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

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Abstract

How does welfare affect the prosperity of mothers and their children? We study this question using a Canadian welfare reform and by linking administrative welfare records to tax returns, nearly all medical spending, and children's educational attainment. Eighty percent of mothers in the complier group found employment within a year, and for many, total income rose despite reduced transfers. We find precise zero effects on total health expenditures for both mothers and children. However, composition changes, including fewer family physician visits, indicate that mothers had less time to seek health care. We find precise zero effects on children's test scores and graduation, but modest reductions of intergenerational transmission of welfare.

Keywords: welfare, health, public.

JEL Codes: H23, H31, I38

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Prior to major reforms that were implemented in the 1990s and early 2000s, welfare systems across North America had a substantial focus on supporting 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 U.S. welfare system, whose name – Aid to Families with Dependent Children (AFDC) – says it all. It was also a focus in Canadian provincial income assistance (IA) programs – the Canadian version of welfare. The reforms represented a dramatic shift in this perspective. They aimed not only to cut welfare caseloads in general but to reduce receipt by families with children, based on a perception that the existing systems created an intergenerational cycle of dependency (Page, 2004).

The impact of these reforms was sizeable. In the U.S., welfare reforms from 1993 to 1996 were followed by a more than 50% drop in the caseload between 1995 and 2000 (Hoynes, 2009). Likewise, IA receipt in British Columbia (BC) – the Canadian province we study – 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 increasing family income, improving feelings of self-efficacy stemming from employment, and providing positive role-modeling for children. Governments touted this as a ‘tough love’ approach designed to cut the cycle of dependency.[1] The most pessimistic critics argued that the reforms would reduce family income, raise stress, and, if work was found, reduce parental time with children – any or all of which might negatively affect health and education outcomes.[2] Our goal is to investigate whether either of these views of the effects of welfare cuts is accurate or whether the effects lie somewhat in between by studying a 2002 reform in BC that dramatically cut welfare caseloads.

[1]This is reflected in the statement by the Minister responsible for IA, in the press release accompanying the announcement of the 2002 BC reform: “These income assistance reforms are designed to help break intergenerational dependency on welfare... The people the ministry serves are truly a resource, and this legislation will give the ministry the tools it needs to provide assistance, create opportunity and help people achieve independence."

[2]One prominent policy commentary 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)

A major innovation of our paper is the novel administrative dataset built to answer this question. We link anonymized individual-level administrative data from a wide range of government ministries, including all outpatient and hospital expenditures, most pharmaceutical spending, school enrollment and scores on standardized assessments, interactions with the foster care system, IA receipt, and income tax returns. As a result, in addition to the income and employment outcomes that are the usual focus of welfare reform investigations (see Ziliak (2015) and Chan and Moffitt (2018) for reviews), we extend the analysis to examine a rich set of outcomes measuring health care use, educational attainment, contact with youth and family services, and children's eventual use of welfare as young adults.

Since the reform was province-wide, we use an identification strategy that leverages one aspect of the reform: a reduction in the age of the youngest child at which a mother in receipt of IA was required to search for work, from 7 years of age to 3. We instrument for IA with a difference-in-differences estimator that compares families who were newly-subject to these work-search requirements with families who were already subject to them.[3]

We show that IA use among the treated group dropped by 5.7 percentage points (p.p.) relative to the control group, with an annual loss in IA benefits of \$11,000 among compliers (equivalent to 12 months of benefits). This is close to the pay from a full-time minimum wage job at the time (35 hours $\times$ 50 weeks $\times$ \$8 = \$14,000). These IA benefit declines were largely offset by increased employment: 79% of the complier group moved into employment, and mean after-tax family income was unaltered in the two years following the reform and increased in subsequent years. However, this average effect hides substantial heterogeneity: about half of complier families saw declines in income to levels that represent real distress, while the other half experienced substantial increases. The sizeable employment effect likely reflects the fact that our identification strategy is based on the expansion of job search requirements. Furthermore, we show that most mothers normally stop using IA as their child ages — hence, this reform can be seen as hastening that exit.

On the health side, we find well-defined zero impacts on total health care expenditures for

[3]Similar work-search requirements (and exemptions for mothers with young children) are embedded in welfare programs elsewhere such as TANF in the U.S.

both mothers and children. This may reflect the small average effect on income, to the extent that income affects health. We do, however, find that general practitioner (GP) visits declined among the complier group. This is consistent with transitions to employment reducing mothers' time available to seek health care for themselves and their children. Reduced GP visits are not associated with detectable drops in expenditures in other notable health categories. Since GPs both provide primary care and serve as gatekeepers for specialist referrals, small effects distributed across health categories may accumulate into a detectable effect on GP visits.

If reductions in GP visits reflect reduced parental time to attend to issues outside of work, we might expect impacts on child educational outcomes through a similar channel. Instead, we find zero impacts on test scores. This aligns with Bastian and Lochner (2022) who show that increased maternal employment induced by the U.S. Earned Income Tax Credit (EITC) reduced mothers' time with children in general, but not time spent on human capital related activities. We also find no impact on the probability of interactions with the Ministry of Child and Family Development which provides foster care and milder family interventions. This further suggests that the decrease in parental time did not lead to substantial breakdowns in child well-being. It is therefore unsurprising that we also find zero effects on high school graduation. We do, however, find modest but imprecise decreases in the likelihood of children using IA when they reach early adulthood. In the absence of effects on educational attainment, plausible mechanisms are role-modelling and information transmission which are emphasized by Dahl and Gielen (2021) and Hartley, Lamarche and Ziliak (2022).

Our overall conclusion is that neither extreme view of the reform's potential impacts was correct. It did not, as the government of the time proclaimed, improve outcomes for mothers and children. Nor was it the catastrophe that critics predicted. Many mothers in the complier group offset lost transfer income through employment. While this shift likely elevated stress and reduced parent-child time, it did not substantially degrade health or education performance. This apparent resilience may reflect Canada's extensive public health and education systems operating as intended: providing services regardless of the income level and income source of families.

A large literature has broadly 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 outcomes 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) does find that welfare reform increased smoking and drinking. Among children, Gennetian et al. (2010) find small effects on parent-reported health using the welfare-to-work experiments.[4] The literature on educational outcomes relies on either variation in work-search requirements under TANF (Herbst, 2016; Washbrook et al., 2011) or the 1990s welfare-to-work experiments (Duncan, Morris and Rodrigues, 2011). These studies find very small positive effects of welfare on test scores, but at younger ages than the children in our setting where we find zero effects. Finally, our intergenerational transmission of welfare estimates, while not directly comparable, are likely more modest in size than prior estimates (Page, 2004; Dahl, Kostol and Mogstad, 2014; Hartley, Lamarche and Ziliak, 2022).

Our contributions to the literature are threefold:

First, the data that we assemble provides crucial advantages relative to past studies. One key advantage is that we observe wide-ranging and detailed outcomes. For instance, while most existing studies focus on self-reported general healthiness, we access hospital records, outpatient billings, and pharmaceutical prescriptions alongside diagnostic codes which allows us to disentangle mechanisms underlying the health effects of welfare reform. Similarly, by linking children's welfare records to (i) health data, (ii) educational attainment, and (iii) welfare receipt in adulthood, we can evaluate, within a single context, three candidate channels through which welfare reform affects children's own reliance on income transfers in the long run. A second advantage is that the linkage of mothers to children allows us to study effects on mothers and children simultaneously, and

[4] A key exception to the use of self-reported outcomes is Leonard and Mas (2008) who study the effects of TANF time-limits on infant mortality. Related U.S. safety net literature also contains three additional exceptions: Almond, Hoynes and Schanzenbach (2011), who study the effect of the roll out of Food Stamps on birth weight; and Hoynes, Miller and Simon (2015) and Evans and Garthwaite (2014), who, respectively, investigate the impact of EITC expansions on birth weight and on the prevalence of risky levels of biomarkers among mothers. The latter two contributions form part of a small literature on the impact of EITC on child and maternal health, which has also relied more heavily on self-reported health measures (Averett and Wang, 2018; Boyd-Swan et al., 2016; Braga, Blavin and Gangopadhyaya, 2020).

therefore to draw comparisons between the effects in the two groups. Finally, the administrative data is free of measurement error from self-reporting. For instance, we observe intergenerational transmission of welfare using caseload data and administrative linkages between mothers and children, which contrasts to existing research that often relies on individuals reporting current program use (e.g., Hartley, Lamarche and Ziliak (2022)).

Second, by using the Canadian context where health insurance is near-universal, we study welfare reform holding insurance coverage constant. This contrasts with U.S. welfare reforms which reduced insurance coverage (Kaestner and Kaushal, 2003; Bitler, Gelbach and Hoynes, 2005; Cawley, Schroeder and Simon, 2006), making it hard to tell whether recorded health changes reflect direct impacts on health or access to health care. To illustrate the importance of the insurance channel, in the one area of health insurance that is more generous for IA recipients (pharmaceuticals), we find that spending decreased following welfare reform.

Third, in contrast to studies of the 1990s U.S. reforms, we do not face confounding effects from a coinciding expansion of the Earned Income Tax Credit (EITC). Isolating the effect of welfare reform and EITC expansion has proven challenging because both programs targeted mothers (Grogger, 2003; Fang and Keane, 2004; Chan, 2013; Kleven, 2021; Schanzenbach and Strain, 2021; Bastian and Jones, 2021). Similarly, the BC labour market was relatively stable throughout the late 1990s and early 2000s, which provides a cleaner setting to study reform than the U.S. which experienced a coinciding economic boom.

The remainder of the paper is structured as follows. Section 1 outlines the history of IA reform in BC. Section 2 overviews the comprehensive data linkage. Section 3 presents the identification strategy and the impacts of the reform on IA use. Section 4 presents the labour market effects for mothers. Section 5 presents effects on health spending for mothers and children. Section 6 includes results for children's educational attainment and IA use as young adults. Section 7 concludes.

1 Institutional Background

The welfare system in BC (known as Income Assistance, or IA) is a system of last resort providing cash transfers and supports for re-entering employment for individuals and families with income and assets below thresholds that are deemed minimally necessary for survival. Applications can be for either Expected to Work (ETW) benefits or Disability Assistance (DA) benefits. ETW benefits, the focus of this paper, have no time limit (unlike TANF) and require recipients to search for work unless young children are present.[5] Over time, the exact parameters changed, but this basic structure remained.

As shown in Figure 1, the caseload increased dramatically over the first half of the 1990s, coinciding with a substantial recession in Canada, reforms that reduced access to Unemployment Insurance, and policy changes that broadened access to IA in 1991. By 1995, over 12% of the population were receiving IA each month. Single mothers made up the largest share of recipients, followed by childless adults and two-parent families. Our focus is on mothers and their children, but we study the effects of welfare access on childless adults in a companion paper (Hicks, 2022*b*).

The large caseload, combined with increased federal deficits that were partly passed on to the provinces, led to a desire to reduce IA outlays. In response, two substantial reforms were implemented in 1996 and 2002 through changes to the Act governing IA, in addition to ongoing regulatory changes, such as a 40% reduction in the number of offices at which a person could apply for benefits (Hicks, 2022*a*). These changes were bellwethers for a shift in outlook toward restricting access to those who were deemed most in need.[6]

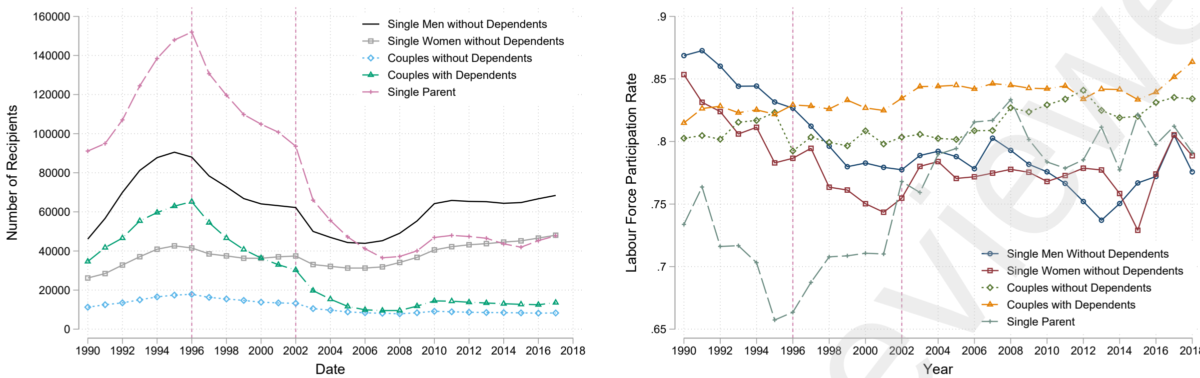
The 2002 changes were most dramatic. They included requiring new applicants to demonstrate two years of financial independence prior to application (a change intended to prevent young people from getting involved with IA); a three-week waiting period after application; and a reduction of the age of youngest child below which a mother was exempt from job search requirements from

[5] DA benefits are higher and without job search requirements but require a demonstrable medical barrier to work.

[6] Green et al. (2021) show that following the 2002 reform, the composition of IA users shifted heavily toward persons with physical and mental health problems.

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 as of January 1st in each year. Panel (b) plots labour force participation rates among persons aged 20 to 60 with less than a university education. Both panels plot trends separately for five family types: single men without dependents, single women without dependents, couples without dependents, couples with dependents, and lone-parent families (the majority of which are female-headed).

7 to 3 years of age.[7] Figure 1 suggests that the 1996 and 2002 changes successfully reduced the caseload, which fell from over 350,000 in 1995 to under 150,000 a decade later. The 2002 caseload reductions were associated with budget cuts at the Ministry that amounted to $580 million or 30% over the 3 years following the reform (Klein and Long, 2003).

The reforms disproportionately impacted families. Between 1995 and 2005, the number of recipients in lone- and two-parent families dropped from 220,000 recipients (including children) to 50,000. The caseload among childless adults, in contrast, dropped from 150,000 to 80,000. The system was transformed from a widely accessed safety net with a strong emphasis on families with children to a much smaller system with greater emphasis on childless adults.[8]

While caseloads dropped dramatically in 2002, the average benefit level for recipient families only fell by 6% (see Figure III.1). This implies that 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

[7] Other changes in 2002 included the elimination of the $100 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. None of these elements of the system varied with the age of the youngest child, which underlies our identification strategy.

[8] In a companion paper (Hicks, 2022*b*), we study the effects of IA receipt on health outcomes of childless adults.

poverty line was $1,914 per month for a family of two.[9]

The reforms are plainly evident in the trajectory of labour force participation rates as shown in panel (b) of Figure 1. Participation rates among lone-parent families (without a university degree) inversely track welfare caseloads, with sizeable increases following both the 1996 and 2002 reforms. By 2004, the longstanding gap in participation rates between single mothers and women without dependents had closed completely, as had occurred following U.S. welfare reform in 1996 (Kleven, 2021). In contrast, the participation rate for couples without dependents was constant, consistent with this group having low welfare participation and minimal changes during the reform period. The participation rates for couples with children is also quite flat for the period between 1998 and 2008, reflecting a relatively stable labour market during this period. This provides a better environment for identifying policy impacts than the late 1990s U.S. which experienced a coinciding labour market boom.

2 Data and Sample Selection

2.1 Data Sources

The data underlying our investigation is a rich linkage of administrative data from different parts of the BC government along with income tax records. We first describe each of the individual administrative datasets and then outline the linkage across the datasets in the following section.

Income Assistance Receipt: We draw on the universe of monthly IA records (beginning in 1989) which indicate the amount of IA received and the category of benefits (ETW versus DA). Since households apply jointly for IA, all members of the household, including children, are recorded.

Employment, Income, and Tax Payments: We utilize tax returns for BC residents to measure employment and earnings from 1998 to 2018. We define extensive margin employment in a calendar year as an individual receiving earnings from a tax-registered employer.[10] We also utilize reported numbers from the main tax form (the T1) to measure all market based income and total

[9] This is Statistics Canada's low income cut-off for cities with over 500,000 inhabitants in 2001.

[10] As indicated in T4 slips, employer-issued documents that summarize earnings (the analog to W-2s in the U.S.).

after-tax income. Market income includes paid and self-employment earnings. Total after-tax income is formed by summing income from all sources, including transfer and alimony income, and then subtracting tax liabilities.

