Do Child Tax Benefits Affect the Wellbeing of Children? Evidence from Canadian Child Benefit Expansions*

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Abstract:

A vast literature has examined the impact of family income on the health and development outcomes of children. One channel through which increased income may operate is an improvement in a family’s ability to provide food, shelter, clothing, books, and other expenditure-related inputs to a child’s development. In addition to this channel, many scholars have investigated the relationship between income and the psychological wellbeing of the family. By reducing stress and conflict, more income helps to foster an environment more conducive to healthy child development. In this paper, we exploit changes in child benefits in Canada to study these questions. Importantly, our approach allows us to make stronger causal inferences than has been possible with the existing, mostly correlational, evidence. Using variation in child benefits across province, time, and family type, we study outcomes spanning test scores, mental health, physical health, and deprivation measures. The findings suggest that child benefit programs in Canada had significant positive effects on test scores, as has been featured in the existing literature. However, we also find that several measures of both child and maternal mental health and well-being show marked improvement with higher child benefits. We find strong and interesting differences in the effects of benefits by sex of the child: benefits have stronger effects on educational outcomes and physical health for boys, and on mental health outcomes for girls.
Introduction

A primary tool for addressing child poverty and increasing the chances for children to succeed in school, and subsequently the labor market, is income transfers to families with children. Almost all developed countries either have direct child benefits or target welfare/social assistance benefits at families with children. These benefits are meant to help achieve several goals. First, by investing in children of lesser means, society aims to provide these children with opportunities to be well-adjusted and productive members of society, with improved educational, and later, labor market outcomes. Second, benefit programs allow society to help achieve distributional and equity goals regardless of the future returns of these transfer programs.

Child benefit programs, as well as social assistance programs that target groups such as single parents with young families, transfer income to expand the budget set of qualifying families. There are several potential mechanisms through which this expansion of family budgets may improve outcomes for children. On the one hand, families may simply use the income to purchase more goods and services, including those goods that are valuable in maintaining basic child welfare and also for enhanced child development (food, clothing, books, etc.). On the other hand, income transfers may have indirect effects such as reducing stress and improving household relations, increasing the chance and opportunities for employment, and others. These indirect channels, while not goods and services purchased with the extra income, may benefit family members, including children, and therefore improve their ability to function, learn, and improve themselves.
These two channels are explored in Mayer (1997) and Yeung et al. (2002), among others, and we borrow terminology from the previous literature here. Direct purchases of resources useful for child development is called the ‘resources’ channel. Improved family relations and emotional well-being is called the ‘family process’ channel. While the majority of the economic literature has focused on the resources channel, our paper instead examines the effects of benefit programs on a wide range of child outcomes which fall naturally under the family process channel. Recent research (Currie and Stabile, 2009; Currie, Stabile, Manivong and Roos, 2008) has documented a strong relationship between early child mental health and not only short-term educational achievement, but longer-term economic outcomes such as welfare take-up. So, findings about the impact on children have important long-run implications into adulthood.

The paper offers two main contributions to the previous work in this area. First, it uses an exogenous source of child benefit income based solely on legislative variation rather than the observed income or other choices made by the family. Second, it uses a detailed Canadian survey of child well-being that includes several important outcomes of interest spanning test scores, mental health, physical health, and poverty measures. This survey allows a more rigorous examination of the family process channel than has been explored in previous work.

Our findings suggest that child benefit programs in Canada had significant positive effects on several measures of both child and maternal mental health and well-being, as well as a few measures of child physical health. We also find evidence of direct effects of child benefits on test scores. Interestingly, we find strong differences in the outcomes improved by child benefits by the sex of the child. For boys, benefits have much stronger effects on educational outcomes and physical health measures. For girls, benefits have much stronger effects on mental health measures. These differences suggest that the channels through which benefits improve outcomes may be multiple and may differ by gender.
Previous work

There is an extensive literature on the broader relationship between income and child health and development.¹ Several difficulties make the study of this relationship complex, including measuring income, distinguishing between permanent and transitory effects, finding exogenous ways to measure income, capturing the effects of other inputs into this production relationship, and finding the proper outcomes to capture the potential effects. Further, documenting a relationship between family income and child outcomes does not in itself help us understand through which channels income works to achieve any measured effects. Mayer (1997) provides a thorough treatment of the sociological theory and developed new empirical evidence based on the NLSY, finding that income was not a strong determinant of long-run outcomes. Blau (1999) presents a discussion of these issues from an economist’s perspective and reviews much of the previous literature that does not deal directly with the issue of the endogeneity of income. Using fixed effects models and the NLSY, Blau concludes that the effects of permanent family income on test scores are small compared to other family and child characteristics, leaving income transfers as a poor policy tool to improve the outcomes of low-income children.

On the other hand, a more recent study by Dahl and Lochner (2008), also using the NLSY, finds reasonably substantial effects of income on child test scores. Dahl and Lochner use changes in the earned income tax credit over several years to exogenously identify income through fixed-effect IV models. This identification strategy relies on federal changes in the benefit structure of the EITC and the tax code over time that has different impacts at different points of the income distribution. Two important contrasts with our work are that we build our

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¹ See an extensive review of the literature of the determinants of child well-being in Haveman and Wolfe (1995).
estimates on across-province and across family-size variation and that we incorporate a much broader set of outcomes than just test scores.

A study in the child development literature by Yeung, Linver, and Brooks-Gunn (2002) focuses on understanding the channels through which family income matters for child development. The authors hypothesize and test the two channels discussed earlier in the introduction, being resources and family process. The authors use the Panel Study of Income Dynamics to test these two perspectives. They find stronger evidence in support of the second hypothesis than the first although these results are primarily based on direct OLS estimates of this relationship including a wide variety of explanatory variables. So, while suggestive, it is difficult to draw causal inferences from this evidence. Morris et al. (2004) examine the effects of U.S. welfare reform and antipoverty policies on child wellbeing at different points in the life cycle. Their findings suggest improvements in child wellbeing as a result of policy changes for children making the transition into middle childhood but negative effects of the same set of policies for children making the transition into early adolescence suggesting that similar policies may indeed have very different implications for different children at different times.

In a Canadian study using the National Longitudinal Survey of Children and Youth, Dooley and Stewart (2004) employ a variety of OLS and fixed-effects models to estimate the relationship between family income and test scores. The results from these models are similar to those found in Blau (1999) in that they find a small relationship between income and test scores. They also find some limited impact of the resources channel through measures of children’s activities (day camps, sports, etc.) and housing amenities.

Previous literature has also explored the relationship between shocks to parental socioeconomic status and child wellbeing by examining the effects of job displacement for the father on later life income by the family (Oreopoulos et al., 2005). The findings suggest that
there are strong intergenerational effects of worker displacement, with children of fathers who were displaced earning less, having higher unemployment and higher take-up of social assistance than otherwise equal children of non-displaced workers.

Our work builds on this literature in a variety of ways. First, we exploit variation over time, across provinces, and across number of children, to develop an exogenous measure of benefit income as an instrument for child benefits. Second, we are able to examine a variety of outcomes, including test scores, but also including a variety of physical and mental health measures. As a result we are able to provide an exogenous estimate of the effects of benefit income on a variety of potential pathways for child development, and on a standard set of test score measures to capture child outcomes.

Policy

The Canadian child benefit system consists of two main benefits. First, the Canada Child Tax Benefit (CCTB) is paid to parents of children age 0 to 17. This is a federal benefit initiated in 1993 that pays the same across the country with two small exceptions.\(^2\) The CCTB is payable for a twelve-month period running from July to June, with the amount dependent upon the reported net income of the parents in the previous calendar year.\(^3\) So, the July 2005 to June 2006 maximum annual benefit of $1,228 per child was based on reported family net income from 2004. Benefits do not depend on earned income specifically, so families with no income still qualify for the benefit. Take-up is high—in many provinces the application is given to families

\(^2\) In Alberta, the benefit is differentiated by age of the child. For example, in 2005 children age zero to six received $1,124 while those age 16 to 17 received $1,423. Until 1997, benefits paid to Quebec residents depended on how many children were in the family, with higher-order children receiving more benefits. We account for each of these exceptions in our benefit calculator.

\(^3\) For 2005, the clawback threshold is $36,378, with a reduction rate of 2.5 percent for income over that threshold for one-child families, and 5 percent for two or more child families.
of newborns and the administration is well-integrated with the tax system so any tax filer who qualifies will be made aware of the transfer. The benefit level was constant in nominal dollars between 1993 and 1999, but has been indexed to CPI inflation since 2000. A small supplement ($86 annually in 2005) is available for a third or higher order child, and another supplement ($243 annually in 2005) was available until 2006 for children age zero to six for those not claiming childcare expenses. The federal benefits are therefore a flat benefit for all families with children, including non-workers, that varies by the number of children and that begins a slow phase-out once an income threshold is reached.

