NBER WORKING PAPER SERIES

MATERNITY LEAVE AND CHILDREN'S COGNITIVE AND BEHAVIORAL DEVELOPMENT

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Working Paper 17105
http://www.nber.org/papers/w17105

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2011

We have benefited from comments from Josh Angrist and seminar participants at the 2011 AEA meetings, UC Davis, Laval, Manitoba, Stavanger, Texas, W.E. Upjohn and Waterloo. We gratefully acknowledge the research support of SSHRC (Baker Grant, #410-2008-0346, Milligan Grant #410-2006-0928). We thank the staffs of the Toronto and B.C. Research Data Centres for their technical support. This paper represents the views of the authors and does not necessarily reflect the views of Statistics Canada or the National Bureau of Economic Research.

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NBER Working Paper No. 17105
June 2011, Revised August 2014
JEL No. I18,J13,J32

ABSTRACT

We investigate the impact of maternity leave on the cognitive and behavioral development of children at ages 4 and 5, following up previous research on these children at younger ages. The impact is identified by legislated increases in the duration of maternity leave in Canada, which significantly increased the amount of first-year maternal care. Our results indicate no positive effect on indices of children's cognitive and behavioral development. We uncover a small negative impact on cognitive scores, which may indicate the timing of the mother/child separation due to the mother's return to work plays a role.

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Introduction

Governments around the world seek to improve child development at young ages as a means both to promote equity and to improve the efficiency of human capital accumulation. Policies range from maternal education and nutrition programs to childcare and early childhood education initiatives.[1] Parental leave policies have been central to this push in more developed countries.[2] In many cases, child development is cited as an important basis for legislative parental leave initiatives. The 'findings' preamble to the American *Family and Medical leave Act* states "…it is important for the development of children and the family unit that fathers and mothers be able to participate in early childrearing…".[3] A recent Australian paid parental leave program was promoted with the claim that "the scheme will give more babies the best start in life. The payment will enable more parents to stay at home to care for their baby full-time during the vital early months of social, cognitive and physical development" (Commonwealth of Australia 2009). An extension of paid maternity leave in the United Kingdom seeks to "…give children the best start in life…" as the "…evidence confirms the value of consistent one-to-one care in the first year of a child's life." (Employment Relations Directorate 2006, p. 2).

In this paper, we study a Canadian reform that expanded parental leave entitlement by about half a year in the first year of life. The reform had a large impact on the time mothers spent at home in their child's first year—about two months averaged across all mothers, or 3 months when averaged across likely eligible mothers.[4] We relate this variation in maternal employment to measures of children's cognitive and non-cognitive development at ages 4 and 5—immediately prior to school entry.

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[1] See, for example, the report prepared for the World Health Organization (Irwin, Siddiqi, and Hertzman 2007) or UNICEF (2006).
[2] The policies in OECD countries are documented in Ray (2008).
[3] See Public Law No. 103-3 §2(a)(2). http://www.dol.gov/whd/fmla/fmlaAmended.htm
[4] See Baker and Milligan (2008b, 2010) and Hanratty and Trzcinski (2009).

Earlier work by Baker and Milligan (2010) examines the effect of this same Canadian policy reform at ages up to 24 months. In that paper, we find a large shift toward maternal care and away from home-based care in the first year of life. This extra maternal care in the first year of life was found to have little impact on health and development indicators up to the age of 24 months. The measures studied in that paper are of parenting and of children's motor and social development, exhibited security, temperament and milestones of physical development (e.g., first steps). As the children affected by this large reform have grown older, we now have the unique opportunity to further evaluate their development as they hit the critical ages at which 'school readiness' can be assessed.

Examining the further progress of this cohort of children who received a substantially larger amount of maternal care is informative for a number of reasons.

1. While not common in economics, follow up studies have been critical to our understanding of the impact of early childhood interventions on child development. For example, the near universal acknowledgment of the profound impact of the Perry Preschool Study is due to the ongoing longitudinal study of the program's children that repeatedly comes to the same conclusion. In contrast, the uncertain lessons from the Head Start program are due to follow up study that suggests the program's initial impacts fade out over time.
2. It is widely acknowledged that the development of very young children is rapid and multidimensional, and that cognitive testing of very young children (2 years or younger) is very challenging (e.g., Bradley-Johnson 2001, Saye 2003). As a result, best practice counsels evaluating children with multiple assessments at different ages.

3. There is evidence from previous research that the developmental impact of maternal employment may not manifest until the child is aged 3 years or older (Brooks-Gunn et al. 2002).[5]
4. Indicators of cognitive and non cognitive development at ages 4 and 5 have well-documented links to future outcomes (e.g., Almond and Currie 2011).

Against this background, many studies of early childhood policies up to now are "one off" analyses. Therefore, we cannot know whether any documented effects fade out at later ages, or if evidence of no effect persists.

Several recent papers have investigated the long-run impact of parental leave expansions on education attainment and early labor market performance.[6] Dustmann and Schonberg (2012) look into changes to Germany's paid leave program that increased paid leave from 2 to 6 months in the late 1970s, and from 6 to 10 months in the mid-1980s.[7] They find little evidence that this expansion of leave affected children's selective school attendance or wages. Rasmussen (2010) examines an increase in paid parental leave, from 14 to 20 weeks, in Denmark in the mid-1980s. She finds no impact on children's high school enrollment and completion, or on grade point average. Liu and Skans (2010) investigate an extension of paid parental leave from 12 to 15 months in Sweden in the late 1980s. They report no average impact on children's test scores and grades at age 16, although there is a positive effect for the children of well-educated mothers. Danzer and Lavy (2012) look at the impact on age 15 test scores of a reform in Austria in 1990, finding no significant impact in the overall sample (although preliminary evidence points to a positive impact for boys born to highly educated mothers). Finally, and in contrast, Carneiro et

[5] Brooks-Gunn et al. (2002) find that the cognitive impact of maternal employment in the children's first year of life manifest by 36 months but not at earlier ages.
[6] A more detailed review of this literature can be found in Baker (2011).
[7] A third reform considered in the analysis is an increase in unpaid leave from 18 to 36 months in the early 1990s.

al. (2011) find the introduction of 4 months of paid leave in Norway in the late 1970s did have positive impacts on children's educational attainment; most notably a reduction in the high school dropout rate. This long-run evidence does not reveal consistent findings of a significant long-run impact of parental leave.

Research on the more immediate effects of maternal employment on child development during the early years has pointed toward a more negative impact.[8] A number of studies report negative impacts of maternal employment anytime in the first year on cognitive development, although the estimates vary in magnitude (e.g., Bernal 2008, Hill et al. 2005, James-Burdumy 2005 and Ruhm 2004).[9] Waldfogel (2006) in her review of the literature concludes "…children whose mothers work in the first year of life, particularly if they work full time, do tend to have lower cognitive test scores at age three and thereafter." (p. 55) There is also evidence that entrance into non parental care in the first year can have negative cognitive and behavioral effects (e.g., O'Brien Caughy et al. 1994, Gregg et al. 2005, Lefebvre et al. 2008, Loeb et al. 2007).[10]

[8] Lucas-Thompson et al. (2010) provides a summary of some of the research cited here as well as of studies on the developmental impact of maternal employment from other fields.

[9] Ruhm (2004) reports reductions in PPVT scores of 7-8 percent of a standard deviation from maternal employment in the first year, with the largest effects from full time employment. James-Burdumy (2005) and Hill et al. (2005) find maternal employment in the first year has smaller negative effects on math and reading scores measured at ages 5-18 (PPVT and the Peabody Individual Achievement Tests). Finally, Bernal (2008) reports that a full year of full time maternal employment in the first five years of life reduces test scores by 0.13 of standard deviation (PPVT and the Peabody Individual Achievement Tests.

[10] O'Brien Caughy et al. (1994) report that entrance into daycare before the first birthday was associated with higher test scores (Peabody Individual Achievement Tests) for lower income children and lower test scores for higher income children. For the U.K. Gregg et al. (2005) find that children who receive informal care from friends and relatives in the first 18 months of life combined with full time maternal employment have lower cognitive outcomes. In the Canadian context, Lefebvre et al. (2008) report that Quebec's universal, low fee childcare program, which serves children from birth, is related to reductions in PPVT scores of just under one-third of a standard deviation. Finally, Loeb et al. (2007) find that entry into non-parental center based care before the age of one can lead to problem behavior. Magnuson et al. (2007), Baker et al. (2008), Datta Gupta and Simonsen (2010), and the research summarized in Belsky (2006) provide further evidence that non-parental care can have negative behavioral effects in some contexts.

Clearly, the sum of this research yields a mixed message. From it, one could construct a research base showing that increased maternal care at young ages alternatively has a positive or no effect. There may be a consistent narrative that links the long and short term research, but uncovering it would require study of the children in the long term research at younger ages, or of the children in the short term studies at older ages.

Four specific features of our analysis enhance its relevance. First, the extension of leave from 26 to 52 weeks is informative for the many OECD countries that currently have short (i.e., 12-39 weeks) maternity leave entitlements (see Ray 2008). Second, the income replacement, provided through the Canadian Employment Insurance system, is relatively modest. It is comparable to the benefits provided in many jurisdictions including Australia and the United Kingdom, as well as the paid leave programs in California and New Jersey.[11] Third, as we discuss below, developmental psychologists observe that some key developmental milestones are achieved in the second 6 months of life. Finally, our primary measure of cognitive development, the Peabody Picture Vocabulary Test (PPVT), is the workhorse of research on the impacts of maternal employment. Because so much research has used this same measure, we can compare our results directly to the existing research that has been cited as a rationale for maternity leave reforms.

Our analysis offers several interesting findings. We find that the expansion of parental leave—and the resulting extra time mothers spent with their child in his/her first year of life—had no positive impact on indices of children's cognitive and behavioral development at ages 4 and 5; this despite the fact it had substantial impacts on the maternal care and non-licensed non-parental care children received in their first year, as well as how long they were breastfed. For

[11] See National Partnership for Women and Families (2012) for a comprehensive description of state-level initiatives across the United States.

our behavioral indices we can rule out all but very modest improvements. For some of our cognitive measures (PPVT) the estimated impact of the reform is small, negative and statistically significant. These results provide precedence for the findings that maternity/parental leaves have no long run impact on children's cognitive achievement at much older ages.

Our finding of small negative effects on some cognitive scores leads to a discussion of how parental leave affects not just the duration, but also the timing, of the mother's return to work.[12] We speculate that it may be due to the fact that some ages are better than others for abrupt changes in the parent-child relationship.

To proceed, we first describe the parental leave reform that forms the basis of our analysis. We next provide the details of our empirical framework and the data we employ. Following the presentation of our main estimates, we bring evidence on the exclusion restriction that underlies our empirical framework and empirically address the impact of some potential confounding factors. The paper closes with a discussion of our findings within the context of the duration and timing of parental leave, and a brief conclusion.

The Reform

Our analysis is based on a reform of Canadian maternity/parental leave (henceforth maternity leave) laws at the end of 2000.[13] Job-protected, uncompensated, maternity leave is provided by provincial labor standards laws, and historically there has been some variation in its

[12] Several papers look at the impact of timing of the return to work. (See Baydar and Brooks-Gunn (1991); Han et al. (2001); Brooks-Gunn et al. (2002); Baum (2003); Berger et al. (2005). Lucas-Thompson et al. (2010) provide a review of studies tracking the timing of return to work.) The novelty we present is interpreting our results of the impact of parental leave through how it might change the timing of the return to work.