Hospital and Outpatient Care: All residents of the province are entitled to health care, covering physician, hospital, and most diagnostic services, without charge.[11] The universe of hospital inpatient visits and hospital-based day surgeries are reported in the Discharge Abstract Database (DAD) (see Appendix I for more details). The main exclusion from this data is emergency room (ER) visits. However, if a patient receives treatment in the ER from a non-hospitalist physician, the physician-cost component of that service will appear in the the Medical Services Program (MSP) data, which we access. The MSP covers all medically-necessary procedures not directly provided by hospitals. We access the universe of MSP procedure-level billings (1991 to 2016) which reports the cost of each procedure and the associated ICD9 diagnosis code. These data cover all health care services for residents in our sample except non-medically-necessary dental and vision services, allied health professionals[12], and ER visits.

Pharmaceuticals: Pharmaceuticals are not universally insured in BC, but they are subsidized on a means-tested basis. IA recipients received full subsidy, but low-income non-IA-recipients received partial subsidies. We rely on *Pharmanet* (1995 to 2016) which tracks all prescriptions filled at community pharmacies regardless of insurance source or coverage. Based on this, we calculate the total annual cost of each person's prescriptions. We also 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 treatment.[13] The largest exclusion from *Pharmanet* is prescription drugs received in a hospital or

[11] Until 2018, the system was funded through premiums that were a function of income, with people with annual income below $20,000 paying no premium. Access to these forms of health care was unrelated to receipt of IA. IA recipients were eligible for supplemental dental insurance, but low income non-IA recipients received an equivalent subsidy.

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

[13] AHFS4 codes Anticonvulsants (28:12) (used for personality disorder treatment), Psycho-therapeutic Agents (28:16), Anti-manic Agents (28:28), Opiate Antagonists (28:10).

mental health center.[14]

Education Records: Education is also a provincial jurisdiction in Canada. BC public schools are fully government funded and offer free student access while private schools require a tuition payment (though they are subsidized by the government). The vast majority of children are in the public school system. We observe all primary and secondary school enrollments, whether in public or private schools, along with high school graduation records, for the years 1990 to 2018. Additionally, BC implemented a mandatory standardized assessment for all grade 7 students beginning in 1999 from which we obtain test scores in reading, writing, and math.[15]

Child Protective Service Records: We observe all interactions (from 1996 to 2018) with the Ministry of Child and Family Development (MCFD) which oversees foster care and less intensive forms of child and family services. We construct variables indicating whether a child had any interaction with MCFD in a given year, and whether they were in foster care.

2.2 Data Linkage and Sample Selection

The tax returns were linked to the IA caseloads in one location while the health and education files were linked to IA caseloads in another. The tax return data is not under provincial jurisdiction and so could not be linked directly to the health and education data. Due to a restriction imposed on the linkage between tax and IA records, we had to restrict that 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 *restricted sample* throughout. The linkage between health and education records to IA recipients contains the universe of persons, or the *full sample*. To maintain a consistent sample between the tax return and health data analyses, we impose the restricted sample criteria on the health data in our baseline analyses. As robustness, we show health results in the full sample to assess any impacts from the sample restriction.

We exclude fathers because it is difficult to confidently link men to children and the links that

[14] It also excludes prescriptions received through the BC Cancer Agency, BC Centre for Excellence in HIV/AIDS, BC transplant society, and BC Renal Agency.

[15] This assessment, called the Foundation Skills Assessment (FSA), is a low-stakes assessment — it does not affect students' final grades or graduation, nor is school funding tied to the results, but the results are released publicly.

can be made are connected to IA receipt. Appendix I describes the linkage of mothers to children. Our approach, based on birth records and MSP insurance records, successfully attributes a mother to over 97% of children in our sample period. As discussed in Section 3, as part of the identification strategy, we restrict the sample to mothers whose youngest child is between ages 4 and 11. We also restrict to mothers aged 20 to 60. Given that the youngest child must be age 4 or older, a 20 year-old mother in our sample would have given birth at age 16.

2.3 Descriptive Statistics and Patterns of Income Assistance Use

Table 1 shows means and standard deviations in 1998-2001 for mothers and children, respectively, in the full sample, the restricted sample, and among persons receiving IA in the year. The first row shows that 32% of the full sample received IA at some time between 1989 and 2001, which illustrates the widespread nature of IA receipt in the 1990s. Single parents make up 19% of the full sample, 44% of the restricted sample, and 67% of active recipients, reflecting the IA system's emphasis on supporting parents.

Unsurprisingly, individuals in the restricted sample are less healthy than in the full sample, with active recipients being the least healthy. A similar, albeit more muted, gradient is evident for children. Children have substantially less interactions with the health care system in general: total health care spending per child is 1/3rd of the adult amount.

Also unsurprisingly, active IA recipients have substantially lower labour market attachment. Their average market income is only $3,900 compared to $10,100 in the restricted sample. Figure III.2 illustrates mothers' income composition from different sources. IA benefits constitute 50% of total income among mothers receiving IA in a given year. The next largest source is child tax benefits administered through the tax system. Market income comes a distant third at approximately 20%.[16] In the restricted sample (regardless of active IA receipt), market income is the largest component.

[16]Some mothers transition between IA and employment during the year, generating simultaneous market income and IA receipt as measured annually. Furthermore, IA recipients can receive small amounts of labour earnings while on IA without full claw back.

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	0.12	0.33	0.38	0.49	1.00	0.00
Months IA Received in Year	1.07	3.16	3.31	4.83	8.69	3.82
IA Benefit Amounts in Year	914.90	2818.53	2821.46	4372.60	7408.23	4027.67
Age	37.74	5.90	34.81	6.15	34.04	6.47
Single Parent	0.19	0.40	0.44	0.50	0.67	0.47
Number Kids	2.11	0.94	2.04	1.02	1.98	1.01
Hospital Costs	130.50	555.33	176.39	649.12	204.28	703.35
Total Outpatient Expenditure	377.59	472.75	441.97	522.42	498.31	557.19
GP Visit Expenditure	128.47	123.96	153.45	140.58	178.51	153.41
Mental Health Expenditure	29.36	88.29	41.52	105.08	54.09	121.84
Injury Expenditure	12.75	39.07	16.25	43.90	18.83	47.41
Cold Expenditure	11.13	23.15	13.05	25.05	14.91	26.88
Prescription Cost	144.17	558.42	160.27	521.52	200.88	532.35
Mental Health Drug Cost	38.87	170.84	50.05	192.13	65.95	220.86
Total Health Care Costs	631.14	1072.70	759.29	1215.58	882.11	1323.61
Health Data Children	Mean	SD	Mean	SD	Mean	SD
Hospital Costs	16.05	81.93	19.80	90.68	21.39	94.09
Total Outpatient Expenditure	148.35	206.76	160.47	218.89	174.94	227.61
GP Visit Expenditure	65.54	69.62	69.28	72.18	77.53	76.27
Mental Health Expenditure	7.88	40.94	11.58	49.44	13.17	52.42
Injury Expenditure	10.70	29.34	11.95	30.85	12.35	31.11
Cold Expenditure	12.76	25.04	13.62	25.86	15.56	27.61
Prescription Cost	21.60	58.41	22.24	60.72	26.30	65.89
Mental Health Drug Cost	0.04	0.27	0.04	0.29	0.05	0.31
Total Health Care Costs	206.57	354.44	227.01	380.68	248.96	396.17
In MCFD Data	0.01	0.11	0.03	0.18	0.04	0.20
In Foster care	0.01	0.10	0.03	0.17	0.03	0.18
Tax Return Data	Mean	SD	Mean	SD	Mean	SD
On Ia	.	.	0.327	0.220	1.00	0.00
Employed Self	.	.	0.617	0.236	0.452	0.248
Fail to File Tax Return	.	.	0.013	0.013	0.021	0.020
Claimed Childcare Credit	.	.	0.152	0.129	0.043	0.041
IA Amount	.	.	2300	4000	7100	4000
Employment Income Self	.	.	9000	12000	3100	5700
After Tax Income Self	.	.	16100	10000	15000	6900
Spousal Market Income	.	.	12500	20000	4100	11000

Note: This table shows means and standard deviations of key variables during 1998 to 2001 for mothers aged 20 to 60 whose youngest child is between 4 and 6 or 8 and 11. The Full Sample contains all such mothers appearing in the administrative files. The Restricted Sample contains mothers who received income assistance (IA) at some point, including potentially as children, from 1989 to 2001. The On IA sample further restricts to mothers receiving IA in the year of observation. Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles.

3 Identification and Estimation Approach

Our objective is to estimate the causal impact of access to welfare on employment, earnings, health and education outcomes. We adopt an identification strategy that exploits the 2002 drop in the age of the youngest child at which a mother is required to search for work.[17]

3.1 First Stage: Impacts on IA Use

To illustrate our identifying variation, in panel (a) of Figure 3 we plot the fraction of mothers receiving IA by the age of their youngest child, for each year between 1998 and 2006.[18] The figure shows two patterns. First, receipt declines as the youngest child ages, from about 45% at age 4 to 33% by age 11 in 1999. This pattern is also evident in the full sample of mothers (see Figure III.3). This reflects a pattern whereby mothers enter IA around the time they give birth, then slowly exit as their children age (see Figure III.4).[19] 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. In 1998, a mother with a youngest child of age 4 was 10 p.p. more likely to receive IA than one whose youngest was 11. By 2006, that difference was only 2 p.p.[20]

To identify the causal effect of IA, we focus on the flattening of the age gradient. This flattening is consistent with the 2002 introduction of job search requirements for mothers with youngest children aged 3 to 6 years old. As such, we define the treatment group as mothers with a youngest child aged 4 to 6. We define the control group as mothers with a youngest child aged 8 to 11 since these mothers 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.[21] We exclude families with a youngest child aged 1 or 2 because they did not become subject to search requirements. We

[17] Appendix II provides details on the enforcement of the job search requirements.

[18] We show corresponding numbers for the full sample of mothers in Figure III.3.

[19] Many low-income mothers do not qualify for maternity benefits through the unemployment insurance system.

[20] A similar initial gradient followed by a flattening during the 1990s U.S. reform is shown in Kleven (2021) among AFDC/TANF recipients.

[21] We define age of youngest child as of December 31st in the calendar year. So a child age 7 at this time would be classified in the control group, but may have been age 6 for most of the calendar year, in which case they effectively belong to the treatment group. In contrast, a child age 6 as of December 31st by definition could only have been in the treatment group for the entire year.

exclude families with youngest children older than 11 because families with older children are less comparable to the treatment group.

Figure 2 illustrates the construction of treatment and control groups. As the youngest child ages, the family transitions from treatment to control. This aging-into-control means that, starting in 2004, the control group will contain families that were previously subject to the work search requirements imposed in 2002 (the "treatment"). This could contaminate the control group if treatment effects are persistent over time. In Section 5.4, we present evidence suggesting this is not a concern.

Figure 2: **Construction of Control and Treatment Groups by Age of Youngest Child**

Control Group   Treatment Group

	Calendar Year								
	Pre-Reform				**Post-Reform**				
	Required to Search for Work if Youngest Child's Age > 6				**Required to Search for Work if Youngest Child's Age > 2**				
Birth Year of Youngest Child	1998	1999	2000	2001	2002	2003	2004	2005	2006
1986	12	13	14	15	16	17	18	19	20
1987	11	12	13	14	15	16	17	18	19
1988	10	11	12	13	14	15	16	17	18
1989	9	10	11	12	13	14	15	16	17
1990	8	9	10	11	12	13	14	15	16
1991	7	8	9	10	11	12	13	14	15
1992	6	7	8	9	10	11	12	13	14
1993	5	6	7	8	9	10	11	12	13
1994	4	5	6	7	8	9	10	11	12
1995	3	4	5	6	7	8	9	10	11
1996	2	3	4	5	6	7	8	9	10
1997	1	2	3	4	5	6	7	8	9
1998	0	1	2	3	4	5	6	7	8
1999		0	1	2	3	4	5	6	7
2000			0	1	2	3	4	5	6
2001				0	1	2	3	4	5
2002					0	1	2	3	4
2003						0	1	2	3

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 white cells with a black outline and the treatment group by shaded cells. Families where the youngest child is neither 8-11 nor 4-6 years old are excluded from the analysis.

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. Parallel pre-reform trends in adults' and children's outcomes support this assumption. They do not, however, guarantee that the two groups reacted similarly to other contemporary policy changes (see footnote 22 for a description of these changes). To support the assumption of similar reactions to other policies, we appeal to the similarity in the ages of children in the two groups and to the fact that other policy changes did not hinge on the age of the youngest child.[22] Most notably, aside from the work-search requirements, the across-the-board tightening of IA in 2002 was not conditioned on the age of the youngest child. The BC labour market was also stable from 1998 through 2004 with the employment rate holding steady near 60 percent.

Our first stage estimating equation is:

$$\mathrm{IA}_{i,t} = \pi_t + \pi_{ay} + \pi \times D_{i,t}\mathrm{Post}_t + \pi_x X_{i,t} + v_{i,t} \quad (1)$$

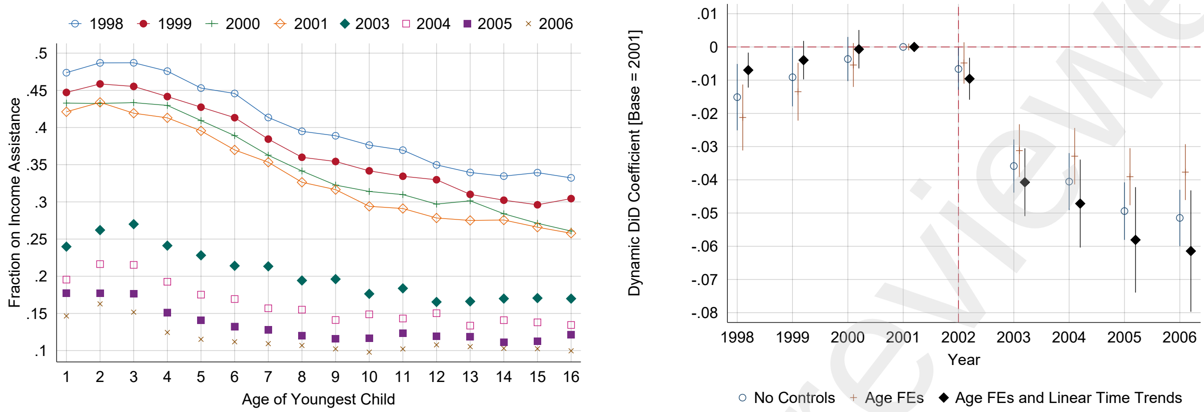
where, $IA_{i,t}$ indicates IA receipt in year t, $D_{i,t}$ is an indicator variable for being in the treatment group, and $Post_t$ is a dummy variable equal to one for the post-2002 years. We present three specifications corresponding to a gradually increasing set of controls, $X_{i,t}$. In the first, we include no controls. In the second, we add fixed effects for the mother's age. In the third, we control for differential linear time trends for treatments and controls. In the analysis below, the majority of outcomes do not require adjustment for pre-trends, but a few do. To perform the pre-trend adjustment, we estimate linear trends of $IA_{i,t}$ and the outcome variables separately for treatment and control using 1998-2001 data, project those trends to 2002-2006, then work with the residuals relative to the trend. To account for sampling variation in the de-trending step, we adjust the standard errors following Newey and McFadden (1994).[23]

[22] Minor reductions in provincial marginal tax rates in 2002 did not have any direct child dimension. The same holds for changes in federal policies that were implemented around that time. In the late 1990s, the federal government, in conjunction with the provinces, introduced a new National Child Benefit which gave families with children a benefit that was not immediately taxed back with other income (Milligan and Stabile, 2011). The provinces were meant to claw back social assistance payments in the same amount. BC had its own BC 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 more substantial increases in the NCB (increases of 13%, 4%, and 14% in 2003, 2004, and 2005, respectively). Importantly, for us, the NCB payments varied by family size but not by age of the youngest child.