The second component of the child benefit system is the National Child Benefit program, begun in 1998.\(^4\) This program is a federal-provincial initiative that features a federally-paid benefit called the National Child Benefit Supplement (NCBS). Provinces, at their discretion, could subtract the NCBS from welfare recipients in their province and use the ‘savings’ to fund different provincial programs and child benefits. This yielded substantial differences in benefits across provinces. In addition, the province of Quebec, while it elected to stay outside the NCB program, instituted major reforms of its child benefits system in 1997 and 2005. The details of each province’s programs are provided in the Appendix. In short, two provinces introduced new transfers that weren’t related to earnings, two provinces introduced earnings-related benefits, and three provinces did both. Across provinces, there were large differences in the structure of benefits across family size. The provincial differences create variation in the flat benefit level, the income threshold, and the reduction rate.

In addition, some provinces have provincially-funded and administered benefits. In particular, Manitoba and Quebec feature unique components to their system and British Columbia introduced a benefit very similar to the National Child Benefit Supplement two years

\(^4\) The National Child Benefit Supplement replaced the Working Income Supplement, which was in place from 1993 to 1997. See details in the Appendix.
before the rest of the country in 1996. These elements are also included in our measure of child benefits.

The net impact of these changes is a large degree of heterogeneity in child benefits across several dimensions: year, province, number of children, and income. There is no explicit dependence of benefits on marital status or education, but benefit levels across those dimensions will vary through differences in family income. Because our empirical strategy will attempt to exploit only the exogenous components of policy variation, we concentrate primarily on three dimensions of heterogeneity (year, province, number of children).

We provide several figures to emphasize the extent of variation in these programs. The first is Figure 1a, which shows a depiction of the benefits for which a two-child two-parent family from Ontario would be eligible through time. The values come from a tax and benefit simulator. Importantly, much of the increase comes for those at $10,000 and $25,000 of income, through the expansion of the National Child Benefit program and the associated provincial program. This is part of the motivation for our focus on the results for lower education families, as they are more likely to be in the lower income ranges that saw the large increases in benefits through the late 1990s. Figure 1b shows how benefits for the same two-child family from Ontario evolve as earnings grow. The large jump between 1994 and 1999 results from the replacement of the $500 federal Working Income Supplement with the Ontario Child Care Supplement for Working Families paying $1100 per year. The further increase in 2004 results from the more-than doubling of the federal National Child Benefit Supplement in the first half of the 2000s. This makes clear from a different angle that most of our variation results from the provincial programs introduced and modified in the late 1990s and early 2000s.

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5 We use the Canadian Tax and Credit Simulator (CTaCS). This is described in Milligan (2010). This is the simulation that will create the instrument we describe below.

6 The National Child Benefit Supplement annual rate for two-child families went from $1,370 in 1999 to $2,806 in 2004.
To further explore the extent of provincial variation, Figure 2 shows the actual average provincial benefits among low education families in our sample. We present the results in three panels, representing families with one, two, and three children. The differences across provinces are strong. Quebec follows its own path with a large reform in 1997. British Columbia moves early by instituting a benefit in 1996. Most other provinces follow in 1998, but some provinces—such as Prince Edward Island—do not institute a provincial benefit. It is these differences across province, through time, and across family size that we exploit in our empirical strategy described below.

**Empirical Strategy**

A standard model for estimating the impact of child benefits on a variety of child outcomes in the policy context described above would be as follows:

\[
Outcome_{pyki} = \alpha_0 + \alpha_1 X_{pyki} + \alpha_2 BEN_{pyki} + \eta_{pyki}
\]  

Where the indexes on the variables represent provinces (p), years (y), number of children (k), and families (i). The vector \(X\) contains observable family level characteristics as well as control variables for time and province effects. \(BEN\) is a measure of benefit income for a family, either reported or derived based on eligibility criteria.

The crucial empirical challenge for estimating the impact of child benefits on outcomes is that individuals who receive benefit income may be different than other households in both observable and unobservable dimensions. Therefore, a standard regression of outcomes on benefit income may lead to biased estimates. OLS regressions of this type show a strong negative relationship between receipt of, or eligibility for, benefit income and outcomes, even controlling

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7 The appendix shows that the province of Quebec is a major and therefore important source of variation in child benefits. Excluding Quebec from the analysis weakens the results presented below, although a good number of the findings still hold, with slightly larger standard errors. These results are available upon request.
for other income sources and a variety of observable characteristics. To overcome this problem, we employ a solution that extracts plausibly exogenous legislative variation in benefits to remove the bias of unobserved correlates of child outcomes. In particular, we use a simulated benefits approach similar to that in Currie and Gruber (1996) where we use variation in benefit eligibility that is unrelated to individual level characteristics. Variation in benefits comes through changes in benefit generosity over time, across provinces, and across number of children. Specifically, the method involves taking a single random sample of families from a data set with detailed income and benefit data and pushing them through a tax and benefit calculator 480 times—once for each of the ten provinces, each of the twelve years between 1994 and 2005, and of four family sizes (0, 1, 2, or 3 children). The tax and benefit calculator we employ is the CTaCS package, which is described in detail in Milligan (2010). The child benefits components of the calculator were developed by looking directly at the legislation and regulations for each province and coding these parameters and program rules into the calculator. We then take the average benefit level for each of these cells. The resulting benefit levels differ across time periods, years, and family sizes only through differences in legislated benefit levels and not by income or any unobservables that may be correlated with income. This simulated benefit sample is then used to instrument for actual calculated benefits that a family is eligible for, based on both family and legislative benefit characteristics.

As in Currie and Gruber (1996), we impute the benefit that each family is eligible for by using the family’s income and demographic information available in our data. We explore the relationship between benefits for which families are eligible and true take-up of benefits below to confirm that take-up levels are sufficiently high such that we are capturing the true benefit levels.

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8 Using a major labor and income survey (described below) we take a 10 percent random sample of families with children across all years from 1996 to 2004. This amounts to 9,818 families. The 10% random sample is sufficiently large that there is considerable support across the entire income distribution. When putting this sample through the calculator for different years, we adjust incomes so that the real distribution of income in the sample is constant across all runs of the calculator.
in the population. For the analyses that use sub-samples of the data, such as education group or marital status, we continue to use the cells from the full sample as instruments.\(^9\)

As described in more detail below, we use two data sets in our analysis. The main source of data, with detailed information on child outcomes is the National Longitudinal Survey of Children and Youth (NLSCY). The NLSCY has basic family income and demographic information that allows us to impute the child benefit that each family is entitled to. Our second data set is the Survey of Labour and Income Dynamics (SLID). We use this survey to simulate the aggregate benefits that we use as instruments. The SLID is preferable for forming the instruments as it has larger samples, was designed to collect detailed data on income and benefit eligibility, and has far more detailed and accurate income information than the NLSCY. The SLID also contains actual benefit receipt information that allows us to test how well our simulated benefits align with reported benefit take-up. We aggregate the benefits to the cell level from the SLID and then merge these cells with the NLSCY.\(^{10}\)

Using both the individual level imputed benefit from the NLSCY, and the simulated aggregate benefit brought in from the SLID we estimate first-stage equations of the following type:

\[
BEN_{\text{pyki}} = \beta_0 + \beta_1 X_{\text{pyki}} + \beta_2 SIMBEN_{\text{pyk}} + \varepsilon_{\text{pyki}}. \tag{2}
\]

The imputed child benefit levels \(BEN_{\text{pyki}}\) are predicted by the set of observable characteristics \(X_{\text{pyki}}\) and the simulated benefit level \(SIMBEN_{\text{pyk}}\). We include not only the main effects of province, years, and number of children, but also the second order interactions of these three factors.

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\(^9\) Specifically, the simulated benefits are collapsed by province, year, and number of children. We have also re-run our analysis using only the low education sample to simulate benefits for this part of the population. Our results are very similar across the two methods.

\(^{10}\) It is possible to avoid using the SLID by forming the province-year-number of children cells using the imputed benefits in the NLSCY. We have tried this approach and found the results quite similar. We prefer the SLID cells because of the advantages described above.
The predicted values from our first-stage can then be used in a second-stage regression using child outcomes, taking the following form:

\[
\text{Outcome}_{pyki} = \alpha_0 + \alpha_1 X_{pyki} + \alpha_2 \hat{\text{BEN}}_{pyki} + \eta_{pyki},
\]

(3)

Both the first and second stages are run in the NLSCY; the SLID is serving only to provide instruments for the analysis in the NLSCY. The predicted value of the child benefit \(\hat{\text{BEN}}_{pyki}\) is used to explain various child outcome measures \(\text{Outcome}_{pyki}\). We include the same \(X_{pyki}\) characteristics in the second stage regression including all second order interactions. In this way, the identification of the impact of child benefits comes through the exclusion of the fully saturated third order interactions of the province, year, and number of children effects. So long as the simulated benefit measure is a good (even if not perfect) predictor of actual benefit eligibility and so long as there are no confounding province-year-number of children trends or policies that invalidate the exclusion restriction, the simulated benefit represents a valid instrument.