[13] Many of the details are reported in Baker and Milligan (2008a).

duration across provinces. In contrast, income replacement during the leave is provided through the federal Employment Insurance (EI) system and there is one standard for the country.

Before December 31, 2000, the duration of income replacement in the EI system was 25 weeks, subject to a 2 week waiting period in which no benefits are received. This comprised a 15 week leave reserved for the mother and a 10 week leave that could be shared between the parents. For children born December 31, 2000 and onwards, the shared 10 week component of the leave was expanded to 35 weeks, bringing total available leave to 50 weeks.[14]

The legislation enacting the changes was introduced to Parliament on April 7, 2000, and received Royal Assent on June 29, 2000. Over the next six months most provinces announced increases in the duration of job-protected leave to 52+ weeks to commence coincidently with the change in the EI law.[15] In many instances the change was not announced or enacted until November or December 2000.[16] By June 2001, all provinces offered job protection of sufficient duration to accommodate the new 50 week EI standard.[17] The change by province is presented in table 1. While there is some slight variation across provinces, the impact of the reform is mostly time series variation before and after 2000/2001.

Only mothers with sufficient attachment (600 hours over the last year) to the labor force at the time of application for leave are eligible. Survey evidence (Statistics Canada 2006) for the 2000 to 2005 period suggests that between 70 and 75 percent of mothers with a child age 0 had

[14] Historically, mothers have taken the vast majority of the leave, although this is (slowly) changing in recent years. Marshall (2008) reports that in 2006, 23 percent of eligible fathers took some parental leave. This average reflects incidence of 56 percent in Quebec (where there is dedicated leave for fathers) and 11 percent in the rest the country. As documented below, observations from Quebec are deleted from our analysis sample.

[15] The changes in provincial mandates were from 29-35 weeks to 52-54 weeks with the exceptions of Alberta, where the change was from 18 weeks to 52 weeks, and Quebec, where the entitlement did not change from a level of 70 weeks.

[16] For example, as late as October 2000 Ontario did not appear on track to make the change but did eventually in December due to public outcry.

[17] Two provinces, Saskatchewan and Alberta, did not change their job protected leave standards until 2001. Unfortunately there are not sufficient observations from these provinces over the 2-6 months of delay to take advantage of this feature of the reform.

insured employment before childbirth. Of these, between 80 and 85 percent make a claim. These percentages show a slight increase from 2000 to 2005 that suggests only a small change in the composition of mothers who take leave before and after the reform (see also Baker and Milligan 2008a).

The Data

The National Longitudinal Study of Children and Youth (NLSCY) is a nationally representative survey of Canada's children. The data we use are based on a longitudinal survey of successive birth cohorts of children up to age 5. The survey is conducted in biennial cycles, so cohorts are interviewed at ages <1, 2, 4 or ages 1, 3, 5. To account for sample attrition, a new top-up sample is added in each cycle to ensure each cycle provides a representative sample of children aged 0-5. There are approximately 2,000 children of each age in each cycle. The 2008/09 data are the final cycle of the survey.

The survey offers three measures of the cognitive development of children aged 4-5. Each of these measures comes from an existing validated model and is comparable to measures used in other studies. The first measure is the Peabody Picture Vocabulary Test-revised (PPVT-R), which has been used extensively in previous studies of child development and is well known in the literature. The second is the Number Knowledge Test, which was developed by a team led by Robbie Case at the Ontario Institute for Studies in Education (Case et al. 1996). The test consists of 30 questions that are used to rank children on a four point scale.[18] It assesses children's understanding of the system of whole numbers, probing their ability to count by rote, quantify small sets of objects, their knowledge of number sequence and their ability to solve

[18] The scale is: 0—the child has not reached the predimensional level, 1—the child has reached the predimensional level (4 year old equivalent), 2—the child has reached the unidimensional level (6 year old equivalent) and 3—the child has reached the bidimensional level (8 year old equivalent).

simple arithmetic problems. The questions and answers are delivered orally, and no aids (e.g., pencil and paper) are allowed. Unfortunately the raw and standardized scores on the 30 questions are only available for later cycles of the NLSCY. The third is the "Who am I?" measure, a test developed by a team led by Molly de Lemos at the Australian Council for Educational Research (de Lemos and Doig, 1999). It consists of copying and writing tasks that help reveal children's understanding and use of symbols. The copying exercises are intended to assess abilities in geometry and the writing tasks are intended to investigate knowledge of the use of numbers, letters and words. The test consists of 10 questions that are each awarded scores between 1 and 4. The overall or total score on the instrument is simply the sum of the scores on the individual questions and therefore ranges in principle between 10 and 40.

We also investigate a number of non-cognitive/behavioral indices. These indices are parent-reported measures based on best practices in the literature.[19] These indicies measure hyperactivity, anxiety, physical aggression and indirect aggression. Each index is built up from a series of questions about the children's reactions to other people and different situations. For each index a higher score implies more problematic behavior. Parent-reported indices are not without their critics. The online appendix to Baker et al. (2008) provides a detailed discussion of these measures.

We select children aged 4 and 5 born in the years 1997-2004. These birth years bracket the changes to the maternity leave laws, yielding four pre-reform cohorts and four post-reform

[19] The measures are based on questions drawn from the Ontario Child Health Study, the Montreal Longitudinal Survey, and the Child Behavior Checklist of T. M. Achenbach. The Ontario Child Health Survey questions are based on items in the Child Behavior Checklist (Achenbach and Edelbrock 1983), modified so that that the symptoms canvassed correspond to the classification of psychiatric disorders in DSM-III-R (the Diagnostic and Statistical Manual of Mental Disorders of the American Psychiatric Association) (Boyle et al. 1993). The questions in the Montreal Longitudinal Survey are based on the Social Behavior Questionnaire. This includes 28 items from the Preschool Behavior Questionnaire (Behar and Stringfield 1974; Tremblay et al. 1992), an adaptation of the Children's Behavior Questionnaire (Rutter 1967) and the Prosocial Behavior Questionnaire (Weir, Stevenson, and Graham 1980; Weir and Duveen 1981).

cohorts. Our objective here is to choose cohorts that are temporally adjacent to the reform to control as much as possible for unobserved time effects. Each birth cohort is surveyed three times between the ages of 0 and 5.[20] Because the survey is conducted biennially, this means children from adjacent birth cohorts are of different ages in their first, second and third surveys. For example, the children of the 2000 birth cohort are surveyed at ages <1, 2 and 4, while the children of the 2001 birth cohort are surveyed at ages 1, 3 and 5.

We omit all observations from Quebec, to account for the fact this province's universal, low fee child care program was extended to children under the age of two in the fall of 2000, and so its effect might be easily confused with the effects of change in maternity leave laws. We also omit children who live in single parent households because concurrent changes in Canada's system of child tax benefits, which disproportionately benefited these families, might confound the inference.[21] Therefore, our results are for children in two parent/adult households, who are the majority beneficiaries of universal maternity leave policies.[22] Our sample includes both mothers who were eligible for the leave and those who might not be eligible. Eligibility depends on the number of hours worked in the 12 months before a claim is made, and we do not observe this information in our dataset.

An overview of our developmental measures for our sample is provided in table 2. We present the sample means for each measure as well as the means for the pre- and post-reform children. For each of the cognitive measures there is a decrease in the index with the reform of between 6 (PPVT) and 13 (Who Am I?) percent of a standard deviation. For the behavioral

[20] To keep the sample representative in a given survey, a top up sample is added to replace lost sample members of the longitudinal sample.
[21] The proportion of children age 0-5 living with two parents in 2000 in the Labour Force Survey is 92.2%. Single-parenthood is much less prevalent in Canada than the United States.
[22] Alberta and Saskatchewan did not change their maternity leave provisions to match the change in the federal EI rules until after December 2000. We therefore also exclude the very small number of children born in Alberta and Saskatchewan in the months between December 2000 and the point when the provincial maternity leave mandate changed a few months later.

measures, in the reform period there is a decrease in both aggression indices and an increase in anxiety and hyperactivity. For these measures the changes are generally smaller—4-6 percent of a standard deviation.

We also use data from the Labour Force Survey (LFS) for the analysis of our exclusion restrictions presented after the main results. The primary purpose of this monthly survey is to collect information on the labor force status of Canadians. We make use of the data on labor force status, as well as questions on reasons for not actively seeking work, weekly and hourly earnings and family structure. We again exclude observations from the province of Quebec and from single-parent households.

Age in years is recorded in the LFS, but exact date of birth is not available. Also, single year age categories are available for ages 0, 3, 4 and 5, but not for ages 1 and 2. Our methods for identifying birth year for the analysis of the LFS are reported in the Appendix.

Empirical Framework

We want to estimate the impact of maternal time on children's developmental outcomes. To fix ideas, consider the empirical model

$$D_i = \alpha + \beta T_i + u_i,$$

where D_i is the developmental outcome, T_i is the period that the mother provides care after birth and u_i is unobserved determinants of the outcome. As has been noted in many previous papers, OLS estimation of this equation is unlikely to retrieve the causal effect of mother's time on development because $E[T_i u_i] \neq 0$. For example, mothers who return to work early (low values of T_i) may have unobserved characteristics that lead their children to have better or worse developmental outcomes. We therefore need to find exogenous variation in T_i to obtain unbiased

estimates of β.

Our approach is to use the variation in time at home induced by the maternity leave reform in an instrumental variables framework. Our first stage equation is

(1) $TFY_i = X_i\phi + Z_i\delta + v_i$,

where TFY_i, the number of months the mother is home with her child in the first year of life, now replaces T_i as a measure of maternal care,[23] *X* are control variables and *Z* is the instrument based on the reform of the maternity leave system.

We have two candidates for *Z*: 1) a dummy variable indicating that the child was born after the change in maternity leave provisions came into effect (i.e., December 31, 2000), and 2) the number of weeks of job protected maternity leave mandated by a province's labor standards law when the child was born, as reported in table 1. Relative to the first instrument, the instrument based on weeks of mandated leave potentially exploits inter-provincial differences in pre reform maternity leave provisions. However, if the EI entitlement was greater than the local job-protection mandate, it is possible the mandates were not well-enforced or followed. Moreover, the R-squared from a regression of the provincial leave mandates on year effects is 0.88; and the addition of province effects raises the R-squared to 0.95. This suggests that our first instrument captures most of the variation in the second. For this reason, in addition to the above-noted issues with the observance of provincial statutes, we therefore rely primarily on the first instrument in the analysis.

Our second stage equation is

(2) $D_i = X_i\lambda + TFY_i\varphi + \varepsilon_i$.

Our specification of *X* is partly directed by any residual concerns about omitted variables in the

[23] The substitution of TFY_i for T_i provides a neat solution for observations with censored values of T_i by limiting the time frame to one year. For example, for children surveyed at older ages, the observation of T_i may be censored because the mother is still at home at the date of the survey.

estimation of (2). In particular, the variation in our primary instrument is perfectly correlated with time. Therefore, it is important to control for any secular trends in the developmental outcomes across birth cohorts that might contaminate the estimates. Our main strategy to address this concern is to use polynomials in time defined at the quarter of birth level. Children born over the eight birth cohorts in our sample (1997-2004) span 32 quarters. We also experiment with other specifications of these time effects and present graphs of the variation in key variables. The other control variables are dummy variables for male children, month of birth, single month of child's age at the survey date, a dummy variable for whether the child is bilingual, dummies for the number of older and younger children, province, city size, and the following characteristics of mothers' and fathers': education (4 categories), age (single year), and place of birth (19 countries).[24]

Note that some additional potential sources of bias are directly addressed by our choice of sample. We omit observations from Quebec and for single parent households due to changes in other policies that might affect developmental outcomes and be picked up by our instruments.

