspective, the average cost of a hospital inpatient visit, an emergency room visit, and a family physician examination were $4000, $220, and $59, respectively. Put differently, every dollar of IA is associated with a -0.0036 dollar change in health costs ($\frac{38}{10,687}$), with bounds of 0.022 ($\frac{235}{10,687}$) and -0.029 ($\frac{-312}{10,687}$).

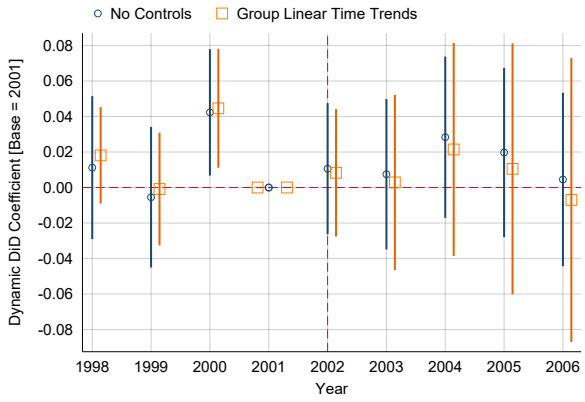
Examining subcategories of health care allows us to investigate whether the zero effect on total costs reflects offsetting effects. Panels (b) through (f) in Figure 6 plot reduced form effects on total health costs in two diagnosis categories – mental health and physical injury[28] – along with the number of visits to emergency rooms (ER), the number of visits to general practitioners (GP), and whether the mother received a pap smear cancer screening.[29] Like total health costs, these subcategories exhibit parallel pre-trends for the control and treatment groups, with the potential exception of mental health spending. GP visits and pap smear screenings are the two variables with the most statistical significance (see Table 5). Pap smear screening is a common form of preventative health care for women of this age group — guidelines recommend screenings every year following the onset of sexual activity and every two years after three normal tests (Health Canada, 2002; BC Cancer Agency, 2006) — and so are a good indicator of whether welfare reform reduced time available for preventative care. GP visits may also encompass preventative care but are a much coarser measure. The IV estimates in Table 5 suggest that access to IA increases the likelihood of receiving a pap smear by 16 percentage points (significant at the 5% level), and increases the number of GP visits by 2.22 (0.35×6.33, significant at the 10% level). These outcomes aside,

[28] ICD9 diagnostic codes 290 to 319 (mental health) and 800 to 900 (physical injuries and poisonings).

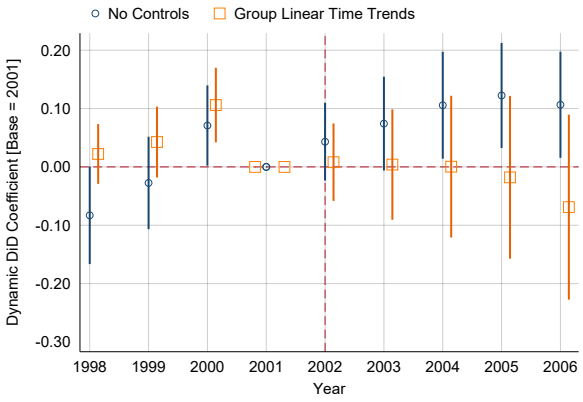
[29] GP visits include consultations, examinations, counseling, minor procedures, etc, provided by a family physician in their office or the patient's home. We identify pap smears conducted during routine pelvic examinations.

Figure 6: **Reduced Form Effects on Insured Health Care, Adults**

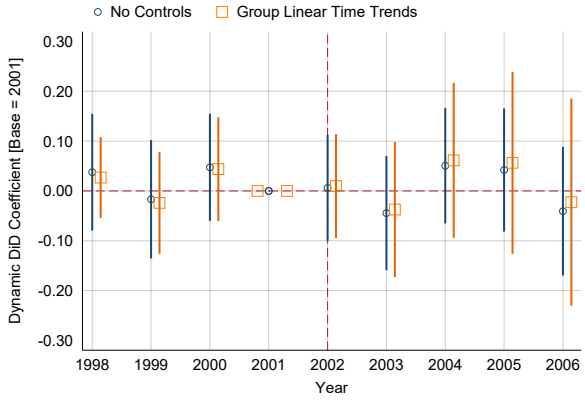
(a) **Total Outpatient and Hospital Costs**



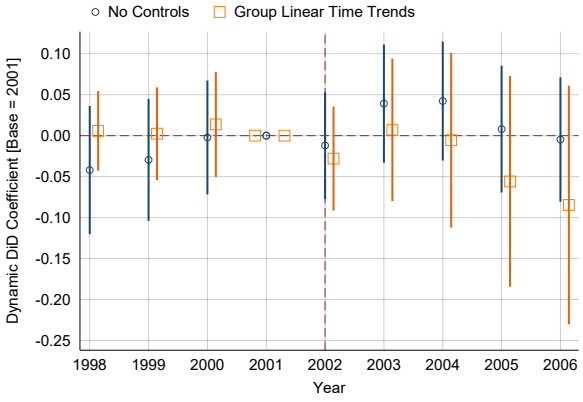
(b) **Mental Health Costs**



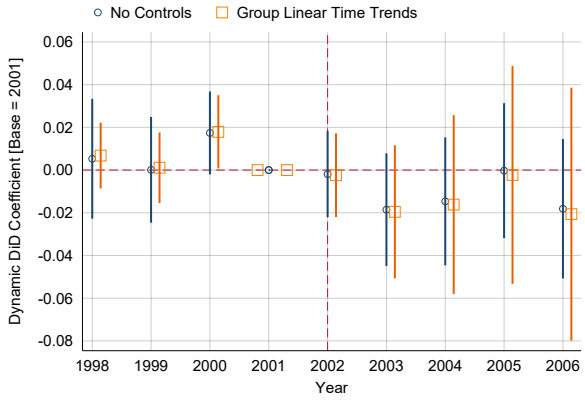
(c) **Injury Costs**



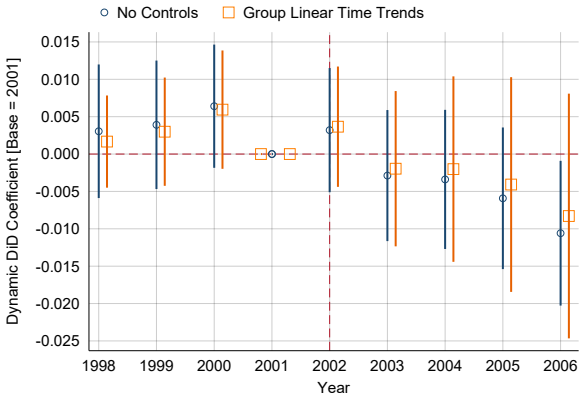
(d) **Number of Emergency Room Visits**



(e) **Number of General Practitioner Visits**



(f) **Received Pap Smear**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals for the sample of adults. The two specifications are described in Section 3.2. Outcomes are normalized as discussed in Section 3.2. Dollar outcomes are winsorized at the 99th percentile. Standard errors are clustered at the individual level.

there is limited evidence of effects on other health categories (injury, mental health, ER visits) but the noisy pre-trends in mental health spending may obscure causal effects.

Table 5: **Treatment Effects of IA on Contemporary Health Outcomes**

Panel A: Adults' Outcomes					
Observations: 461291	No Controls		Time Trends		
	$\hat{\beta}$	SE	$\hat{\beta}$	SE	Mean
Normalized Outcomes					
Hospital and Outpatient Spending	-0.06	0.26	0.12	0.51	638.95
Mental Health Spending	-2.05	0.58	0.88	1.06	56.99
Injury Spending	0.28	0.64	-0.04	1.41	30.17
# ER Visits	-0.76	0.47	0.54	0.97	0.28
# GP Visits	0.35	0.20	0.30	0.37	6.33
Pharmaceutical Days Supplied	0.81	0.28	0.20	0.49	168.36
Pharmaceutical Days Supplied of Contraceptives	2.86	0.54	-0.06	0.95	32.71
Pharmaceutical Days Supplied for Mental Health	-2.25	0.54	-1.94	0.92	41.86
Non-Normalized Outcomes					
Received a Pap Smear	0.16	0.05	0.09	0.11	0.20
Fraction of Drug Costs Paid Out of Pocket	-0.84	0.04	-0.78	0.09	0.66
Received Pharmaceutical	0.33	0.07	0.18	0.14	0.71
Received Pharmaceutical Contraceptive	0.38	0.06	0.14	0.10	0.16
Received Pharmaceutical for Mental Health	-0.09	0.07	0.00	0.13	0.24
Panel B: Children's Outcomes					
Observations: 651463	No Controls		Time Trends		
	$\hat{\beta}$	SE	$\hat{\beta}$	SE	Mean
Normalized Outcomes					
Hospital and Outpatient Spending	0.22	0.45	-0.99	1.35	186.48
Mental Health Spending	0.36	1.02	0.01	2.72	16.59
Injury Spending	-0.48	0.58	-1.23	1.88	14.03
# ER Visits	-0.87	0.60	-0.80	1.91	0.16
# GP Visits	0.21	0.26	-0.69	0.77	2.19
Pharmaceutical Days Supplied	0.15	0.85	-3.15	2.41	32.99
Pharmaceutical Days Supplied for Mental Health	-0.55	2.31	-4.53	6.00	4.49
Normalized Outcomes					
Fraction of Drug Costs Paid Out of Pocket	-0.40	0.09	-0.34	0.25	0.29
Received Pharmaceutical	0.11	0.10	-0.22	0.31	0.42
Received Pharmaceutical for Mental Health	-0.00	0.03	-0.01	0.10	0.03

Note: Estimates of β from equation 3 and standard errors (SE) which are clustered at the individual level. The two specifications are described in Section 3.2. We exclude the year 2002 since this was a partial treatment year. Dollar amounts are in 2002 CAD. The last column shows the unnormalized outcome mean for the treated group in 2001.

The main category of non-universally insured healthcare is pharmaceuticals. The government

fully pays for IA recipients' drugs, but non-IA recipients receive only partial means-tested coverage (see Section 1). Panel (a) of Figure 7 shows that the fraction of mothers' prescription costs paid privately (conditional on filling a prescription) rose sharply in the treatment group after the reform, consistent with the loss of full coverage. Table 5 indicates that losing IA increased the privately-paid share by 84 percentage points. We do not observe private insurance plans which some mothers may receive when transitioning to employment. Nonetheless, we predict this net-of-subsidy price increase to lower medication use, all else equal.

We examine this prediction in the remaining panels of Figure 7. On the extensive margin, there is a clear reduction in pharmaceutical use for mothers (panel (b)). Table 5 suggests that mothers who lose IA are 33 percentage points less likely to fill a prescription during the year. Effects on total drug consumption (combining intensive and extensive margins) are more mixed. The estimate for days supplied of pharmaceuticals is the correct sign but sensitive to the inclusion of trend controls. This suggests the extensive margin effect is driven by mothers who would counterfactually have only minor medication use.

The effect across all drug types masks substantial heterogeneity. As shown in panels (c) and (d), mothers who lose access to IA *increase* medication consumption associated with mental health treatment.[30] Table 5 indicates that losing access to IA because of welfare reform increased days supplied of pharmaceuticals related to mental health by 94 (2.25×41.86) – about three months of treatment. This is potentially consistent with the effects of non-pharmaceutical spending on mental health shown in Figure 6. Taken together, these results provide suggestive evidence that welfare reform increased maternal stress and, therefore, mental health treatment, despite the increase in out-of-pocket drug prices. In contrast, Table 5 shows that mothers who lost IA access were up to 38 percentage points less likely to receive chemical contraceptives (AHFS4 code 68:12).

