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

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Abstract

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

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

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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 US welfare system, whose name – Aid to Families with Dependent Children (AFDC) – says it all. It was also a focus in the Canadian welfare system. 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 US, welfare caseloads dropped by 50% between 1995 and 2000 following the Personal Responsibility and Work Opportunity Reconciliation Act (PRWORA) (Hoynes, 2009). Equally large declines occurred in Canada's welfare system known as Income Assistance, or IA (Kneebone and White, 2014). In British Columbia (BC), the province we study, caseloads dropped by 50% between 1996 and 2004.

The predicted consequences of the reforms ran between two extremes. In the most optimistic outlook, the reforms would improve parental health, child health, and child educational attainment by 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.[1] A Wall Street Journal article described the US reforms as "the greatest advance for America's poor since the rise of capitalism" (Deparle, 2004). The most pessimistic critics argued that the reforms would reduce income, increase stress, and, if work was found, reduce parental time with children – all of which could negatively affect health and education. One prominent policy commentary on the BC reforms stated: "We are deeply concerned that the new welfare rules are a social catastrophe in the making... with profound social and health consequences"(Klein and Long, 2003). Our goal is to investigate which view is accurate, or whether the truth lies somewhere in between.

[1] The BC Minister overseeing IA wrote in a press release: "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."(Ministry of Human Resources, 2002)

A major innovation of our paper is the novel data built to study this question. We link anonymized individual-level administrative data from a wide range of government ministries, including all out-patient and hospital expenditures, most pharmaceutical spending, school enrollment, test scores, IA receipt, income tax returns, and criminal charges. This allows us to examine the holistic impacts of welfare reform together with the impacts on income and employment that are the usual focus (see Ziliak (2015) and Chan and Moffitt (2018) for reviews).

Our setting is the 2002 reform in British Columbia (BC) that dramatically cut welfare access, especially for mothers. We use an identification strategy that leverages a key 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. In a difference in difference setup, we compare families newly subject to these work-search requirements with families already subject to them (i.e., with a control group with children of nearby ages).[2]

Following the reform, IA receipt in the treated group decreased by 7.2 percentage points relative to the control group, with an annual loss in IA benefits of $10,687 among compliers (equivalent to 12 months of benefits). This is close to the pay of a full-time minimum wage job at the time (35 hours × 50 weeks × $8 = $14,000). These benefit declines were offset by increased employment income: 63% of the complier group moved from zero to positive employment income and family after-tax income increased, on average. Since mothers who rely on welfare typically fall below the poverty line, the reform *reduced* poverty rates by raising employment income. However, this average effect hides substantial heterogeneity: a minority of the complier families were pushed deeper into poverty, while the majority experienced varying degrees of increased income. The large employment effect 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. The reform can be seen as hastening that exit.

For health, we find precisely estimated zero impacts on total universally insured healthcare

[2] Similar work-search requirements (and exemptions for mothers with young children) are embedded in welfare programs elsewhere such as TANF in the US (Congressional Budget Office, 2022). Chan et al. (2023) find that the imposition of search requirements in Australia's welfare program substantially reduced welfare receipt.

costs for mothers. For every dollar saved by the government in reduced IA payments, health costs increased by 0.35 cents, with 90% confidence bounds of -2.2 and 2.9 cents. However, total health-care costs include a wide array of services, many of which might *ex-ante* be considered unrelated to IA receipt and some of which may exhibit opposite sign effects. We therefore also examine sub-categories of health services with plausible mechanisms linking them to welfare reform. Most notably, we find suggestive evidence that the reform was associated with a reduction in preventative care and an increase in mental health treatment. These are consistent with the mother's shift to employment causing reduced time available for non-urgent care and heightened stress. In contrast to mothers, we do not find evidence of changes in health outcomes for children (aged 8 to 15).

Unlike hospital and outpatient care, pharmaceuticals are not universally insured in Canada. IA recipients receive full coverage while low-income non-recipients do not. We estimate that (among mothers in the complier group), the fraction of drug costs paid for by the government declined by 84 percentage points, causing a significant increase in the cost of pharmaceutical care for families. Unsurprisingly, we also see reductions in pharmaceutical consumption among mothers, including contraceptives, but not among children.

If the transition to employment reduced parent-child time, we might expect impacts on child education outcomes. However, we find zero impacts on their grade 10 test scores and high school graduation. This aligns with Bastian and Lochner (2022) who show that increased maternal employment induced by the US Earned Income Tax Credit (EITC) reduced mothers' time with children in general, but not time spent on human capital activities. We also find that IA receipt from age 8 to 15 does not affect the likelihood of being charged with a crime when the child becomes a young adult (age 20 to 21). However, we find that IA receipt from age 8 to 15 increases the likelihood of children using IA when they reach early adulthood. In the absence of effects on educational attainment, plausible mechanisms are parental role modeling 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. While the transition to employment increased household income, on average, it likely also increased maternal stress and reduced time available for preventative health care. Despite these changes, the health outcomes and educational performance of children were not significantly affected. This apparent resilience may reflect Canada's public health and education systems operating as intended: providing services regardless of the income level and income source of families. It may also reflect mothers sacrificing time for themselves to preserve outcomes for their children.

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 typically rely on a narrow set of self-reported measures: both Kaestner and Tarlov (2006) and Basu et al. (2016) find limited effects on maternal self-reported mental and physical health, but Basu et al. (2016) find that welfare reform increased smoking and drinking. Among children, Gennetian et al. (2010) find small effects on parent-reported health using welfare-to-work experiments.[3] With respect to educational outcomes, Duncan, Morris and Rodrigues (2011) show that *income* gains from welfare-to-work experiments marginally increased test scores, and Miller and Zhang (2009) find that US welfare reform increased test scores. These studies measure test scores at younger ages than we do, which may explain the discrepancy with our results.[4] Finally, our estimate of intergenerational transmission of welfare is consistent in sign with estimates from elsewhere (Pepper, 2000; Dahl, Kostol and Mogstad, 2014; Hartley, Lamarche and Ziliak, 2022).

Our contributions to the literature are threefold:

First, the data that we assemble provides advantages relative to past studies. For instance, while most existing studies focus on self-reported general health, we access hospital records, outpatient billings, and pharmaceutical prescriptions alongside diagnostic codes, which allows us to partially disentangle mechanisms underlying the effects of welfare reform. The administrative data is also

[3] 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. The US 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, study the impact of EITC expansions on birth weight and 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).

[4] Herbst (2016) and Washbrook et al. (2011) both study effects on infant development from work search exemptions based on the age of the youngest child in TANF. Because almost all states cap the exemption period at 12 months, there's only variation in the child's first year.

free of measurement error, which is often severe in surveys of program participation (Meyer and Mittag, 2019). For instance, we observe intergenerational transmission of welfare using caseload data and administrative linkages between mothers and children, which contrasts with research that relies on individuals reporting current program use (e.g., Hartley, Lamarche and Ziliak (2022)).

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

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

Third, in contrast to studies of the 1990s US reforms, we do not face confounding effects from a coinciding expansion of the Earned Income Tax Credit (EITC). Isolating the effects of welfare reform and EITC expansion has proven challenging (Grogger, 2003; Fang and Keane, 2004; Chan, 2013; Kleven, 2023; Schanzenbach and Strain, 2021; Bastian and Jones, 2021). Similarly, AFDC/TANF reform coincided with substantial shifts to other safety net programs (SSI) (Schmidt and Sevak, 2004), which further complicates the interpretation of US welfare reform.

The paper is structured as follows. Section 1 outlines the history of BC IA reform. Section 2 overviews the data. Section 3 presents the identification strategy. Section 4 presents the labor market effects. Section 5 presents effects on health outcomes. Section 6 includes results for children's educational attainment, and criminal charges and IA as young adults. Section 7 concludes.

1 Institutional Background

1.1 Income Assistance

The welfare system in BC (known as Income Assistance, or IA) is a system of last resort, providing cash transfers and support for re-entering employment. It restricts access to households with income and assets below thresholds 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 have changed, but this basic structure has remained.

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

The large caseload, combined with increased federal deficits that were partially passed on to the provinces, led to a push to reduce IA outlays. To accomplish this, legislative reforms were implemented in 1996 and 2002, along with ongoing regulatory changes.[6][7] The 2002 changes were the 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 accessing IA); a three-week waiting period after application; and a reduction of the age of the youngest child below which a mother was exempt from job search requirements from 7 to 3 years of age.[8][9]

Following the reforms, the caseload fell from 350,000 to under 150,000 between 1995 and

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

[6] Such as a 40% reduction in the number of offices where a person could apply for benefits (Hicks, 2022*b*).

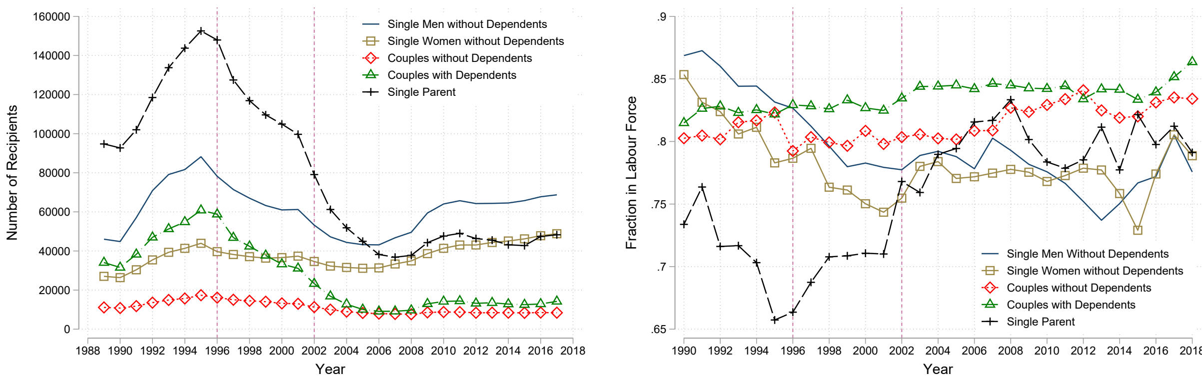
[7] These changes were bellwethers for a shift in outlook towards restricting access to those who were deemed most in need. Green et al. (2021) show that after the 2002 reform, the composition of IA users shifted heavily toward people with physical and mental health problems.

[8] Other changes in 2002 included the elimination of the $200 monthly earnings exemption, an increase in the tax back rate on other income from 75% to 100%, and the elimination of a 'transition-to-work' benefit of $150 per month.

[9] By comparison, in all but one state, TANF sets this age-of-youngest exemption for work search requirements at 12 months or less as of 2009 (Rowe and Murphy, 2008)

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

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



Note: Panel (a) plots the number of recipients (ETW and DA combined) as of January 1st in each year. Panel (b) plots labor force participation rates among persons aged 20-59 with less than a university education. Trends are shown for single men without dependents, single women without dependents, couples without dependents, couples with dependents, and lone parents.

2006. Families were most impacted. Lone and two-parent family recipients (including children) decreased from 220,000 to 50,000. In contrast, the childless adult caseload dropped from 150,000 to 80,000. The system was transformed from a widely accessed safety net with an emphasis on families to a much smaller system with a greater emphasis on childless adults.

In contrast to the substantial changes in access rules, the average benefit level for recipient families only fell by 6% (see Figure III.1). As a result, our study of the 2002 change in access rules is not complicated by changes in benefit rates. A single parent with one child and a couple with two children received monthly benefits of $896 and $1,051 in 2001, respectively. By comparison, the poverty line was $1,972 per month for a family of two and $2,943 for a family of four.[10]

The reforms are evident in the trajectory of labor force participation 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 long-standing gap in participation rates between single mothers and women without dependents had closed completely, as had occurred in the US following welfare reform in 1996 (Kleven, 2023). In contrast, the participation rate for couples without dependents was constant, consistent

[10]Statistics Canada's before-tax low-income cut-offs for cities with over 500,000 inhabitants in 2001 (see Table 11-10-0241-01, Low-income cut-offs (LICOs) before and after-tax by community size and family size, current dollars).

with this group's low welfare use. Participation rate trends for couples with children were also quite flat, reflecting a stable labor market during this period. This provides an easier environment for identifying policy impacts than the late 1990s US which saw a coinciding economic boom.

1.2 Health and Education Institutions

All hospital and medically necessary outpatient care is universally insured in Canada. Pharmaceuticals, dental care, vision, and some medical devices are not universally insured. At the time of the reform, IA recipients received 100% subsidization for pharmaceuticals, while low-income non-recipients only received partial coverage.[11][12]

Education is mostly publicly funded. BC public schools are fully government-funded and offer free student access. Private schools require a tuition payment. The vast majority of children attend public schools and those in the private system are disproportionately from high-income households. The enrolment and test score records we access include both private and public schools.