One assumption—the exclusion restriction—of our instrumental variables strategy is that the reform of the leave provisions affects child outcomes only through the resulting change in mothers' time at home post birth. At least two dimensions of this assumption deserve comment. First, the exclusion restriction means that any other maternal input (such as breastfeeding—see Baker and Milligan 2008a) that may change because of the extra time at home will be picked up by TFY_i. That is, TFY_i will reflect not just the direct effect of extra maternal time, but also any 'downstream' impacts of the extra maternal time on other inputs. In this sense, care must be

[24] The regressions for age standardized PPVT scores omit the month of birth and single month of child's age. The results including these controls are reported in footnotes below. We control for place of birth to account for any differences in immigration patterns. Over this time period, however, there were no strong shifts in the class or source of immigration to Canada.

taken in interpreting the coefficients on TFY_i. Second, we clearly assume the reform had no impact on child development by some other conduit. Our direct evidence of the impact of the reform on other developmental inputs, reported below, provides support for this position.

The other important assumption for our IV strategy is monotonicity—that the instrument has a monotonic effect on mothers' time at home. While in theory it is possible that an increase in parental lead entitlement could lead some mothers to take longer leaves and others to take shorter leaves (Klerman and Leibowitz 1997), empirically the latter effect is not important for the reform we examine (Baker and Milligan 2010). Furthermore, in the current context, theory predicts that mothers taking shorter leaves due to the leave reform would, pre reform, have taken leaves in excess of one year. Therefore any negative impact of the leave reform on mothers' leave would be in the interval $>$ 12 months. Our empirical strategy focuses on the increase in leave taking due to the reform in the interval 0 through 12 months.

A visual depiction of our identification strategy is provided in figures 1 and 2. Here we graph the variation in our measure of maternal care, TFY_i, and our most well-known outcome, PPVT, across the reform.[25] More specifically we graph estimates of year of birth effects from a regression of TFY_i or PPVT on our demographic controls, year of birth effects and no constant.[26] The cohorts 2001-2004 are exposed to the new regime. Therefore, these figures provide a view of the variation identifying our first stage and the reduced form.

In figure 1 there is clear evidence that the reform increased the amount of time mothers' were at home in the first year. A regression of the year of birth effects on a constant and a dummy variable for the reform reveals the pre/post reform increase is 2.218 (0.168) months. Evidence in Baker and Milligan (2008a) indicates between 65 and 75 percent of women were

[25] The sample for both TFY_i and PPVT is children aged 4 and 5 with valid PPVT scores.
[26] The polynomials in quarter are not included here given the specification of year of birth effects.

eligible for the leave over this period, leading to the estimated impact for treated women of between ((1/0.75)*2.218) 2.96 and ((1/0.65)*2.218) 3.41 months.

We relate this increase in TFY_i to any corresponding change in our development indicators. In figure 2 we can clearly see there is no evidence of a corresponding improvement in PPVT scores. In fact, it appears that these scores are marginally lower after the new leave provisions. The estimate here of the pre/post reform change is -1.511 (0.206) points.

Given the time series variation available to us, it might seem natural to implement a regression discontinuity (RD) design. We do not take this approach for a number of reasons. First, the sampling design of the NLSCY prevents a straightforward application of the RD identification strategy to our problem. As noted above, in the NLSCY children of adjacent cohorts are of different ages in their first, second and third interviews. More specifically, the children of the last pre-reform cohort (2000) were age 4 when our developmental indicators were recorded, while the children of the first post reform cohort (2001) were age 5. Therefore a strict regression discontinuity estimator would rely on the age standardization of the developmental scores being correct. Rather than relying on this being true, we sample multiple cohorts symmetrically before and after the reform so that any age effects average out.[27] Second, and related, the sample sizes in the NLSCY, while large for a children's survey, are relatively modest compared to the European administrative registers that are the basis of much of the RD evidence on the long run effects parental leaves. Specifically, the samples in the months directly before and after the reform are small, so our strategy also allows us to bring sufficient sample sizes to the estimation. Third, the evaluation of early childhood programs using a strict RD design, which compares children born months or even days apart, leans very heavily on the assumption that

[27] For example, using the 1999 and 2000 births pre reform and the 2001 and 2002 cohorts post reform ensures that the development measures for all children in both the pre and post reform samples were recorded at age 4 or 5.

program impacts are not offset by the remedial actions of schools and parents. However, in a group of children born in such close temporal proximity, comparisons of pre- and post-reform children could be easily made and would be an obvious reference point for parents and teachers attempting to discover the developmental progress of a child. Parents appear to use other children and their parents as reference points to evaluate the development of their own children (e.g., Glascoe and MacLean 1990). Also, in clinical application parents' evaluations are found to be more useful when they are asked to compare their child to other children (Deimann and Kastner-Koller 2011 and references therein). While our approach is not completely free of these concerns, by sampling multiple cohorts before and after the reform the impact of any such remedial action is diluted relative to its impact in a strict RD design. Furthermore, we observe the children before the remedial opportunities in formal schooling are made available. Finally, we carefully document any differences in developmental inputs provided to pre- and post-reform children at ages 0 through 5 years.

Two-Stage Least-Squares estimates of the impact of Mother's Care on Developmental Outcomes at Age 4 and 5

We now present our main results. First are our Two-Stage Least-Squares (TSLS) estimates, and a demonstration of how they vary with different specifications of the time effects and in different samples. Next, we gather evidence on the exclusion restriction that assumes time at home is the channel through which maternity leave has its impact. Finally, we examine how our results vary with and without controls and investigate the influence of the control variables on our analysis.

In table 3 are our main TSLS estimates of the impact of maternal care from the sample of birth cohorts 1997 through 2004. In column 1 are the results using the dummy variable for treatment as the instrument and conditioning on the demographic controls and the quartic in time measured at the quarter of birth level. The corresponding estimates for the instrument from the first stage are reported in the preceding column. The instrument is very significant in each first stage regression.

The estimate for PPVT is negative and statistically significant at the 10 percent level. It indicates a one month increase in maternal care over the range we study leads to a decrease in this score of 4.8 percent of a standard deviation.[28] Recall that in the discussion of figures 1 and 2 we report estimates of both a first stage and a reduced form for the pre/post variation in the year of birth effects for TFY_i and PPVT based on our treatment dummy variable. They are 2.218 months and -1.511 points respectively. The ratio of this reduced form to the first stage is -0.681, not far off the point estimate of -0.739 in table 3 that is obtained using a different specification of the time effects. Also, the estimate of the first stage matches well with the estimate in table 3. The result for Who Am I? is also negative and statistically significant at the 10 percent level. It indicates a one month increase in maternal care leads to a decrease of 4.7 percent of a standard deviation. Finally, the estimate for Know Your Numbers is positive, again statistically significant at the 10 percent level, and indicates an increase in this score of 5.2 percent of the standard deviation.

The estimates for the behavioral measures are uniformly small and statistically insignificant. In each case the estimated standard error is 2.9 percent or less of the standard deviation of the corresponding measure. This suggests the power to detect changes in the

[28] Adding controls for the child's age and month of birth the estimate is -0.415 (0.486). As we report below (footnote 31) this change results from a change in the estimate for girls moving to this specification.

indicators of at least 5.7 percent of a standard deviation at the 5 percent level. Therefore, any improvement in behavior undetected by our empirical strategy would by implication be quite small.

In columns 2 through 4 we experiment with the control for time effects, by specifying, in turn, a cubic in time, a linear trend in time specified separately for the pre and post reform time periods, and a quadratic in time specified separately for the pre and post reform time periods.[29] The point estimates for PPVT are robust to these innovations although for the pre/post reform quadratic specification the standard errors are much larger. There is more sensitivity for both the point estimates and the statistical significance the other two cognitive measures. Finally the behavioral measures vary in sign and magnitude across specifications, but almost all the estimates are statistically insignificant.

In column 5 we use our second instrument, weeks of mandated job protected leave, which varies at the provincial level. In the preceding column are the estimates for this instrument from the first stage. As explained above, this instrument exploits variation in the change in leave mandate by province (see table 1). The estimated impacts for PPVT and Who Am I? are now larger, but so are their standard errors. The estimate for Know Your Numbers is now negative, but very small. The results for the behavioral measures are in general quite different than their counterparts in column 1 although all remain statistically insignificant.

Overall the estimates for PPVT in table 3 display a good amount of stability across the specifications. The estimates for Who Am I? display a bit more sensitivity, although none is positive. The estimates for the other measures vary more significantly across columns, which

[29] The second stage collapses when we specify a quintic in time. The model is not identified when we use quarter of birth dummy variables since they perfectly predict the instrument.

might be expected given their relatively larger standard errors and lack of statistical significance.[30]

We next examine estimates from a tighter sample around the reform using children born in 1999 through 2002. These estimates are potentially less affected by any secular differences between pre and post reform cohorts, but this benefit comes at the cost of a reduction in sample size of about one-half. The results are reported in table 4. The estimates for PPVT are very similar to the estimates in the previous table and all statistically significant at the 5 percent level. The estimates for both Know Your Numbers and Who Am I? are now of opposite sign of the results in table 3, but in both cases the standard errors are much bigger. For the behavioral measures the standard errors are again much larger and while the point estimates don't always match their counterparts in table 3 are statistically significant. The exception to this conclusion is the results for Hyperactivity, which are negative now statistically significant at the 10 percent level in some specifications.

To try to shed more light on the meaning of the statically significant impacts on some of the cognitive scores in table 3, in table 5 we report estimates separately by sex. It is clear that the negative impact on PPVT is experienced exclusively by boys, while any positive impact on Know Your Numbers is experienced by girls.[31]

Considering the sum of evidence in tables 3-5, the estimates for most outcomes are largely statistically insignificant, typically small, and show some sensitivity to the specifications of the control for time and of the instrument. The exception to this conclusion is the results for PPVT which are more robust, typically statistically significant and consistently negative.

[30] The number of observations varies across the different outcomes. Restricting the sample to children with valid observations on all outcomes leads to similar estimates.
[31] Controlling for child's age and month of birth the estimate for males is -1.443 (0.743) and for females, 0.953 (0.672).

Testing for other channels of impact

A strength of our study is that we can assess the identifying assumption that no other changes in developmental inputs are correlated with our instrument. Bernal and Keane (2011) note that few empirical investigations of the relationship of current developmental outcomes to past environments consider the cumulative impact of past developmental inputs. This is likely because measures of past inputs are not available in most data sets.

In this section, we test the validity of our exclusion restriction by investigating differences in developmental inputs between pre and post reform birth cohorts in the years intervening the first year and the age at which our developmental outcomes are measured. We argue that this is a key step to connect the leave reform to the cognitive and non-cognitive outcomes at ages 4 and 5. As noted in the Introduction, any relationship between leave reforms and later life outcomes may be accentuated or offset by the remedial actions of parents and schools. These actions might be expected if due to the timing of the reform, pre and post reform children were in the same or adjacent grades. Note it is precisely this group of children that are most "informative" in regression discontinuity analyses of leave reforms.

In Baker and Milligan (2010), the care arrangements of children at ages 13-24 months were found to be little changed for the cohorts born after 2000, even though their care arrangements up to 12 months were sharply different than for earlier cohorts with less access to leave. Here, we follow up and extend that analysis to study the income and labor supply patterns in the time following the first year of life.