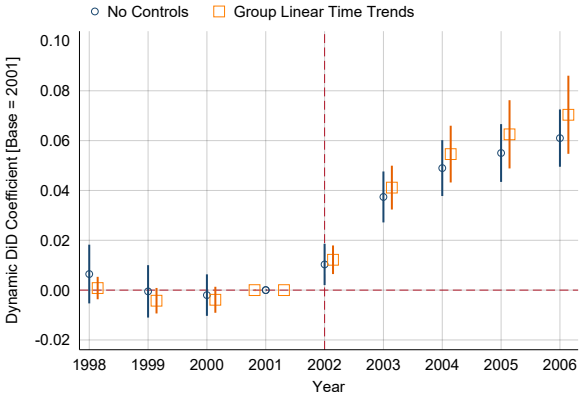
5.2 Children

To study children's contemporary health outcomes, we restrict to children age 8 to 15. Children aged 4-6 are never in the control group: they are either in the treatment group (families with youngest child age 4-6) or not in the sample (youngest child < 4). Our difference-in-difference estimator compares children age 8 to 15 with a younger sibling in the treated range against those

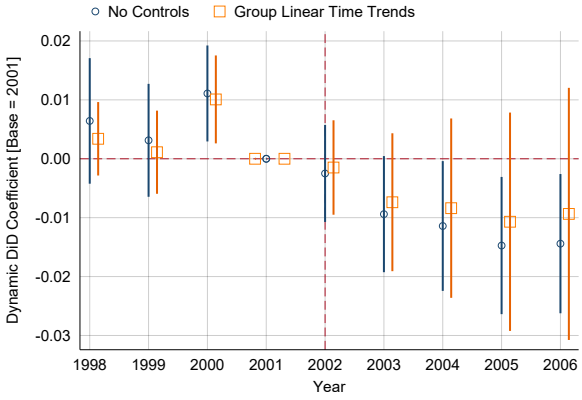
[30] AHFS4 codes for Anticonvulsants (28:12), Psycho-therapeutic Agents (28:16), Anti-manic Agents (28:28), Opiate Antagonists (28:10), and Anxiolytics, Sedatives, and Hypnotics (28:24).

Figure 7: **Reduced Form Effects on Pharmaceutical Outcomes, Adults**

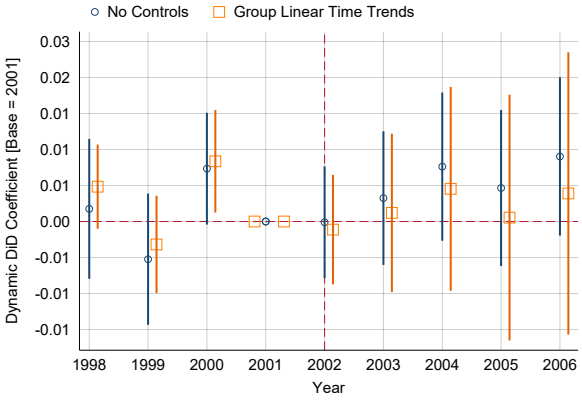
(a) **Fraction of Drug Costs Out-of-Pocket**



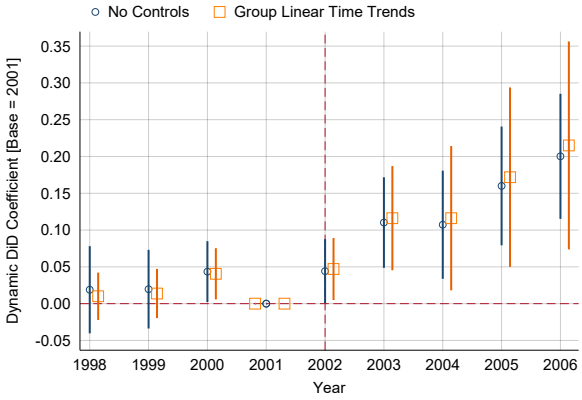
(b) **Received Any Pharmaceutical**



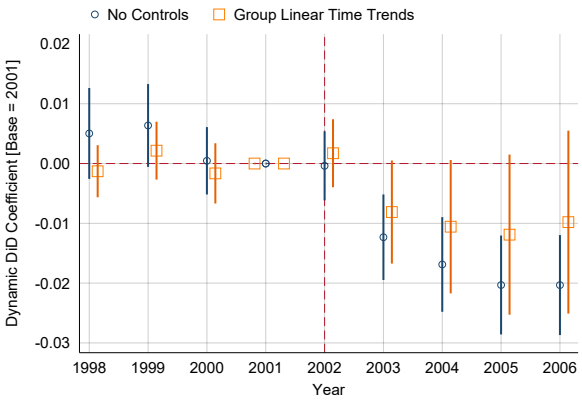
(c) **Received Mental Health Pharmaceutical**



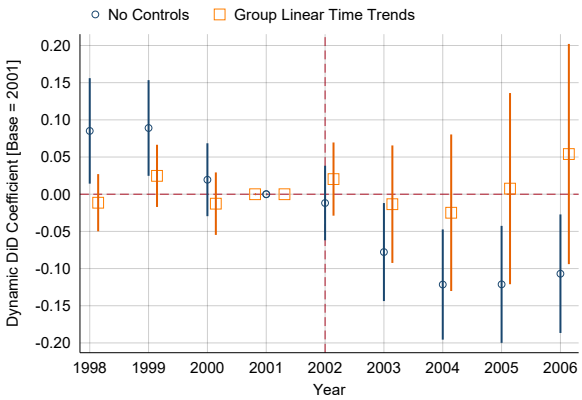
(d) **Days Supplied Mental Health**



(e) **Received Pharmaceutical Contraceptive**



(f) **Days Supplied Contraceptive**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals for the sample of adults. The two specifications are described in Section 3.2. Outcomes are normalized as discussed in Section 3.2. Dollar outcomes are winsorized at the 99th percentile. Standard errors are clustered at the individual level.

who do not.

Reduced form effects on children's health outcomes are shown in Figure 8 with corresponding IV estimates in panel (b) of Table 5. As with mothers, we can rule out modest effects on universally insured health costs for children: the treatment effect is 0.22 (or $41) with the 90% confidence interval bounded by -0.67 (-$97) and 1.11 ($179). We also fail to find effects on treatment for injuries and mental health, ER visits, and GP visits. Unlike for mothers, we do not find compelling evidence that pharmaceutical use changes for children.

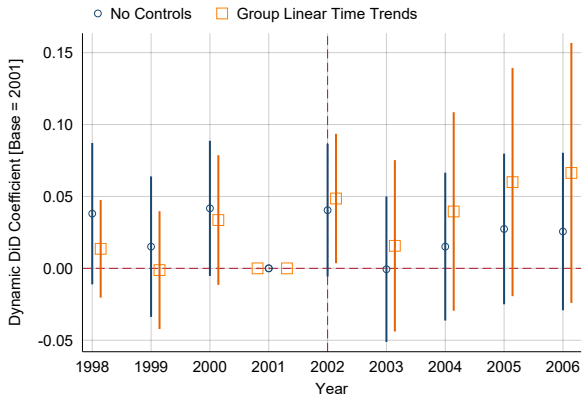
5.3 Taking Stock

The health effects demonstrated in this section are consistent with mothers' transition to employment reducing time available for preventative care and elevating stress. This is perhaps unsurprising since many of the mothers in our sample are single parents facing the pressures of child-rearing and employment. The reduction of pharmaceutical contraceptives is consistent with high price sensitivity found in other contexts (Bailey et al., 2023). Overall, however, the spillover effects of welfare reform on aggregate government health spending were minimal. In Appendix III.5, we show that for every dollar of welfare cuts, governments gained 17 cents in income tax revenue and lost only 0.4 cents from increased health costs.

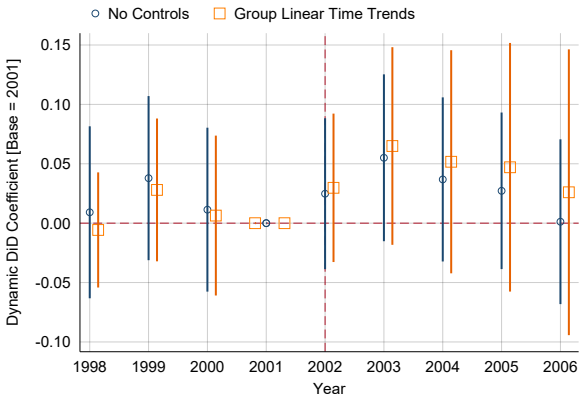
Our results are somewhat difficult to compare to the existing literature on adult outcomes. Using the Behavioral Risk Factor Surveillance System, Bitler, Gelbach and Hoynes (2005) and Basu et al. (2016) find that welfare reform reduced health insurance coverage and increased the likelihood of needing care but finding it unaffordable, while Kaestner and Tarlov (2006) and Basu et al. (2016) find limited effects on self-reported mental and physical health. Basu et al. (2016) find increases in smoking and drinking, perhaps reflecting elevated stress, which could worsen long-term health. In subsequent work using the Survey of Income and Program Participation, Narain et al. (2017) found that welfare reform worsened self-reported health. But, again, it is hard to disentangle the health insurance mechanism from the channels we investigate. A notable exception is Riddell (2020) who shows that the self-sufficiency project in Canada — which financially incentivized single mothers to transition from welfare to employment — reduced feelings of maternal depression 4-5 years after the experiment, at least in part due to elevated employment. Similarly, maternal self-reported mental health and life satisfaction improved after the 1999 introduction of in-work tax credits and welfare case management in the UK (Gregg, Harkness and Smith, 2009).

Figure 8: **Reduced Form Effects on Health Outcomes, Children**

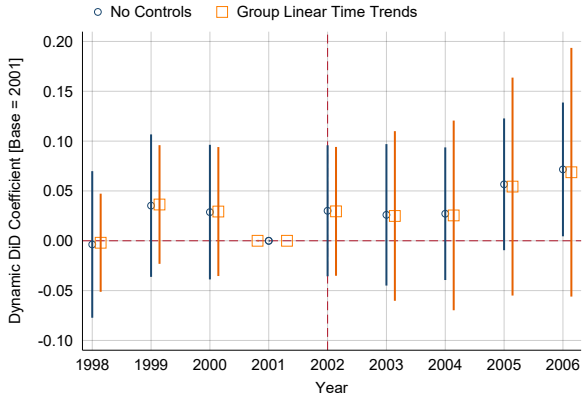
(a) **Total Outpatient and Hospital Costs**



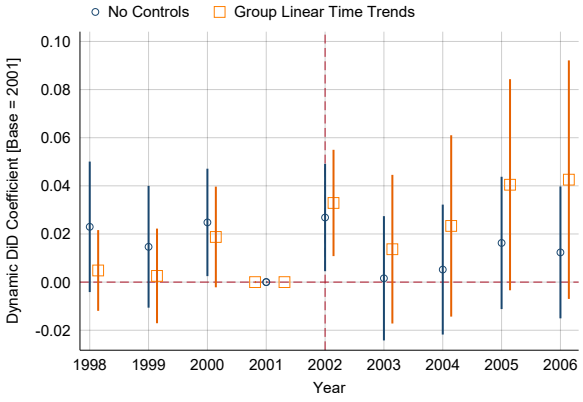
(b) **Injury Costs**



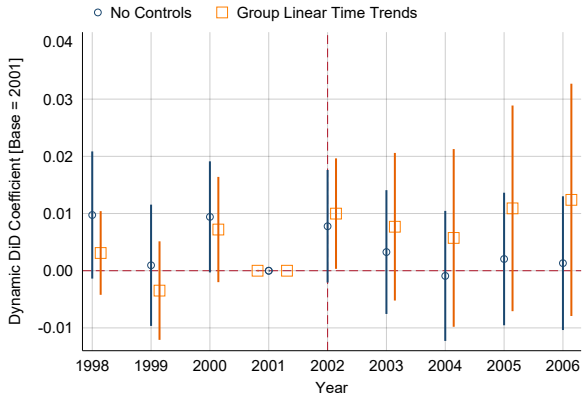
(c) **Number of Emergency Room Visits**



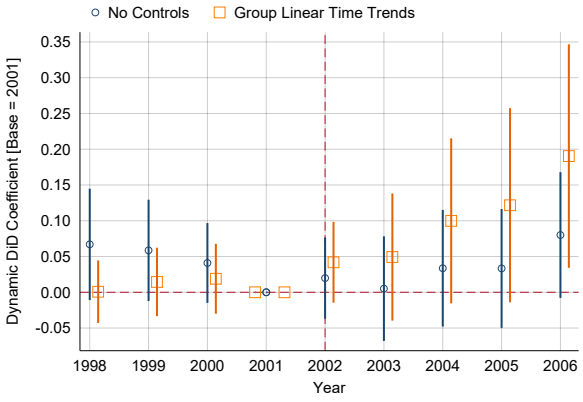
(d) **Number of General Practitioner Visits**



(e) **Received Any Pharmaceutical**



(f) **Pharmaceutical Days Supplied**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals for the sample of children. The two specifications are described in Section 3.2. Outcomes are normalized as discussed in Section 3.2. Dollar outcomes are winsorized at the 99th percentile. Standard errors are clustered at the individual level.