2 Data and Sample Selection

2.1 Data Sources

We access a rich linkage of government administrative data. We first describe each individual administrative dataset and then outline the linkage between the data sets in the following section.

Income Assistance Receipt: We draw on the universe of monthly IA records (beginning in 1989) that indicate the amount of IA received and the category of benefits. All household members are recorded, including children.

Employment, Income, and Tax Payments: We use tax returns from BC residents to measure employment and earnings from 1998 to 2018. We define extensive margin employment in a calendar

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

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

year as an individual receiving earnings from a tax-registered employer.[13] We draw on the main tax form (the T1) to measure total after-tax income. After-tax income is the sum of income from all sources, including transfer and alimony income, minus tax liabilities.

Hospital and Outpatient Care: The universe of hospital inpatient visits and hospital-based day surgeries is reported in the Discharge Abstract Database (DAD) (see Appendix I.2 for more details). Outpatient services are recorded in the Medical Services Program (MSP) data. The MSP is the government's universal insurance program which covers all medically-necessary care not directly provided by hospitals. We access the universe of MSP procedure-level billings (1991 to 2020), including the cost of each procedure and the associated ICD9 diagnosis code. The combined hospital and outpatient data cover most healthcare services, except for non-medically-necessary dental and vision services and allied health professionals.[14] About 70% of emergency room visits are identifiable in the combined DAD and MSP data during our time period (Peterson et al., 2021).

Pharmaceuticals: We access PharmaNet (1995 to 2020), which tracks all prescriptions filled in community pharmacies regardless of insurance coverage, the total cost of the prescription, and the fraction of the cost paid by government insurance. We observe American Hospital Formulary Service (AHFS) Pharmacologic-Therapeutic Classification codes for each prescription, allowing us to examine sub-categories of pharmaceutical use, specifically those related to mental health. PharmaNet excludes prescription drugs dispensed in a hospital or mental health center.[15]

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

Criminal Charge Records: We observe all criminal charges issued in the province from 2001 to 2021, regardless of whether the charge leads to a conviction. With this data, we construct an outcome variable indicating whether children were charged with a crime at age 20 or 21.

[13] As indicated in T4 slips, employer-issued tax slips that report earnings (the analog to W-2s in the US).

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

[15] It also excludes prescriptions received through the BC Centre for Excellence in HIV/AIDS, BC Transplant Society, and BC Renal Agency.

2.2 Data Linkage and Sample Selection

Tax returns are linked to IA caseloads at one data access location, while health and education files are linked to IA caseloads at another. The tax data is not linked directly to the health and education data. Due to a government restriction imposed on the linkage between tax and IA records, we had to restrict the tax sample to all adults who received IA at some point between 1989 and 2001 (the year before the reform), and all children associated with those adults. We refer to this as the *restricted sample* throughout.[16] The link between health and education records to IA recipients contains the universe of residents, which we call the *full sample*. To maintain a consistent sample between the tax data 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.

We exclude fathers because it is more difficult to link men to children and the links that can be made are connected to IA receipt. Appendix I.1 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 part of the identification strategy described in Section 3, we restrict the sample to mothers (age 20 to 60) whose youngest child is between ages 4 and 11. 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. When studying children's outcomes, we restrict to those age 8 to 15, again due to the nature of the identification strategy.

2.3 Descriptive Statistics and Patterns of Income Assistance Use

Table 1 shows descriptive statistics from 1998-2001 for mothers and children in the full sample, the restricted sample, and among IA recipients. Illustrating the widespread nature of IA in the 1990s, 32% ($\frac{246297}{729158}$) of the full sample received IA at least once between 1989 and 2001. Single parents are 52% of the restricted sample and 71% of IA recipients.

Mothers in the restricted sample are less healthy than those in the full sample, and IA recipients

[16] The restricted sample excludes mothers who received DA prior to 2002, since these benefits are not conditional on job search requirements. We do not restrict the sample based on DA receipt after the reform.

are the least healthy. A similar, albeit more muted, gradient is evident in children. Children have substantially fewer interactions with the healthcare system in general: total healthcare spending per child is one-third of the adult amount.

Unsurprisingly, IA recipients have substantially lower labor market attachment. Their average employment income is $3,300 compared to $9,600 in the restricted sample. Figure III.2 shows that IA benefits constitute 48.9% of total annual income among IA recipients. The next largest source is child tax benefits (23.8%), followed by market income (21.4%).[17]

3 Identification and Estimation for Contemporary Outcomes

Our objective is to estimate the causal impact of access to welfare on labor market and health 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.[18]

3.1 First Stage: Impacts on IA Use

To illustrate our identifying variation, in panel (a) of Figure 2 we plot the fraction of mothers who receive IA by the age of their youngest child, for each year between 1998 and 2006. The figure shows two patterns. First, receipt declines as the youngest child ages, from 47.5% at age 4 to 36.6% at age 11 in 1998. This decline is also evident in the full sample of mothers (see Figure III.3). It reflects a dynamic in which mothers enter IA around the time they give birth, then slowly exit as their children age (see Figure III.4).[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, resulting in a flattening of the age gradient. In 1998, a mother with a youngest child of age 4 was 10 percentage points more likely to receive IA than one whose youngest was

[17] Some mothers transition between IA and employment during the year, causing both market income and IA receipt to appear on tax forms. Also, during this period, IA recipients could receive small amounts of earnings while on IA without full claw back.

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

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

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

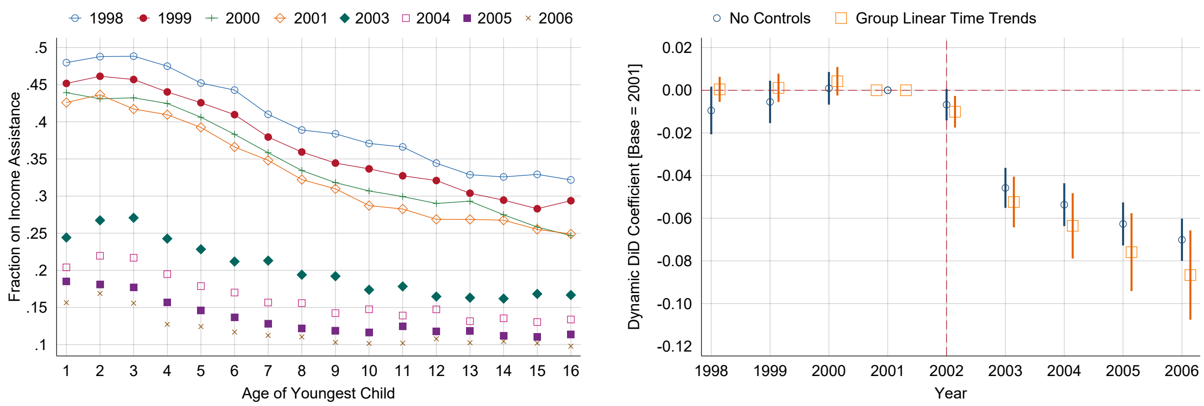
Health Data Adults	Full Sample		Restricted Sample		On IA	
	Mean	SD	Mean	SD	Mean	SD
On IA In Year	0.13	0.33	0.38	0.48	1	0
Months IA Received in Year	1.09	3.17	3.22	4.78	8.56	3.88
IA Benefit Amounts in Year	924	2820	2735	4311	7265	4056
Age	37.74	6	34.92	6.28	34	6.55
Number Kids	2.13	1	2.13	1.12	2.06	1.09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

Note: This table shows means and standard deviations of key variables from 1998 to 2001 for mothers aged 20 to 60 whose youngest child is between 4 and 6 or 8 and 11 years old. 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. The sample of children are those aged 8 to 15 who are in a household whose youngest child is 4 to 6 or 8 to 11 years old. Dollar amounts are rounded to the nearest $100 following the disclosure rules, shown in 2002 CAD, and winsorized at the 99th percentile.

11. By 2006, that difference was only 2 percentage points[20]

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

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



Note: Panel (a) plots the fraction of mothers that received IA for each calendar year and age of youngest child 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 are mothers with youngest child age 8 to 11. The two specifications shown in panel (b) are described in Section 3. Standard errors are clustered at the individual level.

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. 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 0 to 2 because they did not become subject to search requirements. We exclude families with youngest children older than 11 because families with older children are, ex-ante, less comparable to the treatment group.[22]

[20] A similar initial gradient followed by a flattening during the 1990s US reform is shown in Kleven (2023) 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.

[22] We also drop the very small fraction of mothers that received disability assistance before the 2002 reform. Work search requirements did not apply to this group.

Figure 3 illustrates the construction of the treatment and control groups. As the youngest child ages, the family transitions from treatment to control. This means that starting in 2004, the control group contains families that were previously subject to the work search requirements imposed in 2002 (the "treatment"). This could contaminate the control group if treatment effects persist over time. In Section 5.4, we present evidence suggesting that this is not a concern.

This identification strategy assumes that the outcomes of treated families would have evolved parallel to those of control families in the absence of the reform. As we will see, parallel pre-reform trends in outcomes support this assumption. Other contemporary policy changes were small in comparison to welfare reform, did not hinge on the age of the youngest child, and were likely to affect the treatment and control similarly (see the footnote).[23]

Our first-stage estimating equation is:

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

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

In panel (b) of Figure 2, 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. The trend prior to the reform is almost flat. We find similarly parallel pre-trends when using the amount of IA benefits or the number of months of benefits as the dependent

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

Figure 3: **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 the solid outline and the treatment group by the dashed outline. Families where the youngest child is neither 8-11 nor 4-6 years old are excluded from the analysis.

variables (see Figure III.5). We also show results using the de-trended data. After the reform, the treated group shows a relative decline in IA receipt of approximately 5 percentage points.

Table 2 presents estimates of π from equation 1 using three measures of IA use. The results including group-specific linear trends in columns (2), (4), and (6) show that extensive margin IA receipt fell by 7.2 percentage points, months of benefit receipt fell by 0.86, and the annual dollar amount by \$769.50 (in 2002 dollars). The change in dollar amount is an average over the whole treatment group (including mothers who did not change their IA status) and the extensive margin effect is the proportion who changed IA receipt because of the reform. Hence, the estimate of lost IA income for the complier group is $\frac{769.50}{0.072}$ = \$10,687.5. This is equivalent to 1,335 hours of work, or 38 weeks of full-time work, at the provincial minimum wage at the time (\$8/hour). The same calculation for months of receipt implies that compliers lost $\frac{0.858}{0.072} \approx 12$ months of benefits.

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

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

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

Based on the finding that complier mothers would have received IA for 12 months of the year in the absence of reform, we can approximately characterize this group by examining the characteristics of treatment group mothers who received full-time IA before the reform. In Table 3, we present average employment and income among pre-reform observations in which the mother was in the treatment group and received 12 months of IA. Among this group, the average annual benefit amount was \$11,100, which is unsurprisingly similar to the \$10,687 loss of benefits estimated from the first stage. This \$11,100 represented 68% of own total after-tax income ($\frac{\$11,100}{\$16,400}$), while the remaining 32% came from child-related tax benefits and employment income. Of these full-time IA recipients, 22.6% received employment income which amounted to a yearly average of \$3,274.33 ($\frac{740}{0.226}$).[24] So while some mothers supplemented IA income with small amounts of formal work, the majority (77.4%) did not.

3.2 Second Stage: Impacts on Contemporary Outcomes

Normalization of Outcomes: Many of our outcomes ($Y_{i,t}$) are non-negative values that include many zeros, such as health spending or employment income. We normalize these variables by the pre-reform average of the group (treatment or control) to which person i belongs in year t such that the regression outcomes, denoted $\tilde{Y}_{i,t}$, are $\frac{Y_{i,t}}{\bar{Y}_{D=1,2001}}$ and $\frac{Y_{i,t}}{\bar{Y}_{D=0,2001}}$ for treatment and control group observations, respectively. In our difference in difference framework, the treatment effects will be

[24]Before 2002, there was a \$200 monthly earnings exemption for IA recipients. Above that amount, benefits were clawed back by 75 cents for each dollar earned.

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

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

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

percent changes over the group mean. This normalization approach also approximates treatment effects from exponential regression (*e.g.*, Poisson).[25]

This normalization follows one of the suggestions made by Roth and Chen (2023) and is economically meaningful in our context. To illustrate, consider the example of employment income. Our treatment group is mothers with younger children, which means they have lower employment participation rates than the control group. If wages are generally trending upwards for both groups, this will cause non-parallel trends in employment income when expressed in *levels* because of different baseline employment rates. But by dividing by $\bar{Y}_{D,2001}$, we are controlling for differential employment rates, thereby removing the non-parallel trends caused by aggregate wage trends. For binary variables, such as extensive margin employment, we do not normalize, so $\tilde{Y}_{i,t} = Y_{i,t}$.