To proceed we use both the NLSCY and the LFS. Because we do not observe TFY_i in the LFS, we cannot use our instrumental variables strategy for these data. Moreover, we cannot see quarter of birth, so our strategy for the LFS can only exploit cross-year of birth variation. For

these reasons, we investigate any differences in developmental inputs across birth cohorts using a different empirical strategy than the IV strategy we use for the analysis of cognitive and behavioral development that follows.

For both the NLSCY and the LFS, we estimate the reduced form relationship between the inputs and our instrument following a two-step procedure outlined in Baker and Milligan (2010). Briefly, we regress the developmental input on demographic controls and a full set of year of birth effects omitting the intercept. In the second step we regress the 8 estimated year of birth effects from the first stage on an intercept and the instrument—a dummy variable that equals one for birth cohorts exposed to the new maternity leave provisions. The estimates of the parameter on this dummy variable reported below reveal the average difference in the dependent variable between pre and post reform birth cohorts.

We start with the NLSCY data, investigating measures of maternal care in table 6. The first row contains results for TFY_i at different ages between 13 and 71 months. The samples at different ages do not contain all the same children, so the estimated time at home in the first year may change somewhat. However, absent some bias due to cohort attrition and the survey re-sampling procedures, the estimates of the increase in time at home in the first year should be similar when measured at the different points of age. They are in fact very similar varying within a quarter of a month. The estimated year of birth effects underlying these results are reported in table A1 of the appendix. For each group there is a clear break in the estimates between the 2000 and 2001 birth cohorts corresponding to the reform.

In the next row are the results for a 0/1 indicator that the mother has returned to work by the indicated age. At ages 13-24 there is a marginal but not statistically significant decrease in

the proportion who have returned to work. At ages 25-39 and 48-71 months the point estimates are effectively zero and statistically insignificant.

Baker and Milligan (2010) report that the reform had little impact on the non parental care arrangements of these children at ages 13-24 months, this despite the fact their care arrangements up to 12 months were sharply affected. Unfortunately we are unable to extend this analysis to ages 25-71 months due to sampling problems in the non parental care section of the NLSCY in cycle 6 and a revamp of this section of the survey in cycles 7 and 8.[32]

In rows 3 and 4 of table 6 we examine the family circumstances in which the child grew up, through the presence of older and younger siblings. There is little evidence here that the reform had a significant effect on the spacing of births or total fertility to this point in this sample.[33] The estimates for all ages are statistically insignificant. Finally in the last row we report the results using a self reported depression score for the mother as a dependent variable. Chatterji and Markowitz (2005) report a positive effect of maternity leave on mothers' maternal health although at lower durations than the ones under study here. While the estimates for this variable are negative—indicating lower depression—they are all statistically insignificant.

The NLSCY does not provide good information on economic outcomes contemporaneous with the survey such as employment and income. This is in part due to the fact that the survey

[32] Cycles 6-8 are the source of information on the non parental care of post reform children at these older ages. These sampling issues make the comparison of these responses to the responses of pre reform cohorts from earlier waves problematic. The issues include a computer glitch that led to missing values for one-quarter of children in cycle 6.

[33] We investigated the potential impact of differential fertility on the composition of our sample by looking at the fraction of childless couples, the average number of children among those with children, and the fraction of those having a child who are married. No differences in the trend after 2000/2001 was observed.

questions about these outcomes do not have a fixed reference point. [34] We therefore turn to the LFS to investigate these and other inputs.

The results for the LFS are reported in table 7, while the underlying year of birth estimates are reported in Table A2 of the appendix. Note that the age groupings are slightly different than those in table 6 but span the same interval from 13 to 71 months.[35] In the first 4 rows are the results for labor force status as of the survey date. While there is little evidence of an impact of the reform on mothers' employment, there is a statistically significant, but small increase in the probability the employment was full time when the child was between 13 and 59 months of age. Here, full time is defined as working 30 hours per week or more. Note, however, that there is but a faint echo of this result in the estimates for usual weekly hours of work which are all positive, but most less than one hour per week.[36] We note that this NLSCY and LFS evidence that the increase in maternity leave had little impact on mothers' post birth employment is consistent with the results in Baker and Milligan (2008b) and Schonberg and Ludsteck (forthcoming).

The next rows contain the results for real earnings. The results for ages 13-35 months and 36-47 months are after the period of paid leave has ended, but prior to the measurement of developmental indicators. At these ages there is evidence of an increase in family real earnings of around $60 per week, although not in mothers' real earnings or wages. At ages concurrent with the testing there is evidence of an increase in both mothers' and families' real earnings.

[34] In the case of income, the survey respondent (the person most knowledgeable about the child) supplies information for each member of the family. The reference period for the report is the previous 12 months, which is not a calendar year and varies across respondents depending on which month of the year they are interviewed.
[35] As noted earlier, the year of birth is not directly reported in the LFS. Instead, we identify the year of birth for children by selecting a sample of children in December of each year. The regressions also include controls for province, urban/rural residence, mother's and spouse's age and education.
[36] The result for age 36-47 months does echo the larger point estimate for hours for the 25-39 months age group in the NLSCY data

We create real earnings by converting the earnings reports to 2002 dollars using the Consumer Price Index. In the presence of a general upward trend in wages across years, our estimates here based on just the time series variation will attribute to the policy what is really just a trend in real wage growth. For this reason, we have also re-estimated these regressions deflating earnings by the growth in the Industrial Aggregate Wage from Statistics Canada's *Survey of Employment Payrolls and Hours* (catalogue 72-002-XIB). As can be seen in Appendix table A3, using these wage-growth adjusted earnings tells a somewhat different story. They indicate a smaller (by half) increase in family earnings at ages 13-35 months, and little increase at older ages.

If the Consumer Price Index-adjusted results are to be given greater weight there is evidence of a modest increase in family resources for the post reform birth cohorts concurrent with the measurement of the developmental indices that we study. We might expect this difference in family resources to lend a small positive developmental advantage to the post reform cohorts. This would attenuate the negative impact on PPVT we observe in figure 2. However, if instead the wage adjusted results are more informative, there is little earnings advantage for the post reform cohorts.

In the next 5 rows of table 7 are measures of maternal care that are available in the LFS.[37] First up are indicators that the mothers of these children work part-time, are not available for work or not looking for work because they are caring for their own children. We see little evidence here of systematic and significant differences in how the children were cared for at ages 13 through 71 months.

Next are variables that capture whether the mother has been without work since the child was born. To construct these variables we compare the year the mother last worked to the child's

[37] The LFS does not provide any direct information on whether the child is in non-parental care.

year of birth. For the "year before birth" (YBB) variable we code the mother as not working since giving birth if the date of last employment is the year before the year of birth. For the "year of birth" (YOB) measure we code the mother as not working since giving birth if the date of last employment is the same as the year of birth. Using the YBB method we miss some mothers who have in fact stayed at home since their child's birth. Using the YOB method we code some mothers as having stayed at home, who might have instead returned to work for a short period post birth. That said, the two methods lead to very similar conclusions. There is a very modest increase in staying at home at ages 13-35, and no evidence of an impact of the reform at older ages. Note the estimates for ages 13-35 are of similar magnitude to, but opposite sign of, the estimates for the probability the mother has returned to work for this age group in the NLSCY (table 6).

Finally, in the last two rows we look for changes in family structure through the presence of younger or older siblings. Echoing the results from the NLSCY, there is no evidence here of an impact of the reform on the fertility decisions of mothers in our sample.

The results in table 6 and 7 tell largely the same story. There is little systematic evidence in either the NLSCY or LFS data of differences in observable inputs to child development between the pre- and post-reform birth cohorts at ages 1 through 5. This is consistent with our exclusion restriction: the leave reform had concurrent effects on the amount of maternal care children received at ages 0-12 months, but did not affect other inputs across birth cohorts at older ages. That is, our results on cognitive and behavioral measures can be attributed with some confidence directly to the increase in maternal care in the first year of life and are not observably contaminated by changes in other inputs after 12 months.

Robustness Analysis: Trends in Observable Characteristics

By varying the specification of the control for time and also the sample, we investigate whether our results are sensitive to how we account for unobserved differences between the pre and post reform cohorts. In this section we examine any changes in observable characteristics between the pre and post reform cohorts. As noted above, tables 6 and 7 show there are few differences in the observable developmental inputs provided to the children between ages 1 and 5. We now focus on changes in the characteristics of their parents and households.

To start the analysis, in column (1) of table 8 we report the results when we omit all the control variables from the regressions except those for the child's age. Beginning with the cognitive measures, each estimate is negative and statistically significant. The estimate for PPVT is just over half the size of many of the estimates in table 3. The result for Know Your Numbers contrasts with mostly positive estimates in table 3. For the behavioral outcomes there are now negative and statistically significant estimates for aggression and indirect aggression. The results for anxiety and hyperactivity are both a bit bigger than estimates from table 3 but statistically insignificant.

Focusing on the result for PPVT, the difference between the conditional and unconditional estimates suggest there is some observable characteristic of the children's families that is 1) correlated with the reform, and 2) correlated with the outcome variables (e.g., positively correlated with PPVT). To investigate we run OLS regressions of each of the demographic characteristics on dummy variables for year of birth. The results indicate that there are changes in mothers' and fathers' education that appear correlated with the reform. The estimated year of birth effects for the proportions of mothers and fathers with a high school diploma and a university degree are reported in table 9. The results indicate a reduction of the

proportion of mothers with a high school diploma, and a corresponding increase in the proportion with a university degree of roughly 8 percentage points with the 2001 birth cohort. Corresponding but smaller changes in the educational attainment of fathers are also apparent.

One survey issue complicating the interpretation of these results is changes in the questions about education. First in cycle 5 a new open ended category "other education or training" was added to the question about post secondary attainment. Second in cycle 7 the number of categories recording educational attainment was increased. Also, for the first time parents of children who were being followed longitudinally were invited to update their educational attainment. In cycle 5 are the initial interviews of the last pre reform (2000) and first post reform (2001) birth cohorts, while Cycle 7 is the survey in which the developmental results (e.g., PPVT scores) for the first post reform cohorts are observable (birth cohorts 2001 and 2002), so there are changes in the survey questions about educational attainment correlated with the reform.

To gain an independent perspective on this issue we examine data from the LFS on the educational attainment of the mothers of different birth cohorts. There are no changes in the education questions in the LFS over this period. The results are reported in figures 3 and 4. In figure 3 we show the proportion of females by year with a university degree from three groups: 1) married/partnered mothers with a child less than 1 years old, 2) married/partnered mothers with a child aged 1-5, and 3) women of child bearing age (ages 15-45). The first group isolates mothers with a new born allowing us to examine differences among mothers giving birth across the reform. Two points are clear. First there is a strong upward secular trend in the proportion of females and mothers with post-secondary degrees over the period. Second, there is no obvious jump in mothers' educational attainment starting in 2001 after the reform is in place. If anything

there is a jump in the last pre reform year (2000), but the increase is not out of line with similar variation seen in, for example, 1994, 1997 and 2006. Corresponding time series for the proportion of females with a high school degree are reported in figure 4. Again while there is a clear secular trend in these data, there is no evidence of an impact of the reform.

In table 10 are regression results from the LFS corresponding to the table 9 results from the NLSCY. Here we use the sample of parents of children aged 4-5, the ages at which our developmental indicators are measured, to match our sample from the NLSCY. First there is no evidence of significant changes in the education of fathers by the child's birth cohort. Second, while there is no evidence of shifts in the proportion of mothers with a high school diploma by birth cohort, there is evidence of an increase in the proportion of mothers with a university degree starting with birth cohort 2002, one year after the reform. The increase is roughly half of the increase in the NLSCY data.