For children, Gennetian et al. (2010) study the effect of the 1990s welfare-to-work experiments on children aged 3-5 at the time of randomization. The authors report small negative effects on parent-reported child general health when the welfare-to-work transition did not increase household income. They find no effect on child health in families where income increased. Our results are most comparable to the second set of families. Following the 1999 UK reforms that increased maternal employment and income, teenagers' self-reported self-esteem, happiness, and behavior improved (Gregg, Harkness and Smith, 2009). Other studies focus on impacts in utero or among infants (e.g., Hoynes, Miller and Simon, 2015; Hoynes, Schanzenbach and Almond, 2016; Dench and Joyce, 2020), and little work has explicitly studied the effects of welfare, as opposed to other transfer programs (e.g., tax credits (Milligan and Stabile, 2011)).

5.4 Robustness

Transitions from Treatment to Control: We perform the same robustness check described in Section 4.1, but for healthcare use rather than earnings, and plot the results in panel (c) of Figure III.7. The estimates of β are stable as the age gap widens, which implies limited confounding from persistent treatment effects.

Robustness to Sample Restriction: For our baseline estimates, we restricted the sample to mothers (and their children) who received IA at some point between 1989 and 2001 since this is the sample available in the tax return linkage. In Figure III.8 we show that using all families regardless of their IA history leaves the qualitative conclusions unchanged.

Multiple Hypothesis Testing: In Table III.2 we present p-value adjustments that account for multiple hypothesis testing. The first approach adjusts the p-values assuming that the test statistics are independent of each other or positively correlated (Benjamini, Krieger and Yekutieli, 2006). The second approach, the Bonferroni correction, multiplies the p-values by the number of tests, creating highly conservative p-values. Using the former, all tests that were originally significant at the 5% level remain significant. The effect on GP visits for mothers, which was significant at the 10% level, loses significance. Using the Bonferroni values, the effect on pharmaceutical days supplied is only significant at the 10% level but all other results are unchanged.

6 Children's Long-Run Outcomes

Welfare reform in North America was partly motivated by a desire to reduce intergenerational welfare dependency. Candidate channels for intergenerational transmission are the effects of childhood IA receipt on (i) children's physiological development, (ii) their cognitive and emotional development, and (iii) parental role-modeling and parent-to-child information transmission about welfare programs. Section 5.2 found limited effects on children's health oucomes, implying a limited role for the first channel. In this section, we measure the extent of intergenerational transmission and the potential mediating effects through education. We also examine effects on being charged with a crime in young adulthood, which plausibly captures dimensions of human capital that are missed by education outcomes (Dahl and Gielen, 2021).

6.1 Estimating Long-Run Effects of Exposure

To estimate the long-term impacts of IA reform, we adapt the empirical strategy from Section 3. We instrument for IA receipt at ages 8-15 using the fact that adolescents with (or without) younger siblings experienced differential amounts of family IA access before versus after the reforms. We cannot estimate effects of IA exposure at ages earlier than 8 because there is no instrumental variation for those ages. We model children's outcomes as follows:

$$Y_{i,b} = \beta \sum_{s=8}^{15} IA_{i,s} + \psi_b + u_i \tag{4}$$

Where $Y_{i,b}$ is the outcome of child i born in year b, $IA_{i,s}$ is an indicator for IA receipt when the child is age s, and ψ_b and u_i are birth cohort and individual effects. A more complicated model would allow the effect of $IA_{i,s}$ to differ for each s, but for tractability, we estimate a model where $\sum_{s=8}^{15} IA_{i,s}$ is the explanatory variable of interest.[31]

We categorize children into one of three groups based on the age of the *youngest* child in the household: 1 to 3, 4 to 7, and older than 7. Indicators for the first two categories are D_{1-3} and D_{4-7} with the oldest category being the base group. The first stage equation for children of age s is:

[31] To separately identify effects at each age, we need a separate instrument for IA at each age. The difference in differences in Section 3 in theory would allow us to instrument for IA at each age (≥8), but our instruments $D_{4-7,i,s}\text{Post}_s$ are highly correlated across s, which makes it difficult to separately identify the effect of IA for each age s.

$$IA_{i,s} = \pi_{1,s} D_{1-3,i,s} + \pi_{2,s} D_{4-7,i,s} + \pi_{3,s} D_{1-3,i,s} Post_{i,s} + \pi_{4,s} D_{4-7,i,s} Post_{i,s} + \pi_b + v_{i,s} \quad (5)$$

Where π_b are birth year effects and $Post_{i,s}$ indicates that child i was age s after 2002. (π_b and $Post_{i,s}$ are perfectly collinear, so we drop $Post_s$ from equation 5.) The coefficients $\pi_{1,s}$ and $\pi_{2,s}$ are level differences in IA based on the age of youngest child in the house. $\pi_{3,s}$ and $\pi_{4,s}$ are post-reform changes in this age-of-youngest gradient. The relative decline among adolescents in the treatment group ($D_{4-7,i,s} Post_{i,s}$) is the basis of our instrument. Figure 9 shows event study versions of $\pi_{4,s}$.

Summing equation 5 across ages 8 to 15, and assuming coefficients do not vary with s, we get:

$$\sum_{s=8}^{15} IA_{i,s} = \pi_1 \sum_{s=8}^{15} D_{1-3,i,s} + \pi_2 \sum_{s=8}^{15} D_{4-7,i,s} + \pi_3 \sum_{s=8}^{15} D_{1-3,i,s} Post_{i,s} + \pi_4 \underbrace{\sum_{s=8}^{15} D_{4-7,i,s} Post_{i,s}}_{\text{Excluded Instrument}} + \tilde{\pi}_b + v_i \quad (6)$$

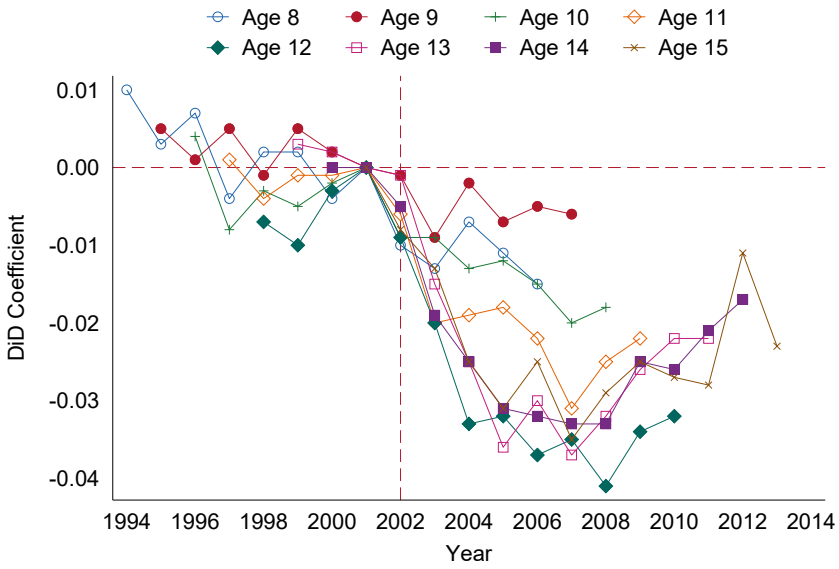
The term $\sum_{s=8}^{15} D_{4-7,i,s} Post_s$ is our excluded instrument for $\sum_{s=8}^{15} IA_{i,s}$ and $\tilde{\pi}_b$ are birth cohort effects. To set up the two-stage least squares, we include the control variables from this first stage into the second stage to get:

$$Y_{i,b} = \beta \sum_{s=8}^{15} IA_{i,s} + \alpha_1 \sum_{s=8}^{15} D_{1-3,i,s} + \alpha_2 \sum_{s=8}^{15} D_{4-7,i,s} + \alpha_3 \sum_{s=8}^{15} D_{1-3,i,s} Post_s + \psi_b + u_i \quad (7)$$

Where we estimate β using two stage least squares with $\sum_{s=8}^{15} D_{4-7,i,s} Post_s$ as the instrument. For robustness, we also estimate a specification that controls for $\sum_{s=1}^{7} IA_{i,s}$ by including the equivalent of equation 6, but summed over ages 1 to 7, as control variables in equation 7.

Outcomes: Our first education performance measure is test scores in Languages and Mathematics in grade 10 (age 15-16), described in Appendix I.3. We average the child's percentage score across subjects, then normalize to standard deviation one across students. Our second educational outcome is high school graduation. To evaluate intergenerational transmission, we measure whether the child received IA when they were 20 or 21 years old, separately from their parents. Our final outcome is whether the child was charged with a criminal offense (in the province) at age 20 or 21. Age 21 is the oldest age we observe for all birth cohorts of children in our sample.

Figure 9: **First Stage At Each Child Age**



Note: Estimates of $\pi_{4,s}$ from the event study version of equation 5 for each age s from 8 to 15. Some age-year pairs are not in the sample because of the birth cohort restrictions. *E.g.*, the coefficients for children of age 8 end in 2006 because the oldest birth cohort is 1998 and they turn 8 in 2006. Standard errors are clustered at the family level.

Sample: We use children born between 1986 and 1998 (inclusive). The 1998 endpoint ensures we observe all cohorts up to age 21, since the data ends in 2019. The 1986 endpoint is the first year we observe family composition. Because the first birth cohort was age 16 in 2002 (i.e., after the reform), we measure IA up to age 15. We restrict to children enrolled in school at ages 8 and 14 to ensure the child was in the province during their adolescence.[32] Because the long-run analysis requires a long time span of data that overlaps considerably with the years used to define the "restricted sample" described in Section 4 (1989-2001), our preferred results use the full sample (subject to the above criteria). Nonetheless, estimates for the restricted sample are qualitatively unchanged (see Table III.3).

6.2 Results

Table 6 shows OLS and 2SLS estimates of equation 7 for each outcome. Unsurprisingly, the OLS estimates indicate that children whose families receive IA have lower test scores, are less likely to graduate high school, are more likely to receive IA as young adults, and are more likely to be charged with a criminal offense as young adults. Below, we discuss the IV estimates.

[32] We condition on age 14, not 15, to avoid selecting on high school drop-out, which can occur as early as age 15. The legal school-leaving age during our period was 16, but there was imperfect compliance.

Education: Effects on education outcomes statistically disappear in the 2SLS. Column (2) indicates that an extra year of IA worsens test scores by a statistically insignificant 0.0051 standard deviation. We rule out effects greater than 0.09 and less than -0.10 with 90% confidence. There are similarly null effects on high school graduation in column (6): a year of IA lowers the probability of high school graduation by a statistically insignificant 0.4 percentage point or 0.5% ($\frac{0.004}{0.8}$).