We first present reduced form event study specifications given by:

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

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

[25]The parallel trends assumption in exponential difference in difference is $\frac{E[Y(0)_{i,t}|D_{i,t}=1, Post_t=1]}{E[Y(0)_{i,t}|D_{i,t}=1, Post_t=0]} = \frac{E[Y(0)_{i,t}|D_{i,t}=0, Post_t=1]}{E[Y(0)_{i,t}|D_{i,t}=0, Post_t=0]}$.

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

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

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

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

4 Labor Market Outcomes

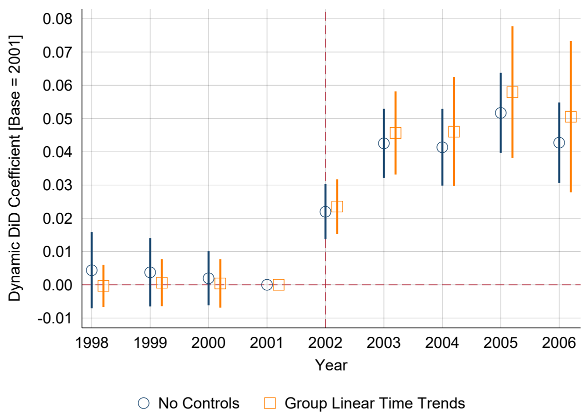
Estimates of γ_s and β are shown in Figure 4 and Table 4, respectively. As shown in panel (a) of Figure 4, there is a sizable increase in the employment rate starting in 2002 among treated mothers. The corresponding estimate of β indicates that 63% of mothers in the complier group entered employment.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

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

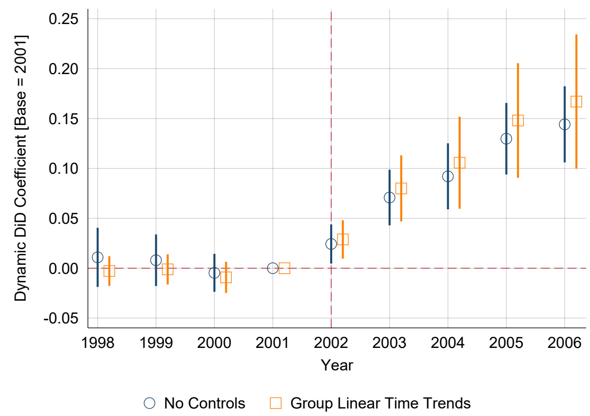
By 2004, mothers' earnings in the treatment group, relative to their 2001 average, had grown

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

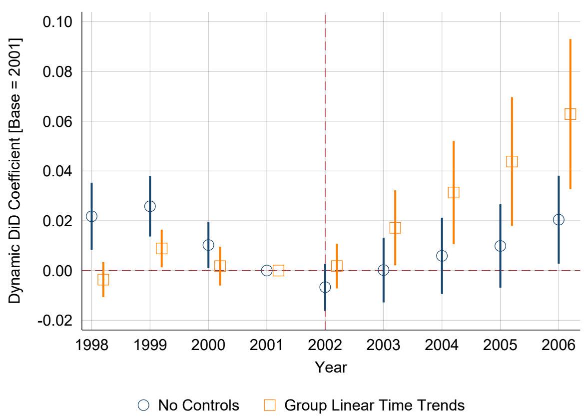
(a) **Extensive Margin Employment**



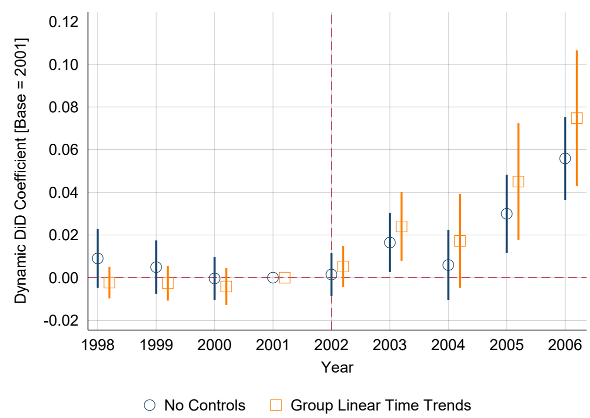
(b) **Individual Employment Income**



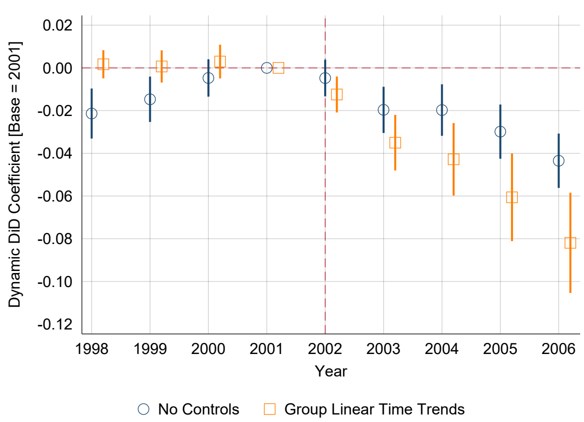
(c) **Individual After-Tax Income**



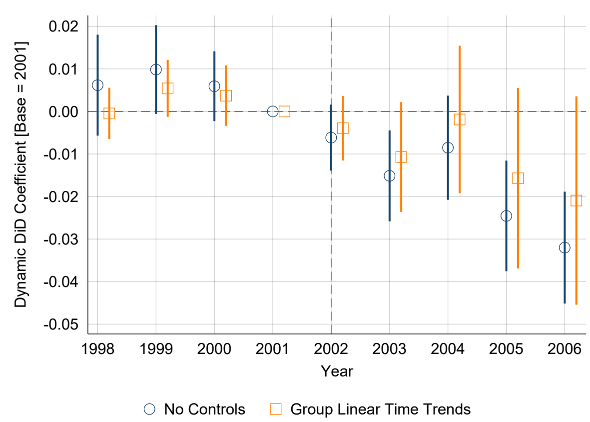
(d) **Family After-Tax Income**



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



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



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

by 10% more than in the control group. To translate the effects on $\frac{Y_{i,t}}{\bar{Y}_{D,2001}}$ into dollar effects, we multiply the estimated β by $\bar{Y}_{D=1,2001}$, as shown in Table 4 for the IV estimates. We estimate that losing IA increases earnings by \$13,726. If the \$13,726 increase came solely from the 63% of mothers that went from zero to positive earnings, then the implied annual earnings of compliers would be \$21,787 ($\frac{\$13{,}726}{0.63}$). To put this in context, at 35 hours a week for 50 weeks, annual earnings of \$21,787 imply an hourly wage of \$12.45. In 2002, the average wage among women in BC was \$16.87 and the minimum wage was \$8.00.[26]

The positive effect on individual after-tax income in Figure 4 suggests that the increased earnings, on average, were more than sufficient to offset the loss of transfer benefits.[27] The IV estimates in Table 4 indicate that losing IA caused individual after-tax income to increase by \$7,802: they lost \$10,687 in IA transfers, gained \$13,726 in employment earnings, and gained \$4,763 (\$7,802 +\$10,687 - \$13,726) in other income (some of which is self-employment income).[28]

The effect on *family* after-tax income is substantially larger than the effect on individual after-tax income: \$15,635 versus \$7,802. Some of this difference comes from an increase in the likelihood of being linked to a spouse in the tax records. Table 4 suggests 18% of complier mothers pushed off IA began reporting a spouse, which increases recorded family income. Distinguishing household formation from reporting changes is difficult because spouses are linked together on tax returns on the basis of self-reporting.[29] So we view the effect on *individual* after-tax income as a lower bound and the effect on *family* after-tax income as an upper bound on the real change in income of the mother's household.

Childcare costs may have offset the income gains of working. In the early 2000s, half-day

[26]We calculate average hourly wages from the Labour Force Survey (Statistics Canada, 2002).

[27]The very small difference in observations between the employment income and after-income regressions is due to tax non-filing. We see after-tax income only if the mother files a T1 tax return, but see employment irrespective of tax filing from employer-issued T4 slips. Mothers are strongly incentivized to file a T1 to get tax benefits, so non-filing is very rare.

[28]We do not observe "under-the-table" income that is not reported by an employer or the mother. In the US, Edin and Lein (1997) interviewed 379 AFDC recipients before welfare reform, and found that 32% to 52% of single mothers had some unreported earnings, and between 69% and 91% received some cash assistance from a family member or boyfriend. How subsequent welfare reform affected those amounts or reporting of earned income is unknown.

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

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

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

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

kindergarten in public schools started at age 5, and as a result, some treated mothers moving into work would require full or after-school childcare. The $7,802 change in individual after-tax income is similar to the cost of formal childcare for a single pre-school-age child in Vancouver.[30] In Appendix III.2, we show that mothers were 20-40 percentage points more likely to claim tax credits for childcare costs after the reform.

4.1 Heterogeneity

To illustrate the heterogeneity underlying the average effects, we estimate equations 2 and 3 while defining the outcome variable $\tilde{Y}_{i,t}$ as an indicator for income exceeding a specific threshold: $\mathbb{1}\{I_{i,t} > \bar{I}\}$. For example, we examine the impact of the reform on the likelihood that a mother has earnings greater than $25,000.[31] In Figure 5, we plot reduced form estimates for thresholds ranging from $2,000 to $50,000 and show IV estimates in Table 5.

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

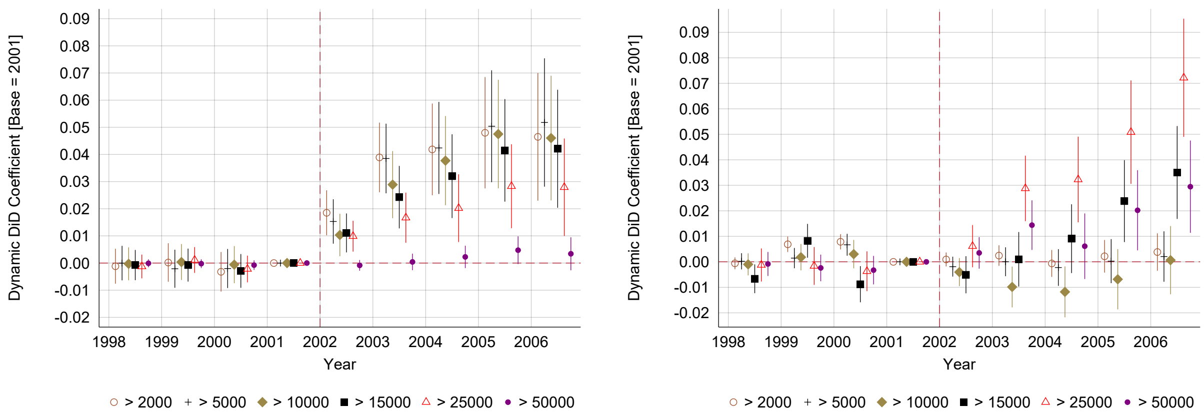
[30] An estimate of median costs of full-time care for a single pre-school-age child in Vancouver in 2019 is $954 per month (MacDonald and Friendly, 2019) – $696 in 2002 dollars, or $8,352 per year.

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

treatment effects grow smaller as the threshold increases, illustrating the substantial heterogeneity in employment outcomes among complier mothers.

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

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



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

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

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

Note: This table shows estimates of β from equation 3 from the specification that allows for differing linear time trends between treated and control (described in Section 3.2) for outcome variables of the form $\mathbb{1}\{I_{i,t} > \bar{I}\}$, where $I_{i,t}$ is income and $\bar{I}$ is some cutoff (displayed in the first column). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. Dollar amounts are in 2002 CAD.

Panel (b) of Figure 5 shows results from the same exercise for after-tax income. The IV estimates in Table 5 indicate that 9% of compliers saw their reported family after-tax income drop below \$10,000. Recall that complier mothers received about \$10,687 in annual benefits (counterfactually). By contrast, about 24% of complier mothers saw their family after-tax income go above $15,000 and 58% saw their income move above $25,000.[32] So while the majority of mothers saw notable increases in income due to employment responses, a minority saw declines that pushed them into deeper poverty.

4.2 Comparisons to US Literature

In a review, Ziliak (2015) reports that welfare reform increased the employment rate of single mothers by 1 to 7 percentage points.[33] This wide range stems from differences in empirical approaches across papers, as well as from the challenge in the US literature of isolating 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; Kleven, 2023; Meyer and Rosenbaum, 2001; Snarr, 2013).[34] Our setting does not face these identification challenges.