[23] The method outlined by Newey and McFadden (1994) can easily be extended to allow for both generated regres-

Figure 3: **Rotation of the Age-of-Youngest Gradient**

(a) **Age-of-Youngest Gradient** (b) **First Stage DiD**



Note: Panel (a) plots the fraction of mothers that received IA for each calendar year and age of youngest child in the family. Panel (b) plots 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. Three specifications are shown: (1) no controls; (2) with mother age fixed effects; (3) with mother age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level.

In panel (b) of Figure 3, we plot an event study version of equation 1 in which we replace the simple $Post_t$ variable 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. In both the specifications with no controls and with mother age effects, the trend prior to the reform is reassuringly flat. We find similarly parallel pre-trends when using either the amount of IA benefits received or the number of months of benefits received as the dependent variables (see Figure III.5). For completeness, we also show results using the de-trended data, which has a flat pre-trend by construction. After the reform, the treated group shows a relative decline in IA receipt of approximately 5 p.p.

Table 2 shows estimates of π from equation 1 using three measures of IA use. The results including group-specific trends are given in column (3). They show that, among the complier group, extensive margin IA receipt fell by 4.9 p.p., months of benefit receipt fell by 0.6, and the annual dollar amount by $547.80 (in 2002 dollars). The change in dollar amount is an average

sand and generated regressors. We use this approach to compute standard errors for our IV estimates in the following sections by exploiting the fact that two-step estimators can be recast as joint GMM estimators.

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 $\frac{547.80}{0.049}$ = \$11,180. This is equivalent to 1,400 hours of work at the provincial minimum wage at the time (\$8/hour), which is close to full-time employment.[24] The same calculation for months of receipt implies that compliers lost $\frac{0.6}{0.049} \approx$12 months of benefits.

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

	Received Any IA (N=565970)			Months of IA (N=565970)			Dollar Amount of IA (N=565970)		
Treat x Post	-0.037	-0.026	-0.049	-0.495	-0.4	-0.601	-521	-451	-547
	(0.003)	(0.003)	(0.003)	(0.031)	(0.031)	(0.031)	(27.6)	(27.6)	(27.6)
			[0.009]			[0.087]			[81]
Age FEs	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Time Trends	No	No	Yes	No	No	Yes	No	No	Yes
KP Fstat	142	68		258	169	169	358	268	

Note: This table shows estimates of π from equation 1, using three measures of IA use: extensive margin IA receipt, the number of months received, and the annual dollar amount received among mothers. Three specifications are shown: (1) no controls; (2) with mother age fixed effects; (3) with mother age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure 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. KP-Fstat is the Kleibergen Paap weak identification F-statistic. Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles.

Based on the finding that complier mothers would have received IA for 12 months of the year in the absence of the reform, we can approximately characterize this group by examining the traits of treatment group mothers that received full-time IA before the reform. In Table 3, we present average employment and income among pre-reform person-year observations in which the mother was in the treatment group and received 12 months of IA. Among this group, the average yearly benefit amount was \$11,100, which is unsurprisingly almost identical to the 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

[24] A mother working 35 hours per week for 50 weeks would have annual hours of 1,750.

of \$3,274.33 ($\frac{740}{0.226}$), or \$270 per month.[25] So while some mothers did supplement IA income with small amounts of work, the majority (77.4%) did not.

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

	Mean	SD	N
Employed Self	0.226	0.175	23170
IA Amount	11100	2300	23170
Employment Income Self	740	2000	23170
After-tax Income Self	16400	5300	22860
After-tax Income Family	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. Among this set of person-year observations, the table shows the average employment and income characteristics. Income variables are rounded to the nearest \$100 following the disclosure rules for data access.

3.2 Second Stage: Impacts on Contemporary Outcomes

To examine the impact of reduced access to IA on employment and health, we proceed in two steps. In the first step, we present reduced form event study specifications given by:

$$Y_{i,t} = \alpha_t + \alpha_{ay} + \times \sum_{s \neq 2001} \gamma_s D_{i,t} \mathbb{1}\{t = s\} + \alpha_x X_{i,t} + \epsilon_{i,t} \tag{2}$$

where $Y_{i,t}$ is the outcome variable, and α_t and α_{ay} are fixed effects for calendar year and age of youngest child. 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. We present three specifications corresponding to different content of $X_{i,t}$, as described in Section 3, plotting the estimated γ_s's in each case. Estimates of γ_s before 2002 serve to test parallel pre-reform trends. Estimates of γ_s from 2002 to 2006 quantify the impact of the reform.

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

$$Y_{i,t} = \beta_t + \beta_{ay} + \beta \text{IA}_{i,t} + \beta_x X_{i,t} + \eta_{i,t} \tag{3}$$

[25] At the time, there was a \$100 monthly earnings exemption for IA recipients. Above that amount, benefits were clawed-back dollar-for-dollar.

where we instrument for $\text{IA}_{i,t}$ using $D_{i,t}\text{Post}_t$ in equation 1. Assuming that the controls are a valid counterfactual for the treatments, the estimate of β represents the causal effect of IA receipt on $Y_{i,t}$. For outcomes where there are parallel pre-trends, our preferred estimates are from the second specification in which we include mother age effects. For the other outcomes, where there is some evidence of pre-trend differences, we focus on the results adjusted for trend differences. We follow Rambachan and Roth (2021) by presenting estimates with and without trend adjustments in all cases in order to show robustness to possible violations of parallel trends.

4 Labour Market Outcomes

We begin by examining how labour market outcomes changed due to the reform. In Figure 4 we plot the reduced form effects, γ_s, and in Table 4, we present the 2SLS estimates of β. In Panel (a) of Figure 4, the outcome is an indicator for whether the mother had earnings from an employer. There is a sizable increase starting in 2002 among treated mothers. The corresponding estimate of β indicates that 79% 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, since mothers tend to move off of IA as their children age (see Figure 3), the reform's effect can be interpreted as hastening the transition to employment that was likely to happen later, rather than a denial of benefits to people who would have been on IA indefinitely.

Panels (b), (c), and (d) show reduced form effects on three measures of income (in 2002 dollars): individual pre-tax employment income, individual after-tax income, and family after-tax income. The latter two are identical for single mothers. In each case, we normalize individual i's income in year t ($I_{i,t}$) by the mean income in 2001 in their group (i.e treatment or control) denoted I_g; that is, we compute each person's percent deviation from their group's pre-reform average $\frac{I_{i,t}}{I_g}$.[26] We observe parallel pre-trends for individual employment income and family after-tax income.

[26] We adopt this normalization because of some evidence of a declining difference in pre-trends for income in dollars. Parallel pre-trends in employment, as seen in panel (a), indicate that any trend in income has to do with wages or hours conditional on working. Our hypothesis is that the decline in earnings for the treated group relative to the control group reflects a common upward trend in weekly wages, weighted by a larger number of weeks worked for the control group than the treatment group in the pre-reform period. If this is true then using percentage changes for each group eliminates this problem.

For individual after-tax income, we need to include a pre-trend adjustment in order to more plausibly claim that we obtain consistent causal estimates. Both individual and family after-tax income, which includes employment income and IA benefits net of taxes, increased albeit much more modestly than employment income and with a delay of a few years. These figures suggest that the movement into employment generated enough earnings, on average, to leave disposable income relatively unchanged in the short run. Over the longer term, disposable income may have even increased.

In Table 4, we present the 2SLS estimates for these outcomes. To translate effects on $\frac{I_{i,t}}{I_g}$ into dollar effects, we multiply the estimated treatment effect β by I_g. We estimate that receiving IA reduces earnings by \$13,948, which is larger than the \$11,035 gained in IA benefits. If the \$13,948 increase came exclusively from the 79% of mothers that went from zero to positive earnings, then the implied annual earnings of compliers would be \$17,655 ($\frac{\$13{,}948}{0.79}$). To put this in context, at 35 hours per week for 50 weeks, annual earnings of \$17,655 imply an hourly wage of \$10.08. In 2002, the average wage among women in BC was \$16.87 and the minimum wage was \$8.[27] The results are therefore consistent with mothers moving to full-time work at a wage that was below average but well above the statutory minimum.

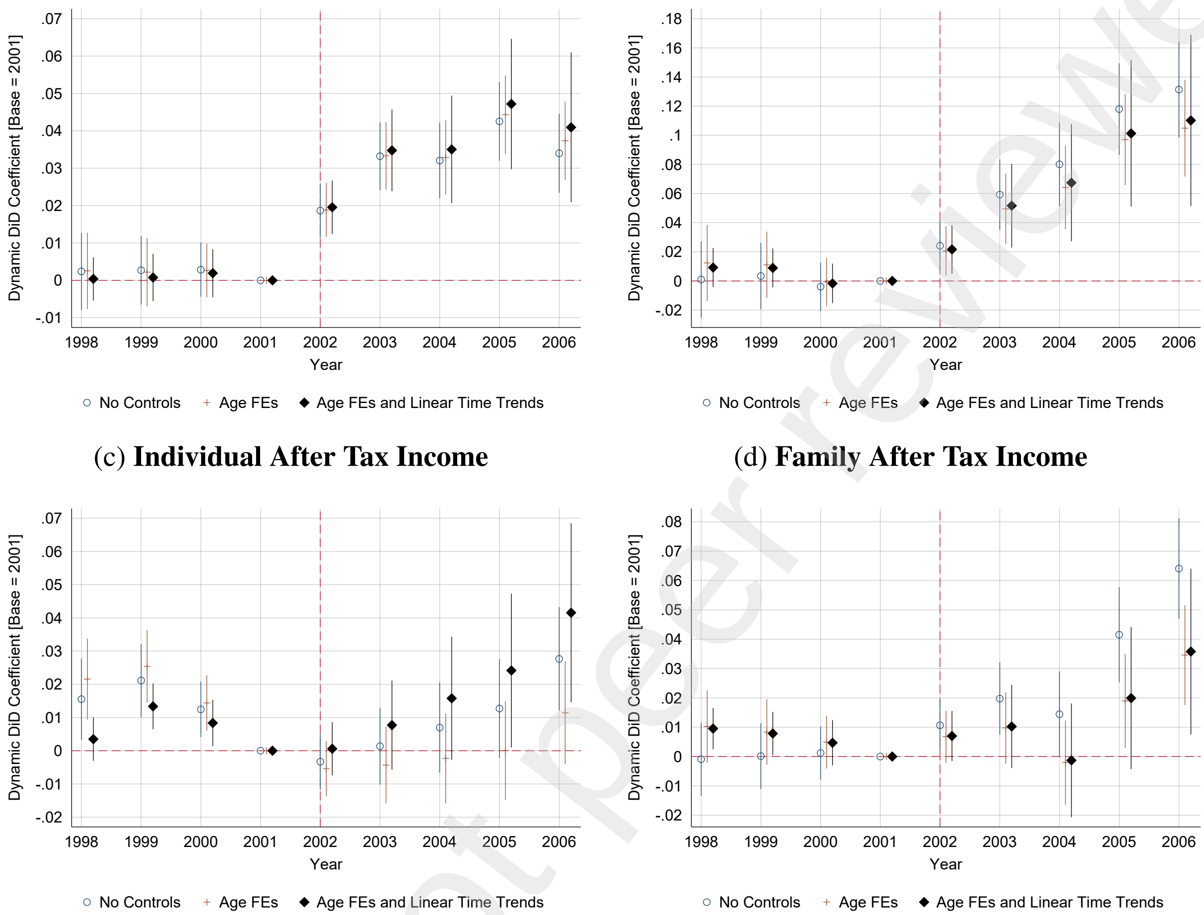
Combining the offsetting effects on earnings and IA income, receiving IA implies a decrease in annual individual and family after-tax income of \$5,380 and \$6,406, respectively, over the four years following the reform. The similarity of the individual and family numbers indicates that changes in family formation and spousal income were trivial. The \$6,406 change in family income is similar to the cost of childcare for a single pre-school-age child in Vancouver.[28] In the early 2000's, half-day kindergarten in public schools started at age 5, and as a result, some treated mothers moving into work would require either full or after-school childcare which may have offset the income gains of working.[29]

[27] We calculate average hourly wages from the Labour Force Survey.
[28] An estimate of median costs of full-time care for a single pre-school-age child in Vancouver in 2019 is \$954 per month (MacDonald and Friendly, 2019) – \$696 in 2002 dollars, or \$8,352 per year.
[29] In Appendix Section III.1, we examine mothers' propensity to claim the childcare tax deduction (and tax filing more generally) as a proxy for increased childcare use.

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**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals. Three specifications are shown: (1) no controls; (2) with mother age fixed effects; (3) with mother age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level. Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles. Income values are also normalized by dividing each value by the mean income in 2001 in the corresponding group (treatment or control).

4.1 Heterogeneity

The average effects on income undoubtedly mask important heterogeneity. To illustrate the distribution of responses, we estimate equations 2 and 3 while defining the outcome variable $Y_{i,t}$ 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 of a mother having earnings over \$25,000.[30] Because 79%

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

Table 4: **Effect on Labour Market Outcomes and Income**

	B	SE	Treated Mean 2001	Implied Effect	N
IA Amount	11035.98	1057.83	.	.	565970
Employed Self	-0.79	0.21	.	.	565970
Normalized Employment Earnings	-1.58	0.50	8800	-13948	565970
Normalized After Tax Income Self	-0.31	0.24	17300	-5380	558230
Normalized After Tax Income Fam	-0.21	0.23	30800	-6406	558230

Note: This table shows estimates of β from equation 3 for the third specification, which includes mother age fixed effects and allows for differing linear time trends between treated and control (using the two-step procedure described in Section 3). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles. Income values are also normalized by dividing each value by the mean income in 2001 in the corresponding group (treatment or control). The implied dollar effect is obtained by multiplying the estimate of β with the treated mean in 2001.

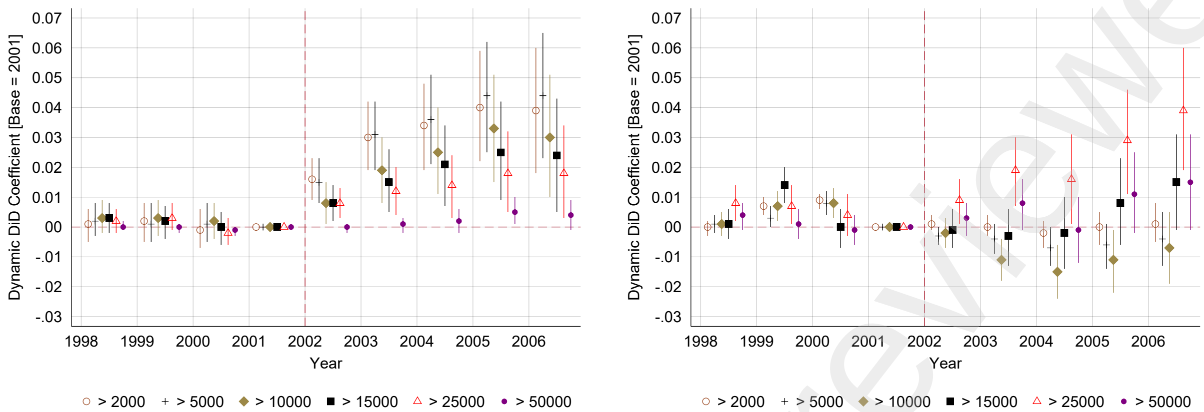
of mothers in the complier group transitioned from zero earnings to employment, a movement in the proportion of mothers with income over a given threshold largely corresponds to a movement from not working to earning over that threshold. In panel (a) of Figure 5, we plot reduced form estimates for thresholds ranging from \$2,000 to \$50,000 for employment income. We view the \$50,000 threshold (\$75,000 in 2022 dollars) as a placebo test because we expect that very few mothers would move into jobs with pay this high. Panel (a) confirms that. At the other extreme, the causal effects on $\mathbb{1}\{I_{i,t} > 2000\}$ and $\mathbb{1}\{I_{i,t} > 5000\}$ are nearly identical, indicating that mothers who entered employment took jobs paying greater than \$5,000.

Table 5 shows the corresponding 2SLS estimates. Again under the assumption that changes in $Pr(I_{i,t} > \bar{I})$ are driven by mothers that started with zero employment income, then the estimate of β divided by 0.79 represents the fraction of employment entrants that entered with jobs paying more than $\bar{I}$ per year. Following this interpretation, we see that 50% ($\frac{.40}{.79}$) of mothers moved into jobs paying more than \$15,000 and 50% took jobs paying less than \$15,000. Thus, relatively equal sized groups entered into jobs paying less money than annual IA income and jobs paying considerably more.