This fits with previous estimates of the effect of transfers on test scores, which tend to be null or small and positive when family income increases (*e.g.* Miller and Zhang (2009); Simonsen, Skipper and Smith (2022); Chen, Osberg and Phipps (2015)). For example, comparing estimates across 1990s welfare-to-work experiments, Duncan, Morris and Rodrigues (2011) estimate that a $1,000 rise in income corresponds to a 0.05-0.06 standard deviation increase in scores, which is similar in size to what Dahl and Lochner (2012) and Bastian and Michelmore (2018) find for EITC expansions. Notably, we measure test scores at an older age: grade 10 versus, for example, grades 4 and 8 in Miller and Zhang (2009). Cognitive development may be more malleable in early childhood (Heckman, 2006), which could explain our slightly more null effects. Alternatively, our null effects may reflect a limited change in family income as our earlier results imply that income gains when mothers transitioned to work might have been offset by fixed costs of work. It may also be that mothers' reduced time at home did not affect their children's schooling. This fits with Bastian and Lochner (2022) and Kalil et al. (2023) who find that employment induced by EITC and welfare reforms did not affect parent-child time spent on human capital enrichment. However, Løken, Lommerud and Reiso (2018) find that Norwegian welfare reform reduced teenage test scores in some households, likely through both income and time effects.

On high school graduation, Miller and Zhang (2012) and Dave, Corman and Reichman (2012) find suggestive evidence that US welfare reform increased graduation rates. Similarly, Hartley, Lamarche and Ziliak (2022) find that a mother receiving AFDC/TANF during their daughter's teenage years *decreases* the probability that the daughter has *more than* a high school degree.

Intergenerational Transmission: Columns (2) and (4) in the second panel show estimated effects on intergenerational transmission. Assuming linearity, a year of IA receipt during adolescence raises own use at ages 20-21 by up to 4 percentage points. Because we find null education effects, parental information transmission and role modeling are the most plausible mechanisms. These mechanisms are also emphasized by Dahl, Kostol and Mogstad (2014) and Hartley, Lamarche and

Table 6: **Long-Run Effects on Children's Outcomes Using the Full Sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	Grade 10 Test Scores				Graduation High School			
$\sum_{s=8}^{15} IA_{i,s}$	-0.1572	-0.0051	-0.1574	0.0095	-0.0671	-0.0040	-0.0672	-0.0128
	(0.0012)	(0.0549)	(0.0012)	(0.0602)	(0.0004)	(0.0202)	(0.0004)	(0.0216)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		93.51		76.75		106.93		87.32
Outcome Mean	0.000	0.000	0.000	0.000	0.802	0.802	0.802	0.802
N	532059	532059	532059	532059	577108	577108	577108	577108
	Received IA at Age 20 or 21				Charge with Crime at Age 20 or 21			
$\sum_{s=8}^{15} IA_{i,s}$	0.0334	0.0452	0.0335	0.0343	0.0069	0.0040	0.0069	0.0050
	(0.0003)	(0.0111)	(0.0003)	(0.0117)	(0.0002)	(0.0065)	(0.0002)	(0.0068)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		106.93		87.32		106.93		87.32
Outcome Mean	0.060	0.060	0.060	0.060	0.018	0.018	0.018	0.018
N	577108	577108	577108	577108	577108	577108	577108	577108

Note: OLS and 2SLS estimates of β from equation 7. The control $\sum_{s=1}^{7} IA_{i,s}$ is described in Section 6.1. Standard errors in parentheses are clustered at the family level. First Stage F is the Kleibergen-Paap F statistic for weak instruments.

Ziliak (2022). The latter finds large intergenerational transmission in AFDC/TANF receipt from mothers to daughters, building on work by Pepper (2000). They measure daughters' participation at ages 19-27 and 24-33, respectively; we measure IA receipt at ages 20 and 21.[33]

The IV estimates are marginally larger than the OLS estimates, inconsistent with pure selection bias. However, Hartley, Lamarche and Ziliak (2022), Dahl, Kostol and Mogstad (2014), and Dahl and Gielen (2021) all find a similar pattern, in US welfare, and Norwegian and Dutch DI, respectively. In our context, which is mostly free of measurement error, the likely explanation is heterogeneous treatment effects: the instrument (maternal work search requirements) isolates effects for teens particularly sensitive to parental information transmission and role modeling.

Crime: Columns (6) and (8) of the second panel show effects on the likelihood of being charged with a criminal offense at age 20 or 21. This is a very rare outcome, which lowers precision, but we find no statistically significant effects. If criminality reflects human capital, this is consistent with

[33] Bastian, Bian and Grogger (2021), who study 1990s US safety net reform broadly, find qualitatively similar suggestive evidence: females exposed to reform during childhood saw increased adulthood labour supply and marginally reduced benefit receipt. Price and Song (2018) find that cash assistance before age 18 (via the Seattle Income Maintenance Experiment) did not increase children's likelihood of receiving SSDI or SSI in adulthood. That experiment did not explicitly test information transmission/role modeling because the cash transfer before 18 was not SSI/SSDI.

the null education effects. Relatedly, Dahl and Gielen (2021) find that reducing parental access to Dutch DI reduced kids' criminal offenses in adulthood.

Extensions: Table III.4 shows that estimates from an IV probit model, as opposed to the linear 2SLS, are qualitatively similar, but smaller in magnitude, for our binary outcomes. Table III.5 shows no gender-based treatment effect heterogeneity for education or criminal charges. However, the intergenerational welfare transmission effect is present *only* for sons, not for daughters. This contrasts with Hartley, Lamarche and Ziliak (2022), who only study daughters.

7 Discussion

We use rich administrative data to estimate the effects of reducing access to welfare on wide-ranging outcomes of both mothers and children. We leverage a large welfare reform involving extending job search requirements to mothers of young children. That reform (in 2002 in British Columbia) was part of a wave of reforms across North America. In our complier group, the reform induced most of the mothers to move from welfare to employment, increasing total income on average — though with a small minority suffering income losses and evidence that childcare costs may have partly offset the income gains. This transition coincided with reduced preventative care and increased mental health treatment for mothers, both consistent with the time pressures and stress of being a working parent. Despite this, mothers largely protected their children from declines in health and education outcomes. Mothers' ability to act as buffers for their children is plausibly facilitated by Canada's extensive public health and education systems.

The reform we study reduced welfare access for relatively healthy and employable persons (Green et al., 2021*b*), and our analysis focuses on families with a youngest child older than age 3. Effects of welfare access may differ for more vulnerable groups, such as refugees (Dustmann, Landersø and Andersen, 2024), families with infant children, or childless adults. Hicks (2023) studies the latter in a companion paper.

Our setting bears similarities and advantages compared to studies of US welfare reforms. In particular, our analysis is not complicated by EITC and macroeconomic expansions. Moreover, Canada's nearly universal health insurance is mostly independent of welfare access, allowing for the identification of welfare access effects rather than the combined effect of insurance and welfare access. In this way, our setting is more generalizable to most non-US developed nations. In other

dimensions, Canadian welfare resembles TANF. Both support households with very low income and assets while imposing high benefit claw-backs for earnings and work search requirements.

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- British Columbia Ministry of Public Safety and Solicitor General [creator] (2021): Corrections - Adult Community and Custody V03. Data Innovation Program, Province of British Columbia [publisher] Data Extract. Approver Year 2023.

Appendix for Online Publication Only

I Data Sources and Variable Definitions

I.1 Notes on Mother-Child Linkages

We draw from two sources to link mothers to children:

1. We use data from the Medical Services Plan (MSP), BC's public health care insurance. Children are typically included in their parents' MSP contract up to age 18, and up to age 24 for children in full-time education. We identify as a child any individual aged 0-18 who appears on a woman's contract, as well as any individual aged 19-24 who appears on their contract and has more than a 16-year age difference with the oldest person on the contract.

2. We use birth records, which include any child born in BC starting in 1985. For almost all children, birth records include the personal identifier of the mother, which allows us to establish mother-child linkages.

Using birth records alone does not allow us to identify (a) non-biological mother-child relationships and (b) biological pairs for children born outside of BC. The MSP linkage fills these gaps. Where MSP and birth records disagree, precedence is given to the latter. Our linking approach successfully attributes a mother to the vast majority of children.

Over time, MSP registrants may change contract number; e.g., if the payer has changed. This may cause artificial breaks in our treatment variable and in our sample of children, possibly correlated with family economic status. To avoid this issue, after identifying the set of children who ever share an MSP contract with a given mother, we assume that the mother-child pair lives together in a given year, *as long as they are observed in the same postal code for the corresponding year*.

I.2 Costing of Hospital-Based Services

Each hospital visit is assigned a Resource Intensity Weight (RIW) based on the case mix of the patient. The RIW is then multiplied by a Cost per Weighted Case, or CPWC, to derive that visit's dollar value cost. The sum of RIW×CPWC within the province equates exactly to total hospital expenditures, although for any given visit, RIW×CPWC may over- or under-estimate the true cost.

I.3 Test Scores

We combine data from two types of evaluations. For school years 1999-2000 to 2002-2003, we use BC's grade-10 Foundation Skills Assessments (FSAs) in literacy and numeracy. The FSA is a low-stakes assessment – it does not affect students' final grades or graduation, nor is school funding tied to the results, but school-level results are publicized. Grade-10 FSAs were discontinued after 2002-2003. To extend the series on grade-10 outcomes further in time, we take advantage of a change in graduation requirements introduced in 2004-2005, including a Language Arts course and a Mathematics course in grade 10. Unlike FSAs, participation and success in these evaluations are directly tied to graduation. To mitigate the effect of this break in data sources, student results are standardized at the year level. As long as changes in incentives affected grade-10 students independently of the age of their youngest sibling, we do not expect the way our grade-10 outcomes are constructed to affect our results. Note that neither data source provides information for the 2003-2004 school year.

I.4 Summary of data sources and variable definitions

Table I.1 summarizes the data sets used in the paper, the years for which the data is available, the sample for which it is available (full vs. restricted), the variables constructed using each data set, and the age of mothers and children at which the different variables are measured.

Table I.1: **Summary of data sources and variable descriptions**

Data source	Years	Sample	Variables	Variable definitions	Mother's age	Child's age[b]
Mothers' and children's contemporary outcomes[a]						
Income assistance receipt[c]	1989-2019	Full	Income assistance (IA) receipt	Equal to one if individual was in receipt of income assistance.	20-60	
			Months of IA receipt	# months of income assistance received.	20-60	
			IA benefits received	Income assistance benefits received.	20-60	
Employment and income	1998-2018	Restricted	Extensive-margin employment	Equal to one if individual received earnings from a tax-registered employer.	20-60	
			Individual employment income	Earnings received from a tax-registered employer.	20-60	
			Individual after-tax income	Individual income from all sources, including transfer and alimony income, minus tax liabilities, as reported on personal income tax records (T1).	20-60	
			Family after-tax income	Family income from all sources, including transfer and alimony income, minus tax liabilities, as reported on personal income tax records (T1).	20-60	
			Marital status (single)	Equal to one if individual self-reported being single in their personal income tax records (T1).	20-60	
Hospital and outpatient care	1991-2020	Full	Hospital and outpatient spending	Expenditures on inpatient care, day surgeries, or medically-necessary outpatient care.	20-60	8-15
			Mental health spending	Hospital and outpatient spending for care associated with a mental health diagnostic code (ICD9 codes 290 to 319).	20-60	8-15
			Injury spending	Hospital and outpatient spending for care associated with an injury diagnostic code (ICD9 codes 800 to 900).	20-60	8-15
			# ER visits	# visits to an emergency room.	20-60	8-15
			# GP visits	# visits to a family physician (general practitioner) for consultations, examinations, counseling, minor procedures, etc.	20-60	8-15
			Received a pap smear	Equal to one if individual received a pap smear as part of a routine pelvic examination.	20-60	
Pharmaceuticals	1995-2020	Full	Received pharmaceuticals	Equal to one if individual filled at least one prescription for pharmaceuticals.	20-60	8-15
			Received pharm. contraceptives	Equal to one if individual filled at least one prescription for pharm. contraceptives.	20-60	
			Received pharm. for mental health	Equal to one if individual filled at least one prescription for pharmaceuticals associated with a mental health AHFS4 code, including anticonvulsants (28:12), psycho-therapeutic agents (28:16), antimanic agents (28:28), opiate antagonists (28:10), and anxiolytics, sedatives, and hypnotics (28:24).	20-60	8-15
			Pharmaceutical days supplied	# days of pharmaceuticals dispensed to individual.	20-60	8-15
			Pharm. days supplied of contraceptives	# days of pharm. contraceptives dispensed to individual.	20-60	
			Pharm. days supplied for mental health	# days of pharmaceuticals dispensed to individual for mental health (see definition of Received pharm. for mental health).	20-60	8-15
			Fraction of drug costs paid out of pocket	Fraction of drug costs paid for privately (private insurer or out of pocket).	20-60	8-15
Children's long-run outcomes						
Education records	1991-2020	Full	Grade 10 test scores	Percent test scores in grade 10, averaged across domains (languages and mathematics) and standardized.		15-16
			High school graduation	Equal to one if child ever graduated from high school.		N/A[d]
Income assistance receipt	1989-2019	Full	Received IA between ages 20 and 21	Equal to one if child received IA at ages 20 or 21, separately from their parents.		20-21
Criminal charge records	2001-2019	Full	Charged with a criminal offense between ages 20 and 21	Equal to one if child was charged with a criminal offence in BC at ages 20 or 21.		20-21

Notes: [a] All contemporary outcomes variables are measured for a given year. [b] Only a subset of contemporary outcomes are measured for children. [c] Income assistance includes both expected-to-work and disability benefits. [d] High school graduation is evaluated using all data available, regardless of children's age.