To summarize, our methodology therefore uses 4 steps. 1. We take a random sample of families from the SLID and simulate the benefits these families would be eligible for in each province-year-number of children combination between 1994 and 2004. 2. We aggregate these simulated benefits up to the province-year-number of children cell level and merge these cells into our estimation sample in the NLSCY. 3. We impute eligible child benefits for each family in our estimation sample using all available family characteristics in the NLSCY. 4. We instrument for eligible child benefits in the NLSCY using the simulated cells merged from the SLID.

An alternative approach is to use a two-sample least squares estimator, using a first stage in the SLID and a second stage in the NLSCY. The two-sample estimator, however, requires a common set of control variables. A key control variable unavailable in the SLID is the age of the child. Since the age of the child is an important control variable for many of our outcomes,
leaving out this control is a concern. In robustness checks, we test the sensitivity of our results to leaving out the age controls and to implementation of a two-sample least squares estimator.

An important challenge to this identification strategy might come from other policy reforms contemporaneous with the changes in child benefits. For example, provincial spending programs introduced as part of the NCB program could have influenced child wellbeing. There were also reforms to provincially-run welfare programs in the mid to late 1990s.11 Similarly, other policy reforms such as the subsidized childcare program in Quebec studied in Baker, Gruber, and Milligan (2008) might affect the environment. However, our inclusion of province-by-year effects goes some way to control for most of these concerns. That is, any impact of new provincial spending programs that affects all family sizes equally will be picked up by the province-year dummies as there is no reason to expect the new programs to have differentially impacted families with different numbers of children. The income benefits are the only aspect of policy that explicitly depends on the number of children of which we are aware, however to the extent that other policies have differing impact on families of different sizes our empirical strategy may suffer.

A key assumption underlying this approach is the exogeneity of the province of residence, year, and number of children. For the province, this assumption would be violated if individuals switched provinces in order to benefit from different incentive structures. We consider this possibility unlikely, as the benefits are unlikely to surpass the costs of moving.12 The number of children may also be influenced by benefits. Assuming that children are exogenous to benefits is standard in the EITC literature in the US (see Hotz and Scholz 2003), but the assumption may be violated if fertility decisions depend on fiscal incentives. Milligan

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11 On both of these points, see Milligan and Stabile (2007) for more policy detail.
12 This claim is supported by recent empirical evidence in Gelbach (2004) who concludes that “. . . evidence suggests little reason for concern (due to welfare migration) in using cross-state variation in welfare generosity to identify incentive effects of the welfare system on other outcome variables.”
(2005) found strong evidence that fertility did respond to fiscal incentives in Quebec’s Allowance for Newborn Children program in the late 80s and early 90s, but found much less evidence of a response among women more likely to be at-risk for being on welfare.

Data

We use two data sources for the study. Our primary source for data is the National Longitudinal Study of Children and Youth (NLSCY). This survey focuses on Canadian children, with data currently available for six biannual cycles spanning 1994-95 to 2004-05. The content of the survey combines extensive parent-reported health, well-being, and developmental information on the child and family with detailed labor market and income information for the parents. The survey initially covered children aged 0 to 11 in cycle 1 and has followed that initial cohort to ages 10 to 21 in cycle 6. Young children were added in each cycle to fill in the gap, allowing cross-sectional coverage of all ages.13

We use all families in each of the NLSCY cycles with children ages 10 and under. We focus on children of these ages as the majority of outcomes of interest are asked on a consistent basis to this subset. The resulting data set comprises approximately 108,000 observations over six cycles. However, for many of the outcomes we examine, the variables are limited to explicit age ranges, making the sample sizes for the analysis considerably smaller that the full data set. Finally, because there is some over-sampling of children in smaller provinces, we use the provided weights to recover population-level results.

The NLSCY contains several variables spanning achievement measures, physical, and mental health including having repeated a grade, a math score, a PPVT score, having been diagnosed with a learning disability, measures of hyperactivity, emotional and anxiety disorders,

13 For cycle 5 in 2002-03, cross-sectional child observations were only added in the age range 0-5. Because the longitudinal cohort was ages 8-19 in cycle 5, this left an unfilled gap at ages 6-7 for cycle 5. Similarly, there is an age gap in the range 6 to 9 for cycle 6.
physical aggression, suffering from hunger, height and weight, and mother’s health status. Means and age ranges for the variables presented in each of the results tables.

Questions are asked of the person most knowledgeable about the child (in 92% of cases this is the mother) about whether the child repeated a grade in past two years. The Peabody Picture Vocabulary Test is administered to children ages 4-6 and is a widely used measure of cognition for preschoolers. In the NLSCY, mathematics tests were administered to children in grades two through ten (beyond the age limits of our sample) and are based on the Canadian Achievement Tests. Response rates for the Math tests are slightly low and various researchers have investigated how these low response rates might bias analysis using the test scores and have concluded that the low response is random, for the most part. The question on learning disabilities asks about whether the child has been diagnosed and is answered by the person most knowledgeable about the child. The questions on mental and emotional health are asked of parents of all children aged 4 through 11 (we list the questions in the data appendix). The responses to these questions are categorized by disorder, and then added together to determine a hyperactivity score (8 questions), an emotional behavior score (8 questions), an aggressive behavior score (6 questions) an indirect aggression score (5 questions), and a prosocial behavior score (10 questions) for the child. The mother’s depression score is again based on a series of twelve questions asked to the child’s mother about her feelings and behavior over the past week.

The child and mother health questions are self-reported based on a 5 point scale for self-assessed health of excellent to poor. We combine the bottom three measures for the child as very

\[\text{14 In cycle 5 the response rate for the mathematics test was 81\%. Currie and Stabile (2006) discuss an analysis of the non-responses to the NLSCY math tests for previous cycles performed by Statistics Canada which reports little difference between responders and non-responders at that time. In the cycle 5 codebook, Statistics Canada notes that the response rate is lower in higher grades, and higher among students who performed well on previous cycle math tests.}\]
few parents report their child to be in poor health. Height and weight measures are also self-reported by the parent as are measures of injuries in the past twelve months, and reports of the child experiencing hunger because of lack of resources to buy food.

Parent reports about their children are sometimes thought less reliable. Parents may hold a more optimistic opinion of their child’s abilities and activities than a disinterested observer. Beyond any bias in their true assessments, parents might also be reluctant to report low achievements out of shame or embarrassment. On the other hand, differences in parent versus expert reports may lie in differences in information—parents may be better informed and thus make more accurate reports. Evidence suggests that parent reports can be reliable in the spheres of motor milestones (Bodnarchuk and Eaton 2004), child health (Spencer and Coe 1996), and behavior and temperament (Clarke-Stewart et al. 2000). However, the validity of the particular measures in the NLSCY may differ from the measures in those studies. A common finding in the literature on validity of parent-reported measures is that the validity of parent-reports for acute events (such as an illness or the reaching of a milestone) is higher than for more general and broad questions.15

The other survey we use is the Survey of Labour and Income Dynamics (SLID), stacking together the public-use cross sections for the years 1996 to 2004. We use this survey both to populate our simulation sample used to generate the simulated child benefits and also for validation of the predictive power of the simulated benefits as the SLID, unlike the NLSCY, has actual reported child benefits for the entire range of years used in our study. The SLID is conducted annually by Statistics Canada with a stratified random sampling of Canadians. With survey weights, the data are potentially nationally representative. The SLID provides detailed information on demographics, and more precise information on income and benefits received.

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15 Baker, Gruber, and Milligan (2008) have an extensive discussion of the measures in the NLSCY in their Appendix B.
over the past year than the NLSCY which allows us to provide more complete income and benefit information to the tax calculator. In particular, the income measures available on the SLID are attached from the respondent’s income tax records, which makes them quite relevant for the tax calculator. The SLID uses the same calendar year definition of income as the tax system; the NLSCY uses a retrospective 12 month period that depends on the interview date. The sample size per year in the SLID is around 35 thousand census families made up of 60 thousand individuals aged 15 and higher.

Results

The first set of results we present shows the mean benefit levels federally, provincially, and in total. We next explore the relationship between our simulated child benefits and actual reported child benefits, in order to gauge the effectiveness of our simulator. We then turn to our analysis of child and family outcomes from the NLSCY. Our main specifications use the simulated benefits to instrument for eligible imputed benefits and rely on within province-year-number of children variation as discussed above. We consider the entire sample of children, as well as samples by sex of the child and also present results for children of mothers with lower levels of education, as these families are more likely to be eligible for child benefits (as shown below). We show our analysis in three groups of outcome variables: education, mental and emotional wellbeing, and health and nutrition. Finally, we present specification checks that use alternative sub-samples and estimation methods.16

16 The models estimates here all include multiple children from the same household due to the sampling in the NLSCY. We have re-estimated all the models using only a single child from each family and find very similar results with little change in the P-values.
For all continuous variables, we have normalized the variables using the mean and standard deviation, so that the coefficients can be interpreted in terms of changes in standard deviations. The key independent variable is the dollar value of child benefits. All dollar values in the paper are transformed to 2004 constant Canadian dollars.