Panel (b) of Figure 5 shows results from the same exercise for after-tax family income. Starting with the extremes again, the reform had no effect on mothers' likelihood to have less than \$5,000 in income, a level of income below any reasonable subsistence definition. By 2003, we observe

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

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



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals from the third specification, which allows for differing linear time trends between treated and control (using the two-step procedure described in Section 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 T4 employment income and panel (b) family after-tax income. Standard errors are clustered at the individual level. Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles.

a 1% drop in the probability of income over \$10,000 and no change in the probability of income over \$15,000. This implies an increase in families with income between \$5,000, and \$10,000 that fits with the earners who took jobs with earnings less than \$10,000. Similarly, the probability of having more then \$25,000 increases following the reform, driven by mothers who re-entered employment with higher annual earnings. The 2SLS estimates in Table 5 summarize these effects, making clear that welfare compresses the income distribution — it represents a binding floor below which mothers cannot fall, while simultaneously reducing higher incomes by disincentivizing work among employable mothers. As a point of reference, in 2003, the poverty line for a three-person family living in a mid-sized city was \$21,216[31]. The reform pushed some families below this poverty line and some above it.

4.2 Comparisons to U.S. Literature

Our results contribute to the debate in the U.S. literature regarding the respective roles of welfare reform, EITC expansion, and macroeconomic conditions, in explaining the growth in single mother employment in the 1990s and early 2000s (e.g., Fang and Keane, 2004; Chan, 2013; Grogger, 2003;

[31] Statistics Canada Table: 11-10-0241-01 (formerly CANSIM 206-0094)

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

	Individual Employment Income		Family After-Tax Income	
Cutoff	B	SE	B	SE
2000	-0.68	0.15	0.08	0.05
5000	-0.72	0.15	0.15	0.07
10000	-0.49	0.14	0.29	0.09
15000	-0.40	0.13	-0.02	0.12
25000	-0.28	0.10	-0.41	0.13
50000	-0.07	0.04	-0.14	0.11

Note: This table shows estimates of β from equation 3 for outcome variables of the form $\mathbb{1}\{I_{i,t} > \bar{I}\}$, where $I_{i,t}$ is income (individual T4 employment income or family after-tax income) and $\bar{I}$ is some cutoff (displayed in the first column). The third specification is used, which includes mother age fixed effects and allows for differing linear time trends between treated and control (using the two-step procedure described in Section 3). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles.

Kleven, 2021; Meyer and Rosenbaum, 2001; Snarr, 2013).[32] In a review, Ziliak (2015) reports that welfare reform increased employment of single mothers by 1-7 p.p. which is comparable to the 4 p.p. we show in Figure 4. This U.S. literature relies in large part on the Current Population Survey whereas we use administrative employment records.

Prior estimates of welfare reforms' impact on incomes often, but not always, find that earnings increases are insufficient to make up for lost transfers (Ziliak, 2015), which contrasts with our average effects. This may in part reflect limitations of U.S. studies that our data allows us to address: namely, the reliance on self-reporting of program participation, which is subject to measurement error and, potentially, to changes in measurement error over time (Ziliak, 2015).[33] Finally, our distributional estimates are consistent with Bitler, Gelbach and Hoynes (2006) who demonstrate that Connecticut's Job-First program had substantially heterogeneous effects on income.

[32] In this vein, several authors have also tried to isolate the role of different elements of welfare reform as determinants of the program participation and employment response (e.g., Fang and Keane, 2004; Grogger, 2003; Mazzolari, 2007). We show that the extension of work requirements is sufficient to generate a large, positive response in employment and income, even in the absence of time limits.

[33] Specifically, Ziliak (2015) writes that "reporting rates of TANF have declined over time, especially in the CPS, and the fact that since 70 percent of TANF is non-assistance, we do not as yet have an understanding of whether survey respondents include an estimate of the cash equivalent of some of this in-kind support" (p. 369).

5 Contemporary Health Care Use

There are four main channels through which welfare reform could affect health outcomes and health care utilization. First, increased disposable income may directly affect health. While cross-sectional estimates of health-income gradients imply strong positive associations, causal work often finds limited evidence of any effect (e.g., Cesarini et al. (2016)). The second channel is direct effects of injury or catching viruses on the job, to the extent that the reform moves people back to employment. Third, employment and reduced leisure time may affect stress levels and subsequent mental health. Fourth, employment decreases time available for preventative care and for seeking treatment for underlying health concerns. This channel helps highlight the difference between health outcomes and health care utilization: it is possible that welfare reform could decrease utilization while also worsening health. In the U.S. context within which much of the existing literature is set, there is a fifth channel: the transition from welfare to employment brings about a change in health insurance status, as people transition from reliance on Medicaid to situations where they may have no health insurance. This is not true in the Canadian context and so our analysis allows us to isolate the effects of the other channels.

There are similar mechanisms through which child health care utilization could be affected, including, again, an increase in disposable income. In addition, more time at work may imply reduced parental time with and supervision of children, which could increase injuries due to unsupervised play. A third channel is the spillover effects of any changes in parental stress as parents move into employment. Finally, when parents spend more time at work, there is less time to take the child to see a health care professional.

We cannot fully distinguish these channels, but can examine effects for different categories of health care use that are likely to be more strongly associated with one channel than others, such as treatments for injuries, mental health illness, and GP visits. Our estimation strategy for these outcomes follows the same approach we used for labour market outcomes. We estimate reduced form event studies from equation 2 and 2SLS treatment effects from equation 3. We normalize each measure of health spending to standard deviation one so that our estimated coefficients are interpreted as multiples of standard deviations, making for easier comparisons across expenditure categories and between adults and children.[34]

5.1 Adults

In panels (a) and (b) of Figure 6, we plot reduced form effects for the two aggregate sub-components of total health care costs: combined hospital and outpatient physician spending, and drug costs. We present the estimates of the causal IA effects for mothers (β from equation 3) in panel (a) of Table 6. The reduced form figures reveal that there are parallel pre-trends between the treatment and controls for both outcomes. We therefore focus on the 2SLS specifications without the pre-trend adjustments, which are more precisely estimated. Our preferred specification corresponds to the second set of columns, including age effects for the mother.

Starting with treatment effects on total health care expenditures, the estimated effect is only 0.05 standard deviations. Moreover, we can rule out effects greater than 0.45 and smaller than -0.36 standard deviations with 95% confidence. The subsequent rows in Table 6 show the effect of the reform on the composition of health care expenditures. For hospital costs and outpatient expenditures, which together make up 80% of health care costs, we again see small effects that are not statistically significant - though, with negative signs in these cases. Pharmaceutical costs, on the other hand, show positive effects on the order of half a standard deviation that are statistically significant at the 5% level.[35] This effect is consistent with the fact that IA recipients get full subsidization for drugs while low-income non-recipients only receive a partial subsidy. The small effects for the other two categories suggests that greater access to drugs does not change health care utilization more generally and, hence, may not change health outcomes markedly. Finally, the overall zero effects could signal that the public health care system works as it is intended to: that

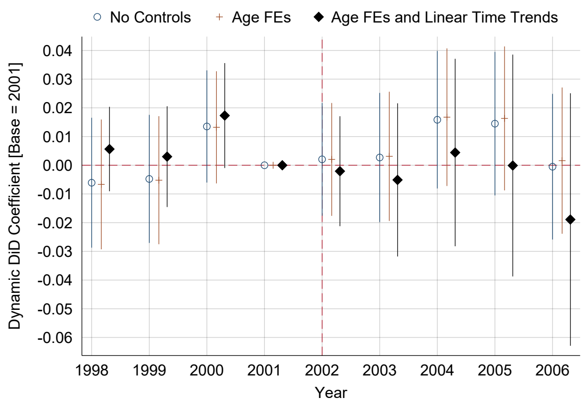
[34] As shown in Table 1, there is substantial variation in expenditures across diagnostic categories and payers as well as between adults and children. For instance, mean outpatient expenditures among adults are more than two times higher than mean hospital costs ($377.59 vs. $130.50).

[35] The standard deviation for drug costs is approximately $500, so our effects show that drug costs increased by $250 ($20/month). This is a non-trivial effect (e.g., blood pressure medication costs approximately $24/month). It could be the difference between having or not access to one's medication.

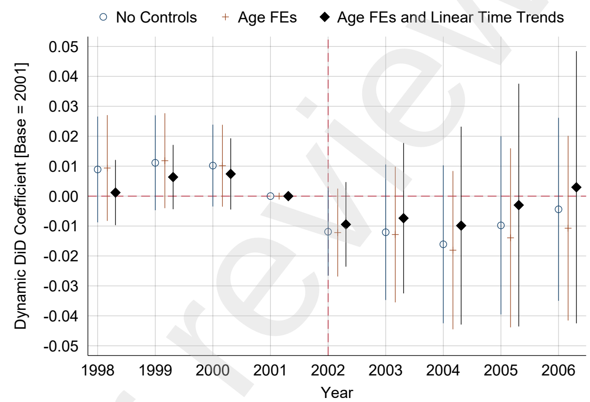
it delivers care equally regardless of a family's level or source of income.

Figure 6: **Reduced Form Effects on Health Care Costs**

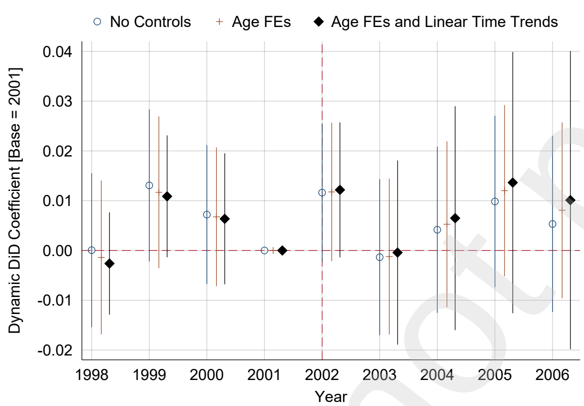
(a) **Outpatient and Hospital Costs, Adults**



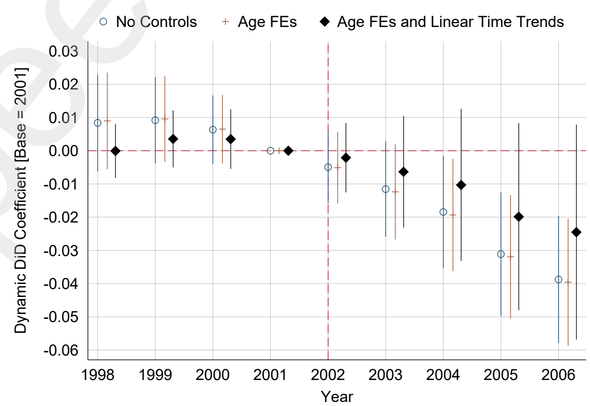
(b) **Pharmaceutical Costs, Adults**



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



(d) **Pharmaceutical Costs, Children**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals. Panels (a) and (b) show estimates for mothers, and panels (c) and (d) children. Three specifications are shown: (1) no controls; (2) with age fixed effects; (3) with age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level. Each outcome is winsorized at the 1st and 99th percentiles, then normalized to mean zero and standard deviation one, such that γ_s are expressed in standard deviation units.

The last five rows of panel (a) of Table 6 contain results for combined hospital and outpatient expenditures in three diagnosis categories — mental health, physical injury, flu and respiratory illness — and for expenditures on GP visits, and pharmaceutical expenditure related to mental health. In Figure III.7, we plot the corresponding reduced-form dynamic effects. The disaggregated expenditures, again, exhibit parallel pre-trends for the control and treatment groups, with the exception of mental health expenditures which exhibit variability in pre-reform differences between treatment and controls. Given this, we focus on the results in the second set of columns, though we discuss results using trend corrections in the third column for mental health spending.

Examining the subcategories allows us to investigate whether the zero effect on total health expenditures reflects underlying, offsetting mechanisms. We do find some evidence of offset. In particular, our estimates imply that IA receipt causes a statistically significant 0.57 standard deviation increase in GP visit costs. This appears to be balanced by a decrease in expenditures for mental-health-related visits: in column (2) we estimate a statistically significant effect of -0.78 standard deviations. When, instead, we include trend corrections, the estimated effect is a statistically insignificant -0.29. The wide confidence interval reflects the pre-reform variability in the differences between treatment and controls which, in turn, is reflected in imprecise estimates for pre-trend differences. On balance, we view this as weakly suggestive evidence that points to negative effects on mental health expenditures. We find no evidence that expenditure on injuries or cold and flu illnesses changes.

Since the results from Section 4 imply that IA receipt significantly decreases employment, the results for GP visits could reflect people on IA having more time available to seek medical treatment. In Canada, because GPs provide primary care and act as gatekeepers for specialist referrals, small effects distributed across specific health categories may accumulate into a detectable effect on GP visits. This may explain the insignificant spending effects we find for specific diagnostic areas, in conjunction with large effects on GP spending. The (weakly suggestive) declines in mental health spending while on IA is consistent with mothers experiencing less stress than they would while working full-time as a low-income single parent.

Our results are somewhat difficult to compare to those in the existing literature. Using the Behavioral Risk Factor Surveillance System, Bitler, Gelbach and Hoynes (2005) 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) does find increases in smoking and drinking, likely reflecting elevated stress, which could cause worse long-run health. In subsequent work using the Survey of Income and Program Participation, Narain et al. (2017) find evidence of welfare reform reducing self-reported health. But, again, it is hard to disentangle the health insurance mechanism from the channels we investigate.[36] Finally, Cesarini et al. (2016) find that large income shocks (lottery winnings) have precise null effects on health care utilization in Sweden (in a health system comparable to Canada's). However, our estimates suggest that the income shock from changing IA status is small, which implies that the health effects we observe are more about time use and employment stress.

5.2 Children

The effects on the health outcomes of children are consistent with what we observe among mothers. In panels (c) and (d) of Figure 6, we present reduced form dynamic estimates for combined hospital and outpatient physician spending, and drug costs, with corresponding IV estimates in panel (b) of Table 6. In our second and third specifications, we include age fixed effects for the child whose health outcomes we are examining (in addition to fixed effects for the age of youngest child in the family). The third specification allows for differential pre-trends, as in the previous section. As with the mothers' outcomes, there is little evidence of differential pre-trends, except for mental health, so we focus on the column (2) results in Table 6.

We can rule out even modest effects of IA on total health care costs for children: the treatment effect is -0.06 standard deviations, with the 95% confidence interval bounded by -0.30 and 0.18. As with mothers, we find zero effects on total hospital and outpatient expenditure, but find that moving onto IA increases pharmaceutical expenditure by 0.55 standard deviations, consistent with differential access to drug insurance for IA and non-IA recipients.

In the remaining rows of Table 6, we show results for expenditures by diagnosis subcategory, and for GP visits. The reduced form dynamic estimates can be seen in Figure III.8. We find

[36] A different strand of literature documents spikes in mortality and morbidity for some recipients of government transfers in the days following cheque issuance (Dobkin and Puller, 2007; Riddell et al., 2006; Evans and Moore, 2011), which might imply a negative effect of transfers on health. But this could be the effect of distributing benefits in concentrated bursts as opposed to the effect of receiving transfers at all.