II Work Search Requirements and Enforcement

Before 2002, as part of receiving Income Assistance, caseworkers worked with clients to provide counseling and referrals to employment and training programs. Recipients could be required to show proof of work at any time. The Income Assistance manual for 1997 for staff members wrote:

> When proof of employment-related efforts is required, the recipient may be directed to complete an S77 (Job Search Statement) form or to provide proof of registration at HRDC and/or proof of contact with union halls and other agencies that produce work opportunities. Explain the proper use of required documents to the recipient.

At a minimum, recipients would have to go through this documentation process once per year, since employable individuals had to re-apply for IA each year. At that re-application, they were interviewed to ascertain whether they had continued seeking other sources of income (i.e., employment). That interview could result in follow-up interviews/investigations, if there was uncertainty about how diligently someone had sought employment, and a new S77 ("Job Search Statement") completed. A modern version of the S77 documentation form is shown below.

The 2002 reform emphasized diverting IA applicants and recipients toward employment.

First, the reform introduced employment plans for IA recipients. During the intake process, welfare workers established applicants' employability and identified any barriers to employment. They then used this information to design detailed employment plans outlining the recipients' obligations to pursue training and/or employment. Employment plans included start and end dates, as well as details about the name of the program or service individuals were expected to participate in and the required activities. Clients were typically required to provide monthly updates but could be required to report more frequently. Clients were informed that their continued benefit eligibility depended on their compliance with the plan (Office of the Ombudsman, 2009). Non-compliance could result in benefit reduction or ineligibility (Klein and Long, 2003).

Second, the government adopted a three-week waiting period as part of the application process, during which applicants were required to demonstrate work search activities. Applicants were instructed to contact their welfare worker to report these activities two weeks into this period. At this stage, they could be asked to undertake additional efforts in the remaining week or to restart the search altogether. Klein and Long (2003) and Wallace, Klein and Reitsma-Street (2006) report that the three-week waiting period often lasted up to six weeks until activities reported by applicants were deemed satisfactory by welfare workers. Based on data from 2002-2005, Wallace, Klein and Reitsma-Street (2006) find that 4.8% of application denials were related to non-compliance with the initial work search requirements. This is likely an underestimate, as an additional 25.8% of denied applications were due to failure to provide the required information, including – but not limited to – documents required as part of the job search (updated resume, completed Work Search Activities Record). Exemptions from the three-week work search exist for people in severe hardship. But, at the time of the reform, this exemption did not extend to parents of young children who would be exempted from looking for work while receiving welfare (Office of the Ombudsman, 2009).

In summary, work search and training requirements were present before 2002, but enforcement intensified after 2002. This is evident in the Ministry's service plan for 2002/2003 – 2004/2005 (BC Ministry of Human Resources, 2002) and in the reactions of advocates and critics of the reform (Clemens and Emes, 2001;Klein and Long, 2003; Wallace, Klein and Reitsma-Street, 2006).

Ministry of Social Development and Poverty Reduction

THIS IS A MANDATORY FORM FOR APPLICANTS & MUST BE RETURNED TO THE MINISTRY

WORK SEARCH ACTIVITIES RECORD

The personal information requested on this form is collected under the authority of and will be used for the purpose of administering the *Employment and Assistance Act* and the *Employment and Assistance for Persons with Disabilities Act*. The collection, use and disclosure of personal information is subject to the provisions of the Freedom of Information and Protection of Privacy Act. Any questions about this information should be directed to your Employment and Assistance Centre.

LAST NAME	FIRST NAME		BIRTH DATE (YYYY MMM DD)
ADDRESS		POSTAL CODE	TELEPHONE

REASONABLE WORK SEARCH ACTIVITIES CASE NUMBER (If APPLICABLE) _____ SR NUMBER (If APPLICABLE) _____

Examples of work search activities:

- Preparation of (i.e. drafting, typing, photocopying) resume and/or cover letters, when completed in combination with employer contacts
- Telephone inquiries to potential and specific employers
- Fact finding interviews, when completed in combination with employer contacts
- Responding to newspaper ads, internet
- Cold calling potential employers
- Networking with friends, relatives, neighbors previous employers, colleagues or other social contacts
- Submitting applications for employment
- Submitting letters and/or resumes for employment
- Participating in employment interviews
- Attending workshops for resume preparation or employment search

INSTRUCTIONS: List date, type of activity (e.g. resume preparation, personal interview, application, telephone call, networking, etc.), location of activity, a contact name and phone number and the results of all activities that you have done to improve your opportunities of finding work. Please refer to the Work Search Toolkit for work search ideas and activities that will assist you to find employment. Prior to submitting this form, sign and date the declaration and notification at the bottom of page 2 (reverse) of this form.

DATE OF ACTIVITY	TYPE OF ACTIVITY	LOCATION OF ACTIVITY	CONTACT NAME AND PHONE NUMBER	RESULTS OF YOUR ACTIVITY

Security Classification: MEDIUM SENSITIVITY

BRITISH COLUMBIA | Ministry of Social Development and Poverty Reduction

THIS IS A MANDATORY FORM FOR APPLICANTS & MUST BE RETURNED TO THE MINISTRY

WORK SEARCH ACTIVITIES RECORD

DATE OF ACTIVITY	TYPE OF ACTIVITY	LOCATION OF ACTIVITY	CONTACT NAME AND PHONE NUMBER	RESULTS OF YOUR ACTIVITY

(ADD ADDITIONAL PAGES IF NECESSARY)

IF YOU HAVE HAVE NOT LOOKED FOR WORK, PLEASE INDICATE WHY.

☐ HOSPITALIZED ☐ OVER 65 YEARS OF AGE ☐ MEDICAL OR PHYSICAL CONDITION WHICH PRECLUDES EMPLOYMENT ☐ FLEEING ABUSE

☐ OTHER (EXPLAIN) _____

DECLARATION AND NOTIFICATION

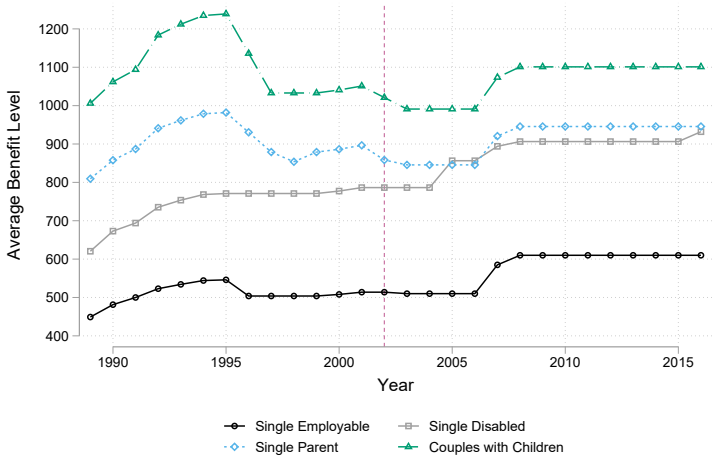
I declare that all the information I have provided in this form is true and complete. I understand the accuracy of the information I provide will be checked by comparing it against information held by other governments, private agencies and individuals. I understand that the BC government may verify and obtain information to confirm my eligibility.

SIGNATURE	PRINT NAME	DATE (YYYY MMM DD)

III Supplemental Results

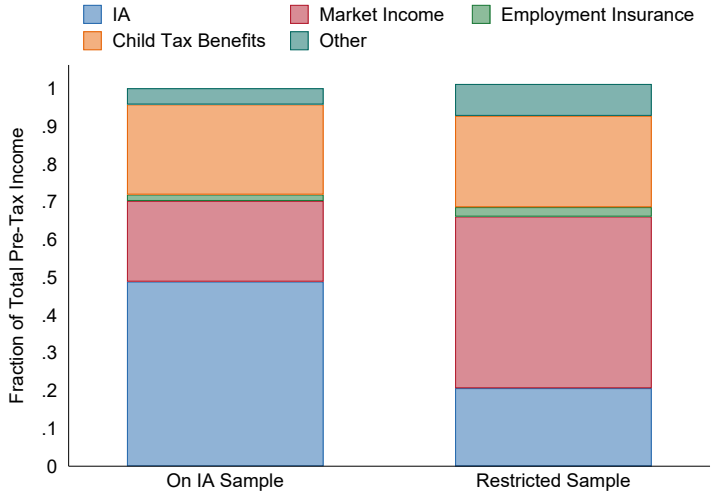
III.1 Income Assistance reform and income sources

Figure III.1: **Average Benefit Rates**



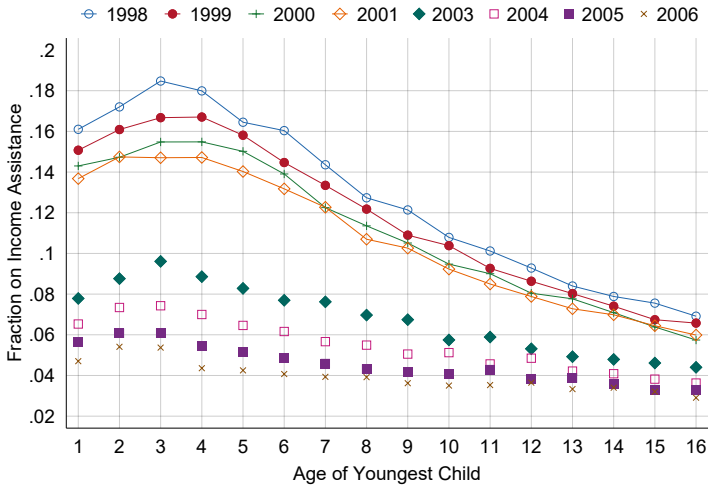
Note: This figure plots average benefit level amounts for different recipient groups: single employable adults; single adults with a disability designation; lone parents with one child, age 2; and couples with two children, ages 10 and 15. Source: National Council of Welfare (various years) and Caledon Institute (various years).

Figure III.2: **Income Composition**



Note: This figure illustrates the fraction of mothers' individual after-tax income from different sources, among those that receive IA during a given year and among the broader sample ('restricted sample'). Child tax credits are refundable credits offered through the tax system. Employment Insurance is Canada's unemployment insurance program. Market income refers to all employment and self-employment income. IA refers to IA benefits recorded on the T5007 tax slip.