Estimated effects on earnings and income are also ambiguous. Both Moffitt (1999) and Schoeni and Blank (2000) report that pre-PROWRA state waivers increased earnings. However, Moffitt (1999) finds effects concentrated among high school graduates, whereas Schoeni and Blank (2000) report effects driven by high school drop-outs. Moreover, Moffitt (1999) finds no statistically significant effect on family income while Schoeni and Blank (2000) report a large positive impact.[35] Importantly, these studies rely on survey data that substantially misreports program participation, and to a lesser extent, earnings (Ziliak, 2015; Meyer and Mittag, 2019). The ambiguity may also reflect treatment effect heterogeneity. Bitler, Gelbach and Hoynes (2006) demonstrates very heterogeneous treatment effects from Connecticut's Job-First program, similar to our analysis.

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

[33] This is the range of estimates across studies. It does not factor in the confidence intervals of those estimates.

[34] 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).

[35] Moffitt (1999) provides results for women irrespective of their education, as well as separately by education level. Among low-educated women, he finds a statistically insignificant impact on earnings. In all samples, he finds no impact on family income. Methodological differences may explain the differing results of Schoeni and Blank (2000) and Moffitt (1999). E.g., Moffitt (1999) measures earnings and income in levels, whereas Schoeni and Blank (2000) use the logarithm. Schoeni and Blank (2000) also analyzed PRWORA in 1996, and find that it decreased welfare use among women with a high school degree or less, with no impact on their labor market outcomes.

We can calculate implied IV estimates as the ratio of labor market effects over welfare participation effects reported in Moffitt (1999) and Schoeni and Blank (2000)'s analyses of state waivers. Moffitt (1999)'s results suggest that annual earnings increased by \$34,287.50 ($\frac{274.30}{0.008}$) among women who lost welfare due to state waivers. Among low-educated women, Schoeni and Blank (2000) find effects consistent with a 229 percentage point ($\frac{0.0197}{0.0086}$) increase in employment, a 585% ($\frac{0.0503}{0.0086}$) increase in own earnings, and a 706% ($\frac{0.0607}{0.0086}$) increase in family income. The large effects may reflect confounding from the macroeconomic or EITC expansions.

5 Health Outcomes

There are multiple channels through which welfare reform could affect health outcomes, mostly centered around the shift to employment. Increased disposable income may directly affect health. The shift to employment may also elevate the risk of injury and illness while on the job, magnify mental stress, and decrease the time available for preventative health care. Potential mechanisms for children are similar, including, again, increased household income. Increased parental employment may also reduce parental time with and supervision of children, which could increase injuries due to unsupervised play, and reduce the time available to take children to health care professionals. Changes in parental stress could spill over to children's well-being. We cannot fully separate these mechanisms, but we can examine effects across categories of health care that are more strongly associated with specific mechanisms, such as treatments for injuries, mental health, primary care visits, and preventive care such as pap smear screenings.

In the US, there is an additional channel: welfare reform changed health insurance coverage as some mothers transitioned from Medicaid to being uninsured or having employer-sponsored insurance. This is not true in Canada, with the exception of pharmaceuticals: welfare recipients received full coverage while low-income non-recipients did not. To illustrate the importance of the insurance channel, we estimate effects separately for pharmaceutical use.

5.1 Adults

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

For universally insured hospital and outpatient spending, which make up 80% of health costs, the estimated effect is -0.06 (or -\$38). We can rule out effects greater than 0.37 (or \$235) and smaller than -0.49 (or -\$312) with 90% confidence. To put that into perspective, the average cost of a hospital inpatient visit, an emergency room visit, and a family physician examination were \$4000, \$220, and \$59, respectively. Put differently, every dollar of IA is associated with a 0.35 cent decrease in health costs ($\frac{38}{10{,}687}$), with bounds of 2.2 cents ($\frac{235}{10{,}687}$) and -2.9 cents ($\frac{-312}{10{,}687}$).

Examining subcategories of health care allows us to investigate whether the zero effect on total health costs reflects offsetting effects. Panels (b) through (f) plot reduced form effects on total health costs in two diagnosis categories – mental health and physical injury[36] – along with the number of visits to emergency rooms, the number of visits to general practitioners, and whether the mother received a pap smear.[37] Like total health costs, these subcategories exhibit parallel pre-trends for the control and treatment groups, with the potential exception of mental health spending. The two variables with the most statistical significance (see Table 6) are general practitioner (GP) visits and whether the mother received a pap smear. Pap smears are among the most common forms of preventative health care for women of this age group — guidelines recommend pap smear screenings every three years — and so are a good indicator of whether gaining access to IA allows mothers more time for preventative care. GP visits may also encompass preventive care, but are,

[36] Physical injuries are ICD9 diagnostic codes ranging from 800 to 900 (injuries and poisonings). Mental health treatment is ICD9 codes 290 to 319.

[37] GP visits include services provided by family physicians – consultations, examinations, counseling, minor procedures, etc. – in their office or at the patient's home. We identify pap smears in the MSP records.

of course, a much coarser measure. The IV estimates in Table 6 suggest that access to IA increases the likelihood of receiving a pap smear by 16 percentage points (significant at the 5% level), and increases the number of GP visits by 2.22 (0.35×6.33, significant at the 10% level). These outcomes aside, there is limited evidence of major effects on other health categories (injury, mental health, emergency room visits) but in the case of mental health spending, the non-parallel trends may obscure important causal effects.

The primary category of non-universally insured medical spending is pharmaceuticals. Drugs for IA recipients are fully paid for by the government, but non-IA recipients receive only partial means-tested coverage (see Section 1). Panel (a) of Figure 7 shows the reduced form effect of IA reform on the fraction of mothers' pharmaceutical costs paid out-of-pocket (conditional on filling a prescription). Immediately after the reform, the out-of-pocket share rose sharply in the treatment group, consistent with the loss of IA and thus full drug coverage. Table 6 indicates that losing IA is associated with an 84 percentage point increase in the out-of-pocket share. This likely overstates the true effect, since we do not observe private supplemental insurance plans which some mothers may receive when transitioning to employment. Nonetheless, this net-of-subsidy drug price increase is predicted to lower pharmaceutical consumption, all else equal.[38]

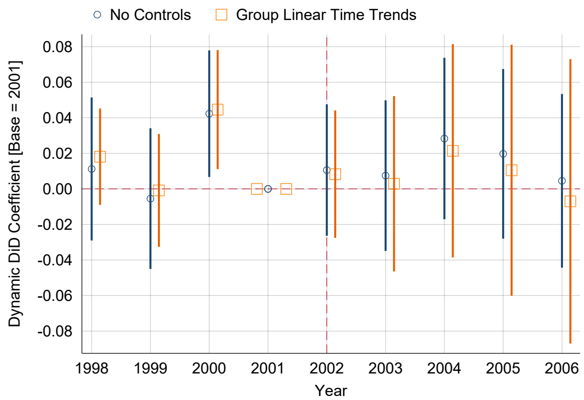
We examine this prediction in the remaining panels of Figure 7. On the extensive margin, there is a clear reduction in pharmaceutical use for mothers (panel (b)). The estimate in Table 6 suggests that losing IA causes mothers to be 33 percentage points less likely to fill a pharmaceutical prescription during the year. Effects on total drug consumption (combining intensive and extensive margins) are more mixed. The estimate in Table 6 for total days supplied of pharmaceuticals (summed across all drug types) is the correct sign, but sensitive to the inclusion of trend controls. This suggests that the extensive margin effect is driven by mothers who would counterfactually have only small pharmaceutical consumption.

The effect across all drug types masks substantial heterogeneity. As shown in panels (c) and (d), mothers who lose access to IA *increase* their consumption of pharmaceuticals associated with

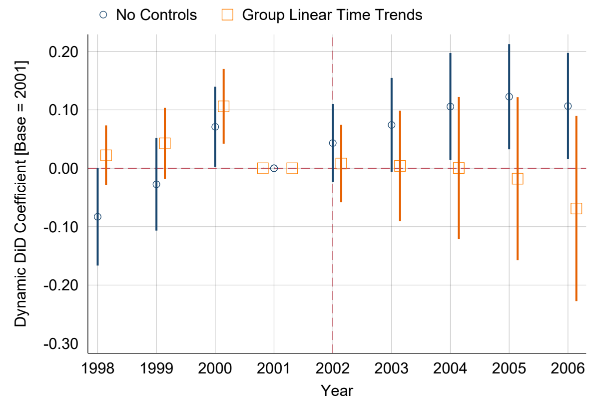
[38] In a meta-analysis, Goldman, Joyce and Zheng (2007) find drug elasticities typically in the range of -0.20 to -0.60.

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

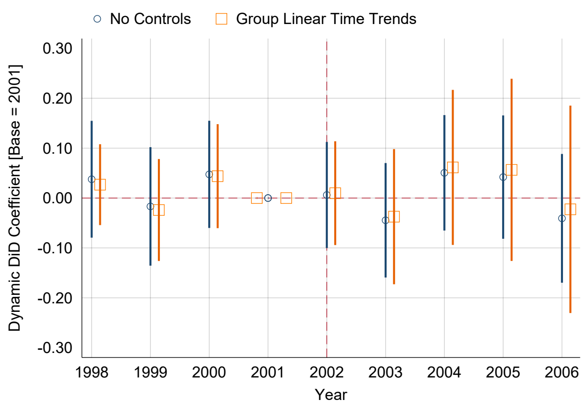
(a) **Total Outpatient and Hospital Costs**



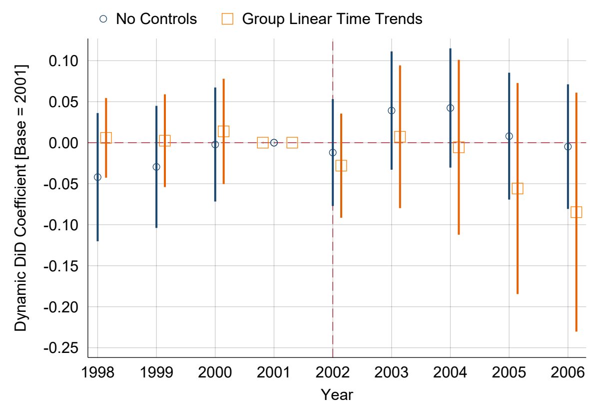
(b) **Mental Health Costs**



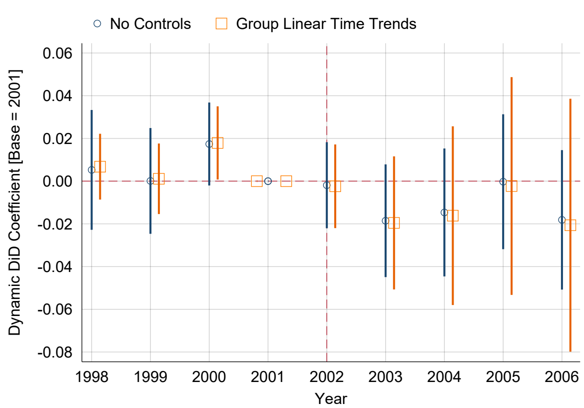
(c) **Injury Costs**



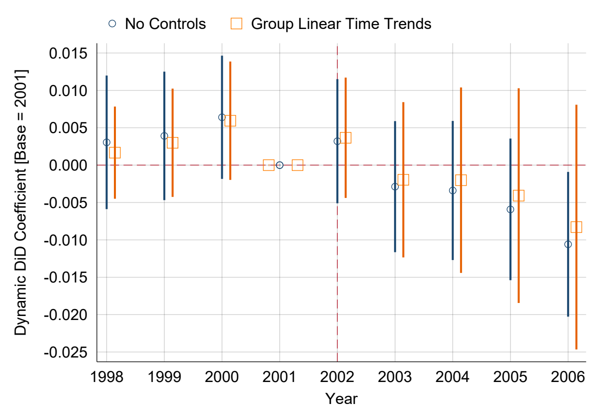
(d) **Number of Emergency Room Visits**



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



(f) **Received Pap Smear**



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

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

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

Note: This table shows estimates of β from equation 3 and standard errors (SE) which are clustered at the individual level. The two specifications are described in Section 3.2. We exclude the year 2002 since this was a partial treatment year. Dollar amounts are in 2002 CAD. The last column shows the outcome mean (not normalized) for the treatment group in 2001.

mental health treatment.[39] The estimates in Table 6 indicate that losing access to IA because of

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

welfare reform increased days supplied of pharmaceuticals related to mental health by 94 (2.25×41.86) – about three months of treatment. This is potentially consistent with the effects of non-pharmaceutical spending on mental health shown in Figure 6. Taken together, these results provide suggestive evidence that welfare reform increased stress among mothers and therefore treatment for mental health issues, despite the increase in out-of-pocket drug prices. In contrast, the estimates in Table 6 suggest that mothers who lost access to IA were up to 38 percentage points less likely to receive pharmaceutical contraceptives (AHFS4 code 68:12). It's unsurprising that chemical contraceptive use declines when the price rises, given the prevalence of non-chemical alternatives.