Table 6: **Treatment Effects of IA on Contemporary Health Outcomes**

Panel A: Adult's Outcomes									
Sample Size: 436171	No Controls			Age FEs			Age FE & Time Trend		
	B	Min	Max	B	Min	Max	B	Min	Max
Total Health Care Spending	0.03	-0.27	0.32	0.03	-0.37	0.43	0.13	-0.55	0.80
Hospital and Outpatient Spending	-0.14	-0.42	0.14	-0.23	-0.61	0.16	0.19	-0.50	0.88
Hospital Spending	-0.21	-0.46	0.05	-0.24	-0.59	0.10	0.54	-0.12	1.19
Outpatient Expenditure	0.01	-0.29	0.31	-0.13	-0.54	0.28	-0.29	-1.03	0.44
Drug Costs	0.34	-0.02	0.69	0.54	0.05	1.02	0.14	-0.53	0.82
GP Visits	0.58	0.26	0.89	0.57	0.14	0.99	0.44	-0.31	1.19
Mental Health Spending	-0.67	-0.99	-0.34	-0.85	-1.31	-0.39	-0.22	-0.97	0.53
Injury Spending	0.05	-0.20	0.30	0.01	-0.32	0.35	0.00	-0.69	0.70
Cold and Flu Spending	0.10	-0.17	0.37	0.07	-0.30	0.44	0.44	-0.30	1.17
Drug Costs Mental Health	-0.09	-0.42	0.25	0.04	-0.41	0.50	-0.52	-1.22	0.18
Panel B: Children's Outcomes									
Sample Size: 914366	No Controls			Age FEs			Age FE & Time Trend		
	B	Min	Max	B	Min	Max	B	Min	Max
Total Health Care Spending	0.22	-0.01	0.46	0.19	-0.05	0.43	0.04	-0.48	0.56
Hospital and Outpatient Spending	0.02	-0.20	0.24	-0.04	-0.26	0.19	-0.07	-0.59	0.45
Hospital Spending	-0.10	-0.29	0.10	-0.12	-0.32	0.08	-0.04	-0.53	0.46
Outpatient Expenditure	0.12	-0.12	0.35	0.05	-0.19	0.28	-0.02	-0.56	0.52
Drug Costs	0.66	0.39	0.93	0.70	0.42	0.97	0.32	-0.21	0.84
GP Visits	0.59	0.34	0.83	0.43	0.19	0.68	0.24	-0.32	0.80
Mental Health Spending	0.15	-0.08	0.38	0.20	-0.04	0.44	-0.32	-0.84	0.20
Injury Spending	-0.15	-0.35	0.05	-0.10	-0.30	0.10	-0.08	-0.59	0.44
Cold and Flu Spending	0.32	0.10	0.55	0.18	-0.04	0.41	0.02	-0.52	0.56
Drug Costs Mental Health	0.43	0.18	0.68	0.49	0.24	0.75	-0.37	-0.92	0.17
Interaction with Child Services	-0.03	-0.07	0.02	-0.02	-0.07	0.03	-0.05	-0.15	0.06
In Foster Care	-0.03	-0.07	0.01	-0.02	-0.06	0.02	-0.08	-0.18	0.02

Note: This table shows estimates of β from equation 3 and 95% confidence intervals (indicated by Min and Max). Three specifications are shown: (1) no controls; (2) with age fixed effects; (3) with age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. Each outcome is winsorized at the 1st and 99th percentiles, then normalized to mean zero and standard deviation one, such that β are treatment effects expressed in standard deviation units.

a statistically significant positive effect of IA receipt on GP visits of 0.44 standard deviations — similar to the 0.57 standard deviation effect for mothers. This suggest that welfare not only affords mothers more time to consult a physician for their own health, but also for the health of their children. Again, there is no corresponding statistically significant change in expenditures for other categories. This, again, likely reflects the fact that GPs are general gatekeepers to health care, while knock-on effects that come from seeing GPs are likely spread diffusely across many diagnostic categories.

Overall, previous research finds that cash and in-kind transfer programs have either no or small positive effects on child health. However, it focuses largely 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 reform, as opposed to other transfer programs (e.g., tax credits (Milligan and Stabile, 2011)). Perhaps closest to our work, Gennetian et al. (2010) study the effect of the 1990s welfare-to-work experiments on children age 3-5 at the time of randomization. For those families where mothers transitioned from welfare to employment without an increase in disposable family income, the authors report small negative effects on parent-reported child general health. They find no effect on child health in families where both employment and income increased. Our results are most comparable to the second set of families. Finally, Cesarini et al. (2016) estimate small impacts of lottery winnings on child health. Again, our results are more likely to reflect changes in parental time availability. In turn, this determines a parent's ability to take their child to GP visits, and potentially more proactive health care utilization.

5.3 Interaction with Child Services

The final two rows of Table 6 show treatment effects on the likelihood that a child appears in the Ministry of Child and Family Development (MCFD) records and that a child is in foster care, respectively. MCFD provides services and supports to families, in addition to foster care, such as respite services for parents of children with complex health needs. Hence, interactions with MCFD identify parents and/or children who may need extra support, and in some cases may be indicative of unsafe or unstable home environments. Both dimensions could be affected by the loss of access to welfare through the impact on family income and parental time and stress. However, we find no evidence that IA receipt affects the likelihood of a child being associated with MCFD services or in foster care. Panel (f) in Figure III.8 shows that the treatment and control groups exhibit parallel pre-trends for interaction with child services.[37]

5.4 Robustness Checks and Multiple Hypothesis Testing

Confounding Persistence of Treatment Effects: Treatment group mothers will enter the control group as their youngest child ages from the treatment group (ages 4 to 6) to the control group (ages 8 to 11). This contaminates the control group if treatment effects are persistent over time. For example, consider a mother with a youngest child of age 5 who is removed from IA in 2003 due to the reform, and suppose that causes an increase in health spending that persists for three years. When she moves to the control group in 2006 (as her child turns 8), we will observe increased expenditures among the control group, leading to downward biased treatment effect estimates.

In our baseline estimates, we exclude mothers whose youngest child is age 7 to guard against short-term persistence of this nature. As a robustness check, we estimate β from equation 3 in specifications in which we sequentially widen the age gap between treatments and controls: dropping families with a youngest child aged 7-8, 6-8, and 6-9. As shown in Figure III.10, the estimates of β are stable as the age gap widens, indicating that our estimates are not confounded by persistence.

This is also supportive evidence for families with a youngest child aged 8 to 11 being an effective control group. If older child families are different from younger child families in how they react to other policy changes and general economic conditions, then we would expect comparability problems to increase as we focus on families further apart in their children's ages, causing differences in estimated effects to emerge. But that is not what we observe.

Robustness to Sample Restriction: For our baseline health estimates, we restricted the sample to mothers (and their children) that received IA at some point between 1989 and 2001, since this is the sample available in the tax return linkage. In Figure III.9 and Table III.2 we show that using all mothers regardless of IA history leaves the qualitative conclusions unchanged, albeit with less precise estimates. The imprecision reflects the weaker first stage caused by including mothers that

[37] The closest related evidence comes from the effect of Medicaid expansions in the US on child maltreatment (Brown et al., 2019) and foster care intake (Beland, Huh and Kim, 2021). Both papers find that Medicaid expansions reduced interaction with child services, with the probable mechanism being reduced financial stress and improved mental health of parents.

are not on the margin of welfare use.

Multiple Hypothesis Testing: In Table III.3 we present two p-value adjustments that account for higher type 1 error introduced by multiple hypothesis testing. The first approach adjusts 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, among the tests that were originally significant at the 5% level, only the coefficient on prescription costs among adults loses significance. Using the very conservative Bonferroni values, the only results that retain statistical significance at the 5% level are mental health expenditure among mothers and GP visit expenditure for children.

6 Children's Long-Run Outcomes

As discussed in Section 1, welfare reform in North America was motivated, in part, by a desire to reduce intergenerational cycles of dependency on welfare; that is, to promote conditions for the development of children that would allow them to rely on earned income as adults, rather than on transfers. In this section, we examine whether welfare reform accomplished this, and if so, through what mediating channels. Three candidate channels are the effects of IA receipt on (i) children's physiological development, (ii) their cognitive and emotional development and human capital attainment, and (iii) parental role-modelling and parent-to-child information transmission about welfare programs. Section 5.2 found only limited effects on child health, thereby ruling out the first channel. In this section, we examine how IA receipt affects children's propensity to receive IA when they transition to adulthood (intergenerational dependence) and potential mediating effects through educational performance.

6.1 Outcomes and Sample Selection

The first measure of educational performance we consider is scores on a standardized test given to all students in Grade 7 - the Foundational Skills Assessment (FSA). The FSA comprises three domains: reading, writing, and math. For each child, we take the average of percent scores across all three domains, then standardize the average to standard deviation one across students. Approximately 14% of children do not write one or more of the FSAs and non-participation is significantly associated with childhood socio-economic characteristics. We therefore also estimate effects on non-participation, defined as not writing any of the three tests.

The second educational outcome we measure is whether a child graduates from high school. The legal school leaving age during our sample period was 16 and children who do not graduate often drop out at age 17. For this reason, we measure exposure to IA from age 8 to 16 and examine how it affected dropping out at older ages. The third outcome is whether the child received IA – separately from their parents – when they were age 20 or 21.

We restrict the set of children to those born between 1986 and 1996. The 1996 end point ensures that we observe all children through to age 21. The 1986 cutoff limits the window to generations that were at most 16 years old at the time of the 2002 reform. We further restrict the sample to children enrolled in school at ages 8 or 9 and at ages 15 or 16. Children that are only sporadically observed likely left the province. Finally, to be consistent with our specifications for other outcomes, we drop child-years in which the age-of-youngest is less than 4 years old.

6.2 Estimating Long-Run Effects of Exposure

In this section, we adapt the empirical strategy presented in Section 3 to link exposure to IA during childhood with long-run outcomes. Our specification for children's long-run outcomes is:

$$Y_{i,b} = \beta_{1-7} \sum_{s=1}^{7} IA_{i,s} + \beta_{8-16} \sum_{s=8}^{16} IA_{i,s} + \psi_b + \alpha_{1-7} \sum_{s=1}^{7} T_{i,s} + \alpha_{8-16} \sum_{s=8}^{16} T_{i,s} + u_{i,b} \tag{4}$$

where, $Y_{i,b}$ is the outcome of child i born in year b, $IA_{i,s}$ is an indicator for IA receipt in the household when the child was age s, $T_{i,s}$ is an indicator for whether child i at age s lived with a youngest child age 4-6 years old, and ψ_b are birth year fixed effects that capture cohort effects.

By construction, any child under age 8 lives in a family where the youngest child is aged 4-

6 years old.[38] Therefore, our instrument provides no variation in IA for children under age 8[39]. For this reason, we split IA exposure over two age ranges: 0 to 7 years old, for which we have no instrument, and 8 to 16 years old, for which we instrument for IA following the identification strategy presented in Section 3.[40] Hence, the explanatory variable of interest in this specification is the sum of years children are in receipt of IA for each age range. Although our identification strategy does not offer an instrument for $\sum_{s=1}^{7} IA_{i,s}$, we keep it in the specification since it helps pick up its own effect and those of correlated omitted variables (Stock and Watson, 2011).

Equation 4 is a restricted version of a more general model in which outcomes are general functions of IA receipt at any age during childhood and of being in the treatment group at any age. Our specification does not allow for dynamic complementarity whereby IA receipt at age s changes the treatment effect of IA receipt at ages greater than s. It also imposes the same sized effect of IA receipt for any year within each of the two age ranges.[41]

We approach the first stage similarly. At a given age s, extensive margin IA receipt is:

$$IA_{i,s} = \pi_{1,s} T_{i,s} \text{Post}_s + \pi_{2,s} T_{i,s} + \gamma_b + \epsilon_{i,s} \tag{5}$$

And summing across years provides the first stage equation for $\sum_{s=8}^{16} IA_{i,s}$:

$$\sum_{s=8}^{16} IA_{i,s} = \sum_{s=8}^{16} \pi_{1,s} T_{i,s} \text{Post}_s + \sum_{s=8}^{16} \pi_{2,s} T_{i,s} + \gamma_b + \sum_{s=8}^{16} \epsilon_{i,s} \tag{6}$$

We provide two implementations of this strategy. First, we assume that effects are constant across ages; i.e., that $\pi_{1,s} = \pi_1$, $\pi_{2,s} = \pi_s$, for $s = 8, \ldots, 16$. Hence, we get:

[38] Recall that we drop children in families where the age of youngest child is 0-3 years old.

[39] In theory, the instrument provides variation in IA for children age 7, but in-line with the previous section where families whose youngest was age 7 were dropped, we focus on children aged 8 and above.

[40] When estimating impacts on Grade 7 test scores, we measure IA exposure from age 8 to 11, rather than 8 to 16.

[41] The assumption of constant treatment effects within the two age ranges is not necessary for identification in principle. To separately identify effects at age 8, 9, 10 and so on, we need a separate instrument for IA at each age. The difference-in-differences presented in Section 3 in theory would allow us to instrument for IA at each age (>8), but the instruments $T_{i,s}\text{Post}_s$ are highly correlated across s, which makes it difficult to provide enough exogenous variation in IA for each age s.

$$\sum_{s=8}^{16} IA_{i,s} = \pi_1 \sum_{s=8}^{16} T_{i,s}\text{Post}_s + \pi_2 \sum_{s=8}^{16} T_{i,s} + \gamma_b + \sum_{s=8}^{16} \epsilon_{i,s} \tag{7}$$

Where the instrument is $\sum_{s=8}^{16} T_s\text{Post}_s$, the number of years a child was in the treatment group in the post-reform era. We also estimate the more flexible specification of separately instrumenting for $IA_{i,s}$ at each age s, such that the first stage is equation 6 and the second stage includes $\sum_{s=8}^{16} \alpha_s T_{i,s}$ rather than $\alpha_{8-16} \sum_{s=8}^{16} T_{i,s}$.

6.3 Results

Table 7 shows OLS and 2SLS estimates of equation 4 with consecutively richer specifications. All specifications control for $\sum_{s=1}^{7} T_{i,s}$, $\sum_{s=8}^{16} T_{i,s}$, and birth year fixed effects. Results for this baseline model are shown in columns (1) and (5). Columns (2) and (6) further control for $\sum_{s=1}^{7} IA_{i,s}$ which corresponds to the exact implementation of equation 4. Columns (3) and (7) include fixed effects for birth year and number of children in the household, allowing each to differ by gender of the child, to account for the effects of household size changes over time. Finally, columns (4) and (8) show results using the more flexible first stage, equation 6.

Unsurprisingly, the OLS estimates indicate that children whose families receive IA are less likely to participate in testing, have lower scores conditional on writing the test, are less likely to graduate secondary school, and are more likely to receive IA as young adults.

All of these effects statistically disappear in the 2SLS results. Column (8) indicates that an extra year of IA (from age 8 to 11) causes Grade 7 test scores to increase by 0.0089 standard deviation, a result that is neither economically nor statistically significant. The lack of effect on test scores is robust across all specifications. The results for graduation are similarly non-significant but less precisely estimated. Column (8) shows that an extra year of IA (from 8 to 16) increases graduation probabilities by 2.11 p.p. on a base of 61.6 p.p. but the estimated coefficient is approximately the same size as its standard error.[42] Table III.4 shows that the null effects are present for both genders.

[42]The base graduation rate is low because of the sample restriction described in Section 2, and because even full-sample graduation rates in the 1990s were below 80%.

Why is there so little effect on educational outcomes? The first answer may be that disposable income did not increase substantially. Section 4 outlined how the \$6,400 increase in after-tax income when mothers were pushed off IA might have been offset by fixed costs of work, implying little change in family resources. But a zero effect on schooling must also mean that changes in mother's time at home had little effect on children's educational outcomes. This fits with Bastian and Lochner (2022) who use time-survey data to show that EITC-induced increases in employment did not crowd-out parental time spent on human capital enrichment with children.

Our results match the summary of the effects on child test scores of 1990s randomized welfare-to-work experiments presented in Duncan, Morris and Rodrigues (2011). When the individual experiments are pooled together, the authors estimate a \$1,000 increase in income corresponds to a 0.06 standard deviation increase in child achievement. Dahl and Lochner (2012) and Bastian and Michelmore (2018) find similarly-sized estimates using EITC expansions in the U.S.[43] In contrast, existing estimates of transfer impacts on high school graduation vary widely. Hartley, Lamarche and Ziliak (2022) estimate that a mother receiving AFDC/TANF during a daughter's teenage years substantially increases the probability that the daughter has *no more* than a high-school degree (though they do not isolate the effect on high school graduation per se). In contrast, Bastian and Michelmore (2018) find that EITC expansions in the U.S. that occurred when a child was 13 to 18 years old improved graduation (though they also find offsetting negative effects for EITC received from age 6 to 12).