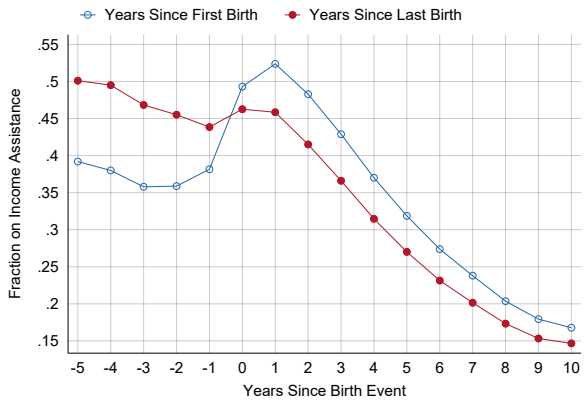
Figure III.3: **Age-of-Youngest Gradient Using the Full Sample**



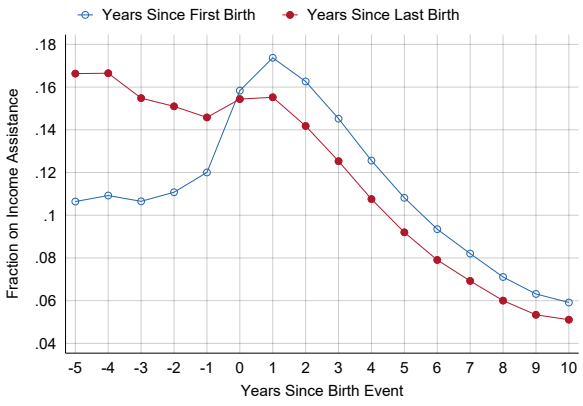
Note: This figure plots the fraction of mothers that received income assistance for each calendar year and age of youngest child in the family.

Figure III.4: **Entry into Income Assistance Receipt Around Child Birth**

(a) **Restricted Sample**



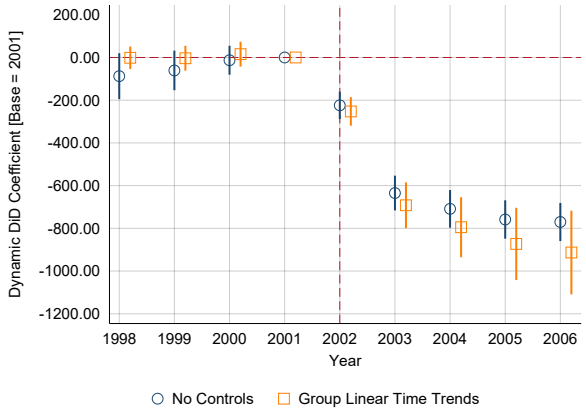
(b) **Full Sample**



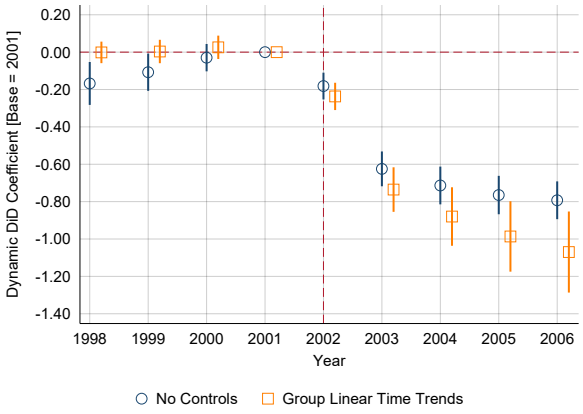
Note: This figure plots the income assistance receipt around the birth of a mother's first child and her last child, conditional on those birth events taking place between 1994 and 2002, respectively. Time 0 denotes the year of birth. Panel (a) uses the restricted sample and panel (b) uses the full sample.

Figure III.5: **First Stage Difference in Difference with Alternative Definitions of IA**

(a) **Dollar Amount of Annual IA Receipt**



(b) **Months of IA Received**



Note: Both panels plot estimates of π from a dynamic version of equation 1 and 95% confidence intervals. The treated group are mothers with youngest child age 4 to 6 and the control group, mothers with youngest child age 8 to 11. The outcome in panel (a) is the dollar amount of IA receipt during the year. The outcome in panel (b) is the number of months of IA receipt in a given year. The two specifications shown are described in Section 3.2. Standard errors are clustered at the individual level. Dollar amounts are expressed in 2002 CAD and winsorized at the 99th percentile.

III.2 Extensive Margin Elasticity Calculation

We provide a back-of-the-envelope calculation of the extensive margin elasticity with respect to $1-\tau$ where τ is the average income tax rate. The elasticity of interest is:

$$\epsilon = \frac{\%\Delta\text{Employment Rate}}{\%\Delta(1-\tau)} \tag{III.1}$$

We define the numerator as the percent change in the employment rate in the treated group, relative to the control group, due to welfare reform. This equals $\frac{0.0454}{0.57} = 0.079$.

We define the denominator as the percent change in $1-\tau$ in the treatment group, relative to the control group, due to welfare reform. This requires further notation:

- E is earnings *if a mother chooses to work*.
- Tw is taxes minus transfers (i.e. net taxes) *if a mother chooses to work*.
- Tn is taxes minus transfers (i.e. net taxes) *if a mother chooses to not work*.
- $\tau = \frac{Tw-Tn}{E}$.

So $\%\Delta(1-\tau) = \frac{\Delta Tn}{E-Tw+Tn}$ under the simplifying assumption that welfare reform only affects net taxes of non-working mothers (Tn). We calculate ΔTn based on the first stage results in Table 2 column (6), which show that treatment group mothers lost \$769.50 on average due to welfare reform (about \$10,700 per mother in the complier group). We estimate pre-reform E as the average earnings among mothers in the treatment group who were working in 2001. That value is \$15,550. Similarly, for this same group, we calculate Tw as the difference between individual market income and after-tax income, which is -\$5,050, meaning they get 5,050 in net transfers due to Canada's progressive tax structure.[34] And finally, we calculate Tn as the net taxes for non-working mothers in the treatment group, amounting to \$9,500 in net transfers on average. And so, we calculate:

$$\%\Delta(1-\tau) = \frac{769.5}{15300+5050-9500} = 0.071 \tag{III.2}$$

And therefore the elasticity is:

$$\epsilon = \frac{\%\Delta\text{Employment Rate}}{\%\Delta(1-\tau)} = \frac{0.079}{0.071} = 1.11 \tag{III.3}$$

This should be viewed as a ballpark estimate due to ignored considerations such as how to account for multi-adult-household decisions, the allocation of net taxes in multi-adult households, and childcare costs. For example, adding childcare costs as a tax on work, thus increasing Tw, would increase the denominator and therefore lower the elasticity.

Our estimate is on the upper end of estimated elasticities for lower-income mothers: McClelland and Mok (2012) report a range from 0.3 to 1.2, Gelber and Mitchell (2012) report a range of "0.35 to 1.7" (p. 873), and Goldin, Maag and Michelmore (2022) report a range of 0.7 to 1.0 for single mothers. Kleven (2024)'s estimates for the 1990s EITC expansion are 0.63, but closer to zero for smaller EITC expansions in other periods. Given EITC expansions are arguably less salient than welfare reform, it's likely not surprising that we find a larger estimate.

[34] This too is a simplifying assumption, as minor forms of income other than taxes and transfers may contribute to the difference between market income and after tax income.

III.3 Childcare in BC, Childcare Deductions and Non-Filing

Unlike the US, welfare reform in BC was not accompanied by improvements in the availability and affordability of childcare services. Parents relied on private for-profit or non-profit regulated or unregulated centres. Availability and affordability were key issues. The CCPA (2015) reports that between 1998 and 2006, regulated childcare spaces – either centre- or family-based – were available for less than 25% of children aged 0-5 years old in BC. Most of these spaces were dedicated to part-day care (Doherty, Friendly and Beach, 2003).

A means-tested childcare subsidy for low-income parents existed for regulated and unregulated centres (Doherty, Friendly and Beach, 2003). In 2001, school-age kids were eligible for $147 per month for part-time care, and $173 for full-time care. Four-year-old kids were eligible for $184 and $368 for part- and full-day care, respectively. The subsidies were clawed-back at 50 cents per dollar of income when net family income rose above $18,984 for a single parent with one child or $23,016 for a two-parent family with two children, with subsidies ceasing completely for families with income greater than $27,816 and $31,846, respectively (Friendly, Beach and Turiano, 2003; Friendly et al., 2007). By contrast, centre-based child care for school-age kids (part-time) cost $197 on average, and full-time care for pre-school age kids cost $494 on average. So for families with income below the thresholds, the subsidies were about 75% of childcare costs (Doherty, Friendly and Beach, 2003). In 2002, an estimated 35,000 children benefited from partial or full subsidies (Forer et al., 2003), about 15% of the 248,600 children aged 0-5 in BC at the time.

In 2002, the province decreased the income thresholds above which the subsidy was reduced (Kershaw, 2004; Klein and Long, 2003). By 2003, the income thresholds for receiving a full subsidy were reduced to $16,764 and $20,796 and the clawback rate was 60% (Friendly, Beach and Ferns, 2005; Friendly et al., 2007). This change would not have impacted mothers on welfare, as their family income is typically at or below the cutoffs. It may have impacted the amount of subsidy available to mothers transitioning to employment who experienced an increase in income.

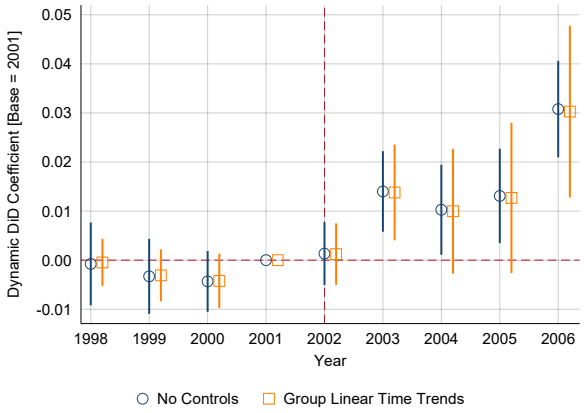
All parents could deduct out-of-pocket childcare expenses (for regulated and unregulated centres) from their taxable income (up to a cap) to reduce their tax liability. We examine this deduction as a proxy for childcare use below. Panel (a) of Figure III.6 shows a positive effect of the reform on claiming childcare deductions. The estimates in Table III.1 indicate that mothers who lost IA due to the reform became 38 percentage points more likely to claim the childcare deduction, consistent with mothers transitioning to work.

We expect that 92 to 96% of mothers appear in the tax data we use. Frenette, Green and Picot (2004) compare tax records to the Canadian Census of Population and find that approximately 96% of Canadians are observed in tax files. Similar to our tax data, their comparison uses personal income tax records combined with third-party tax forms. Drawing from death records, Green et al. (2021*a*) estimate that an additional 4% of Canadians appear in neither tax forms nor the census. They also find that women and parents are more likely to file a T1, suggesting that at most 8% of mothers may be unobserved in our tax data.

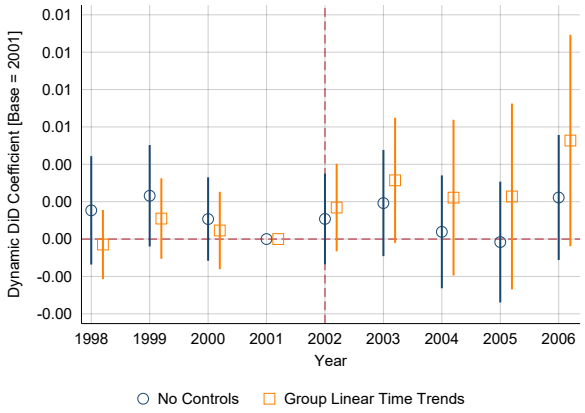
Panel (b) of Figure III.6 shows the reduced form effect of the reform on filing a T1 tax return, conditional on having a third-party tax slip issued. Here we see no effect. This is unsurprising because a number of refundable tax credits, most notably child tax benefits, are only delivered to mothers if they file a T1 return, so mothers are strongly incentivized to do so.

Figure III.6: **Reduced Form Event Studies for Child Care Deductions and Tax Non-Filing**

(a) **Claiming Childcare Tax Deduction**



(b) **Fail to File Tax Return**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals. The two specifications shown are described in Section 3.2. Standard errors are clustered at the individual level.