5.2 Children

Reduced form effects on children's health outcomes are shown in Figure 8 with corresponding IV estimates in panel B of Table 6. As with mothers, we can rule out modest effects on universally insured health costs for children: the treatment effect is 0.22 (or \$41) with the 90% confidence interval bounded by -0.67 (-\$97) and 1.11 (\$179). We also fail to find effects on treatment for injuries and mental health, ER visits, and GP visits. Unlike for mothers, we also do not find compelling evidence that pharmaceutical use declines for children.

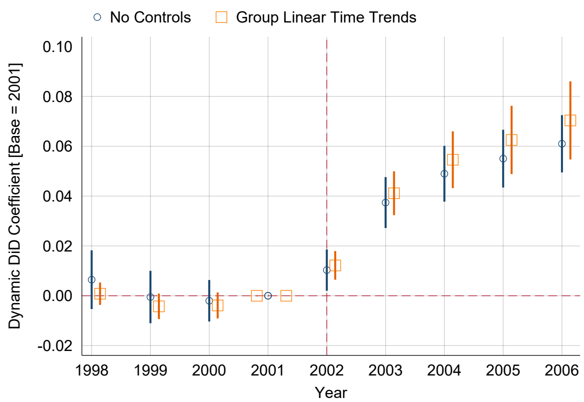
5.3 Taking Stock

The health effects demonstrated in this section are consistent with the rise in employment (documented in Section 4) causing mothers to have less time for preventative care and increased stress. This is perhaps unsurprising since many of the mothers in our sample are single parents facing the pressures of child-rearing and employment. The reduction of pharmaceutical contraceptives is consistent with high price sensitivity found in other contexts (Bailey et al., 2023). Overall, however, the spillover effects of welfare reform on aggregate government health spending were minimal. In Appendix III.4, we show that for every dollar of welfare cuts, governments gained 17 cents in income tax revenue and lost only 0.4 cents from increased health costs.

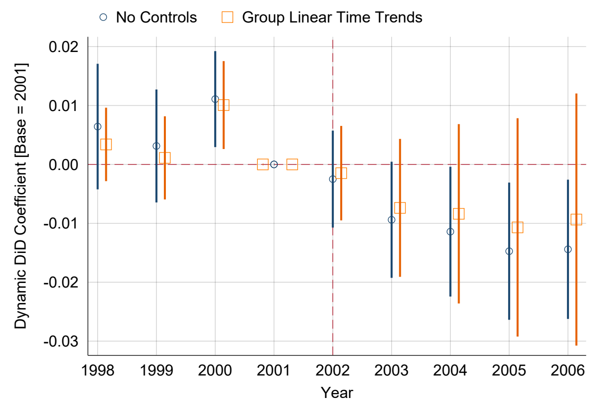
Our results are somewhat difficult to compare to those of the existing literature on adult outcomes. Using the Behavioral Risk Factor Surveillance System, Bitler, Gelbach and Hoynes (2005)

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

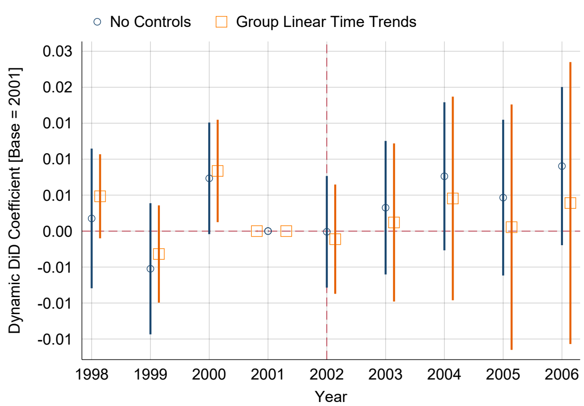
(a) **Fraction of Drug Costs Out-of-Pocket**



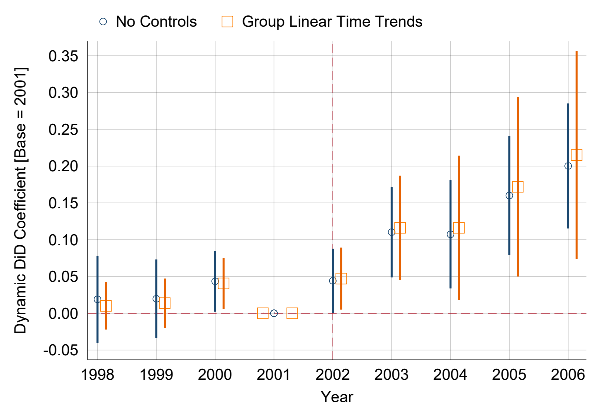
(b) **Received Any Pharmaceutical**



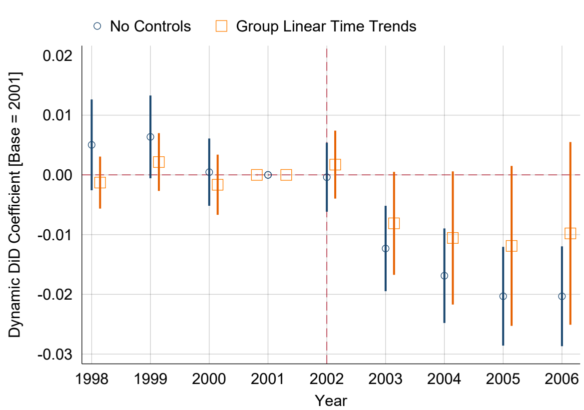
(c) **Received Mental Health Pharmaceutical**



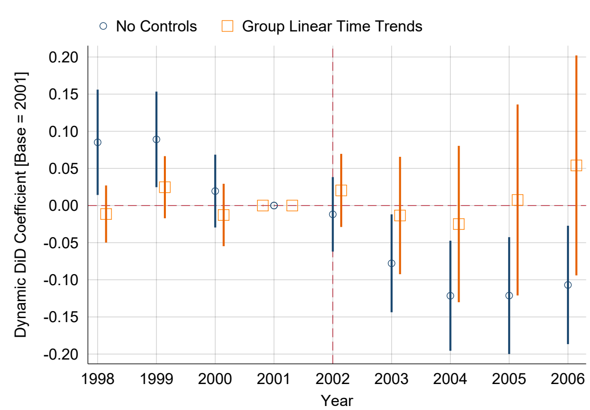
(d) **Days Supplied Mental Health**



(e) **Received Pharmaceutical Contraceptive**



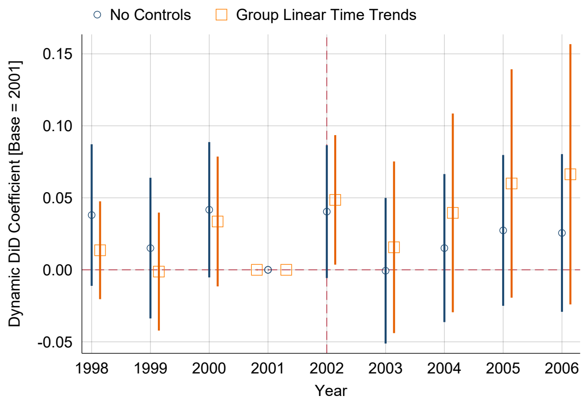
(f) **Days Supplied Contraceptive**



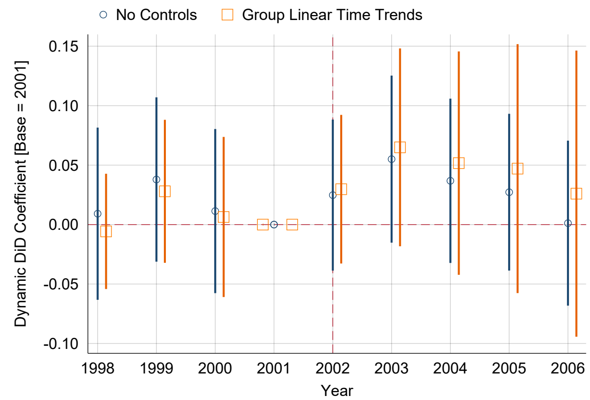
Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals for the sample of adults. The two specifications are described in Section 3.2. Outcomes are normalized as discussed in Section 3.2. Dollar outcomes are winsorized at the 99th percentile. Standard errors are clustered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

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

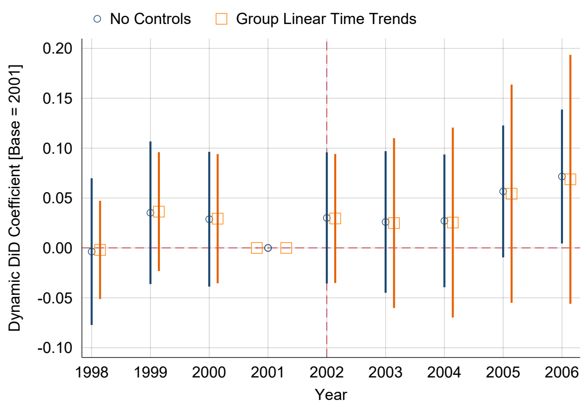
(a) **Total Outpatient and Hospital Costs**



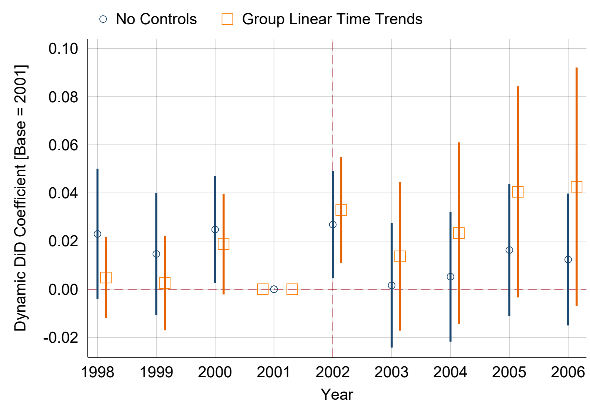
(b) **Injury Costs**



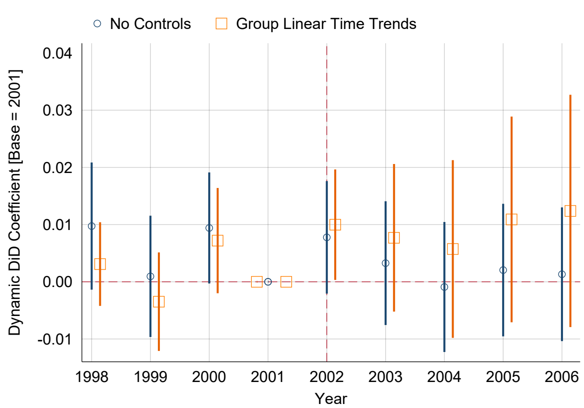
(c) **Number of Emergency Room Visits**



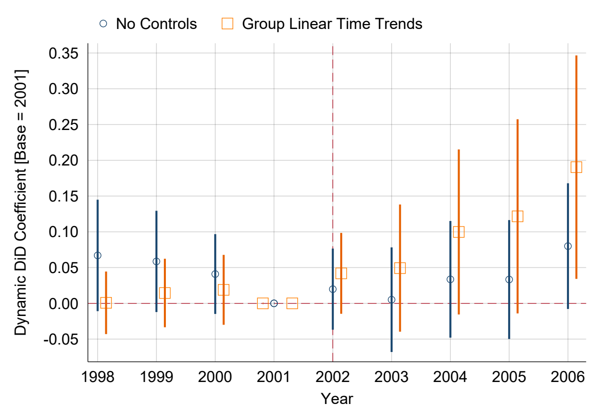
(d) **Number of General Practitioner Visits**



(e) **Received Any Pharmaceutical**



(f) **Pharmaceutical Days Supplied**



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

and Basu et al. (2016) find that welfare reform reduced health insurance coverage and increased the likelihood of needing care but finding it unaffordable, while Kaestner and Tarlov (2006) and Basu et al. (2016) find limited effects on self-reported mental and physical health. Basu et al. (2016) find increases in smoking and drinking, perhaps reflecting elevated stress, which could worsen long-term health. In subsequent work using the Survey of Income and Program Participation, Narain et al. (2017) found that welfare reform worsened self-reported health. [40] But, again, it is hard to disentangle the health insurance mechanism from the channels we investigate. A notable exception is Riddell (2020), who shows that the self-sufficiency project in Canada — which provided a strong financial incentive to single mothers to transition from welfare to employment — reduced feelings of maternal depression 4-5 years after the experiment, at least in part due to elevated employment. Cesarini et al. (2016) find that large income shocks generally have precise null effects on health care utilization in Sweden, in a health system comparable to Canada's.