Benefit levels

Table 1 presents the mean and standard deviation of benefit levels at the federal and provincial level, as well as the total. We show the results across all observations and broken down by mother’s education. This is important to understand where our variation is coming from and to motivate our sample choices and robustness checks. The data used for this table is the 2004 SLID for families with a child under or at age 10, with all results in 2004 dollars.

The first row indicates that 85 percent of Canadian families receive some child benefits, with an average amount of $2,174—this includes those with zero in the average. Not shown in the table, a breakdown by income shows that the proportion of families with income under $60,000 receiving some child benefits is almost 1—in the SLID take-up appears close to universal. Only 28.8 percent of families receive any provincial benefits, reflecting both more narrow income targeting and also that several provinces have no provincial benefit. The average benefit is only $222, but among those receiving any benefit it is $769.

The last four rows break down the sample into groups by maternal highest level of education. For high school graduates and drop-outs, over 90 percent are receiving benefits. This reflects low income levels for these families, not an education-related take-up rate. While the proportion receiving benefits remains relatively high across all education groups, the average benefit does decline sharply, reflecting the phase out of benefits at higher income levels. The
provincial benefits are positive for only 14.6 percent of families, with a much lower average amount. This is important because our identifying variation comes not from province (and family size) level variation. For higher education families, the provincial benefits are not sizeable.

**The relationship between simulated benefits and reported child benefits**

We begin with an analysis of the relationship between our simulated child benefits and actual reported child benefits in the SLID. This analysis allows us to validate the accuracy of our simulated benefits. These estimations are performed using the person files of the SLID for the years 1996 to 2004. The results appear in Table 2. Overall, the results suggest that our simulated benefit strongly predicts actual reported benefits. Each result in the table comes from a separate regression of reported child benefits on simulated benefits with a set of standard control variables. The controls include year dummies, province dummies, number of children dummies, respondent and spouse education and parent age group dummies, a marital status indicator, and age of youngest child dummies. A standard error, clustered at the province level, is reported beneath in parentheses. The different rows of the table show results from different subsamples of the SLID data. The columns show results using different formulations of the policy variable.

The first column shows results using a difference-in-differences specification that exploits only province-year variation. We show the results for this more limited source of variation for the purpose of better understanding how much we rely on the differences across family size in our richer specification. In the province-year models, the simulated benefit cells are aggregated at the province-year level (as opposed to the province-year-number of kids level as described above) and then merged back with the individual level data. The second column uses a triple-difference specification with simulated benefits varying on a province-year-number of children basis—this is the main specification used in our analysis and includes all two-way
interaction terms. The third column is also a triple difference specification, but uses a measure of child benefits that adjusts for the reduction of welfare benefits resulting from the NCB clawback. That is, it accounts for the net change in income.\footnote{For these simulations, families in the simulation sample who had social assistance income and were in a ‘clawback’ province had their benefit level adjusted to account for the clawback. See Milligan and Stabile (2007) for details on the clawback.}

The first result is a regression using families with a child age 0 to 17, which captures any families potentially eligible for child benefits. The reported coefficient of 0.941 indicates that an extra $1 of simulated benefits is predicted to increase reported child benefits by $0.941. The result is highly significant and indicates that the simulated benefits are a very precise and accurate predictor of reported child benefits. The coefficient is little-changed in the triple difference specifications in columns two and three. The next row restricts the sample to children age 0 to 10, which is the age range we use for the NLSCY analysis to follow. In this sample, the province-year specification shows an increase in actual benefits of $1.354 for every $1 of simulated benefits. In the second and third columns, the estimated coefficient falls back under $1.00. In the subsample containing only those observations where the respondent has high school education or less, the point estimates in the triple-difference specifications are slightly lower at $0.860 and $0.868, but remain highly significant. As with all of our results here, we cannot reject the hypothesis that the coefficient equals 1.0. Overall, the analysis of the relationship between our simulated benefits and actual child benefits in the SLID allows for firm confidence that the simulated benefits are good predictors of actual benefits.

\textit{Effects of Child Benefits on Child Outcomes}

We now present the main results that examine the effects of child benefits on three sets of outcomes. These results use the National Longitudinal Survey of Children and Youth, which, as noted above, has detailed measures on educational outcomes, mental/behavioral outcomes, and
physical health outcomes. All our results are IV results where the instrument is the simulated benefit measure derived from the SLID, as discussed above. First stages from these results are reported along with the first set of outcome measures. As we discussed earlier, the NLSCY has weaker income information and incomplete child benefit information. For this reason we impute benefits to our NLSCY observations and instrument for these using the simulated benefit measure.18

The first row of results in Table 3 shows our first-stage coefficients. For all samples, the simulated cell benefit from the SLID is a strong predictor of the individual benefit imputed in the NLSCY. The coefficients range from 0.481 to 0.654. These coefficients may be less than was seen in Table 2 because of measurement error with the individual imputations in the NLSCY. In particular, then income measures in the NLSCY may be less accurate. We now turn to presenting the results of each set of outcomes in turn.

**Educational Outcomes**

Table 3 contains the results for education outcomes. For each dependent variable, we report the number of observations, the age range covered by the variable, the mean and standard deviation, and finally the coefficient on the benefit variable in four different specifications. The number of observations varies primarily because of differing age ranges for the dependent variables. For example, the PPVT scores are available only for children between ages 4 and 6. Reported in the table are the second stage coefficients on child benefits, using the simulated SLID benefits as an instrument. Our results generally show improvements in educational

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18 It is possible to aggregate the NLSCY imputed benefits into cells and use these for instruments, instead of the cells from the SLID. If we do so, the evaluation regressions show that the NLSCY cells are not as close a fit as those presented in Table 2 above, but remain strong and significant predictors of eligible benefits with coefficients ranging from 0.47 to 0.66.
outcomes, although these improvements appear to be concentrated among lower education families, and among boys more than girls.

The first row reports whether the child has ever repeated a grade. In the full sample, the significant coefficient of 0.027 suggests that an increase of $1,000 in benefits leads to a 2.7 percentage point increase in the probability of having repeated a grade. This result does not persist in the lower education sample including both sexes, and neither for the lower education sample broken down by sex. This leaves the result inconclusive.

The math and PPVT scores show a small positive, but insignificant, relationship between benefits and test outcomes for the entire sample. However, the results in the low education sample show positive and significant relationships for the math score, and a large but imprecise estimate for the PPVT. For the math score, the coefficient is 0.069, indicating an increase of 6.9 percent of a standard deviation for an increase in $1,000 of benefits. When the lower education sample is broken down by sex we find stark differences. The coefficient for math scores for boys is 0.231 and for the PPVT is 0.365, implying very sizable increases in test scores for boys. In contrast, there is no evidence of an impact for girls.

The final row of Table 3 displays the result for a binary variable describing whether the child has been diagnosed with a learning disability, as answered by the parent. The mean of this variable is 0.969, reflecting the fact that very few children have been diagnosed with a learning disability. The estimated coefficient in the full sample is not statistically distinguishable from zero, but in the high school or less sample the estimated coefficient is a significant 2.8 percentage points. Once again, once the sample is broken down by sex we see that this result is driven by boys with a significant coefficient of 6.3 percentage points, and a small and insignificant coefficient for girls. Of all our educational measures, this finding is perhaps most surprising given that we are measuring educational outcomes more or less contemporaneously.
with the benefit income. However, this result might be explained by the persistence in benefit income receipt over time and the fact that children can respond quite quickly to changes in the learning environment at home.

Overall, the evidence shows some positive impact on educational outcomes. These results appear to be concentrated among boys of families from lower education households (as measured by the educational status of mother). However, since the full sample includes many families who were not recipients of child benefits, we would expect any impact to be diluted. So, the stronger effects in the lower education sample are consistent with expectations. The magnitude of the results is very comparable to other recent work. For example, Dahl and Lochner (2008) find that a $1000 increase in income leads to an increase in math scores of 6% of a standard deviation versus our 7.3% for boys and girls combined. They are slightly larger than those found by Blau (1999) who finds approximately a quarter of a standard deviation increase in test scores from $10,000 worth of income.