The last panel of Table 7 shows effects of childhood IA receipt on IA use when the child becomes a young adult. The estimated impact in all specifications, apart from column (8), implies a statistically insignificant 2 p.p. increase in IA receipt at ages 20 and 21 (relative to a mean of 13 p.p.). It is important to recall that the reform we study coincided with the introduction of a two-year financial independence test. This test, explicitly designed to reduce the inflow of young adults into

[43] A key exception is the Self-Sufficiency Project (SSP) experiment run in BC in the 1990s, which finds substantial positive effects of a wage subsidy that induced increased employment. The majority of children in the SSP wrote the Peabody Vocabulary test from ages 4 to 7, whereas we are measuring standardized tests in math, reading, and writing at age 12. One interpretation is that income support may be more influential in early cognitive development (as argued by Heckman (2006)).

Table 7: **Long-Run Effects on Childhood Outcomes**

	OLS				IV			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Outcome: Non-Participate in Standardized Test								
# Years on IA ($\sum_{s=8}^{12} IA_{i,s}$)	0.0219	0.0184	0.0183	0.0176	-0.0249	-0.0281	-0.0227	-0.0037
	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0192)	(0.0201)	(0.0198)	(0.0258)
First Stage F					122.72	161.40	165.05	25.96
Outcome Mean	0.103	0.103	0.103	0.103	0.103	0.103	0.103	0.103
N	107329	107329	107329	107329	107329	107329	107329	107329
Outcome: Average Standardized Test Score Among Participants								
# Years on IA ($\sum_{s=8}^{12} IA_{i,s}$)	-0.1061	-0.0767	-0.0770	-0.0776	0.0048	0.0197	-0.0109	0.0089
	(0.0027)	(0.0031)	(0.0030)	(0.0031)	(0.0863)	(0.0922)	(0.0890)	(0.1027)
First Stage F					65.45	83.23	86.87	17.64
Outcome Mean	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002	-0.002
N	95311	95311	95311	95311	95311	95311	95311	95311
Outcome: High School Graduation								
# Years on IA ($\sum_{s=8}^{16} IA_{i,s}$)	-0.0383	-0.0292	-0.0292	-0.0300	0.0324	0.0344	0.0165	0.0211
	(0.0005)	(0.0006)	(0.0006)	(0.0006)	(0.0223)	(0.0220)	(0.0209)	(0.0199)
First Stage F					86.36	108.46	112.87	21.92
Outcome Mean	0.616	0.616	0.616	0.616	0.616	0.616	0.616	0.616
N	165147	165147	165147	165147	165147	165147	165147	165147
Outcome: Income Assistance Use at Age 20 and 21								
# Years on IA ($\sum_{s=8}^{16} IA_{i,s}$)	0.0266	0.0216	0.0215	0.0219	0.0214	0.0209	0.0260	0.0036
	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0140)	(0.0142)	(0.0139)	(0.0131)
First Stage F					86.36	108.46	112.87	21.92
Outcome Mean	0.129	0.129	0.129	0.129	0.129	0.129	0.129	0.129
N	165147	165147	165147	165147	165147	165147	165147	165147
Controls:								
$\sum_{s=1}^{7} IA_{i,s}$		Y	Y	Y		Y	Y	Y
Birth Year × Sex			Y	Y			Y	Y
# Kids × Sex			Y	Y			Y	Y
IV(s):								
$\pi \sum_{s=8}^{16} T_s \times Post_s$					Y	Y	Y	
$\sum_{s=8}^{16} \pi_s T_s \times Post_s$								Y

Note: This table shows OLS and 2SLS estimates of β_{8-16} from equation 4 (and β_{8-11} for grade 7 test scores). N Years on IA is $\sum_{s=8}^{16} IA_{i,s}$ in the last two panels, and $\sum_{s=8}^{11} IA_{i,s}$ in the first two. Four specifications are shown, for both OLS and 2SLS. All specifications control for membership in the treatment group between 8 and 16 years old and birth year fixed effects. The second adds a control for IA receipt at ages 1-7. The third adds fixed effects for birth year and number of children in the household, allowing each to differ by sex of the child. The fourth uses the more flexible first stage described by equation 6. Standard errors are clustered at the family level and shown in parentheses.

IA, started at age 18 and stipulated that applicants had to have earned at least \$7,000 in income in each of the past two years to qualify for benefits. The policy would have impacted treatments and controls alike, leaving less room for a clear treatment effect to emerge. Given this, while we cannot reject a null hypothesis of a zero effect, the mainly positive span of the 95% confidence interval suggests that firmly concluding there is a lack of intergenerational transmission is not warranted.

Much of the existing literature does find evidence of positive intergenerational transmission of welfare. Hartley, Lamarche and Ziliak (2022) (building on earlier work from Pepper (2000)) report very large intergenerational transmission in AFDC/TANF receipt from single mothers to their daughters. However, both papers measure children's welfare participation in adulthood at an older age than we do. Whereas we measure IA receipt at ages 20 and 21, Hartley, Lamarche and Ziliak (2022) and Pepper (2000) measure daughters' participation when they are 19-27 and 24-33 years old, respectively. Existing work on the transmission of income shows that intergenerational income correlation is increasing in children's age (Chen, Ostrovsky and Piraino, 2017; Connolly, Haeck and Lapierre, 2021), which might explain the discrepancy between our results and previous findings.[44]

7 Conclusion

We provide perhaps the most comprehensive analysis of the consequences of welfare reform on the well-being of mothers and their children, made possible by coordinating data access across multiple government domains. Moreover, we do so in a setting where identification is not complicated by a coinciding EITC expansion and economic boom, and where health insurance coverage is not tied to welfare receipt. Contrary to claims made on both ends of the political spectrum, we find that forcing welfare recipients with school-aged children to search for work did not substantially alter health care utilization nor children's long-run prosperity. In Appendix IV, we show that for every dollar of welfare cuts, federal and provincial governments gained 14 cents of additional revenue from increased income taxes and only 1.6 cents from reduced health care expenditures. This is because of a large transition from welfare to full-time employment among mothers, which

[44]In a somewhat different context, prominent examples from Norway (Dahl, Kostol and Mogstad, 2014) and the Netherlands (Dahl and Gielen, 2021) both indicate that children are more likely to use disability insurance (DI) as adults if their parents received it during the child's teenage years. DI programs, however, typically provide more income than non-disability programs, such as BC IA, which increases the general attractiveness of enrolling.

boosted family income on average, at the expense of parental time spent with children and time available to visit a family physician. However, average effects mask considerable heterogeneity — some families faced income declines that put them below the poverty line while others experienced sizeable increases.

Our finding of zero effects on most health and education outcomes may be a reflection of a universal health care and education system operating as intended — providing equal access to services regardless of one's income level and income source. This universality may underlie the apparent resilience of mothers. Despite the decreased time at home and potentially elevated stress associated with employment, mothers largely protected their children from degradation of health and education outcomes. Extrapolating beyond this complier group, however, should be done cautiously. The reform, by design, reduced welfare access for healthier and more employable persons (Green et al., 2021). Removing welfare access to less employable and less healthy persons is unlikely to generate the same results.

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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. First, we use MSP registrants' contract number. Children are typically included in their parents' contract up to age 18, and up to age 24 for children in full-time education. Hence, 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 next oldest person on the contract. We also use birth records, which include any child born in BC between 1985 and 2017. For most children, birth records include the personal identifier of the mother, which allows us to establish mother-child linkages. Combining the two sources allows us to balance coverage and accuracy. Where MSP and birth records disagree, precedence is given to the latter. Our approach to link mothers and children successfully attributes a mother to the vast majority of children in British Columbia over our analysis period.

Mother-child linkages are used to construct our treatment variable and to identify children whose outcomes may have been affected by the reform. MSP registrants may change contract number because the payer has changed or because premiums have lapsed. 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 associated with a mother, we assume that all children 0-17 years old live with her, as long as they are observed in BC 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 provinces equates exactly to total hospital expenditure in the province, although for any given visit, RIW×CPWC may over- or under-estimate the true cost.

II Work Search Requirements and Enforcement

As part of receiving Income Assistance, caseload management officers often work with clients to provide counselling and referrals to employment and training programs. Recipients can be required to show proof of work at any time. The Income Assistance manual for staff members writes:

> 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 to seek out 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 on the next page.

Ministry of Social Development and Poverty Reduction

BRITISH COLUMBIA

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

HR0077(16/03/07)

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.

- ☐ HOSPITALI ED
- ☐ OVER 6 YEARS OF AGE
- ☐ MEDICAL OR PHYSICAL CONDITION WHICH PRECLUDES EMPLOYMENT
- ☐ FLEEING ABUSE
- ☐ OTHER (EXPLAIN) ______________________________

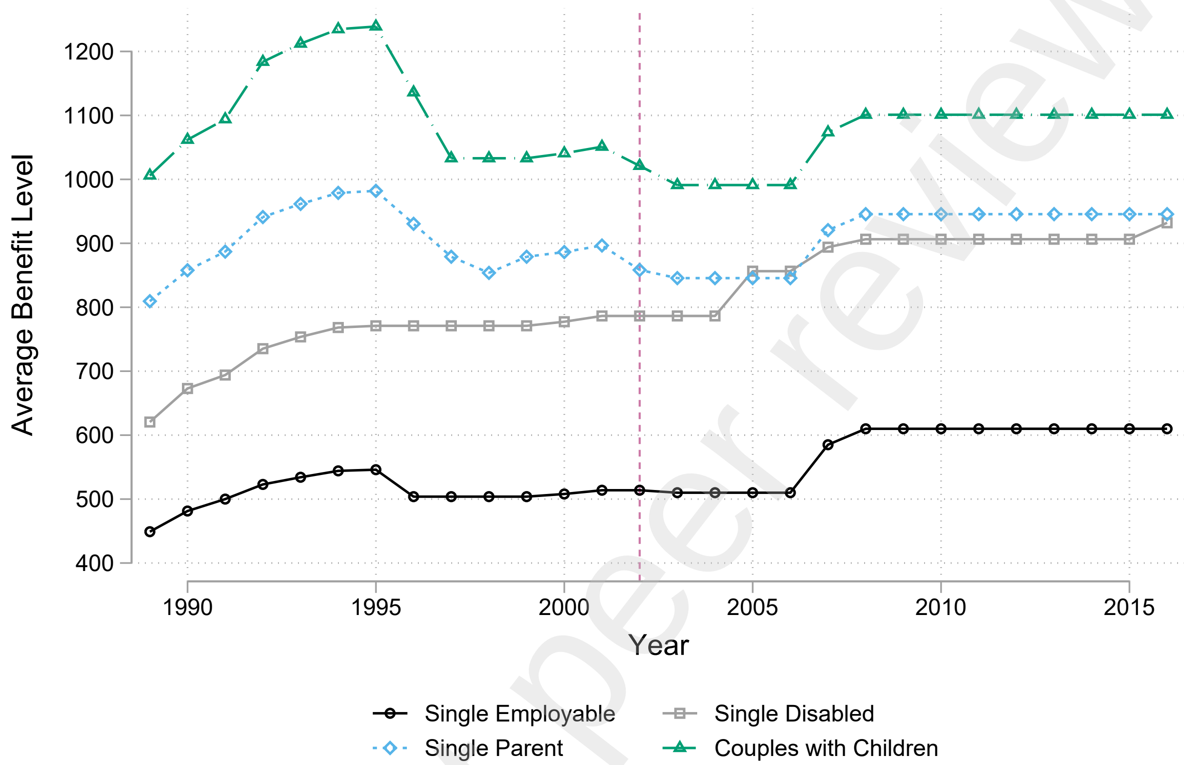
DECLARATION AND NOTI ICATION

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)

HR0077(16/03/07)
Security Classification: MEDIUM SENSITIVITY

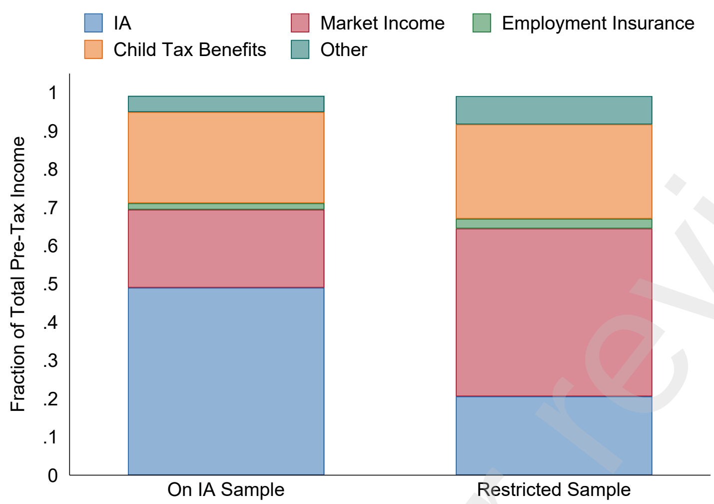
III Supplemental Results

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



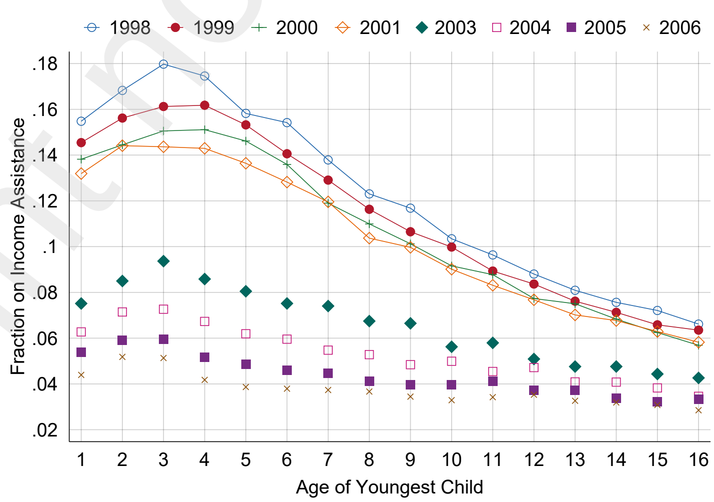
Note: This figure plots average benefit level amounts for different recipient groups: single employable adults; single adults with a disability designation; lone parents with one child, age 2; and couples with two children, ages 10 and 15. Source: National Council of Welfare (various years) and Caledon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

Figure III.2: **Income Composition**



Note: This figure illustrates the fraction of mothers' individual total after-tax income coming from different sources; first among those that receive IA during a given year, and second among the broader analysis 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 corresponds to IA received as recorded on the T5 tax slip.

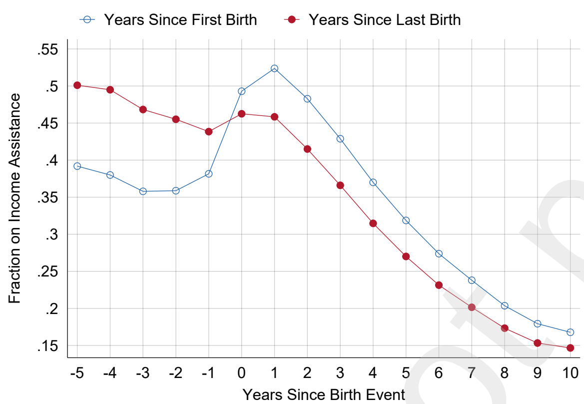
Figure III.3: **Age-of-Youngest Gradient Using the Full Sample**



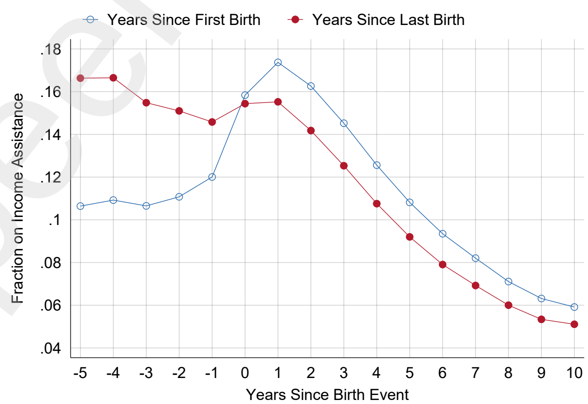
Note: This figure plots the fraction of mothers that received income assistance for each calendar year and age of youngest child in the family.