We interpret these LFS results as suggesting that the story told in table 9 is partly the result of the changes in the educational questions in the NLSCY, or some other sample issue.[38] Mechanically the changes in parental education in the NLSCY do account for the differences in the conditional and unconditional estimates for PPVT. This can be seen in the second column of table 8 where we report estimates simply adding mothers' and fathers' education as control variables. This brings the estimate for PPVT in line with the previous results. Note also the estimated decreases in Know Your Numbers and Who Am I? scores are marginally larger relative to the unconditional counterparts. The decrease in these scores is larger once we account for the recorded increase in the education of the parents.

[38] Another change in the survey in cycle 7 is that children being followed longitudinally were interviewed even if they had not responded in all previous cycles. In previous cycles children followed longitudinally were interviewed in a current wave only if they had responded in all previous waves. As noted above there is a top up sample in each cycle to account for any longitudinal attrition in the birth cohorts. In any event omitting these observations from the sample does not materially affect the results.

What accounts for the difference in the conditional and unconditional estimates for Know Your Numbers and the behavioral measures? The answer is in table 8, column 3. Here we add a very simple control for any secular trends in the data: a linear trend. With the notable exceptions of PPVT and Who Am I?, the estimates for most of the other outcomes switch signs and/or lose statistical significance with this change in specification. Exceptions are indirect aggression and hyperactivity—in these cases the estimates start to converge to the estimates in table 3 when higher order terms in time are specified (i.e., a cubic). This implies that the statistically significant estimates for these outcomes in column 1 of table 8 cannot be distinguished from a simple trend in the data. More generally the estimates for these outcomes are sensitive to the specification of controls for secular trends. However, this is not the case for PPVT and to a lesser extent for Who Am I?.

We recognize that the changes in parental education over time documented in the LFS potentially put post reform children at an advantage developmentally, all else equal, to their pre reform cohorts. This would present a stronger challenge to our inferences if we had found that the maternity leave reform had resulted in an increase in the developmental scores. We would not know what part of the effect was due to the reform and what part was due to the increasing education of the parents. As it is, however, we find no impact on some of our measures and small negative impacts on others.

Figures 3 and 4 tell us that a secular trend in mothers' education over the period in which the reform was implemented might affect our estimates. The trend appears to be stronger in the recorded education variables of the NLSCY. It seems likely that this is in part due to the changes in NLSCY education questions correlated with the reform. Therefore, once we use the NLSCY education variables as control variables if there is a bias it would be negative—that is our

conditional estimates provide upper bound estimates of a negative impact of the reform on PPVT and Who Am I? scores. However, given the unconditional estimates for PPVT and Who Am I? are negative and statistically significant, we can rule out any positive impact of the reform on these cognitive measures.

Discussion

The results presented in table 3 provide little evidence that the Canadian maternity leave reforms had a measurable *positive* impact on the cognitive and behavioral outcomes of children at ages 4 and 5. Together with the evidence of Baker and Milligan (2010), the results indicate that the Canadian reform led to little improvement in a wide variety of measures of young children's development and well-being at ages 0 through 5 years. We believe this is a significant conclusion. First, as noted in the introduction, a positive impact is assumed in legislation enacting maternity leave in many developed countries. Second, the reforms did have a substantial impact on the maternal care children receive in their first year of life, with consequent impacts on inputs thought significant to development such as full time maternal employment, non-licensed non-parental care, breastfeeding and exclusive breastfeeding duration. Third, these findings provide precedence for the evidence that there is no impact of maternity leave on cognitive outcomes measured at older ages (Dustmann and Schonberg 2012, Liu and Skans 2009, Rasmussen 2010). Finally, the stated target of maternity leave policies is often the developmental outcomes of children at ages just prior to school entry. It is precisely at these ages that we find no positive effect.

The point estimates for some of our measures of behavioral development, while statistically insignificant, are sometimes in the expected direction—better behavior. However, in

general the results for these outcomes are quite unstable across changes in sample and specification. Also, the standard errors are small enough to rule out changes in these outcomes of more than 5½ percent of a standard deviation, so any improvement in behavior is by implication small.

While our results are consistent with analyses of the impacts of this Canadian reform at younger ages, and more generally with studies of the impact of maternity leave reforms on long run outcomes, they do contrast with the results of the cited literature on the impact of maternal employment on child outcomes. One possible account of this disagreement is the different identification strategies used in these two literatures. Most of the direct evidence of the impact of maternity leave is based on policy reforms, which are arguably exogenous to unobserved determinants of child outcomes. Studies of the impact of maternal employment on child outcomes more commonly attempt to account for the possible endogeneity of maternal employment by controlling for an extensive array of observable characteristics.

We also note that the reform we examine not only increased the duration of leave but also reduced the minimum employment needed for benefit entitlement from 700 hours to 600 hours in the 12 months preceding the claim. This change was made in response to advocacy claiming that given the typical spacing of births, many women were unable to work enough after giving birth to establish entitlement for a subsequent birth. It is possible that the women newly entitled under this part of the reform are so selected from the population to generate a bias to our estimates. Unfortunately the NLSCY does not have sufficient information on the pre birth employment of mothers to pursue this hypothesis.

The results for PPVT and Who Am I? are not of the expected sign and are statistically significant. In interpreting these negative impacts it is important to note that they are small. In

the conditional results the impact on PPVT is 4.8 percent of a standard deviation, while the impact on Who Am I? is 4.7 percent of a standard deviation. Given the previous discussion of the recording of parental education in the NLSCY, these estimates are likely to be upper bounds.

We argue that the relative impacts by sex add credibility to the inference. PPVT captures skills in vocabulary, a precursor to reading. As reported by Fryer and Levitt (2010), males persistently score lower in reading tests in the primary years, as early as at fall enrollment in kindergarten. It is therefore perhaps not surprising that for these skills it is males who suffer the greater consequence to an "upset" to developmental progress in the early years.

A remaining question is why an increase in maternal care could have a negative, albeit small, impact on cognition. One possibility is the post-reform increase in maternal care the children experienced at ages 0-12 months. It is certainly true that some observational studies find early childhood education, for which maternal care is often a substitute, has positive impacts on cognition. However, it is not obvious what sort of critical early childhood instruction is provided to children at ages 0-12 months when in non-parental care. Another possibility is some impact of the reform on development inputs children received after their mothers' returned to work. However, as documented in Baker and Milligan (2010), the reform did not change the care arrangements children were in at ages greater than 12 months. Also, the analysis in table 7 directly tests and rejects the hypothesis that there were systematic changes in a variety of other developmental inputs. Finally, the reform we examine not only increased the duration of leave but also reduced the minimum employment needed for benefit entitlement from 700 hours to 600 hours in the 12 months preceding the claim. This change was made in response to advocacy that claimed that given the typical spacing of births, many women were unable to work enough to establishment entitlement before their second, third, or subsequent birth. It is possible that the

women newly entitled under this part of the reform are so selected from the population to generate the negative impact on children's cognitive scores. Unfortunately the NLSCY does not have sufficient information on the pre birth employment of mothers to pursue this hypothesis.

We think a potentially promising avenue starts at the widely held, but to our knowledge not well researched, assumption that child development is monotonically increasing in the amount of maternal care in the first year. Importantly, parental leave expansions not only change the duration of maternal care but also the timing of the date of separation. It is possible that these two effects push against each other at certain ages. More care may be better, but the developmental consequences of mothers' return to work may vary in an unrestricted way over the first year. Simply put, that there may be better and worse times to make this transition over the first year from a developmental perspective.

The reform we analyze increased the maternal care in the first year by about 3 months for the treated, changing the return to work on average from just short of 6 months post birth to just shy of 9 months. Developmental psychologists observe that some key milestones are achieved in the second 6 months of life. Potentially important here are the development of stranger anxiety and separation anxiety.[39] Stranger anxiety, which is generally observed emerging around 6-8 months, is the tendency of the child to express distress and wariness at the approach of a stranger. In earlier months such an approach might be met instead with a smile and curiosity. Separation anxiety refers to a child's distress from being separated from his/her parent or primary caregiver. It is thought to relate to the development of object permanence—the appreciation that objects and people continue to exist when out of sight—and emerge around the 8th month.

[39] Our brief discussion of these milestones follows Scher and Harel (2009).

At a mother's return to work, a child is separated from a parent and in many cases introduced to a stranger. Relative to a return to work before 6 months, a return between 6 and 12 months places these events in precisely the interval in which a child develops anxiety about them. More precisely, the impact of the maternity leave reform we study potentially increases the stress a child experiences when attaching to a new non parental caregiver.

Neither the direct impact on stress indicators nor any consequences for cognitive and behavioral development of the mechanism we investigate have, to our knowledge, been directly investigated (see Gunnar and Quevedo 2007 for a general discussion of stress and development in infancy). There is evidence that maternal stress transmitted to the child in utero (O'Dinnell et al. 2009, Bergman et al. 2010) or post-natally through breast milk (Glynn et al. 2007) can negatively impact cognitive behavioral development (see also Glover 2011 and the references therein). Also, after the emergence of "separation protest", entry into non parental care leads to persistent elevation of cortisol levels (Ahnert et al. 2004). Furthermore the expression of stranger or separation anxiety may increase parental stress that may in turn have consequences for the child.

While the additional stress from parent/child separation at the 9th month rather than the 5th provides a potential mechanism for the small, negative cognitive effects we find, or an offset of otherwise positive effects, the means to test this hypothesis are not available in our data. We do observe that the maternity leave reform we analyze does not have a statistically significant impact on our measure of anxiety at ages 4 and 5 (table 3), although this is clearly not definitive. However, from a developmental perspective further research on this and associated issues is clearly critical to initiatives to refine maternity and parental leave provisions in many countries. Among other impacts, maternity leave reform typically affects the timing of mothers' return to

work, and therefore of the parent/child separation, over the first year. Our reading of the literature is that open questions remain on how child development varies with the timing of this separation.

Conclusions

We investigate the impact of a change in Canada's maternity leave laws on children's cognitive and behavioral development at ages 4 and 5. The change in the law increased the duration of job-protected, partially-compensated leave from approximately 6 months to one year. This led to large contemporaneous changes in important inputs to children's development: maternal care, maternal full time employment, unlicensed non-parental care, and breastfeeding duration.

We find that these changes had no positive impact on indices of behavioral and cognitive development. For our behavioral indices we can rule out all but very modest improvements. Our estimates for the cognitive indices are small, negative, and statistically significant in two of three cases. For example, for PPVT we estimate that a one month increase in maternal care over the range we examine leads to a reduction of at most 5.7 percent of a standard deviation. These findings together with those in Baker and Milligan (2010) provide a fairly comprehensive document of how the Canadian reform had little positive impact on a wide variety of developmental indices for children at ages 0-5 years. Follow-up study of these children at older ages will clearly further enhance our understanding of this reform.

This said, we cannot rule out impacts on still more outcomes. For example we are unable to investigate the impact of our reform on children's health outcomes because Canada does not

have an ongoing survey of children's health. Ruhm (2000) and Tanaka (2005) provide evidence of a positive impact of maternity leave on child health.

Our results provide precedence for findings in the literature that maternity leave reforms have no impact on cognitive outcomes measured at high school or later. They also highlight the possibility that child development is not monotonically increasing in the amount of maternal care received in the first year—there may be better and worse times for mothers to make the transition back to work in this period. Because "more is better" appears to be the working assumption of maternity leave laws in many countries, there is clearly a need to better understand the developmental consequences of mothers' return to work over the ages typically spanned by these policies.