**Mental and Emotional Wellbeing**

We now turn to indicators of mental and emotional wellbeing. Recent literature has highlighted the importance of early mental health problems for long-term educational and labor market success (Currie et al., 2008). These dependent variables take the form of scores, aggregated up from responses to individual questions. We report the questions from the questionnaire in the appendix. These scores have been developed in accordance with established practices in developmental psychology. Baker, Gruber, and Milligan (2008) provide some detail on studies of the validity of these measures. For the regressions, we have again scaled the variables by the mean and standard deviation so that coefficients estimates reflect the proportion of a standard deviation resulting from a $1,000 change in benefits.
In general our results show evidence of child benefits improving mental health scores. Results are strongest for the entire sample, with similar patterns (but larger standard errors) in the lower education sample. The overall relationship between child benefits and mental health is stronger for the girls than the boys. Table 4 presents the results for each of the measures. The first row of the table shows the results for the hyperactivity-inattention score. There is a negative and significant impact in the full sample. In the low education sample, the estimated coefficient is larger in magnitude, but the precision of the estimate is weak, rendering the coefficient insignificantly different from zero. The second row of the table studies the pro-social behavior score. As can be seen in the appendix, these questions reflect how much the child helps other children. The coefficients here are negative, but not significant. For emotional disorder-anxiety, the point estimates are uniformly negative, but not statistically significant anywhere but the full sample. Taken together, this evidence is not very informative about the emotional impact of child benefits.

We now turn to measures of aggression. The next row shows the results for conduct disorder-physical aggression, measuring violent acts towards others. The impact with the full sample is negative and statistically significant and is practically identical for the sample from lower education households. When we break down the sample, however, it appears that this result is driven by the girls in the sample. The estimated impact of a thousand dollar increase in child benefits is -0.153, which implies an impact of 15.3 percent of a standard deviation. The estimated impact on boys is small and insignificant. The table now moves from physical to indirect aggression. Indirect aggression measures social rather than violent conflict with other children. The results here are strongly significant in the low-education sample, and again are driven by a very large result for girls. An extra thousand dollars of benefit is predicted to change girls’ indirect aggression score by 21.7 percent of a standard deviation.
We close this analysis with an examination of the depression score of the mother. The depression questions are asked of the person most knowledgeable, so to keep the responses consistent we selected only the mothers who were the respondent. The results for this dependent variable are negative and very strong, indicating a significant improvement on maternal depression of increased child benefits. The coefficient on simulated benefits in the full sample is -0.101, which rises to -0.196 in the low-education sample. There are differences in the point estimates for boys and girls, but the confidence intervals overlap.

To summarize, several indicators of emotional and behavioral wellbeing indicate that increased child benefits improve the outcomes of children and their mothers. The results are particularly strong for physical aggression and for maternal depression. Unlike the test score results where it appears that the effects are strongest for the boys from lower education households, the effects on mental well-being are concentrated among girls from lower education households in two of the five measures of mental health, and are slightly stronger among the mothers of girls from lower education families for our measure of maternal mental health. While there is little comparable direct evidence on the relationship between benefit income and mental health, the change in mental health is a significant one. For example, using evidence from Currie and Stabile (2009) to link the effect estimated here to future educational outcomes, the fifth to a quarter standard deviation improvement in indirect aggression found here would result in improvements in test scores of approximate 2.5 to 5 percent of a standard deviation and a decline in the probability of repeating a grade of approximately 1 percent. These estimated indirect impacts on future education are only slightly smaller than the direct contemporaneous effects on education reported above.
Health and Nutrition

The final set of variables we analyze looks at the health outcomes of children and their mothers. We find less evidence overall that child benefits have improved physical health. However, among children from less educated families there is some evidence that fewer children are malnourished (particularly among boys) but the evidence is somewhat mixed and so we are cautious about drawing any strong conclusions about physical health.

These results are reported in Table 5, following the same format as the previous two tables. In the first row is a dummy variable for never having experienced hunger. The mean of this variable is 0.987, reflecting the fact that very few children in the NLSCY have ever experienced hunger. The results in the full sample show no change in this variable when simulated benefits increase, but in the low education sample the coefficient is 0.014, indicating a small improvement. For hunger, the result is much stronger among boys than girls.

Parent assessments of the child’s general health level show changes for the worse in the specifications for both sexes together, but show improvements in overall health for boys only (for this variable, the positive coefficients indicate a worsening of health). For height, there is a fairly large and significant impact on height for low-education boys, but little effect for girls. Again, given that benefits are more or less contemporaneous with the outcomes we are measuring, we are cautious about interpreting the change in height as a function of contemporaneous benefits. To the extent that current benefits are correlated with past benefits, this might be part of a delayed improvement in health. However, for weight, there is no significant effect evident.

Overall, the health results show some indications that hunger is reduced, but this appears to have little impact on the general health of the child. For the subsample of just boys from lower education household, the effects are much more pronounced with an increase in the
number of boys who never experience hunger and improved height and lower obesity.\textsuperscript{19} Our results of a $1000 increase in benefit income leading 1 to 3 percentage point reduction in the share going hungry among low education families are quite large. Between 1977 and 1991 an average 2 to 4 percent of families in the US reported that they did not have enough to eat, although this rate is considerably higher (10 times as high) among families below the poverty line than among those above it (Lewit and Kerrebrock, 1997).

\textbf{Robustness Checks:}

We next turn to examining some specification and robustness checks for our models. We focus most of these analyses on the low-education sample as these are the children most likely to receive child benefits, and accordingly where we find the majority of the effects of these benefits on outcomes.

First, we check for the consistency of our inferences with an approach that adjusts for multiple outcomes that may be correlated with each other. In particular, we implement the family-wise error correction developed by Westfall and Young (1993) and described in Anderson (2008). These results appear in Table 6. The first column reproduces our results from tables 3, 4, and 5 and the second column reports the original $p$-value. The third column in Table 6 shows the adjusted $p$-value using the approach described by Anderson (2008).\textsuperscript{20} We implement this across the three ‘families’ of results, being those in tables 3, 4, and 5. As expected, in general the results show higher $p$-values. However, our key results on the math score, aggression, and depression are robust to this test.

\textsuperscript{19} Further analysis (not reported here, but available on request) suggests that the height and BMI changes are focused mainly on kids over three years of age. While the change of 14 percent of a standard deviation seems large, it translates into a change of about 1.6 cm or 0.6 inches over the course of the year.

\textsuperscript{20} We thank Michael Anderson for making his code available on his website.
Next, we re-estimate our main specifications using two sample instrumental variables. Specifically we use the SLID for our first stage model of the relationship between simulated child benefits and actual child benefits received and the NLSCY for our second stage examining the relationship between child benefits and child outcomes.\textsuperscript{21} In this case we take advantage of the fact that the SLID has both high quality family income information that we use to construct the simulated benefit instrument and information on reported child benefits received. A major drawback of the SLID is that it does not have important demographic information on children such as the age of the child, meaning that we must also exclude these controls from our second stage regression in the NLSCY. To test how important these controls are to any differences in the results between specifications, we also run our main specification using the same estimation strategy as our base result, but restricting to those controls available in the two-sample estimation. This allows us to see how much of any difference between the base results and the two sample results are driven by the change in the set of control variables.

Our two sample results are reported in Table 6, along with the results for the restricted set of controls. The omitted controls turn out to be important ones in some cases. In particular, the math score result in Table 6 shows an insignificant 0.037 coefficient when we use the same methodology as our base results, but restricted to the two-sample set of controls. We also notice larger discrepancies for other results—such as height and weight—for which age is particularly important control. For our other results, the two-sample estimates are broadly consistent with what we find using our base methodology. For example, the maternal depression results show a coefficient of -0.200, compared to -0.196 in our base results. The standard errors are larger, but in most cases the inference of statistical significance remains plausible.

\textsuperscript{21} The coefficient estimates are taken from the first stage estimated in the SLID and used to predict the instrument in the NLSCY which is then used in the second stage. The standard errors are corrected based on Jappelli et al. (1998).
The next set of robustness checks tries the results in different subsamples. We run three alternative regression models to examine the sensitivity of our results to a) excluding the province of Quebec, b) including province-specific linear trends and c) including only observations with mothers having a university degree.

In our description of the provincial variation in benefits over time we note that Quebec’s child benefits are considerably different from those in other provinces. As such we test whether excluding Quebec from our analysis has important effects on our conclusions. The results are also reported in Table 7. Some of the results lose significance. While we still find some evidence of the effects of child benefits in mental health improvements for mother and child, the results are weaker. This suggests that Quebec is an important source of variation for our empirical strategy. It is important to note that our full specification does include a set of Quebec-time and Quebec-family size interactions, so this should mitigate some concerns that our results are overly affected by Quebec-specific trends or fixed family behavior patterns.

The next column in Table 7 examines how sensitive our specifications are to the inclusion of separate time trends for each province by number of kids group in our model along with province, time and number of children dummies as well as the second order interactions of all three of these sets of dummy variables (all of which are included in all our previous estimates). Including these interacted time trends soaks up much of the variation in our models and the resulting estimates of the effects of the child benefits are mainly insignificant. Given that the model includes three sets of second-order interactions across province, time, and number of children, and the interaction between provincial time trends and number of children, the fact that our results fall down under the rigors of this demanding specification is perhaps not surprising, but we include it here for completeness.Finally, we examine the effects of child benefits on the population least likely to receive benefits and least likely to benefit from the income—children
of university graduates. We expect that among this sub-sample, our results should be much weaker. While there is still a fraction of university graduates who receive child benefits, this fraction is smaller. However, a dollar in benefits may have different impact on different types of families, so it is more difficult to interpret this specification as a ‘placebo’ test. Our findings, reported in Table 7, show little positive impact of benefits on education outcomes. While we do find a few positive effects of the benefits (on height and physical aggression for example), there are also effects going in the other direction. We interpret this pattern as much weaker than what we observed in the low-education sample, giving us a stronger sense of what part of the population is driving our main results.