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

(a) **Restricted Sample**



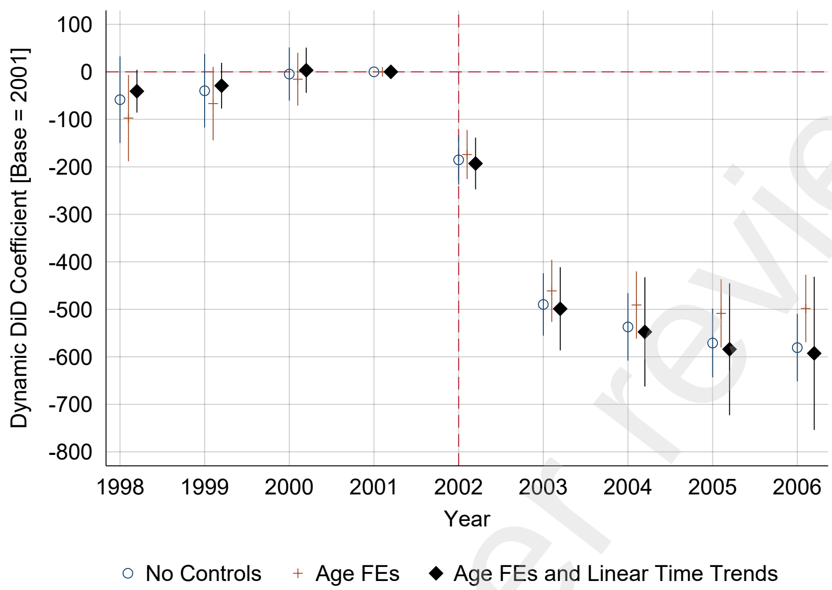
(b) **Full Sample**



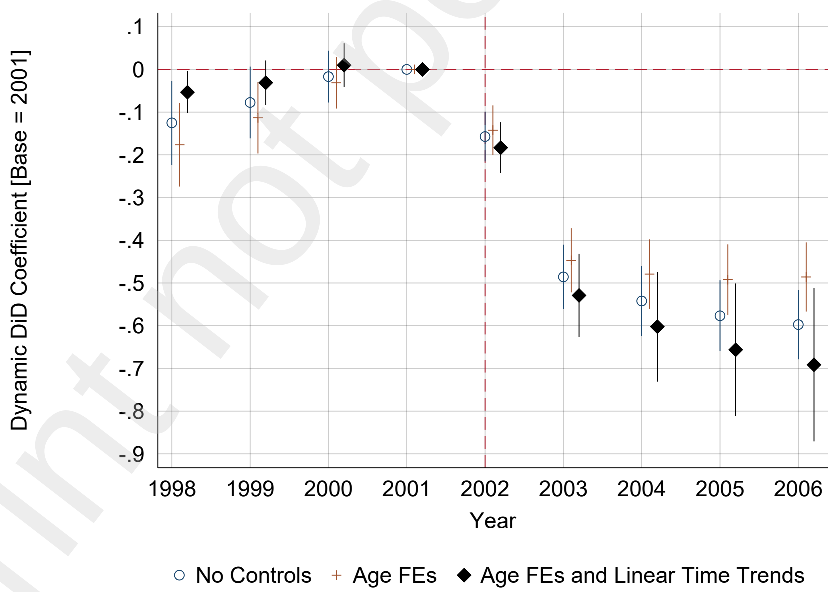
Note: This figure plots the income assistance receipt around the birth of a mother's first child and her last child, conditional on those birth events taking place between 1994 and 2002, respectively. Time 0 denotes the year of birth. Panel (a) uses the restricted sample and panel (b) uses the full sample.

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. Three specifications are shown: (1) no controls; (2) with mother age fixed effects; (3) with mother age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level. Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles.

III.1 Childcare Deductions and Non-Filing

We do not observe childcare expenditures, but we do observe income tax deductions for childcare expenses.[45] Panel (a) of Figure III.6 shows no discernible effect in the reduced form event study estimates for claiming child care deductions. Likewise, Table III.1 shows no discernible treatment effect on claimed deductions. However, this is not conclusive evidence that mothers did not increase their use of childcare. In general, low-income families are much less likely to claim the childcare tax deduction than higher-income families, even after accounting for differences in hours worked (Heidinger, Findlay and Guèvremont, 2020). In our data, Table 1 shows that only 15% of mothers who received IA prior to the 2002 reform claimed the child care credit.[46] This may be because of the administrative cost of claiming the deduction – parents must retain receipts that support expenses –, because unpaid child care is used or child care is paid under the table, or because parents are not aware of the deduction or do not see it as worthwhile given their taxable income.

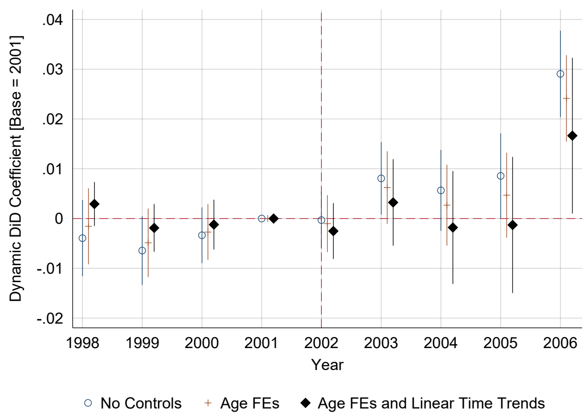
Low claiming of childcare deductions raises the question of whether the reform affected tax-filing more broadly. A number of refundable tax credits, most notably child tax benefits, are only delivered to mothers if they file a T1 return. Recognizing this, the Ministry responsible for provincial IA required and encouraged IA recipients to file their T1. We define a non-filer as a mother that received a tax slip from either an employer (T4) or the government (T4A, T5007) indicating receipt of employment or transfer income, but who did not file their main tax return (T1) during tax filing season. As shown in Table 1, the non-filing rate between 1998 and 2001 among the restricted sample is 1.3% and 2.1% among IA recipients, consistent with the refundable tax credits available to filers. Panel (b) of Figure III.6 plots the reduced form estimates and the final row of Table III.1 the estimated treatment effects of IA on non-filing. The estimated effects of access to IA on non-filing range from 0 to -.05 in the first two specifications (both statistically insignificant), indicating that the cuts to IA may have reduced tax filing, but since these treatment effects are marginal, we view this as unlikely to be a first-order effect on tax credit income.

[45] In Canada, tax payers can claim child care expenses for dependent children under age 16, so long as child care is used in order to engage in employment or to attend school.

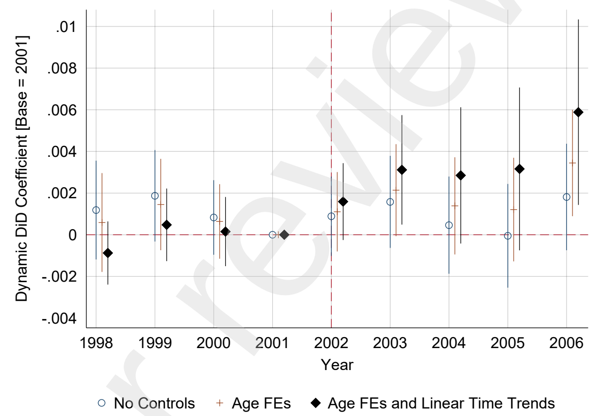
[46] A more minor reason could be the presence of subsidized childcare for low income mothers But this seems unlikely to alter our results because only 3,657 families received a subsidy in 2003. Source: https://www.bcbudget.gov.bc.ca/Annual_Reports/2003_2004/caws/caws_performance_link10.htm

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

(a) **Claiming Childcare Tax Credit**



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



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals. Three specifications are shown: (1) no controls; (2) with mother age fixed effects; (3) with mother age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level.

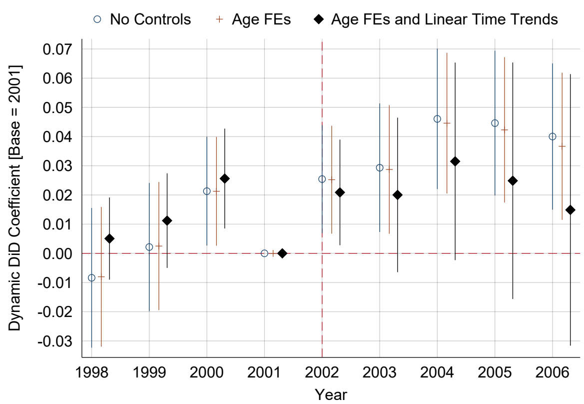
Table III.1: **Claimed Childcare Tax Deductions and Tax Non-Filing**

	No Controls			Age FEs			Age FEs & Time Trends			
	B	Min	Max	B	Min	Max	B	Min	Max	N
Claimed Childcare Credit	-0.31	-0.47	-0.15	-0.29	-0.52	-0.06	-0.01	-0.34	0.32	502660
Fail to File Tax Return	0.00	-0.04	0.04	-0.05	-0.12	0.01	-0.08	-0.17	0.01	565970

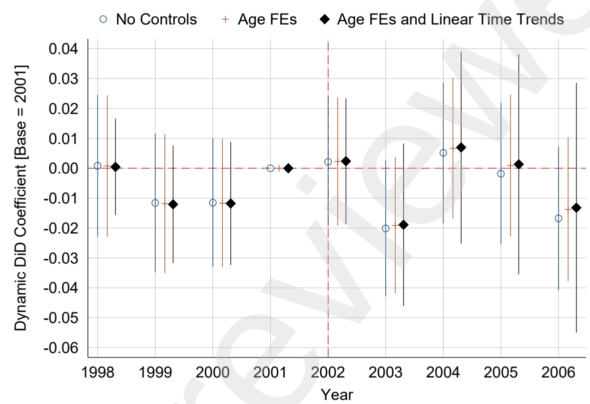
Note: This table shows estimates of β from equation 3 and 95% confidence intervals (indicated by Min and Max), for two outcome variables: whether the mother reported child care tax deductions on her tax return, and whether she failed to file a tax return. Three specifications are shown: (1) no controls; (2) with mother age fixed effects; (3) with mother age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level.

Figure III.7: **Reduced Form Effects on Adults by Sub Category**

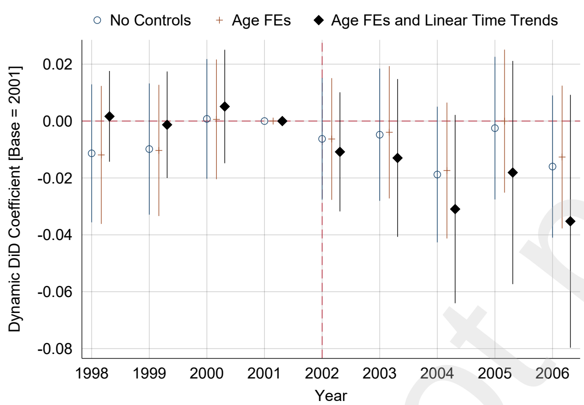
(a) **Expenditure on Mental Health**



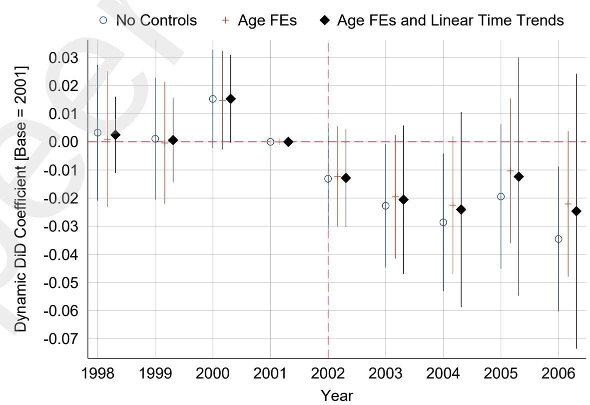
(b) **Expenditure on Injuries**



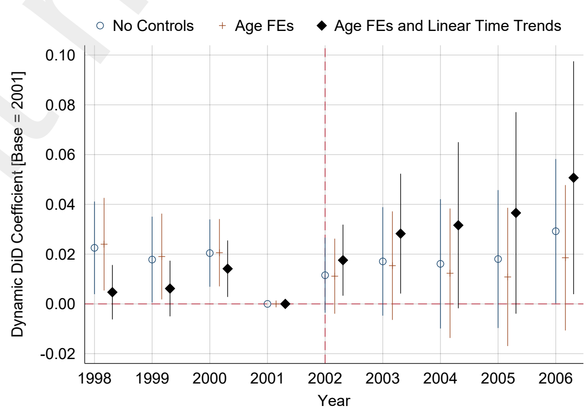
(c) **Expenditure on Flu and Colds**



(d) **Expenditure on GP Office Visits**

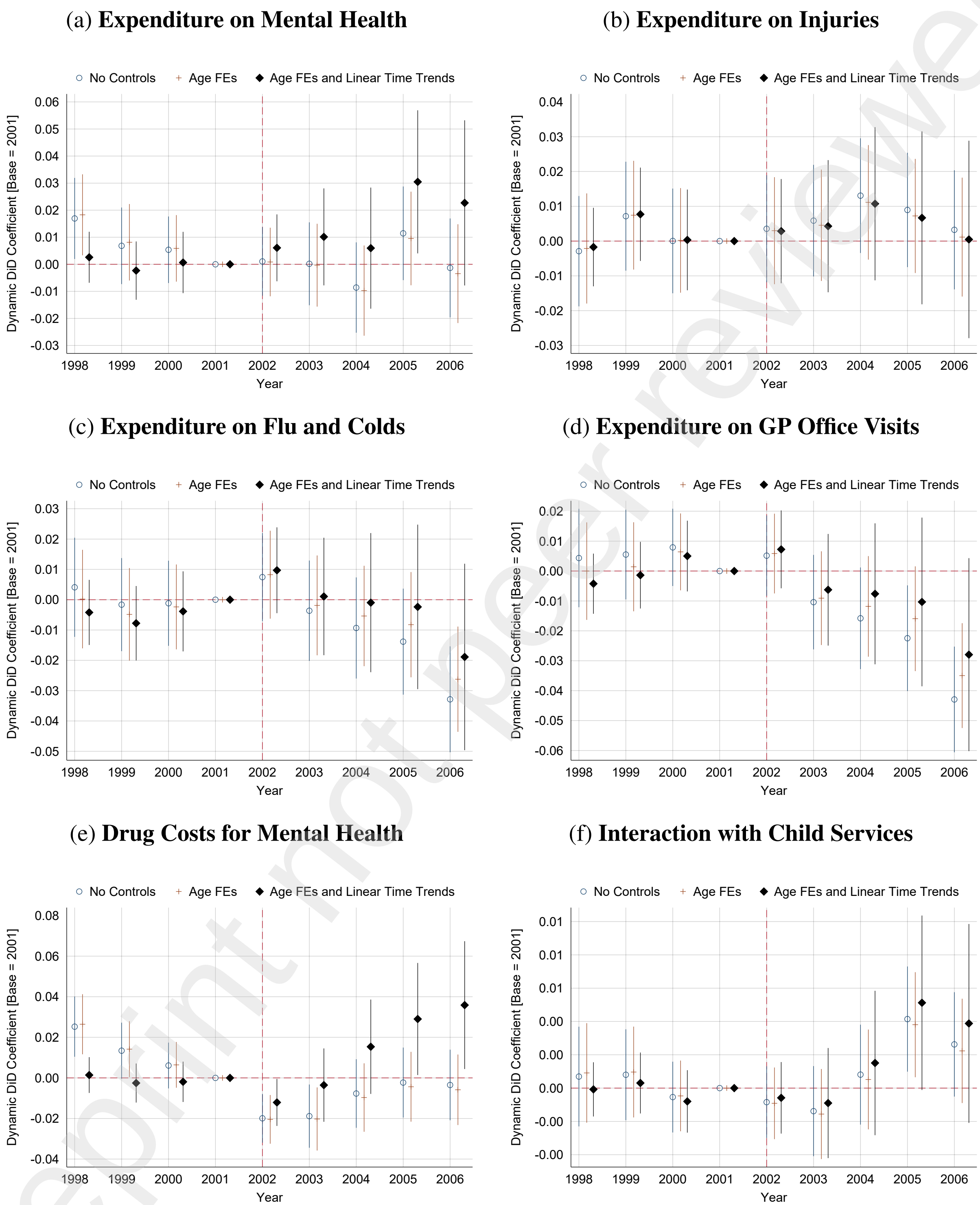


(e) **Drug Costs for Mental Health**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals, for the sample of mothers. Three specifications are shown: (1) no controls; (2) with mother age fixed effects; (3) with mother age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level. Each outcome is winsorized at the 1st and 99th percentiles, then normalized to mean zero and standard deviation one, such that γ_s are expressed in standard deviation units.