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Table 1: Mandated, Job Protected, Unpaid Maternity/Parental Leave by Province

Province	Weeks Pre Reform	Weeks Post Reform	Date of Change
Newfoundland	29	52	December 31 2000
P.E.I.	34	52	December 31 2000
Nova Scotia	34	52	December 31 2000
New Brunswick	29	54	December 31 2000
Quebec	70	70	
Ontario	35	52	December 31 2000
Manitoba	34	54	December 31 2000
Saskatchewan	30	52	June 14 2001
Alberta	18	52	February 7 2001
British Columbia	30	52	December 31 2000

Notes: The reform of the Employment Insurance benefit entitlement for maternity/parental leave increased the duration of benefits from 25 to 50 weeks effective December 31 2000.

Table 2: Mean Values of Developmental Indicators

Indicator	N	Full Sample	Pre Reform	Post Reform
PPVT	9950	101.87	102.28	101.40
		(15.37)	(15.15)	(15.62)
Know Your	9970	1.37	1.39	1.34
Numbers		(0.58)	(0.59)	(0.58)
Who Am I?	9478	24.77	25.16	24.35
		(6.29)	(6.04)	(6.53)
Aggression	10971	1.64	1.68	1.60
		(1.88)	(1.91)	(1.85)
Indirect	10766	0.52	0.55	0.48
Aggression		(1.08)	(1.09)	(1.07)
Anxiety	10971	1.98	1.95	2.01
		(1.93)	(1.90)	(1.97)
Hyperactivity	10945	3.93	3.88	3.98
		(2.65)	(2.66)	(2.65)

Notes: The data are from the NLSCY. Full sample are means for the birth cohorts 1997-2004. Pre reform denotes the 1997-2000 birth cohorts, while post reform denotes the 2001-2004 birth cohorts. Standard deviations in parentheses.

Table 3: Two Stage Least Squares Estimates of the Impact of Maternal Care on Developmental Outcomes, 1997-2004 sample

Instrument			Treatment				Leave	
	N	1st Stage	1	2	3	4	1st Stage	5
PPVT	9950	2.216***	-0.739*	-0.681*	-0.692**	-0.635	0.056***	-0.941
		(0.218)	(0.382)	(0.377)	(0.294)	(0.411)	(0.009)	(0.600)
		[103.23]					[41.09]	
Know Your Numbers	9970	2.019***	0.030*	0.024	0.025*	0.023	0.047***	-0.007
		(0.234)	(0.018)	(0.017)	(0.014)	(0.019)	(0.009)	(0.028)
		[74.13]					[27.14]	
Who Am I?	9478	2.098***	-0.297*	-0.235	-0.521***	-0.179	0.049***	-0.497*
		(0.241)	(0.163)	(0.153)	(0.132)	(0.175)	(0.009)	(0.277)
		[75.52]					[27.46]	
Aggression	10971	2.090***	-0.010	-0.000	-0.027	0.007	0.047***	-0.014
		(0.223)	(0.053)	(0.052)	(0.043)	(0.059)	(0.009)	(0.092)
		[87.42]					[30.03]	
Indirect Aggression	10766	2.072***	0.009	0.019	0.048**	0.015	0.046***	0.030
		(0.226)	(0.028)	(0.27)	(0.023)	(0.031)	(0.009)	(0.048)
		[84.27]					[28.84]	
Anxiety	10971	2.083***	0.007	0.013	0.019	0.019	0.047***	0.117
		(0.223)	(0.056)	(0.055)	(0.045)	(0.061)	(0.009)	(0.097)
		[87.24]					[30.14]	
Hyperactivity	10945	2.087***	0.000	0.007	0.079	-0.013	0.047***	0.177
		(0.224)	(0.076)	(0.076)	(0.063)	(0.085)	(0.009)	(0.137)
		[87.04]					[31.92]	
Control for Time			Quartic	Cubic	Pre/Post Policy Linear	Pre/Post Policy Quadratic		Quartic

Notes: The data are from the NLSCY. All estimates are from two stage least squares regressions using the indicated instrument controlling for demographic characteristics and the indicated specification of time effects. 1st stage estimates are for the TSLS estimates in column (1) and (5). *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Standard errors in parentheses, F-stats in square brackets. N is number of observations.

Table 4: Two Stage Least Squares Estimates of the Impact of Maternal Care on Developmental Outcomes, 1999-2002 sample

Instrument		Treatment		
	N	1	2	3
PPVT	4049	-0.769**	-0.781**	-0.811**
		(0.377)	(0.377)	(0.379)
Know Your Numbers	4051	-0.088	-0.087	-0.088
		(0.148)	(0.145)	(0.149)
Who Am I?	3832	0.385	0.225	0.274
		(0.814)	(0.785)	(0.795)
Aggression	4540	0.067	0.049	0.053
		(0.138)	(0.133)	(0.134)
Indirect Aggression	4445	0.039	0.037	0.039
		(0.076)	(0.075)	(0.075)
Anxiety	4543	-0.048	-0.041	-0.040
		(0.152)	(0.145)	(0.147)
Hyperactivity	4523	-0.309	-0.348*	-0.334*
		(0.205)	(0.199)	(0.200)
Control for Time		Linear	Quadratic	Pre/Post Policy Linear

Notes: The data are from the NLSCY. All estimates are from two stage least squares regressions using the indicated instrument controlling for demographic characteristics and the indicated specification of time effects. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Standard errors in parentheses. N is number of observations.

Table 5: Two Stage Least Squares Estimates of the Impact of Maternal Care on Developmental Outcomes by Sex, 1997-2004 sample

Instrument	Treatment			
	Male Child		Female Child	
	Estimate	N	Estimate	N
PPVT	-1.516***	5057	0.244	4893
	(0.571)		(0.523)	
Know Your	0.021	5066	0.048*	4904
Numbers	(0.025)		(0.026)	
Who Am I?	-0.372	4837	-0.214	4641
	(0.237)		(0.219)	
Aggression	0.053	5592	-0.091	5379
	(0.076)		(0.077)	
Indirect	-0.017	5500	0.051	5266
Aggression	(0.039)		(0.042)	
Anxiety	0.031	5593	-0.041	5378
	(0.077)		(0.081)	
Hyperactivity	0.048	5580	-0.060	5365
	(0.112)		(0.107)	

Notes: The data are from the NLSCY. . All estimates are from two stage least squares regressions using the indicated instrument controlling for demographic characteristics and the quartic specification of time effects. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Standard errors in parentheses. N is number of observations.

Table 6: Estimated impact of longer maternity leave mandates on observable inputs to childhood development from the NLSCY

	Age 13-24 Months	Age 25-39 Months	Age 48-71 Months
Time mother home in first year	2.015**	2.194**	2.278**
	(0.131)	(0.139)	(0.130)
N	8307	8520	11111
Mother returned to work post-Birth	-0.025	-0.001	0.002
	(0.019)	(0.026)	(0.014)
N	8307	8520	11111
Child has younger sibling(s)	-0.016	0.003	-0.006
	(0.017)	(0.021)	(0.016)
N	8307	8520	11111
Child has older sibling(s)	-0.015	-0.014	-0.003
	(0.015)	(0.010)	(0.026)
N	8307	8520	11111
Depression Score - Mother	-0.165	-0.136	0.117
	(0.144)	(0.177)	(0.157)
N	7283	7327	9963

Notes: The reported statistics are from an 8 observation regression of the indicated input by year of birth on a constant and a dummy variable for birth cohorts exposed to the new maternity leave provisions. N indicates the number of observations from the first step regression. * and ** indicate statistical significance at the 10% and 5% levels respectively. Standard errors in parentheses.

Table 7: Estimated impact of longer maternity leave mandates on observable inputs to childhood development from the LFS

	Age 13-35 Months	Age 36-47 Months	Age 48-59 Months	Age 60-71 Months
Mother Employed	-0.001	0.030	-0.003	-0.002
	(0.005)	(0.018)	(0.011)	(0.007)
Mother Employed Full Time	0.026**	0.043**	0.032**	0.004
	(0.008)	(0.014)	(0.011)	(0.006)
Mother's Usual Weekly Hours	0.765**	0.923**	1.078*	0.428
	(0.290)	(0.324)	(0.443)	(0.353)
Mother Not in the Labor Force	0.000	-0.018	0.003	0.009
	(0.005)	(0.017)	(0.011)	(0.007)
Mother's Real Weekly Earnings	23.616	21.721	42.953**	29.605*
	(12.015)	(22.924)	(11.480)	(13.314)
Mother's Real Hourly Earnings	0.237	0.242	0.737**	0.646*
	(0.211)	(0.409)	(0.241)	(0.301)
Economic Family's Real Weekly Earnings	29.820**	64.879*	62.024**	63.874**
	(11.060)	(28.620)	(18.603)	(10.136)
Mother working PT to care for own children	-0.021*	-0.010	-0.015	0.008
	(0.010)	(0.007)	(0.011)	(0.007)
Mother not available for work-caring for own children	0.0013**	0.0014**	-0.0002	-0.0005
	(0.0004)	(0.0003)	(0.0010)	(0.0013)
Mother not looking for work-caring for own children	0.0020	0.0003	0.0009	0.005
	(0.0019)	(0.0016)	(0.0028)	(0.003)
Stay at Home Mother (YBB)	0.016**	-0.012	0.002	-0.018**
	(0.006)	(0.008)	(0.012)	(0.004)
Stay at Home Mother: (YOB)	0.019**	-0.010	-0.000	-0.011
	(0.007)	(0.011)	(0.011)	(0.007)
Mother has younger children	0.006	0.022	0.016	0.005
	(0.007)	(0.009)	(0.010)	(0.010)
Mother has older children	0.006	-0.021	-0.018**	-0.014
	(0.005)	(0.015)	(0.006)	(0.020)

Notes: The reported statistics are from an 8 observation (7 for ages 13-35) regression of the indicated input by year of birth on a constant and a dummy variable for birth cohorts exposed to the new maternity leave provisions. * and ** indicate statistical significance at the 10% and 5% levels respectively.

Table 8: Two Stage Least Squares Estimates of the Impact of Maternal Care on Developmental Outcomes, 1997-2004 sample, Robustness Analysis

Instrument		Treatment		
	N	1	2	3
PPVT	9950	-0.405***	-0.784***	-0.684**
		(0.135)	(0.130)	(0.310)
Know Your	9970	-0.021***	-0.032***	0.025*
Numbers		(0.005)	(0.005)	(0.014)
Who Am I?	9478	-0.222***	-0.335***	-0.410***
		(0.052)	(0.050)	(0.137)
Aggression	10971	-0.063***	-0.054***	-0.044
		(0.017)	(0.017)	(0.044)
Indirect	10766	-0.039***	-0.034***	0.050**
Aggression		(0.009)	(0.009)	(0.023)
Anxiety	10971	0.029	0.016	0.001
		(0.018)	(0.017)	(0.045)
Hyperactivity	10945	0.032	0.057**	0.055
		(0.025)	(0.024)	(0.063)
Demographic Controls		No	Parents' Education	Parent's Education
Control for Time		None	None	Linear

Notes: The data are from the NLSCY. All estimates are from two stage least squares regressions using the indicated instrument controlling for the indicated specifications of demographic characteristics and time effects. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively. Standard errors in parentheses. N is number of observations.