For children, Gennetian et al. (2010) study the effect of the 1990s welfare-to-work experiments on children aged 3-5 at the time of randomization. The authors report small negative effects on parent-reported child general health when the welfare-to-work transition did not increase household income. They find no effect on child health in families where income increased. Our results are most comparable to the second set of families. Other studies focus on impacts in utero or among infants (e.g., Hoynes, Miller and Simon (2015), Hoynes, Schanzenbach and Almond (2016), Dench and Joyce (2020)), and little work has explicitly studied the effects of welfare, as opposed to other transfer programs (e.g., tax credits (Milligan and Stabile, 2011)). Cesarini et al. (2016) estimates mostly null impacts of lottery winnings on child health (birth outcomes, drug consumption), although they report increased all-cause hospitalization.

5.4 Robustness

Confounding Persistence of Treatment Effects: Treated mothers enter the control group when their youngest child ages from the treatment group (ages 4 to 6) to the control group (ages 8 to 11).

[40] Relatedly, Dobkin and Puller (2007); Riddell et al. (2006); Evans and Moore (2011) find spikes in mortality and morbidity for some transfer recipients in the days after cheque issuance, likely the effect of distributing benefits in concentrated bursts.

This contaminates the control group if treatment effects persist over time. For example, consider a mother with a youngest child of age 5 who exits IA in 2003 due to the reform, and suppose that causes an increase in health spending that persists for three years. When her child turns 8 in 2006, she will move to the control group, causing expenditures to rise in the control group, leading to downward-biased treatment effects. 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 samples in which we widen the age gap between treatment and control: dropping families with a youngest child age 7-8, 6-8, and 6-9. For example, when dropping observations with the youngest child age 6-9, persistent treatment effects only confound the control group if the health effect persists for 5 years. The estimates of β are stable as the age gap widens (shown in Figure III.7), which implies there are no persistent effects to raise concerns.

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

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

Fertility: Finally, we find no detectable treatment effect on fertility (see Figure III.9), indicating that our results are not biased by selective sample attrition driven by endogenous fertility.[41]

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

6 Children's Long-Run Outcomes

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

6.1 Estimating Long-Run Effects of Exposure

We adapt the empirical strategy presented in Section 3 to examine the effect of IA receipt between ages 8 and 15 on long-term outcomes. Our model of the relationship between IA receipt and long-run outcomes is:

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

Where $Y_{i,b}$ is the outcome of child i born in year b, $IA_{i,s}$ is an indicator for IA receipt when the child is age s, and ψ_b and u_i are birth cohort and individual effects. A more complicated model would allow the effect of $IA_{i,s}$ to differ for each s, but for tractability, we estimate a model where $\sum_{s=8}^{15} IA_{i,s}$ is the explanatory variable of interest.[42] We focus on ages 8 to 15 because the reform that we use for exogenous variation only provides identifying variation for children older than 7.

We categorize children into one of three groups based on the age of the *youngest* child in the

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

household: 1 to 3, 4 to 7, and older than 7. Indicators for the first two categories are D_{1-3} and D_{4-7} with the oldest category being the base group. The first stage equation for children of age s is:

$$IA_{i,s} = \pi_{1,s} D_{1-3,i,s} + \pi_{2,s} D_{4-7,i,s} + \pi_{3,s} D_{1-3,i,s} Post_s + \pi_{4,s} D_{4-7,i,s} Post_s + \pi_b + v_{i,s} \tag{5}$$

Where π_b are birth year effects and $Post_s$ indicates that the child was age s after the 2002 reform. The coefficients $\pi_{1,s}$ and $\pi_{2,s}$ represent level differences in IA based on the age of the youngest child in the household. The coefficients $\pi_{3,s}$ and $\pi_{4,s}$ represent changes in this age-of-youngest gradient following the reform. The relative decline among children in the treatment group ($D_{4-7,i,s}Post_s$) is the basis of our instrument. In Figure 9, we show event study versions of $\pi_{4,s}$ for each age s to demonstrate that parallel pre-trends hold and $\pi_{4,s} < 0$ for each age s.

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

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

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

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

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

Outcomes: Our first measure of educational performance is test scores in Languages and Mathematics at grade 10 (age 15-16), described in detail in Appendix I.3. For each child, we take the average percent score across domains and then standardize it to standard deviation one across students. Our second educational outcome is whether a child graduates from high school.[43] To measure intergenerational transmission, we construct a variable indicating whether the child received IA when they were 20 or 21 years old, separately from their parents. Our final outcome is whether the child was charged with a criminal offense (in the province) at age 20 or 21.

Sample: Our sample is children born between 1986 and 1998. The 1998 endpoint ensures that we observe all birth cohorts up to age 21. Our earliest birth cohort is 1986, the first year in which we observe family composition. The first birth cohort turns age 16 at the earliest in 2002 (i.e., after the reform), which is why we measure IA up to age 15. We restrict the sample to children enrolled in school at age 8 and age 14.[44] Our analysis period for the long-run outcomes goes as far back as 1994, overlapping considerably more with the years used to define the restricted sample (1989-2001). For this reason, our preferred results use the full sample (subject to the cohort and enrollment criteria), although we present estimates for the restricted sample as well.

6.2 Results

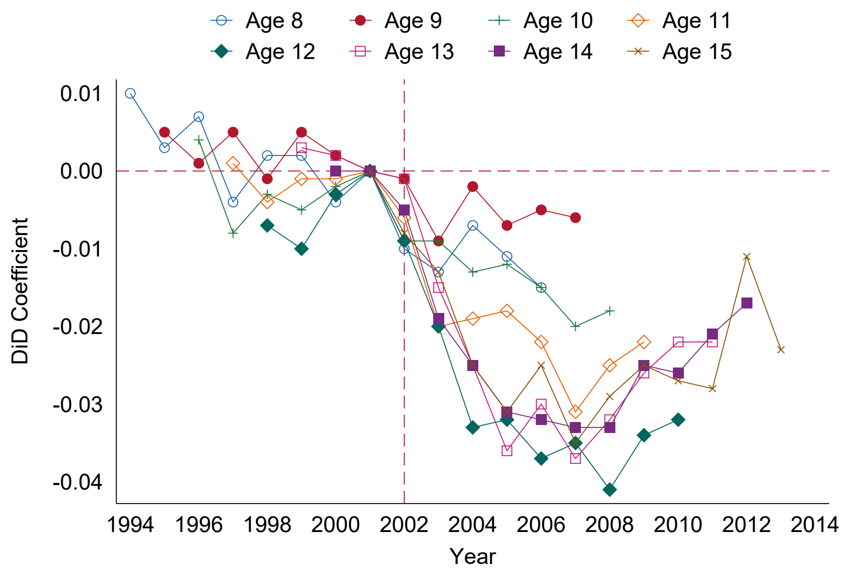
Table 7 shows OLS and 2SLS estimates of equation 7. Unsurprisingly, the OLS estimates indicate that children whose families receive IA have lower test scores, are less likely to graduate from secondary school, are more likely to receive IA as young adults, and are more likely to be charged with a criminal offense as a young adult. This is also evident in comparing the outcome means in the full sample and the restricted sample. For instance, children of mothers who received IA at some point between 1989 and 2001 had test scores 0.450 standard deviations below the full sample and were 18 percentage points less likely to graduate high school.

All of the effects on education outcomes statistically disappear in the 2SLS results. In panel A, column (2) indicates that an extra year of IA worsens test scores by a statistically insignificant 0.0051 standard deviations. We rule out effects greater than 0.09 and less than -0.10 with 90%

[43] The legal school-leaving age during our sample period was 16.

[44] This ensures the child was in the province during the exposure period (8-15). We condition on age 14, rather than 15, to avoid selecting on high school drop-out which sometimes occurs as early as age 15.

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



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

confidence. There are similarly null effects on high school graduation in panel B: the estimate in column (2) suggests that an extra year of IA lowers the probability of high school graduation by a statistically insignificant 0.4 percentage points or 0.5% ($\frac{0.004}{0.8}$).

Why is there so little effect on educational performance? It may be that disposable income did not increase enough to have an effect. Section 4 outlined how the increase in after-tax income when mothers were pushed off IA might have been offset by fixed costs of work. A zero effect on education might also mean that changes in the mother's time at home had little effect on children's schooling. This fits with Bastian and Lochner (2022) who show that EITC-induced increases in employment did not crowd out parental time spent on human capital enrichment with children.

Duncan, Morris and Rodrigues (2011) show that the 1990s welfare-to-work experiments typically affected child test scores only when the welfare-to-work transition elevated family income. Comparing estimates across studies, they estimate that a \$1,000 increase in income corresponds to a 0.05 to 0.06 standard deviation increase in scores.[45] Miller and Zhang (2009) find that US

[45] Dahl and Lochner (2012) and Bastian and Michelmore (2018) find similarly-sized effects of EITC expansions.

welfare reform increased test scores for low-income children, which is consistent with Duncan, Morris and Rodrigues (2011) if reform also increased family income. Simonsen, Skipper and Smith (2022) find that a Danish welfare reform that *reduced* disposable income had no short-term effect on test scores.[46] Our results contrast with these studies in that we find no effect on test scores despite elevated family income. However, we measure test scores at an older age: grade 10 versus, for example, grade 4 to 8 in Miller and Zhang (2009). Cognitive development may be more malleable in early childhood (Heckman, 2006), which could explain the contrast. On high school graduation, Miller and Zhang (2012) find that graduation rates increased in low-income areas, relative to high-income places, following US welfare reform. Similarly, Hartley, Lamarche and Ziliak (2022) find that a mother receiving AFDC/TANF during their daughter's teenage years decreases the probability that the daughter has *more than* a high school degree.[47]

Panel C shows 0 to 4 percentage point effects of teenage IA receipt on IA use at ages 20 and 21 ("intergenerational transmission"). Because we find no effects on education outcomes, the most plausible mechanism is that exposure to IA during teenage years influences later IA use through parental information transmission and role-modeling. This mechanism is emphasized by Hartley, Lamarche and Ziliak (2022) who report large intergenerational transmission in AFDC/TANF receipt from mothers to their daughters (building on work by Pepper (2000)).[48]

Panel D shows effects on the likelihood of being charged with a criminal offense at age 20 or 21. This is a very rare outcome, which lowers precision, but we find no evidence that welfare exposure during teenage years affects the likelihood of being charged in young adulthood. If criminal behavior also reflects a form of human capital investment from parents, this is consistent with the null effect on educational outcomes. Closely related work from Dahl and Gielen (2021) finds that reducing parental access to disability insurance in the Netherlands reduced children's serious crim-

[46] They did find negative effects on other survey-based measures of children's school experience.

[47] Similarly, Dave, Corman and Reichman (2012) find some evidence that US reform raised school *enrolment* for females aged 15 to 20, and Bastian and Michelmore (2018) find that family EITC receipt when the child is age 13 to 18 improves graduation.

[48] Hartley, Lamarche and Ziliak (2022) and Pepper (2000) measure daughters' participation when they are 19-27 and 24-33 years old, respectively, whereas we measure IA receipt at ages 20 and 21. Other examples from Norway (Dahl, Kostol and Mogstad, 2014) and the Netherlands (Dahl and Gielen, 2021) indicate that children are more likely to use disability insurance as adults if their parents received it when the child was a teen.

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

	Full Sample				Restricted Sample			
	OLS (1)	IV (2)	OLS (3)	IV (4)	OLS (5)	IV (6)	OLS (7)	IV (8)
Panel A: Grade 10 Test Scores								
$\sum_{s=8}^{15} IA_{i,s}$	-0.1572	-0.0051	-0.1574	0.0095	-0.0882	0.0151	-0.0894	0.1079
	(0.0012)	(0.0549)	(0.0012)	(0.0602)	(0.0014)	(0.0513)	(0.0014)	(0.0807)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		93.51		76.75		67.04		30.73
Outcome Mean	0.000	0.000	0.000	0.000	-0.450	-0.450	-0.450	-0.450
N	532059	532059	532059	532059	184346	184346	184346	184346
Panel B: High School Graduation								
$\sum_{s=8}^{15} IA_{i,s}$	-0.0671	-0.0040	-0.0672	-0.0128	-0.0396	0.0030	-0.0402	0.0482
	(0.0004)	(0.0202)	(0.0004)	(0.0216)	(0.0005)	(0.0201)	(0.0005)	(0.0317)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		106.93		87.32		80.06		36.97
Outcome Mean	0.802	0.802	0.802	0.802	0.620	0.620	0.620	0.620
N	577108	577108	577108	577108	199387	199387	199387	199387
Panel C: Received Income Assistance Between Ages 20 and 21								
$\sum_{s=8}^{15} IA_{i,s}$	0.0334	0.0452	0.0335	0.0343	0.0271	0.0282	0.0274	0.0013
	(0.0003)	(0.0111)	(0.0003)	(0.0117)	(0.0004)	(0.0129)	(0.0004)	(0.0189)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		106.93		87.32		80.06		36.97
Outcome Mean	0.060	0.060	0.060	0.060	0.127	0.127	0.127	0.127
N	577108	577108	577108	577108	199387	199387	199387	199387
Panel D: Charged With a Criminal Offense Between Ages 20 and 21								
$\sum_{s=8}^{15} IA_{i,s}$	0.0069	0.0040	0.0069	0.0050	0.0040	0.0002	0.0041	-0.0073
	(0.0002)	(0.0065)	(0.0002)	(0.0068)	(0.0002)	(0.0075)	(0.0002)	(0.0108)
Controls for $\sum_{s=1}^{7} IA_{i,s}$			✓	✓			✓	✓
First Stage F		106.93		87.32		80.06		36.97
Outcome Mean	0.018	0.018	0.018	0.018	0.036	0.036	0.036	0.036
N	577108	577108	577108	577108	199387	199387	199387	199387

Note: Shown are OLS and 2SLS estimates of β from equation 7. The controls for $\sum_{s=1}^{7} IA_{i,s}$ are described in Section 6.1. Standard errors in parentheses are clustered at the family level. The “full sample" is all children in BC. The “restricted sample" is children of mothers who received Income Assistance (IA) at least once before the 2002 reform. First Stage F is the Kleibergen-Paap F statistic for weak instruments.

inal offenses in adulthood, while Deshpande and Mueller-Smith (2022) find that denying disability insurance to *18 year olds* raises their chance of being charged and incarcerated in the US.