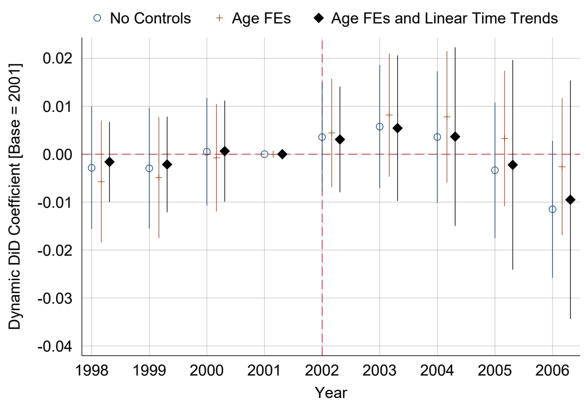
Figure III.8: **Reduced Form Effects on Children by Sub Category**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals, for the sample of children. Three specifications are shown: (1) no controls; (2) with child age fixed effects; (3) with child age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level. Each outcome is winsorized at the 1st and 99th percentiles, then normalized to mean zero and standard deviation one, such that γ_s are expressed in standard deviation units.

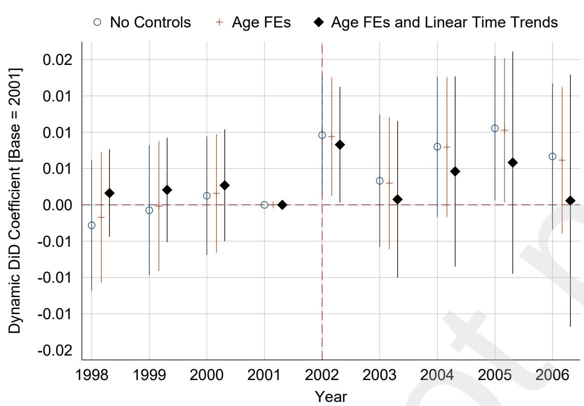
Figure III.9: **Reduced Form Effects on Health Care Costs Using the Full Sample**

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

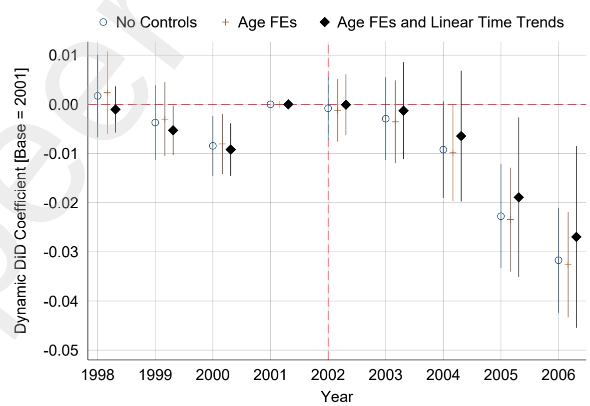


(b) **Pharmaceutical Costs, Adults**

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



(d) **Pharmaceutical Costs, Children**



Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals, using the full sample rather the baseline analysis sample that restricts to mothers that received IA before 2001 (as described in Section 2). Panels (a) and (b) show estimates for mothers, and panels (c) and (d) children. Three specifications are shown: (1) no controls; (2) with age fixed effects; (3) with age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Standard errors are clustered at the individual level. Each outcome is winsorized at the 1st and 99th percentiles, then normalized to mean zero and standard deviation one, such that γ_s are expressed in standard deviation units.

Table III.2: **Treatment Effects of IA on Health Outcomes Using the Full Sample**

Panel A: Adult's Outcomes

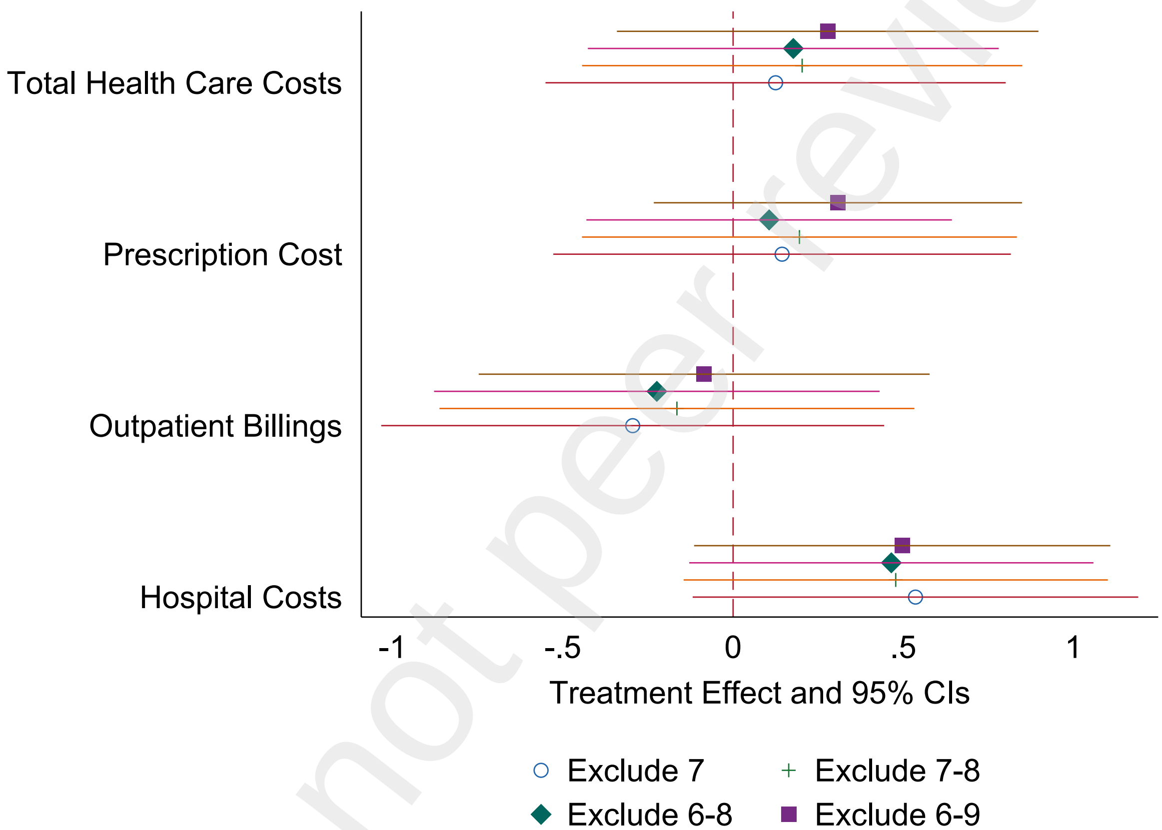
Sample Size: 1348746	No Controls			Age FEs			Age FEs & Time Trends		
	B	Min	Max	B	Min	Max	B	Min	Max
Total Health Care Spending	0.22	-0.04	0.47	0.05	-0.32	0.41	-0.28	-3.13	2.56
Hospital and Outpatient Spending	-0.01	-0.24	0.23	-0.28	-0.63	0.06	-0.05	-2.89	2.80
Hospital Spending	-0.10	-0.32	0.11	-0.27	-0.58	0.04	1.03	-1.70	3.76
Outpatient Expenditure	0.17	-0.09	0.42	-0.15	-0.52	0.22	-1.55	-5.06	1.97
Drug Costs	0.46	0.16	0.76	0.62	0.19	1.05	0.39	-2.34	3.11
GP Visits	0.64	0.37	0.91	0.41	0.02	0.80	1.00	-2.16	4.16
Mental Health Spending	-0.34	-0.60	-0.07	-0.65	-1.04	-0.26	-0.34	-3.48	2.80
Injury Spending	0.25	0.03	0.46	0.15	-0.16	0.45	-0.73	-3.72	2.27
Cold and Flu Spending	0.10	-0.14	0.33	0.02	-0.32	0.36	1.45	-1.83	4.73
Drug Costs Mental Health	0.28	-0.02	0.57	0.42	0.00	0.84	-0.41	-3.31	2.50

Panel B: Children's Outcomes

Sample Size: 2773940	No Controls			Age FEs			Age FEs & Time Trends		
	B	Min	Max	B	Min	Max	B	Min	Max
Total Health Care Spending	0.03	-0.17	0.23	0.06	-0.14	0.27	0.16	-0.92	1.24
Hospital and Outpatient Spending	-0.25	-0.45	-0.06	-0.23	-0.42	-0.03	-0.09	-1.17	0.99
Hospital Spending	-0.07	-0.25	0.10	-0.06	-0.23	0.12	-0.33	-1.36	0.71
Outpatient Expenditure	-0.33	-0.54	-0.13	-0.31	-0.52	-0.10	0.17	-0.94	1.28
Drug Costs	0.46	0.23	0.68	0.49	0.27	0.72	0.62	-0.47	1.72
GP Visits	-0.07	-0.28	0.15	-0.05	-0.27	0.16	1.59	0.31	2.87
Mental Health Spending	0.14	-0.06	0.34	0.14	-0.06	0.33	-1.08	-2.21	0.05
Injury Spending	-0.26	-0.43	-0.08	-0.20	-0.38	-0.03	-1.76	-3.03	-0.49
Cold and Flu Spending	-0.12	-0.32	0.08	-0.13	-0.33	0.06	0.41	-0.74	1.55
Drug Costs Mental Health	0.32	0.10	0.53	0.35	0.14	0.57	-1.90	-3.24	-0.56
Interaction with Child Services	0.02	-0.01	0.04	0.02	-0.00	0.04	-0.01	-0.14	0.13
In Foster Care	0.01	-0.01	0.04	0.02	-0.01	0.04	-0.07	-0.20	0.06

Note: This table shows estimates of β from equation 3 and 95% confidence intervals (indicated by Min and Max). Three specifications are shown: (1) no controls; (2) with age fixed effects; (3) with age fixed effects and allowing for differing linear time trends between treated and control (using the two-step procedure described in Section 3). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. Each outcome is winsorized at the 1st and 99th percentiles, then normalized to mean zero and standard deviation one, such that γ_s are expressed in standard deviation units.

Figure III.10: **Testing for Confounding Persistence of Treatment Effects**



Note: This figure plots estimates of β from equation 3 and 95% confidence intervals, for the sample of mothers, while consecutively widening the age gap between treatment and control. Our baseline estimates exclude families where the youngest child is aged 7 (Exclude 7). The results correspond to the third specification, which includes mother age fixed effects and allows for differing linear time trends between treated and control (using the two-step procedure described in Section 3). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. Each outcome is winsorized at the 1st and 99th percentiles, then normalized to mean zero and standard deviation one, such that β are expressed in standard deviation units.

Table III.3: **Adjusted P-Values for Multiple Hypothesis Testing**

	Sample	Effect	SE	Unadjusted p	Sharpened q	Bonferroni p
Hospital and Outpatient Costs	Adults	-0.229	0.197	0.245	0.291	1.000
Total Prescription Cost	Adults	0.538	0.247	0.029	0.063	0.412
Outpatient Exp GP Visits	Adults	0.567	0.218	0.009	0.027	0.130
Injury Expenditure	Adults	0.015	0.172	0.931	0.751	1.000
Colds and Flu Expenditure	Adults	0.071	0.188	0.706	0.695	1.000
Mental Health Expenditure	Adults	-0.850	0.234	0.000	0.002	0.004
Hospital and Outpatient Costs	Children	-0.035	0.115	0.761	0.695	1.000
Total Prescription Cost	Children	0.700	0.140	0.000	0.001	0.000
Outpatient Exp GP Visits	Children	0.434	0.126	0.001	0.003	0.008
Injury Expenditure	Children	-0.105	0.102	0.303	0.309	1.000
Colds and Flu Expenditure	Children	0.184	0.116	0.113	0.170	1.000
Mental Health Expenditure	Children	0.201	0.121	0.097	0.170	1.000
Interaction with Child Services	Children	-0.018	0.024	0.453	0.406	1.000
In Foster Care	Children	-0.019	0.022	0.388	0.373	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 four columns present baseline estimates for the second specification, which includes age fixed effects. The first one calculates sharpened False Discovery Rate (FDR) q-values following the procedure outlined in Benjamini, Krieger and Yekutieli (2006). 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.

Table III.4: **Effects on Children's Education and Young Adulthood Outcomes, by Sex**

	OLS		IV	
	(1)	(2)	(3)	(4)
	Female	Male	Female	Male
Outcome: Non-Participate in Standardized Test				
N Years on IA	0.0144	0.0221	-0.0262	-0.0183
	(0.0012)	(0.0014)	(0.0230)	(0.0325)
First Stage F			104.01	74.09
Outcome Mean	0.085	0.121	0.085	0.121
N	52351	54978	52351	54978
Outcome: Average Standardized Test Score Among Participants				
N Years on IA	-0.0709	-0.0829	0.0166	-0.0455
	(0.0041)	(0.0043)	(0.1054)	(0.1431)
First Stage F			58.15	36.24
Outcome Mean	0.111	-0.110	0.111	-0.110
N	46535	48776	46535	48776
Outcome: High School Graduation				
N Years on IA	-0.0287	-0.0298	0.0319	-0.0052
	(0.0008)	(0.0008)	(0.0241)	(0.0353)
First Stage F			88.32	42.44
Outcome Mean	0.658	0.575	0.658	0.575
N	80687	84460	80687	84460
Outcome: Income Assistance Use at Age 20 and 21				
N Years on IA	0.0234	0.0197	0.0293	0.0221
	(0.0007)	(0.0006)	(0.0171)	(0.0225)
First Stage F			88.32	42.44
Outcome Mean	0.143	0.115	0.143	0.115
N	80687	84460	80687	84460

Note: This table reproduces columns (3) and (7) of Table 7, separately for female and male children. It shows OLS and 2SLS estimates of β_{8-16} from equation 4 (and β_{8-11} for the test score outcomes, since the grade 7 test is written at age 12). N Years on IA is $\sum_{s=8}^{16} IA_{i,s}$ in the last two panels, and $\sum_{s=8}^{11} IA_{i,s}$ in the first two. The specification also includes fixed effects for birth year and number of children in the household, allowing each to differ by sex of the child. Standard errors are clustered at the family level, to account for inter-family correlations between siblings, and shown in parentheses. *Mean* denotes the mean of the outcome variable.

IV Spillovers Accounting

In this section we calculate the net fiscal costs from the government's perspective associated with the causal point estimates of granting IA on health care expenditure and tax revenue. Income taxes paid to provincial and federal governments measured in tax returns are straight forward. Whether to include federal taxes in the accounting depends on whose perspective one takes: the provincial government, or also the federal government (which provides block transfers to provinces for health and social programs). We include federal taxes since health care expenditure is also partially funded through federal transfers to provinces.

Table IV.1 shows that the government loses \$20 per child granted IA in reduced health expenditures and \$164 adult health care costs, in total less than 2% of the \$11,036 direct cost of granting IA benefits. In contrast, IA-induced employment losses cost the government \$1565 (1281+ 384) in lost tax revenue, which is approximately 14% of the direct cost. Summing both health care costs and tax spillovers, we estimate that every dollar spent on direct IA expenditure costs an extra 17 cents.

Table IV.1: **Change in Government Fiscal Position from an IA Recipient**

Children Health Care Costs	-20.118
Adult Health Care Costs	-164.577
Federal Tax Self	-1281.68
Provincial Tax Self	-384.374
Total Indirect Costs	-1850.75
Direct Cost of IA Benefits	-11036
Ratio of Indirect to Direct	0.17

Note: This table shows estimates of β from equation 3 for the third specification, which includes mother age fixed effects and allows for differing linear time trends between treated and control (using the two-step procedure described in Section 3). Outcome variables represent health care costs and tax revenues from different sources. The coefficients are interpreted as the estimated change in the governments' fiscal position from granting a person IA (at the annual frequency). Dollar amounts are expressed in 2002 CAD and winsorized at the 1st and 99th percentiles.

Despite capturing the universe of government health expenditure and income tax revenue, this calculation naturally does not include all potential spillover costs to the government. We could, for instance, use the point estimates on high school graduation and intergenerational dependency to get noisy estimates of discounted long-run costs, but we do not push the data that far.