Table 9: Mothers and Fathers Educational Attainment by Child's Birth Cohort - NLSCY

YOB	Mother High School Grad	Mother University Grad	Father High School Grad	Father University Grad
1998	0.006	0.015	-0.009	-0.000
	(0.014)	(0.014)	(0.014)	(0.014)
1999	-0.000	0.016	0.002	-0.011
	(0.014)	(0.015)	(0.025)	(0.014)
2000	0.000	0.026*	-0.027*	-0.010
	(0.015)	(0.016)	(0.015)	(0.016)
2001	-0.072***	0.098***	-0.051***	0.048***
	(0.013)	(0.016)	(0.014)	(0.015)
2002	-0.072***	0.074***	-0.059***	0.035**
	(0.013)	(0.016)	(0.014)	(0.016)
2003	-0.067***	0.098***	-0.071***	0.057***
	(0.012)	(0.014)	(0.013)	(0.014)
2004	0.084***	0.109***	-0.048***	0.032**
	(0.013)	(0.017)	(0.015)	(0.016)

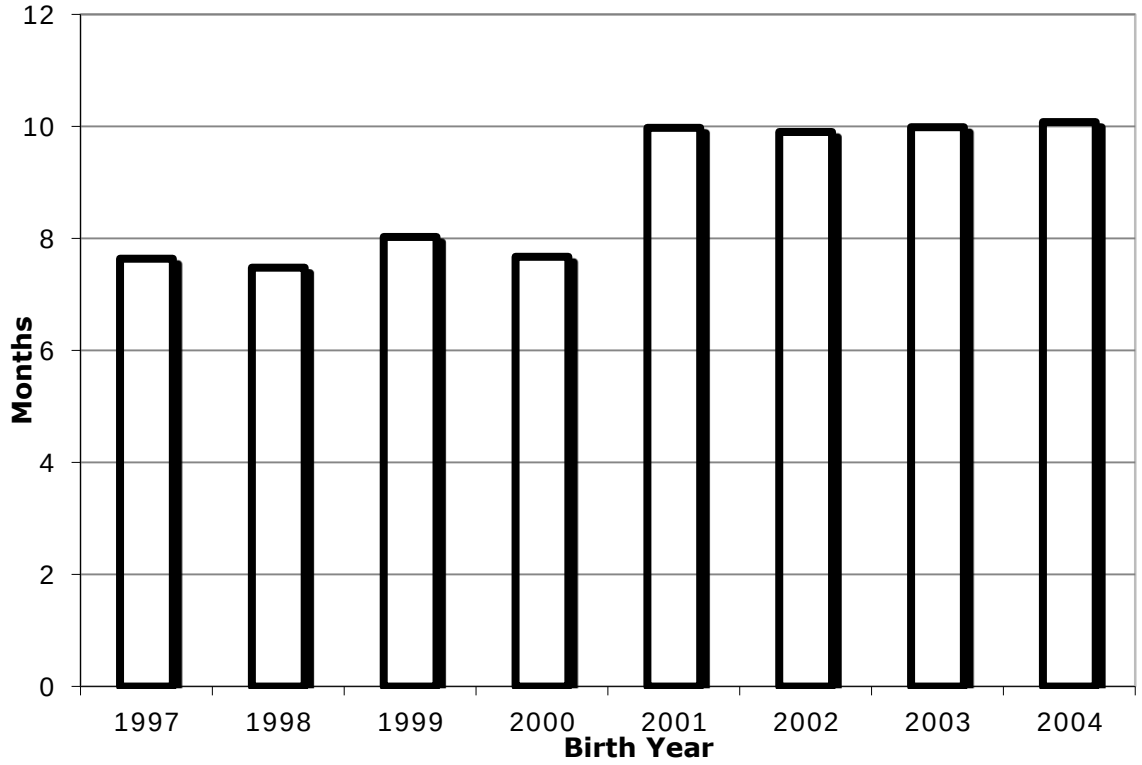
Notes: The data are from the NLSCY. All estimates are from regressions of the indicated dummy variable for educational attainment on dummy variables for the child's year of birth. 1997 is the omitted birth cohort. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

Table 10: Mothers and Fathers Educational Attainment by Child's Birth Cohort-LFS

YOB	Mother High School Grad	Mother University Grad	Father High School Grad	Father University Grad
1998	0.005	0.013	0.001	-0.008
	(0.022)	(0.019)	(0.022)	(0.018)
1999	0.010	-0.014	0.039	-0.024
	(0.022)	(0.018)	(0.022)	(0.018)
2000	-0.013	0.002	-0.033	0.015
	(0.024)	(0.020)	(0.024)	(0.021)
2001	0.003	0.009	0.025	-0.016
	(0.023)	(0.019)	(0.022)	(0.018)
2002	-0.018	0.058***	0.004	0.006
	(0.023)	(0.020)	(0.023)	(0.019)
2003	0.007	0.049**	0.011	0.015
	(0.023)	(0.020)	(0.023)	(0.019)
2004	0.016	0.045**	0.022	0.019
	(0.023)	(0.020)	(0.023)	(0.019)

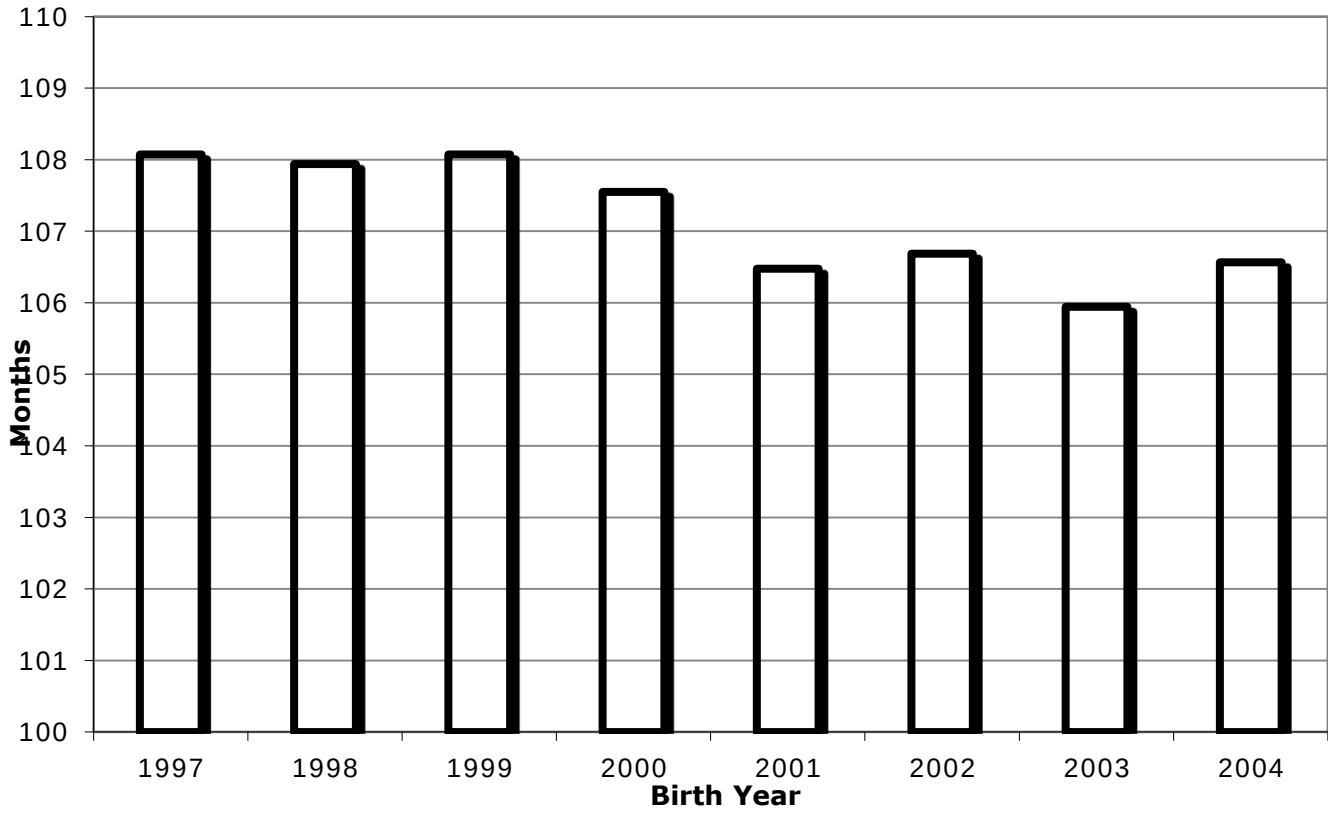
Notes: The data are from the LFS. All estimates are from regressions of the indicated dummy variable for educational attainment on dummy variables for the child's year of birth. 1997 is the omitted birth cohort. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

Figure 1: Number of Months Mother at Home in First Year: Estimated Year of Birth Effects



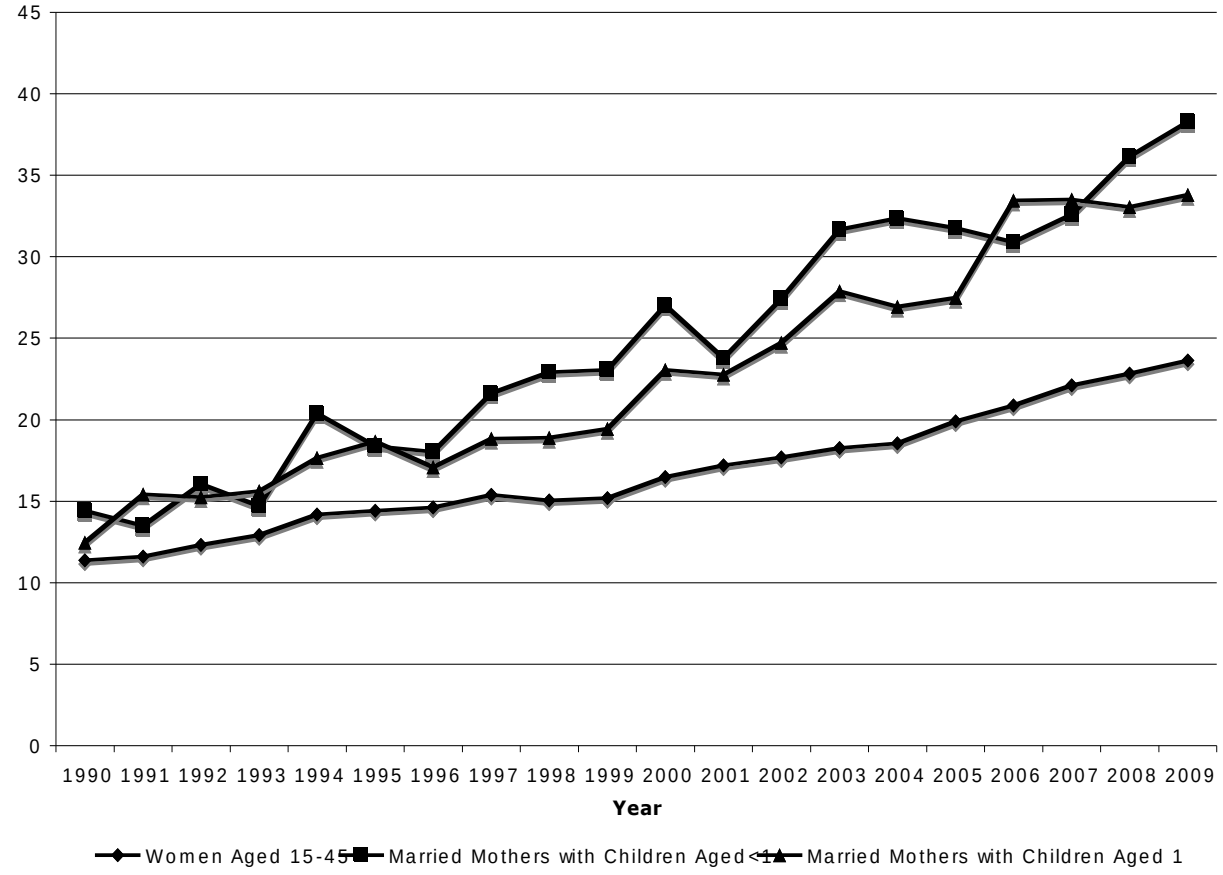
Notes: Estimates are from a regression of mothers' time at home in the first year on demographic controls and year of birth effects (no constant). The data are from the NLSCY.