Conclusions

In this paper, we study the impact of child benefits on measures of education, emotional and behavioral wellbeing, and health. We find indications that increased child benefits led to improved test scores, decreased aggression and maternal depression, and a reduction in hunger. Our empirical approach based on exogenous policy changes makes us more confident these results are causal than has been possible with the existing, mostly correlational, literature.

A particularly striking finding in our results is the large difference between the effects of benefit income on boys versus girls. Further, these differences depend on the type of outcome being examined, although they are quite consistent within type of outcome (various health measures versus various education measures). On many of our education and physical health measures we find considerably larger effects for boys. For many of our mental health variables we find considerably larger effects for girls. Finding such differences between sexes is consistent with evidence from other studies examining the impact of various programs on children of
various ages (see, for example, Angrist et al (2006), Dynarski (2005) for differences at the college level, and Anderson (2008) for differences at the pre-school level).

Most of the economics research on child benefits has focused on the labor market, educational, and direct-consumption aspects of increased child benefits. We take our findings as evidence that a broader set of outcomes should be included in any assessment of the costs and benefits of expanded transfer payments to families with children.
References


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Oreopoulos, Phil, Page, Marianne, and Ann Huff Stevens (2005), “The Intergenerational Effects


### Table 1: Benefit levels by education group

<table>
<thead>
<tr>
<th>Education Group</th>
<th>Observations</th>
<th>Federal Benefits</th>
<th>Provincial benefits</th>
<th>Total benefits</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Greater than zero</td>
<td>Amount</td>
<td>Greater than zero</td>
</tr>
<tr>
<td>All observations</td>
<td>5134</td>
<td>0.850 (0.357)</td>
<td>2174 (2418)</td>
<td>0.288 (0.453)</td>
</tr>
<tr>
<td>High school dropout</td>
<td>484</td>
<td>0.978 (0.146)</td>
<td>3651 (2821)</td>
<td>0.493 (0.500)</td>
</tr>
<tr>
<td>High school graduate</td>
<td>763</td>
<td>0.933 (0.249)</td>
<td>2835 (2445)</td>
<td>0.361 (0.481)</td>
</tr>
<tr>
<td>Some post-high school</td>
<td>2835</td>
<td>0.884 (0.320)</td>
<td>2199 (2369)</td>
<td>0.294 (0.456)</td>
</tr>
<tr>
<td>University degree</td>
<td>1052</td>
<td>0.672 (0.470)</td>
<td>1103 (1756)</td>
<td>0.146 (0.353)</td>
</tr>
</tbody>
</table>

Notes: Data come from the 2004 SLID. The table shows the proportion of observations with child benefits greater than zero and the average child benefits (including those with zero). This is repeated for federal benefits, provincial benefits, and total benefits. Beneath each mean is the standard deviation in parentheses. Each row represents a different sample.
Table 2: The Relationship Between Simulated Benefits and Actual Child Benefits in the SLID

<table>
<thead>
<tr>
<th>Type of variation in policy variable</th>
<th>Nobs.</th>
<th>(1) province-year</th>
<th>(2) province-year-number children</th>
<th>(3) province-year-number children, net measure</th>
</tr>
</thead>
<tbody>
<tr>
<td>All kids age 0-17</td>
<td>85396</td>
<td>0.941 (0.104)</td>
<td>0.905 (0.105)</td>
<td>0.884 (0.102)</td>
</tr>
<tr>
<td>Just kids age 0-10</td>
<td>55959</td>
<td>1.354 (0.141)</td>
<td>0.979 (0.135)</td>
<td>0.966 (0.131)</td>
</tr>
<tr>
<td>Kids age 0-10</td>
<td>17704</td>
<td>0.980 (0.260)</td>
<td>0.860 (0.177)</td>
<td>0.868 (0.179)</td>
</tr>
<tr>
<td>Just highschool or less</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Regressions using the Survey of Labour and Income Dynamics. Regressors include year dummies, province dummies, respondent and spouse age group dummies, respondent and spouse education group dummies, age of youngest child dummies, and a marital status indicator. The second and third columns also include interaction terms for province*year, year*number of children, and province*number of children.
### Table 3: IV Results: Educational Outcomes

<table>
<thead>
<tr>
<th>First Stage Coefficients</th>
<th>All Education groups sample</th>
<th>High School or Less sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Number of Observations</td>
<td>Age</td>
</tr>
<tr>
<td></td>
<td>Range</td>
<td>(Std. Dev.)</td>
</tr>
<tr>
<td>Child has ever repeated a grade</td>
<td>37629</td>
<td>4-10</td>
</tr>
<tr>
<td></td>
<td>[0.169]</td>
<td>[0.003]**</td>
</tr>
<tr>
<td>Scaled math score</td>
<td>16804</td>
<td>6-10</td>
</tr>
<tr>
<td></td>
<td>[89.27]</td>
<td>[0.037]</td>
</tr>
<tr>
<td>Scaled PPVT score</td>
<td>29874</td>
<td>4-6</td>
</tr>
<tr>
<td></td>
<td>[15.35]</td>
<td>[0.021]</td>
</tr>
<tr>
<td>NOT been diagnosed with</td>
<td>52304</td>
<td>6-10</td>
</tr>
<tr>
<td>learning disability</td>
<td>[0.173]</td>
<td>[0.005]</td>
</tr>
</tbody>
</table>

Notes: Data is the NLSCY. The table shows the number of observations, age range, mean, and standard deviation for each dependent variable in the first three columns. The last four columns report coefficients on simulated benefits for a regression with the indicated variable as the dependent variable. Regressions include the full set of control variables indicated in text. Standard errors are reported beneath the estimates, with one star for results significant at the 10 percent level and two stars for those significant at the 1 percent level of significance.
Table 4: IV Results: Mental and Emotional Wellbeing Outcomes

<table>
<thead>
<tr>
<th>Regression coefficients</th>
<th>All Education groups sample</th>
<th>High School or Less sample</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Number of Observations</td>
<td>Age Range</td>
</tr>
<tr>
<td>Hyperactivity-inattention score</td>
<td>58916</td>
<td>4-10</td>
</tr>
<tr>
<td></td>
<td>[3.373]</td>
<td>[0.015]**</td>
</tr>
<tr>
<td>Prosocial behavior score</td>
<td>42545</td>
<td>4-10</td>
</tr>
<tr>
<td></td>
<td>[3.887]</td>
<td>[0.057]</td>
</tr>
<tr>
<td>Emotional disorder - Anxiety score</td>
<td>58987</td>
<td>4-10</td>
</tr>
<tr>
<td></td>
<td>[2.411]</td>
<td>[0.023]**</td>
</tr>
<tr>
<td>Conduct disorder - physical aggression score</td>
<td>58958</td>
<td>4-10</td>
</tr>
<tr>
<td></td>
<td>[1.868]</td>
<td>[0.032]**</td>
</tr>
<tr>
<td>Indirect aggression score</td>
<td>56634</td>
<td>4-10</td>
</tr>
<tr>
<td></td>
<td>[1.562]</td>
<td>[0.026]</td>
</tr>
<tr>
<td>Mother's Depression Score</td>
<td>98602</td>
<td>0-10</td>
</tr>
<tr>
<td></td>
<td>[5.348]</td>
<td>[0.016]**</td>
</tr>
</tbody>
</table>

Notes: Data is the NLSCY. The table shows the number of observations, age range, mean, and standard deviation for each dependent variable in the first three columns. The last four columns report coefficients on simulated benefits for a regression with the indicated variable as the dependent variable. Regressions include the full set of control variables indicated in text. Standard errors are reported beneath the estimates, with one star for results significant at the 10 percent level and two stars for those significant at the 1 percent level of significance.
Table 5: IV Results: Health and Nutrition Outcomes