Table III.1: **Claimed Childcare Tax Deductions and Tax Non-Filing**

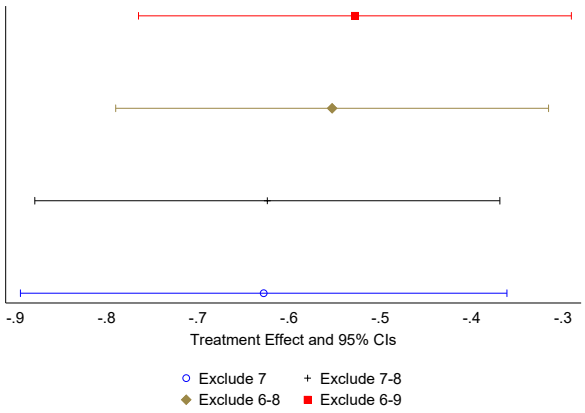
	No Controls		Group Time Trends		
	B	SE	B	SE	N
Claimed Childcare Credit	-0.38	0.06	-0.23	0.10	444840
Non Filer	0.00	0.02	-0.04	0.03	444840

Note: This table shows estimates of β from equation 3 and standard errors which are clustered at the individual level, for two outcome variables: whether the mother reported child care tax deductions on her tax return, and whether she failed to file a tax return. The two specifications shown are described in Section 3.2. We exclude the year 2002 since this was a partial treatment year.

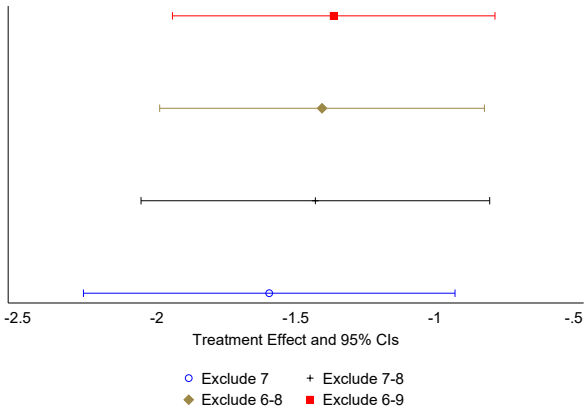
III.4 Robustness Results

Figure III.7: **Testing for Confounding Persistence of Treatment Effects**

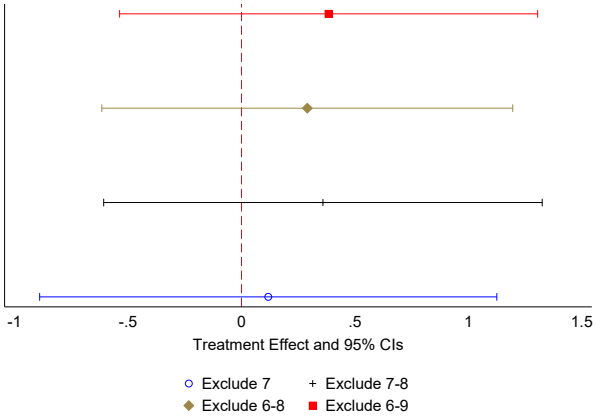
(a) **Extensive Margin Employment**



(b) **Individual Normalized Earnings**



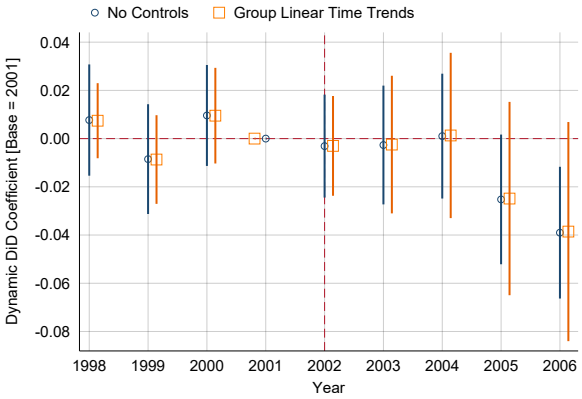
(c) **Normalized Hospital and Outpatient Spending**



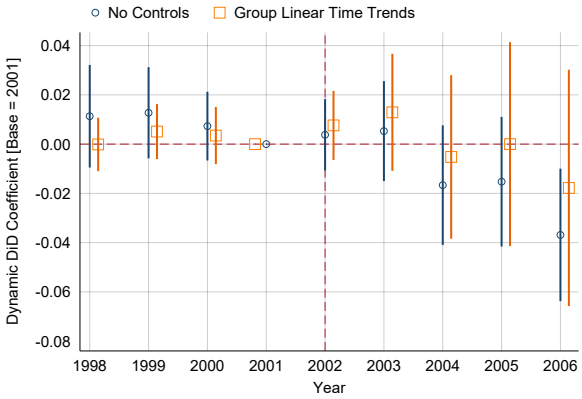
Note: Estimates of β and 95% CIs from equation 3, using the second specification which allows for differential time trends, for mothers, while consecutively widening the age of youngest child gap between treatment and control. Our baseline estimates exclude families whose youngest child is aged 7 (Exclude 7).

Figure III.8: **Reduced Form Effects on Health Care Costs Using the Full Sample**

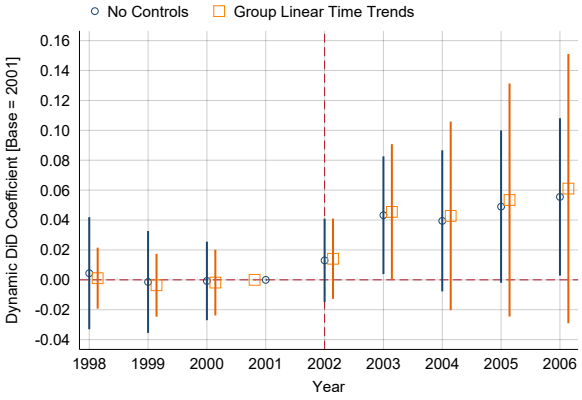
(a) **Outpatient and Hospital Costs, Adults**



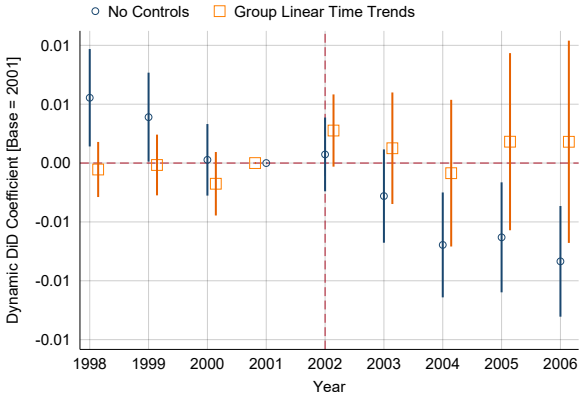
(b) **Pharmaceutical Days Supplied, Adults**



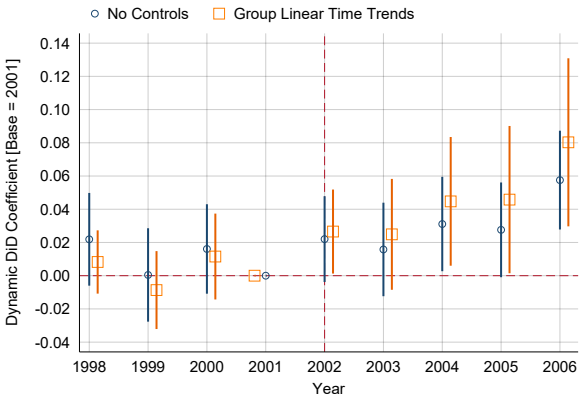
(c) **Pharma Days Supplied Mental Health, Adults**



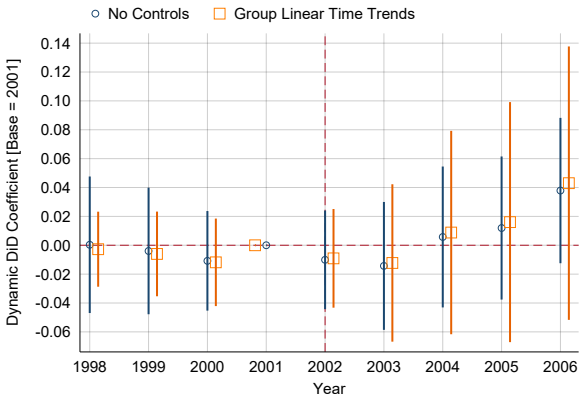
(d) **Received Pharmaceutical Contraceptive, Adults**



(e) **Outpatient and Hospital Costs, Children**



(f) **Pharmaceutical Days Supplied, Children**



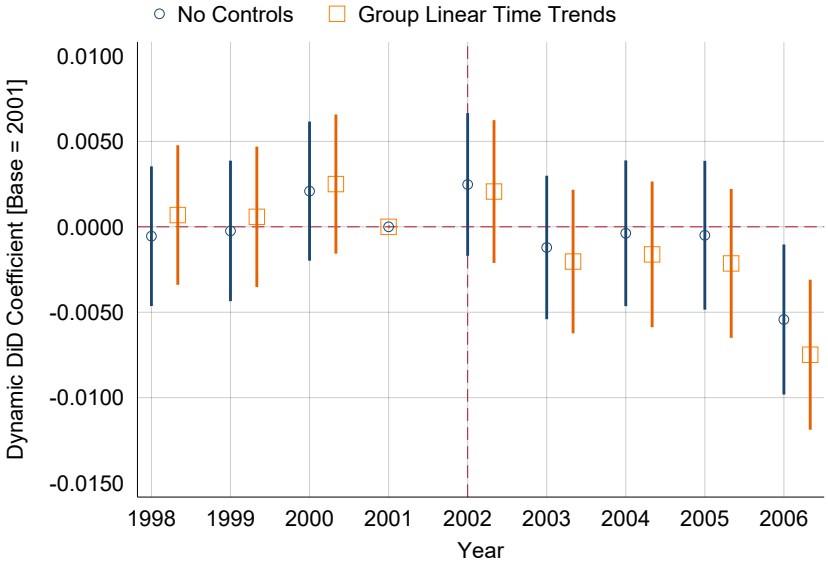
Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals, using the full sample rather than the baseline analysis sample that restricts to mothers that received IA before 2002 (as described in Section 2). Panels (a), (b), (c), and (d) show estimates for mothers, and panels (e) and (f), children. Standard errors are clustered at the individual level. Spending variables are winsorized at the 99th percentile and normalized as described in Section 3.2

Table III.2: **Adjusted P-Values for Multiple Hypothesis Testing**

	Sample	Effect	SE	Unadjust p	Sharp q	Bonf p
Received Pharmaceutical	Adults	0.325	0.073	0.000	0.001	0.000
Received Pharmaceutical Contraceptive	Adults	0.375	0.056	0.000	0.001	0.000
Received Pharmaceutical for Mental Health	Adults	-0.094	0.067	0.161	0.257	1.000
Received a Pap Smear	Adults	0.164	0.052	0.002	0.005	0.034
# GP Visits	Adults	0.347	0.200	0.083	0.170	1.000
# ER Visits	Adults	-0.755	0.469	0.107	0.201	1.000
Pharmaceutical Days Supplied	Adults	0.810	0.283	0.004	0.010	0.088
Pharmaceutical Days Supplied of Contraceptives	Adults	2.863	0.541	0.000	0.001	0.000
Pharmaceutical Days Supplied for Mental Health	Adults	-2.249	0.543	0.000	0.001	0.001
Hospital and Outpatient Spending	Adults	-0.056	0.263	0.831	0.759	1.000
Injury Spending	Adults	0.281	0.639	0.660	0.703	1.000
Mental Health Spending	Adults	-2.053	0.580	0.000	0.002	0.008
Received Pharmaceutical	Children	0.107	0.102	0.294	0.417	1.000
Received Pharmaceutical for Mental Health	Children	-0.001	0.034	0.977	0.870	1.000
# GP Visits	Children	0.209	0.255	0.412	0.443	1.000
# ER Visits	Children	-0.872	0.599	0.145	0.256	1.000
Pharmaceutical Days Supplied	Children	0.146	0.846	0.863	0.759	1.000
Pharmaceutical Days Supplied for Mental Health	Children	-0.552	2.309	0.811	0.759	1.000
Hospital and Outpatient Spending	Children	0.220	0.454	0.628	0.703	1.000
Injury Spending	Children	-0.483	0.584	0.408	0.443	1.000
Mental Health Spending	Children	0.365	1.017	0.720	0.735	1.000

Note: This table shows adjustments to p-values that account for the increased Type I error caused by multiple hypothesis testing. The first one calculates sharpened False Discovery Rate (FDR) q-values following the procedure outlined in Benjamini, Krieger and Yekutieli (2006), denoted “Sharp q". This approach works well under the assumption that p-values are independent or positively correlated. The second method uses the highly conservative Bonferroni adjustment which multiplies p-values by the number of tests conducted.