7 Discussion

We use rich administrative data to provide consistent estimates of the effects of reducing access to welfare on a wide range of outcomes for both mothers and children. Our estimates make use of a substantial welfare reform involving the extension of job search requirements to mothers of younger children. That reform (in 2002 in the Canadian province of British Columbia) was part of a wave of reforms that took place across North America. We find that the reform induced most of the affected mothers to move from welfare to employment, resulting in total income increasing on average – though, with an important minority suffering income losses. This transition coincided with reduced preventative care and increased mental health treatment for mothers, both consistent with the time pressures and stress of being a working parent. Despite this, mothers largely protected their children from declines in health and education outcomes. For example, mothers reduced GP visits for themselves but not for their children. Similarly, the increase in maternal time pressure did not affect teenagers' educational performance. Mothers' ability to act as buffers for their children is plausibly facilitated by Canada's extensive public health and education systems.

Our setting bears both similarities and advantageous differences compared to studies of US welfare reforms. In particular, our analysis is not complicated by EITC and macroeconomic expansions. Moreover, Canada's universal health care means that changes in welfare status do not entail changes in health insurance coverage, allowing for the identification of welfare access effects rather than a combination of insurance and welfare effects. In other dimensions, Canadian IA resembles TANF. Both provide support of last resort, conditional on having sufficiently low income and assets, with high benefit claw-backs for earned income, and work search requirements. As a result, we view our results as relevant outside of the Canadian context.

Finally, the reform we study reduced welfare access for relatively healthy and employable persons (Green et al., 2021) and our analysis focuses on families with a youngest child age 4 or older. The effects of welfare reform may differ for more vulnerable groups, for families with infant children, and for childless adults (which Hicks (2022*a*) examines in a companion paper).

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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 information from the Medical Services Program (MSP), BC's public health care insurance plan. 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 oldest person on the contract. We also use birth records, which include any child born in BC starting in 1985. 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. On the one hand, using birth records alone does not allow us to identify mother-child pairs coming from other provinces or other countries. On the other hand, using MSP data alone does not always yield a clear linkage for mother-child pairs where children are older or where mother and child are relatively close in age. 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 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. Over time, MSP registrants may change contract number; e.g., if the payer has changed. This may cause artificial breaks in our treatment variable and in our sample of children, possibly correlated with family economic status. To avoid this issue, after identifying the set of children associated with a mother, we assume that a mother-child pair lives together, as long as they are observed in the same postal code for the corresponding year.

I.2 Costing of Hospital-Based Services

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

I.3 Test Scores

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

II Work Search Requirements and Enforcement

As part of receiving Income Assistance, case workers often work with clients to provide counseling and referrals to employment and training programs.[49] 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.

The 2002 reform likely made the work search requirements more onerous. As reported by Klein and Pulkingham (2008):

> People on income assistance must complete "employability screens" and "client employability profiles," which are then used as a basis for caseworkers to develop an "employment plan" that sets out the client's work search, training and/or job placement obligations. These "agreements" must be signed by those on assistance, and failure to abide by the terms of one's plan is grounds for the suspension of benefits.

[49] Case workers were sometimes called Financial Assistance Workers.

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

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

BRITISH COLUMBIA | Ministry of Social Development and Poverty Reduction

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

WORK SEARCH ACTIVITIES RECORD

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

(ADD ADDITIONAL PAGES IF NECESSARY)

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

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

☐ OTHER (EXPLAIN) ______________________________

DECLARATION AND NOTIFICATION

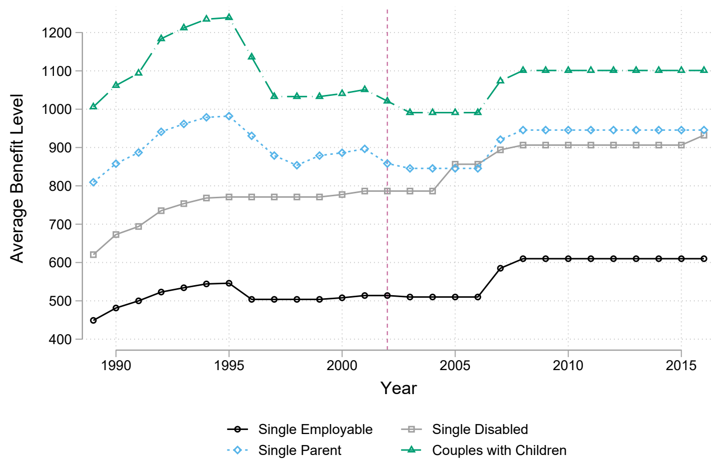
I declare that all the information I have provided in this form is true and complete. I understand the accuracy of the information I provide will be checked by comparing it against information held by other governments, private agencies and individuals. I understand that the BC government may verify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

SIGNATURE	PRINT NAME	DATE (YYYY MMM DD)

III Supplemental Results

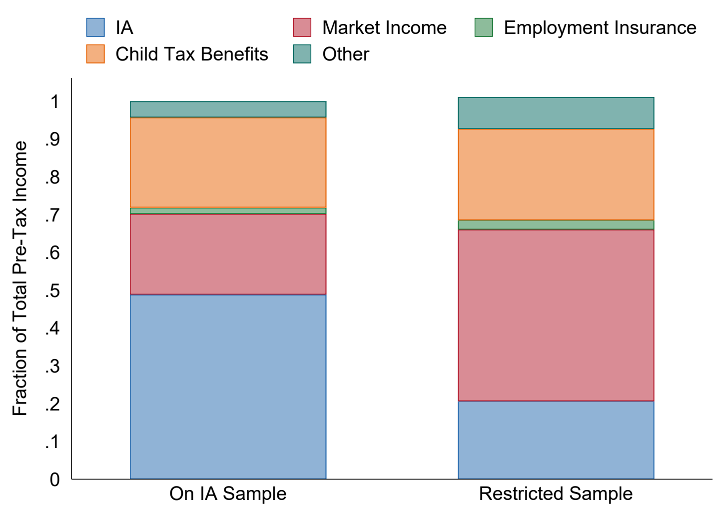
III.1 Income Assistance reform and income sources

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



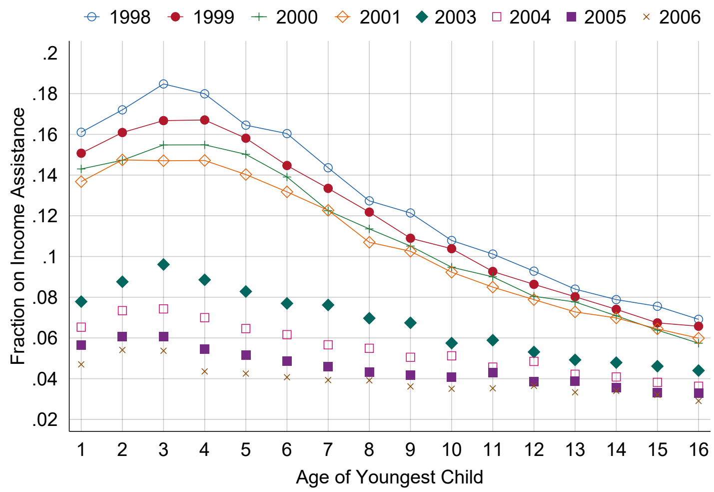
Note: This figure plots average benefit level amounts for different recipient groups: single employable adults; single adults with a disability designation; lone parents with one child, age 2; and couples with two children, ages 10 and 15. Source: National Council of Welfare (various years) and Caledon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

Figure III.2: **Income Composition**



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

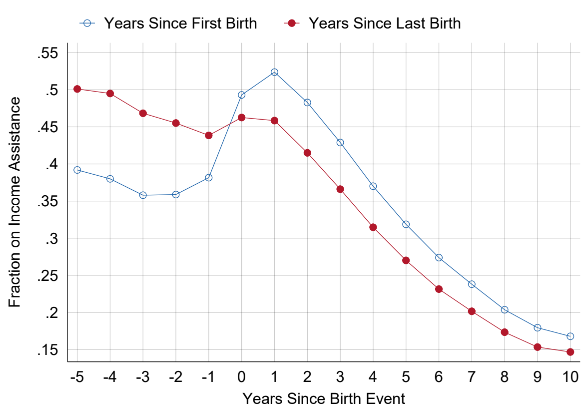
Figure III.3: **Age-of-Youngest Gradient Using the Full Sample**



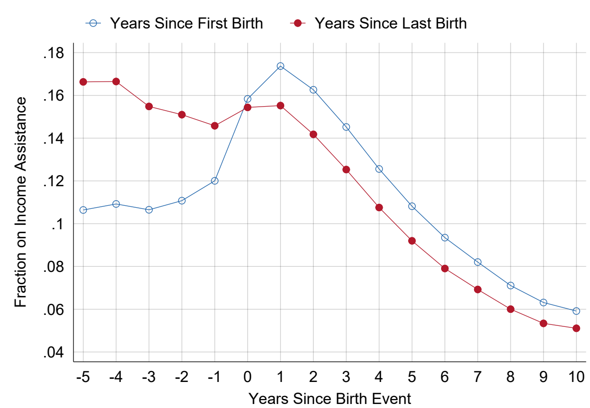
Note: This figure plots the fraction of mothers that received income assistance for each calendar year and age of youngest child in the family.

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

(a) **Restricted Sample**



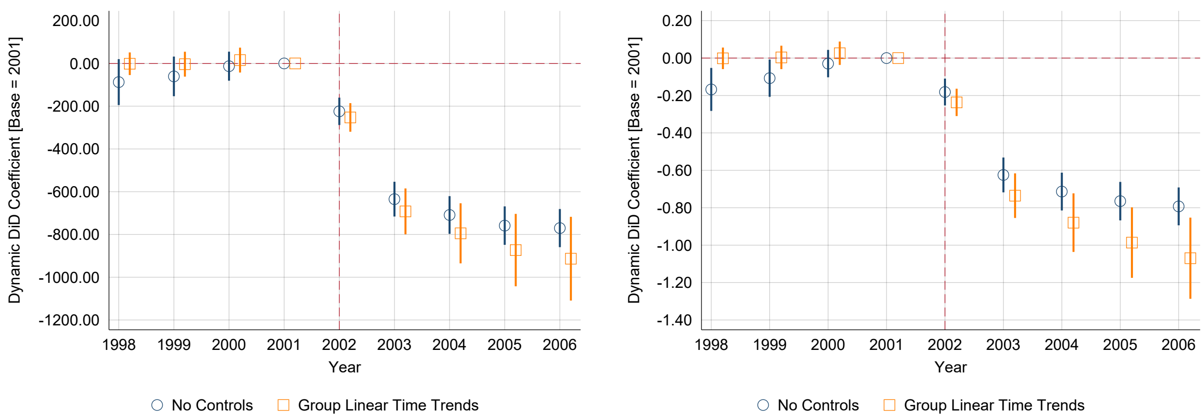
(b) **Full Sample**



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

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

(a) **Dollar Amount of Annual IA Receipt** (b) **Months of IA Received**



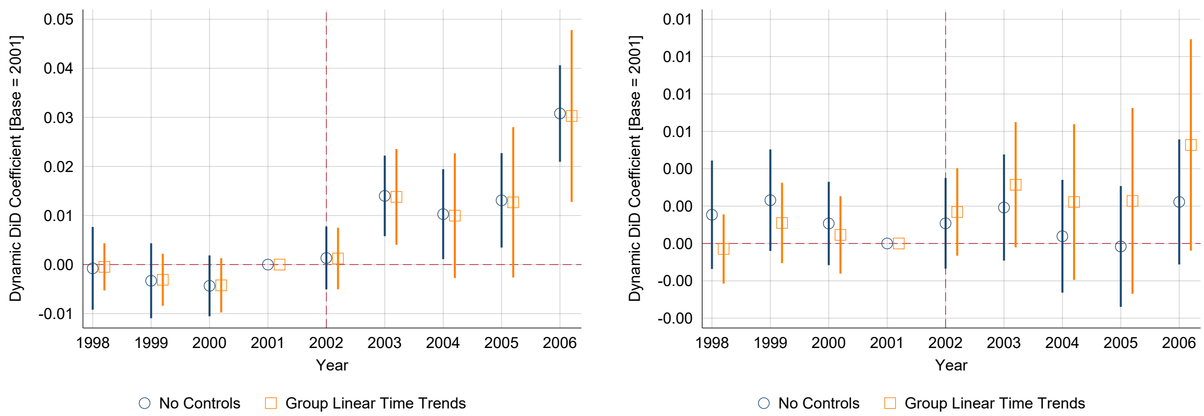
Note: Both panels plot estimates of π from a dynamic version of equation 1 and 95% confidence intervals. The treated group are mothers with youngest child age 4 to 6 and the control group, mothers with youngest child age 8 to 11. The outcome in panel (a) is the dollar amount of IA receipt during the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