<table>
<thead>
<tr>
<th>Regression coefficients</th>
<th>All Education groups sample</th>
<th>High School or Less sample</th>
<th>Both</th>
<th>Just</th>
<th>Just</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Number of Observations</td>
<td>Age Range</td>
<td>Mean (Std. Dev.)</td>
<td>Regression coefficient</td>
<td>Sexes</td>
</tr>
<tr>
<td>Never experienced hunger because of lack of money to buy food</td>
<td>82134</td>
<td>2-10</td>
<td>0.987 [0.111]</td>
<td>-0.002 [0.003]</td>
<td>0.014 [0.005]*</td>
</tr>
<tr>
<td>In general, child is in good/fair/poor health</td>
<td>108917</td>
<td>0-10</td>
<td>0.118 [0.323]</td>
<td>0.005 [0.003]</td>
<td>0.018 [0.011]</td>
</tr>
<tr>
<td>Current height in metres and centimetres</td>
<td>91533</td>
<td>0-10</td>
<td>1.086 [0.245]</td>
<td>-0.010 [0.007]</td>
<td>0.043 [0.023]*</td>
</tr>
<tr>
<td>Current weight of child in kilograms</td>
<td>102424</td>
<td>0-10</td>
<td>21.225 [9.752]</td>
<td>-0.016 [0.008]</td>
<td>-0.039 [0.037]</td>
</tr>
<tr>
<td>injured in last 12 months</td>
<td>108869</td>
<td>0-10</td>
<td>0.094 [0.292]</td>
<td>0.009 [0.004]*</td>
<td>-0.002 [0.011]</td>
</tr>
<tr>
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<td>0.011 [0.009]</td>
<td>-0.001 [0.010]</td>
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</table>

Notes: Data is the NLSCY. The table shows the number of observations, age range, mean, and standard deviation for each dependent variable in the first three columns. The last four columns report coefficients on simulated benefits for a regression with the indicated variable as the dependent variable. Regressions include the full set of control variables indicated in text. Standard errors are reported beneath the estimates, with one star for results significant at the 10 percent level and two stars for those significant at the 1 percent level of significance.
Table 6: Robustness: Inference and Specification

<table>
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<tr>
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<tr>
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<tr>
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<td>**</td>
<td>[0.032]</td>
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<tr>
<td>Hyperactivity-inattention score, 4-11</td>
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<td>0.253</td>
<td>0.304</td>
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<tr>
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<td></td>
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<td>Conduct disorder - physical aggression score</td>
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<tr>
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<td>[0.031]**</td>
<td>[0.100]**</td>
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<td>Mother's Depression Score</td>
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<td>[0.005]*</td>
<td>[0.007]*</td>
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<tr>
<td>In general, child is in good/fair/poor health</td>
<td>0.018</td>
<td>0.122</td>
<td>0.244</td>
<td>0.019</td>
<td>-0.016</td>
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<tr>
<td></td>
<td>[0.011]</td>
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<td></td>
<td>[0.010]*</td>
<td>[0.011]</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Current height in metres and centimetres</td>
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<tr>
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<td></td>
<td>[0.057]</td>
<td>[0.073]</td>
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<tr>
<td>injured in last 12 months</td>
<td>-0.002</td>
<td>0.834</td>
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<td>[0.012]</td>
<td>[0.011]*</td>
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<tr>
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<td>0.936</td>
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<td>[0.012]*</td>
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</tr>
</tbody>
</table>

Notes: Data is the NLSCY, using the low education sample. The first column repeats the results from Tables 3, 4, and 5. The second column shows p-values corresponding to the results in the first column. The third column shows p-values adjusted for multiple outcomes. The fourth column shows the results using the restricted set of control variables. The final column shows the results for the Two Sample Instrumental Variables estimation. All regressions include the control variables indicated in the text. Standard errors clustered on province are reported beneath the estimates, with one star for results significant at the 10 percent level and two stars for those significant at the 1 percent level of significance.
## Table 7: Robustness: Samples

<table>
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<tr>
<th></th>
<th>Base Results</th>
<th>Without Quebec</th>
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<td>Scaled math score</td>
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<td>[0.015]**</td>
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<td>[0.136]</td>
</tr>
<tr>
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<td>[1.350]</td>
<td>[0.008]*</td>
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<tr>
<td>Hyperactivity-inattention score, 4-11</td>
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</tr>
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<td>[0.090]</td>
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<td>-0.171</td>
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<tr>
<td>Conduct disorder - physical aggression score</td>
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<td>-0.088</td>
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<td>[0.053]*</td>
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<td>[0.040]**</td>
<td>[0.222]*</td>
<td>[0.228]</td>
<td>[0.135]</td>
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<tr>
<td>Mother's Depression Score</td>
<td>-0.196</td>
<td>-0.229</td>
<td>0.343</td>
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<td></td>
<td>[0.060]**</td>
<td>[0.107]*</td>
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<td>[0.094]</td>
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<tr>
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<td>-0.032</td>
<td>-0.018</td>
</tr>
<tr>
<td></td>
<td>[0.005]*</td>
<td>[0.020]</td>
<td>[0.056]</td>
<td>[0.006]*</td>
</tr>
<tr>
<td>In general, child is in good/fair/poor health</td>
<td>0.018</td>
<td>-0.020</td>
<td>-0.307</td>
<td>-0.028</td>
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<td>[0.024]</td>
<td>[0.232]</td>
<td>[0.029]</td>
</tr>
<tr>
<td>Current height in metres and centimetres</td>
<td>0.043</td>
<td>0.141</td>
<td>0.197</td>
<td>0.045</td>
</tr>
<tr>
<td></td>
<td>[0.023]*</td>
<td>[0.135]</td>
<td>[0.089]*</td>
<td>[0.012]**</td>
</tr>
<tr>
<td>Current weight of child in kilograms.</td>
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<td>0.043</td>
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<td>[0.035]</td>
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<tr>
<td>injured in last 12 months</td>
<td>-0.002</td>
<td>-0.024</td>
<td>0.049</td>
<td>0.005</td>
</tr>
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<td>[0.011]</td>
<td>[0.064]</td>
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<td>[0.008]</td>
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<tr>
<td>Mother health status is excellent</td>
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<td>[0.023]</td>
</tr>
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</table>

Notes: Data is the NLSCY. The first column repeats the low-education results from Tables 3, 4, and 5. The second column shows results without Quebec, using the low education sample. The third column shows results including linear provincial trends with the low-education sample. The final column shows the results for the university graduates sample. All regressions include the control variables indicated in the text. Standard errors clustered on province are reported beneath the estimates, with one star for results significant at the 10 percent level and two stars for those significant at the 1 percent level of significance.
Figure 1a: Total benefits for an Ontario family with two children through time

Note: Simulations based on CTaCS tax and benefit simulator. The lines show the total federal plus provincial benefit level for a two parent family in Ontario with children age 5 and 8.
Figure 1b: Total benefits for an Ontario family with two children at different income levels

Note: Simulations based on CTaCS tax and benefit simulator. The lines show the total federal plus provincial benefit level for a two parent family in Ontario with children age 5 and 8.
Figure 2a: Provincial benefits across provinces through time for one child families

Note: Graphed are the average provincial benefits in each year for each province in our SLID sample of low education families.
Figure 2b: Provincial benefits across provinces through time for two child families

Note: Graphed are the average provincial benefits in each year for each province in our SLID sample of low education families.
Figure 2c: Provincial benefits across provinces through time for three child families

Note: Graphed are the average provincial benefits in each year for each province in our SLID sample of low education families.
Appendix A: National Child Benefit and Provincial Programs

**Federal:**

The National Child Benefit Supplement began in 1998. The rates for 2005-06 were $1,722 annually for a first child, $1,502 for the second, and $1,420 for a third child. These rates are approximately triple what was in place in 1998. The family income threshold for the clawback of these benefits in 2005 was $21,480. The clawback rates were 12.2% for one child families, 22.8% for two child families, and 32.9% for three or more child families.

From 1993 to 1997, there was a federal Working Income Supplement. This benefit paid up to $500 to a family with children. It was phased in at a rate of 8% for earned income greater than $3,750 and phased out at a rate of 10% for family incomes over $20,921. This benefit was removed with the introduction of the National Child Benefit Supplement.

Unless otherwise mentioned, the provincial benefits described below were administered by the Canada Revenue Agency and integrated in one monthly payment with the federal CCTB and NCBS.

Several provinces reduced provincial social assistance payments dollar for dollar with the NCBS payments. Other provinces adjusted their social assistance payment schedule. These details are noted for each province below.

**Newfoundland and Labrador:**

The Newfoundland Child Benefit was introduced in 1999. In 2005, the annual rate for a first child was $250, $326 for a second child, $350 for a third child, and $375 for a fourth child. The clawback of these benefits was in the income range $17,397 to $21,480. An additional supplement for children age zero was added in 2001. This paid $540 annually in 2005.

Social assistance payments were not clawed back, but the adjustment for family size in social assistance payments was changed in 1999 when the Newfoundland Child benefit was introduced.

**Prince Edward Island:**

No child benefit program. Full reduction of NCBS payment on provincial social assistance payments.

**Nova Scotia:**

The Nova Scotia Child Benefit started in 1998. Families with 1 to 3 children receive different payments. Initially in 1998, the payments ranged from $250 for the first child to $136 for a third (or higher) child. In 2001, the payments for third and higher children increased substantially. In 2005, the payment rates were $445 annually for a first child, $645 for a second, and $720 for a third. These benefits are clawed back at high clawback rates for incomes over $16,000.