Figure III.9: **Reduced Form Effects on Maternal Fertility**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals. The outcome is an indicator for whether the mother conceives a child in that year, defined as appearing in the birth records in the following year. The two specifications shown are described in Section 3.2. Standard errors are clustered at the individual level.

Table III.3: **Long-Run Effects on Children's Outcomes Using the Restricted Sample**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	Grade 10 Test Scores				Graduation High School			
$\sum_{s=8}^{15} IA_{i,s}$	-0.0882	0.0151	-0.0894	0.1079	-0.0396	0.0030	-0.0402	0.0482
	(0.0014)	(0.0513)	(0.0014)	(0.0807)	(0.0005)	(0.0201)	(0.0005)	(0.0317)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		67.04		30.73		80.06		36.97
Outcome Mean	-0.450	-0.450	-0.450	-0.450	0.620	0.620	0.620	0.620
N	184346	184346	184346	184346	199387	199387	199387	199387
	Received IA at Age 20 or 21				Charge with Crime at Age 20 or 21			
$\sum_{s=8}^{15} IA_{i,s}$	0.0271	0.0282	0.0274	0.0013	0.004	0.0002	0.0041	-0.0073
	(0.0004)	(0.0129)	(0.0004)	(0.0189)	(0.0002)	(0.0075)	(0.0002)	(0.0108)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		80.06		36.97		80.06		36.97
Outcome Mean	0.127	0.127	0.127	0.127	0.036	0.036	0.036	0.036
N	199387	199387	199387	199387	199387	199387	199387	199387

Note: OLS and 2SLS estimates of β from equation 7. Results in this table use only the restricted sample of children whose mothers received IA at some point between 1989 and 2001. The control $\sum_{s=1}^{7} IA_{i,s}$ is described in Section 6.1. Standard errors in parentheses are clustered at the family level. First Stage F is the Kleibergen-Paap F statistic for weak instruments.

Table III.4: **Long-Run Effects on Children's Outcomes, Probit Specification**

	Graduation High School			
	OLS	IV	OLS	IV
$\sum_{s=8}^{15} IA_{i,s}$	-0.0498	0.0136	-0.0499	0.0038
	(0.0003)	(0.0204)	(0.0003)	(0.0215)
Outcome Mean	0.802	0.802	0.802	0.802
	Received IA at Age 20 or 21			
	OLS	IV	OLS	IV
$\sum_{s=8}^{15} IA_{i,s}$	0.0189	0.0198	0.0189	0.0127
	(0.0001)	(0.0116)	(0.0001)	(0.0122)
Outcome Mean	0.060	0.060	0.060	0.060
	Charged with Crime at Age 20 or 21			
	OLS	IV	OLS	IV
$\sum_{s=8}^{15} IA_{i,s}$	0.0040	0.0037	0.0041	0.0036
	(0.0001)	(0.0063)	(0.0001)	(0.0064)
Outcome Mean	0.018	0.018	0.018	0.018
N	577108	577108	577108	577108
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓

Note: OLS and IV estimates of β from probit versions of equation 7. See Section 15.7.2 of Wooldridge (2010) for details — this approach assumes that the endogenous variable ($\sum_{s=8}^{15} IA_{i,s}$) is continuous, which is not true here, and so this approach is only an approximate robustness check. The control $\sum_{s=1}^{7} IA_{i,s}$ is described in Section 6.1. Average marginal effects are shown and standard errors are calculated using the delta method. Standard errors in parentheses are clustered at the family level.

Table III.5: **Long-Run Effects on Children's Outcomes By Gender Using the Full Sample**

	Daughters							
	Grade 10 Test Scores				Graduation High School			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
$\sum_{s=8}^{15} IA_{i,s}$	-0.1527	0.0743	-0.1529	0.1054	-0.0631	0.0116	-0.0632	0.0041
	(0.0016)	(0.0777)	(0.0016)	(0.0910)	(0.0006)	(0.0260)	(0.0006)	(0.0289)
First Stage F		50.74		38.56		64.18		49.65
Outcome Mean	0.115	0.115	0.115	0.115	0.827	0.827	0.827	0.827
N	259919	259919	259919	259919	282015	282015	282015	282015
	Received IA at Age 20 or 21				Charged with Crime at Age 20 or 21			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
$\sum_{s=8}^{15} IA_{i,s}$	0.0373	0.0226	0.0374	0.0157	0.0042	-0.0008	0.0042	0.0021
	(0.0005)	(0.0155)	(0.0005)	(0.0173)	(0.0002)	(0.0060)	(0.0002)	(0.0066)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		64.18		49.65		64.18		49.65
Outcome Mean	0.064	0.064	0.064	0.064	0.008	0.008	0.008	0.008
N	282015	282015	282015	282015	282015	282015	282015	282015
	Sons							
	Grade 10 Test Scores				Graduation High School			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
$\sum_{s=8}^{15} IA_{i,s}$	-0.1617	-0.0580	-0.1619	-0.0586	-0.0710	-0.0188	-0.0711	-0.0286
	(0.0016)	(0.0744)	(0.0016)	(0.0746)	(0.0006)	(0.0295)	(0.0006)	(0.0294)
First Stage F		52.31		50.22		56.11		53.07
Outcome Mean	-0.109	-0.109	-0.109	-0.109	0.779	0.779	0.779	0.779
N	272140	272140	272140	272140	295093	295093	295093	295093
	Received IA at Age 20 or 21				Charged with Crime at Age 20 or 21			
	OLS	IV	OLS	IV	OLS	IV	OLS	IV
$\sum_{s=8}^{15} IA_{i,s}$	0.0297	0.0689	0.0297	0.0523	0.0095	0.0088	0.0096	0.0073
	(0.0005)	(0.0164)	(0.0005)	(0.0157)	(0.0003)	(0.0118)	(0.0003)	(0.0117)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		56.11		53.07		56.11		53.07
Outcome Mean	0.056	0.056	0.056	0.056	0.027	0.027	0.027	0.027
N	295093	295093	295093	295093	295093	295093	295093	295093

Note: OLS and 2SLS estimates of β from equation 7, separately for daughters and sons. The control $\sum_{s=1}^{7} IA_{i,s}$ is described in Section 6.1. Standard errors in parentheses are clustered at the family level. First Stage F is the Kleibergen-Paap F statistic for weak instruments.

III.5 Spillovers Accounting

We calculate the net fiscal costs from the government's perspective associated with the causal effect of granting IA on healthcare expenditure and tax revenue. Income taxes paid to provincial and federal governments measured in tax returns are straightforward. Whether to include federal taxes in the accounting depends on whose perspective one takes: the provincial government, or also the federal government. We include federal taxes since the federal government provides block transfers to provinces for health and social programs. Table III.6 shows that granting IA to one recipient family, on average, increases child health costs by $41 per child (82$ for a two-child household), decreases adult health cost by $38, and costs the government $1296 + $564 = $1860 in lost tax revenue. The combination of these indirect costs adds up to $1863, which is 17.4% of the direct cost of providing IA ($10,687.50). This calculation naturally does not include all potential spillover costs to the government. We could, for instance, use the point estimates on intergenerational transmission to get noisy estimates of discounted long-run costs.

Table III.6: **Change in Government Fiscal Position from an IA Recipient**

Adult Hospital and Outpatient Costs	-38
Child Hospital and Outpatient Costs	41
Lost Federal Individual Income Tax	-1296
Lost Provincial Individual Income Tax	-564
Indirect Change in Government Fiscal Position of an IA Recipient	-1857
Direct Change in Government Fiscal Position of IA Recipient	-10,687.50
Ratio of Indirect over Direct Costs	0.174

Note: This table shows the estimated fiscal spillovers of an IA recipient, based on the causal IV estimates derived in the main text. Dollar amounts are expressed in 2002 CAD.

IV Standard error correction

We follow Newey and McFadden (1994) closely for the correction of the standard errors. Let i denote a person-year observation. Consider y_i, an outcome of interest, and $\tilde{y}_i$, its detrended version; namely, $\tilde{y}_i$ is obtained by regressing y_i on group-specific linear time trends using pre-reform data, then calculating the residual value for all years. In addition, consider $\tilde{x}_i$, a $(1\times k)$-vector of regressors including a detrended version of IA receipt, the independent variable of interest; z_i, a $(1 \times \ell)$-vector of exogenous regressors including the instrument for IA receipt; and w_i, a $(1 \times s)$-vector of variables used to detrend y_i and IA receipt. To lighten notation, denote $h_i = (\tilde{y}_i, \tilde{x}_i, z_i, w_i)$. The estimation proceeds in two steps. First, y_i and IA_i are detrended by running the following regressions on pre-reform data and computing the residual values for all years:

$$y_i = w_i\delta + \xi_i \tag{IV.1}$$

$$IA_i = w_i\gamma + e_i \tag{IV.2}$$

Second, the main equation of interest is estimated using $\tilde{y}_i$ and $\tilde{x}_i$:

$$\tilde{y}_i = \tilde{x}_i\beta + u_i \tag{IV.3}$$

Newey and McFadden (1994) show that this type of two-step estimator can be formulated to fit into the GMM framework. Specifically, the estimators $\hat{\delta}$, $\hat{\gamma}$, and $\hat{\beta}$ respectively solve:

$$N^{-1}\sum_i g(h_i, \hat{\beta}, \hat{\delta}, \hat{\gamma}) = 0, \qquad g(h_i, \beta, \hat{\delta}, \hat{\gamma}) = z_i'(\tilde{y}_i - \tilde{x}_i\beta) = z_i'u_i \tag{IV.4}$$

$$N^{-1}\sum_i q(h_i, \delta) = 0, \quad \text{where} \quad q(h_i, \delta) = d_i w_i'(y_i - w_i\delta) = d_i w_i'\xi_i \tag{IV.5}$$

$$N^{-1}\sum_i m(h_i, \gamma) = 0, \qquad m(h_i, \gamma) = d_i w_i'(IA_i - w_i\gamma) = d_i w_i'e_i \tag{IV.6}$$

where $d_i = 1\{year_i < 2002\}$; that is, only pre-reform years are used to estimate δ and γ. Under standard regularity conditions, it then follows that:

$$\sqrt{N}\left(\hat{\beta} - \beta\right) = -G_\beta^{-1} \times N^{-\frac{1}{2}} \sum_i \left[g\left(h_i, \beta, \delta, \gamma\right) - G_\gamma M^{-1} m(h_i, \gamma) - G_\delta Q^{-1} q(h_i, \delta)\right] + o_p(1) \tag{IV.7}$$

where:

$$G_\beta = E\left[\nabla_\beta\, g\left(h_i, \beta, \hat{\delta}, \hat{\gamma}\right)\right] \tag{IV.8}$$

$$G_\delta = E\left[\nabla_\delta\, g\left(h_i, \beta, \delta, \hat{\gamma}\right)\right] \tag{IV.9}$$

$$G_\gamma = E\left[\nabla_\gamma\, g\left(h_i, \beta, \delta, \gamma\right)\right] \tag{IV.10}$$

$$Q = E\left[\nabla_\delta\, q(h_i, \delta)\right] \tag{IV.11}$$

$$M = E\left[\nabla_\gamma\, m(h_i, \gamma)\right] \tag{IV.12}$$

The asymptotic variance of $\hat{\beta}$ is then straightforward to derive. In estimating it, we allow for within-individual correlation across observations (within-family, in the case of the children results), following Cameron and Miller (2015).