III.2 Childcare Deductions and Non-Filing

We do not observe childcare expenditures, but we do observe income tax deductions for childcare expenses.[50] Panel (a) of Figure III.6 shows a positive effect on claiming child care deductions. The estimates in Table III.1 indicate that mothers who lost IA due to the reform became 38 percentage points more likely to claim the childcare credit, consistent with mothers transitioning to work. Panel (b) of Figure III.6 shows the reduced form effect of filing a T1 tax return. Here we see no effect. This is unsurprising because a number of refundable tax credits, most notably child tax benefits, are only delivered to mothers if they file a T1 return, so mothers are strongly incentivized to do so.

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

(a) **Claiming Childcare Tax Credit** (b) **Fail to File Tax Return**



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

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

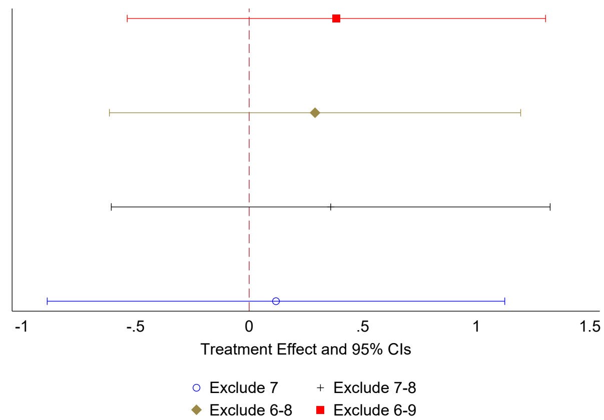
	No Controls		Group Time Trends		
	B	SE	B	SE	N
Claimed Childcare Credit	-0.38	0.06	-0.23	0.10	444840
Non Filer	0.00	0.02	-0.04	0.03	444840

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

[50]Taxpayers can claim childcare expenses for dependent children under age 16 only if childcare is used to facilitate employment or school attendance. Most childcare in BC is privately provided. Only 3,657 families received a provincial childcare subsidy in 2003. Source: https://www.bcbudget.gov.bc.ca/Annual_Reports/2003_2004/caws/caws_performance_link10.htm

III.3 Robustness Results

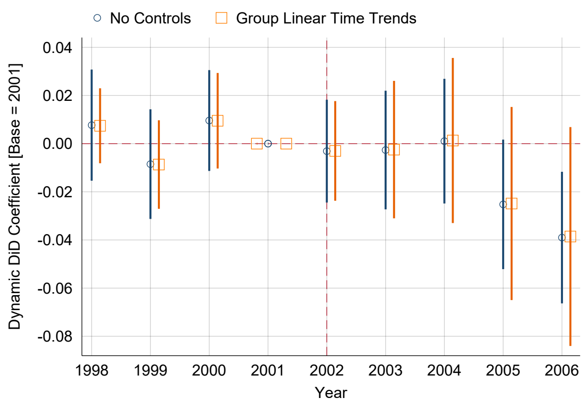
Figure III.7: **Testing for Confounding Persistence of Treatment Effects**



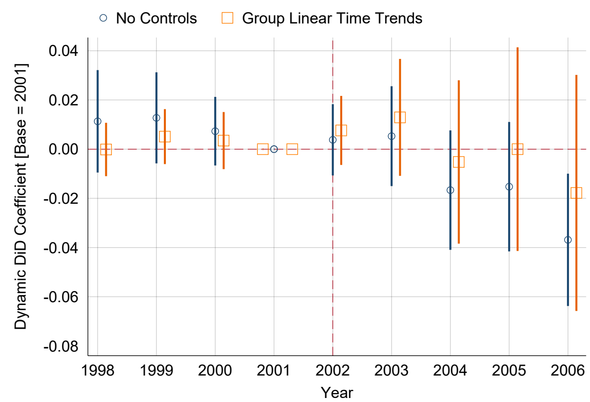
Note: This figure plots estimates of β from equation 3 and 95% confidence intervals, for the sample of mothers, while consecutively widening the age gap between treatment and control. The outcome is total hospital and outpatient spending, normalized as described in the text. Our baseline estimates exclude families where the youngest child is aged 7 (Exclude 7). The results correspond to the second specification, which allows for differing linear time trends between treated and control (using the two-step procedure described in Section 3.2). We exclude the year 2002 since this was a partial treatment year. Standard errors are clustered at the individual level. The outcome is winsorized at the 99th percentile.

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

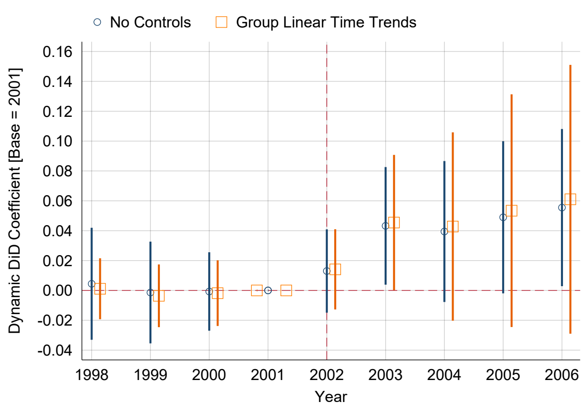
(a) **Outpatient and Hospital Costs, Adults**



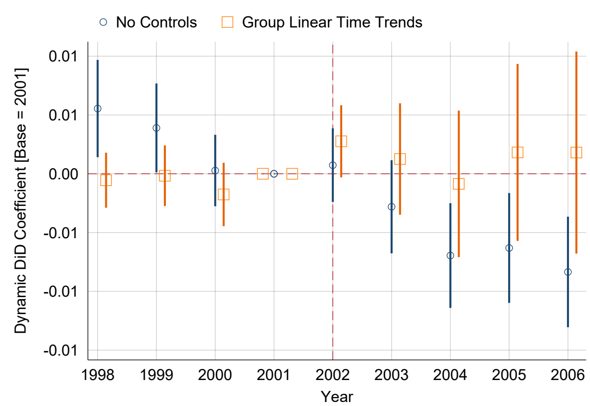
(b) **Pharmaceutical Days Supplied, Adults**



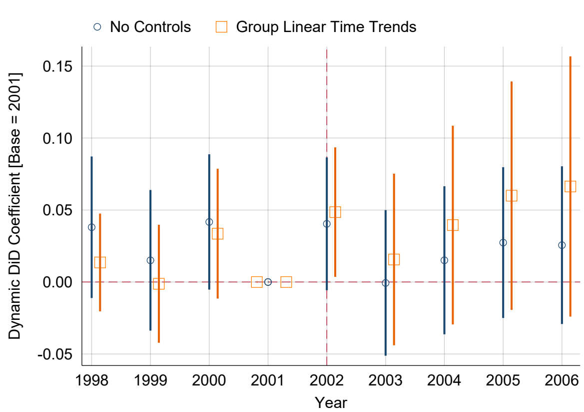
(c) **Pharma Days Supplied Mental Health, Adults**



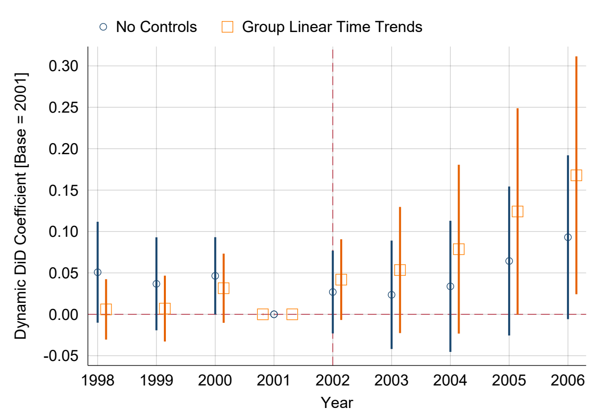
(d) **Received Pharmaceutical Contraceptive, Adults**



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



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



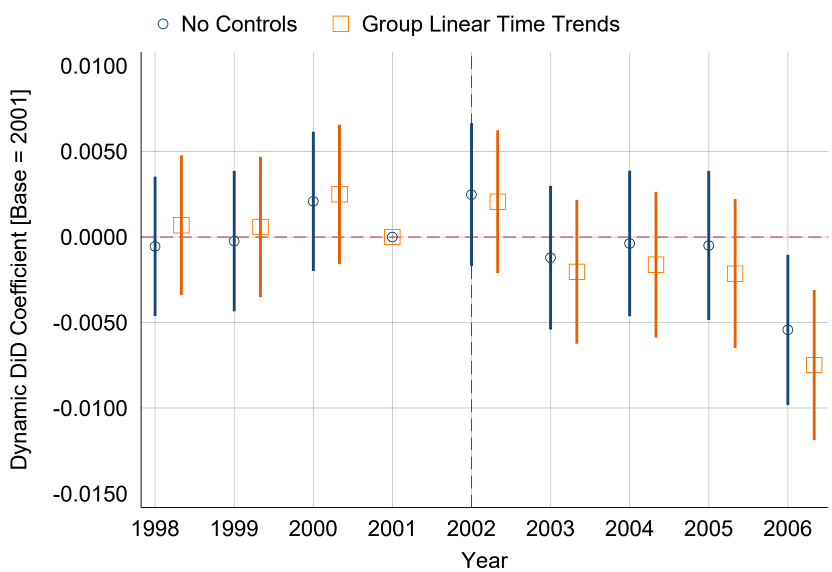
Note: This figure plots estimates of γ_s from equation 2 and 95% confidence intervals, using the full sample rather than the baseline analysis sample that restricts to mothers that received IA before 2002 (as described in Section 2). Panels (a), (b), (c), and (d) show estimates for mothers, and panels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

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

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

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

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



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

III.4 Spillovers Accounting

We calculate the net fiscal costs from the government's perspective associated with the causal effect of granting IA on healthcare expenditure and tax revenue. Income taxes paid to provincial and federal governments measured in tax returns are straightforward. Whether to include federal taxes in the accounting depends on whose perspective one takes: the provincial government, or also the federal government. We include federal taxes since the federal government provides block transfers to provinces for health and social programs. Table III.3 shows that granting IA to one recipient family, on average, increases child health costs by \$41 per child (82\$ for a two-child household), decreases adult health cost by \$38, and costs the government \$1296 + \$564 = \$1860 in lost tax revenue. The combination of these indirect costs adds up to \$1863, which is 17.4% of the direct cost of providing IA (\$10,687.50). This calculation naturally does not include all potential spillover costs to the government. We could, for instance, use the point estimates on intergenerational transmission to get noisy estimates of discounted long-run costs.

Table III.3: **Change in Government Fiscal Position from an IA Recipient**

Adult Hospital and Outpatient Costs	-38
Child Hospital and Outpatient Costs	41
Lost Federal Individual Income Tax	-1296
Lost Provincial Individual Income Tax	-564
Total Annual Indirect Cost of an IA Recipient	1863
Direct Cost of IA Benefits	10,687.50
Ratio of Indirect over Direct Costs	0.174316

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

IV Standard error correction

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

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

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

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

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

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

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

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

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

where $d_i = 1\{year_i < 2002\}$; that is, only pre-reform years are used to estimate δ and γ.

Under standard regularity conditions, it then follows that:

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

where:

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

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

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

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

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

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