Full reduction of social assistance payments until 2001. After 2001, the social assistance payment structure for family size was adjusted instead.
New Brunswick:
The New Brunswick Child Tax Benefit was introduced in 1997, before the national NCB program reached its start. The benefit is $250 per child annually, and has not changed since 1997. The benefit is clawed back at 2.5% or 5% for family net income over $20,000. In addition, there is a Working Income Supplement of $250 annually that is phased in at 4% for earned income over $3,750 and clawed back at a rate of 5% for family net income over $20,921.

New Brunswick never reduced social assistance payments.

Quebec:
Until 1997, residents of Quebec were eligible for a family allowance, an allowance for young children, and an allowance for newborn children. These amounts increased with the number of children in the family and did not depend on family income. In 1997, these were combined into a new family allowance. The rates for the new family allowance were $2,275 per child for a single parent family and $975 per child for a two-parent family. These amounts were clawed back starting at incomes of $15,332 for singles and $21,825 for two-parent families. However, the clawback only took benefits down to a minimum benefit level that was $80 annually for one and two child families and $975 for three child families. But, for those with family net incomes higher than $50,000 these ‘minimum’ benefits were clawed back at a rate of 5%. In Quebec there was also an earned income benefit called APPORT in place from 1988 to 2004. In 2004, this benefit was phased in for earnings over $1,200 at a rate of 35% until an earnings level of $11,370 (two-parent) or $7,790 (one-parent), and then clawed back at 43% after that. At the peak benefit level the benefit amount was quite large, but take-up of this benefit was not high.

In 2005 a new Child Assistance program replaced the family allowance and a new Work Premium replaced APPORT.

There was no reduction in social assistance payments for NCBS income, but social assistance and other child benefits were reformed in 1997.

Ontario:
Ontario introduced the Ontario Child Care Supplement for Working Parents in 1997. The initial rates were $400 per child age 0 to 6, clawed back at 4% for net family income over $20,000. In 1998, the amounts were revamped and largely stayed the same until 2005. From 1998, the amount was phased in with earned income over $5000, at a rate of 20% for 1998 and 21% from 1999 to 2005. The 2005 benefit amount was $1,100 per child age 0 to 6 for a one-parent family and $1310 for a two-parent family. The clawback rate was 8% for incomes over $20,000.

Full reduction of social assistance for NCBS payments until July 2004. From 2004, the increments to NCBS were protected from the reduction.
Manitoba:
There was no new benefit specifically part of the NCB program in Manitoba, but a pre-existing benefit called CRISP was in existence since the 1980s. It required a separate provincial application and social assistance recipients were not eligible. In 2005, CRISP paid $360 annually per child, with a clawback rate of 2.083% for incomes over $12,384. These amounts had not changed in nominal terms since the 1980s.


Saskatchewan:
The Saskatchewan Child Benefit was introduced in 1998. In the first year, it paid $900 annually to a one child family, $1,104 for a second child, and $1,176 for a third. As the NCBS increased in the following years, the Saskatchewan Child Benefit was decreased downward dollar for dollar, so that by 2005 it paid only $7 annually for a 2nd child and $86 for a third. It is clawed back at high rates for family net incomes over $15,921. Additionally, there is a working income supplement in Saskatchewan. In 2005 the amount ranges from $2,385 for a one child family to $4,293 for a five child family. It is phased in for earnings over $1,500 at rates between 25% and 45% and clawed back at a 20% rate for incomes over $14,640. There is a supplement for children under age 13 that pays an extra 25% on top of the regular employment supplement.

There was no reduction in social assistance payments, but as noted above the Saskatchewan Child Benefit shrunk dollar for dollar with NCBS increases through time.

Alberta:
Alberta has an employment-related child benefit. It was introduced in 1997 with a phase-in rate of 8% for earnings over $6,500 up to a maximum of $250 for one child and $500 for two or more. The benefit is clawed back at a rate of 4% for incomes over $25,000. Between 1998 and 2004, the benefit maximum was set at $500 for one child and $1000 for two or more children, but was otherwise similar to 1997. The benefit changed again for the 2005 year.

There is full reduction of social assistance payments for the NCBS benefits.

British Columbia:
The BC Family Bonus was introduced in 1996, two years before the NCB program. The Bonus rate was $1,236 per child and was clawed back at a rate of 8% for one child and 16% for two or more for incomes higher than $18,000. These amounts were increased to $1,332, 9%, 18%, and $20,500 in 2001 and have remained constant since. However, the NCBS is subtracted from the BC Family Bonus, rendering it to zero by 2005 since the NCBS is now larger than the prescribed BC Family Bonus payments. There is also a BC Earned Income Benefit that was introduced in 1998. It pays differing amounts for each child and is phased in between earnings levels of $3,750 and $10,000. It is clawed back for incomes higher than $20,921 at high rates. The amounts for 2005 are $365 for the first child, $370 for the second, and $372 for the third or higher. Until 2003, the amount for the first child was $605, with $405 for the second and $330 for the third.

There is no reduction of social assistance for the NCBS payments, but the NCBS payments reduced the BC Family Bonus as described above.
Appendix B: Questionnaire extracts

Mental Health Variables


a) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Can't sit still, is restless or hyperactive?
b) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is distractible, has trouble sticking to any activity?
c) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Fidgets?
d) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Can't concentrate, can't pay attention for long?
e) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is impulsive, acts without thinking?
f) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Has difficulty awaiting turn in games or groups?
g) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Cannot settle to anything for more than a few moments?
h) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is inattentive?

2. Emotional Disorder Score in Cycle 1 (1994). Variable ABECS08. Questions:

a) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Seems to be unhappy, sad or depressed?
b) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is not as happy as other children?
c) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is worried?
d) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Cries a lot?
e) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Appears miserable, unhappy, tearful, or distressed?
f) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is nervous, highstrung or tense?
g) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Has trouble enjoying %him/her%self?
h) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is too fearful or anxious?


a) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Gets into many fights?
b) HOW OFTEN WOULD YOU SAY THAT %FNAME%: When another child accidentally hurts %him/her% (such as by bumping into %him/her%), assumes that the other child meant to do it, and then reacts with anger and fighting?
c) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Physically attacks people?
d) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Threatens people?
e) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Is cruel, bullies or is mean to others?
f) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Kicks, bites, hits other children?

4. Prosocial Behaviour Score in Cycle 1 (Variable: ABECS07) ages 4-11

The total score varies from 0 to 20, a high score indicating prosocial behavior

a) Using the answers never or not true, sometimes or somewhat true, or often or very true, how often would you say that %fname%: shows sympathy to someone who has made a mistake?
b) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Will try to help someone who has been hurt?
c) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Volunteers to help clear up a mess someone else has made?
d) HOW OFTEN WOULD YOU SAY THAT %FNAME%: If there is a quarrel or dispute, will try to stop it?
e) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Offers to help other children (friend, brother or sister) who are having difficulty with a task?
f) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Comforts a child (friend, brother, or sister) who is crying or upset?
g) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Spontaneously helps to pick up objects which another child has dropped (e.g. pencils, books, etc.)?
h) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Will invite bystanders to join in a game?
i) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Helps other children (friends, brother or sister) who are feeling sick?
j) HOW OFTEN WOULD YOU SAY THAT %FNAME%: Takes the opportunity to praise the work of less able children?
5. **Indirect Aggression Score** in Cycle 1 ages 4-11
The total score varies from 0 to 10, a high score indicating behavior associated with indirect aggression.

a) **HOW OFTEN WOULD YOU SAY THAT %FNAME%**: When mad at someone, tries to get others to dislike that person?
b) **HOW OFTEN WOULD YOU SAY THAT %FNAME%**: When mad at someone, becomes friends with another as revenge?
c) **HOW OFTEN WOULD YOU SAY THAT %FNAME%**: When mad at someone, says bad things behind the other's back?
d) **HOW OFTEN WOULD YOU SAY THAT %FNAME%**: When mad at someone, says to others: let's not be with him/her?
e) **HOW OFTEN WOULD YOU SAY THAT %FNAME%**: When mad at someone, tells the other one's secrets to a third person?

6. **Adult Health Depression Score**

a) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I did not feel like eating; my appetite was poor.
   1 RARELY OR NONE OF THE TIME (LESS THAN 1 DAY)
   2 SOME OR A LITTLE OF THE TIME (1-2 DAYS)
   3 OCCASIONALLY OR A MODERATE AMOUNT OF TIME (3-4 DAYS)
   4 MOST OR ALL OF THE TIME (5-7 DAYS)

b) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I felt that I could not shake off the blues even with help from my family or friends.

c) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I had trouble keeping my mind on what I was doing.

d) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I felt depressed.

e) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I felt that everything I did was an effort.

f) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I felt hopeful about the future.

g) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: My sleep was restless.

h) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I was happy.

i) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I felt lonely. FREQ WTD

j) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I enjoyed life.

k) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I had crying spells.

l) **HOW OFTEN YOU HAVE FELT OR BEHAVED THIS WAY DURING THE PAST WEEK**: I felt that people disliked me.