Figure 2: PPVT: Estimated Year of Birth Effects



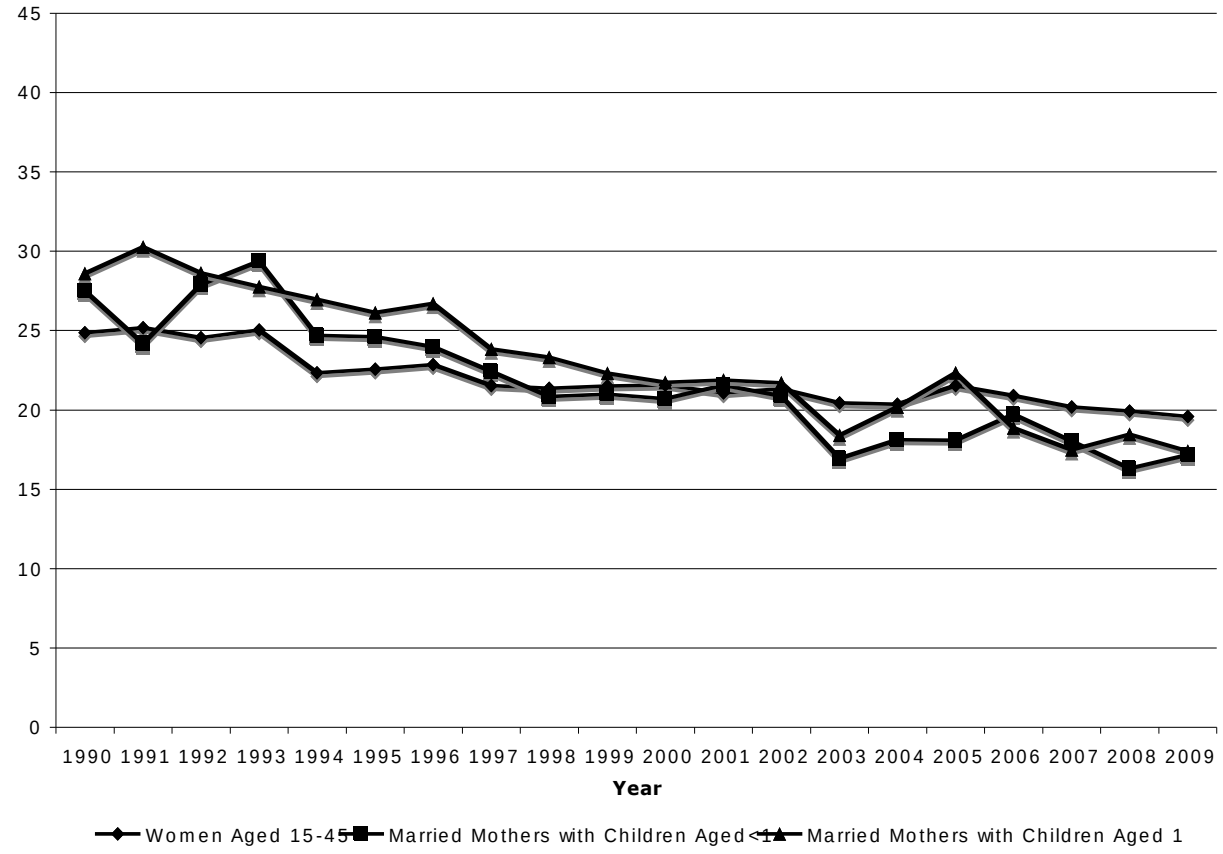
Notes: Estimates are from a regression of children's age standardized PPVT score on demographic controls and year of birth effects (no constant). The data are from the NLSCY.

Figure 3: Changes in the Proportion of Females with a University Degree – LFS Data



Notes: Graphed is the proportion of females with a university degree using data from the LFS. The three categories graphed are all women aged 15-45, married mothers with children age less than 1, and married mothers with children age 1 to 5.

Figure 4: Changes in the Proportion of Females with a High School Diploma – LFS Data



Notes: Graphed is the proportion of females with a high school diploma using data from the LFS. The three categories graphed are all women aged 15-45, married mothers with children age less than 1, and married mothers with children age 1 to 5.

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Appendix

Data Methods for the Labour Force Survey Sample

Age in years is recorded in the LFS, but exact date of birth is not available. By sampling from either the December or January surveys we can identify year of birth with a relatively small amount of error for single year categories.[40] For the December sample, we assume the birthday is in the current year. For the January sample, we assume the birthday was in the previous year. Since the reference week for the monthly survey is the week containing the 15th day. This means our coding will miss people born in the first half of January (for the January sample) or the last half of December (for the December sample). Any impact of this error is attenuated by the fact that our ultimate objective is to divide the cohorts by whether they faced the new maternity leave regime. However, the miscoding between the 2000 and 2001 birth cohorts means that some children will erroneously be designated as "treated" and vice versa. We report results using the December sample, but the results are very similar using the January samples.

Single year age categories are available for ages 0, 3, 4 and 5, but not for ages 1 and 2. As a result in the December 2000 or January 2001 surveys, those aged 2 will have been exposed to the original leave regime while those aged 1 will have been born during the reform period. To address this problem we do not sample from the December 2000 or January 2001 surveys for this age group. Denoting cohorts by the birth year of the one year olds, for this age group we use four cohorts before the reform (1997 to 2000) and three cohorts born after the reform (2002 to 2004).

[40] January and December are at the trough of the seasonal birth cycle in Canada. Compared to the U.S. the peak in monthly births in Canada occurs earlier than in the U.S., and the relative distance between peak and trough is greater. See He and Earn (2007).

Table A1: Estimates of Measures of Developmental Inputs by Year of Birth: NLSCY Data

	N	1997	1998	1999	2000	2001	2002	2003	2004
13-24 months									
Time mother home in first year	8307	8.436 (0.305)	8.061 (0.381)	8.624 (0.322)	8.297 (0.378)	10.501 (0.317)	10.325 (0.378)	10.382 (0.323)	10.258 (0.386)
Mother returned to work post-Birth	8307	0.779 (0.040)	0.834 (0.050)	0.749 (0.042)	0.792 (0.048)	0.743 (0.042)	0.766 (0.050)	0.772 (0.042)	0.772 (0.051)
Child has younger sibling	8307	0.166 (0.025)	0.175 (0.031)	0.121 (0.026)	0.137 (0.029)	0.104 (0.026)	0.158 (0.031)	0.132 (0.026)	0.141 (0.026)
Child has older sibling	8307	0.483 (0.042)	0.493 (0.053)	0.506 (0.044)	0.515 (0.051)	0.515 (0.044)	0.459 (0.052)	0.496 (0.044)	0.467 (0.053)
Depression Score-Mother	7283	3.857 (0.408)	4.061 (0.515)	4.236 (0.431)	4.474 (0.519)	3.859 (0.428)	3.919 (0.513)	4.095 (0.437)	4.095 (0.529)
25-39 months									
Time mother home in first year	8520	8.242 (0.276)	8.085 (0.305)	8.711 (0.302)	8.298 (0.316)	10.444 (0.299)	10.524 (0.306)	10.635 (0.291)	10.508 (0.316)
Mother returned to work post-Birth	8520	0.934 (0.032)	0.926 (0.036)	0.854 (0.035)	0.959 (0.037)	0.930 (0.035)	0.949 (0.037)	0.892 (0.035)	0.898 (0.037)
Child has younger sibling	8520	0.412 (0.034)	0.355 (0.037)	0.323 (0.037)	0.356 (0.039)	0.360 (0.036)	0.336 (0.038)	0.380 (0.036)	0.381 (0.038)
Child has older sibling	8520	0.491 (0.037)	0.519 (0.041)	0.511 (0.040)	0.491 (0.043)	0.508 (0.040)	0.479 (0.042)	0.487 (0.039)	0.483 (0.042)
Depression Score-Mother	7327	3.154 (0.373)	3.182 (0.410)	2.821 (0.424)	2.705 (0.437)	3.175 (0.410)	2.806 (0.425)	2.796 (0.397)	2.540 (0.434)
48-71 months									
Time mother home in first year	11111	7.621 (0.293)	7.525 (0.315)	8.089 (0.308)	7.644 (0.339)	10.007 (0.307)	9.914 (0.337)	10.083 (0.307)	9.988 (0.345)
Mother returned to work post-Birth	11111	0.961 (0.030)	0.969 (0.032)	0.925 (0.032)	0.967 (0.035)	0.966 (0.032)	0.977 (0.035)	0.953 (0.032)	0.933 (0.035)

Child has younger sibling	11111	0.499 (0.038)	0.481 (0.041)	0.468 (0.040)	0.491 (0.044)	0.457 (0.040)	0.453 (0.033)	0.489 (0.040)	0.489 (0.040)
Child has older sibling	11111	0.631 (0.037)	0.626 (0.041)	0.630 (0.040)	0.581 (0.044)	0.666 (0.040)	0.584 (0.044)	0.637 (0.040)	0.570 (0.045)
Depression Score-Mother	9963	3.726 (0.352)	3.673 (0.384)	3.412 (0.377)	3.965 (0.416)	3588 (0.376)	3.813 (0.429)	3.744 (0.373)	4.103 (0.425)

Notes: Each row presents analysis of the indicated dependent variable. Reported are the regression coefficients on year of birth effects for the indicated cohort. Estimates are conditional on the other demographic controls described in the text. N is sample size. Robust standard errors are in parentheses.

Table A2: Estimates of Measures of Developmental Inputs by Year of Birth: LFS Data

	N	1997	1998	1999	2000	2001	2002	2003	2004
Age 13-35 months									
Mother Employed	12260	0.257 (0.028)	0.257 (0.028)	0.251 (0.029)	0.245 (0.029)	N.A.	0.249 (0.029)	0.260 (0.029)	0.244 (0.029)
Mother Employed Full Time	12260	0.263 (0.028)	0.269 (0.028)	0.277 (0.029)	0.275 (0.029)	N.A.	0.282 (0.029)	0.299 (0.029)	0.309 (0.029)
Mother's Usual Weekly Hours	7801	35.139 (1.056)	35.447 (1.066)	35.814 (1.062)	36.027 (1.097)	N.A.	36.096 (1.071)	36.224 (1.065)	36.796 (1.072)
Mother Not in the Labor Force	12260	0.610 (0.028)	0.608 (0.028)	0.617 (0.029)	0.619 (0.028)	N.A.	0.614 (0.028)	0.606 (0.029)	0.621 (0.029)
Mother's Real Weekly Earnings	6656	230.796 (21.776)	233.557 (21.809)	240.716 (22.121)	244.111 (22.208)	N.A.	249.198 (22.298)	248.631 (22.618)	284.904 (22.831)
Mother's Real Hourly Earnings	6656	6.027 (0.516)	5.903 (0.516)	6.228 (0.528)	6.086 (0.530)	N.A.	6.086 (0.534)	6.100 (0.543)	6.711 (0.545)
Economic Family's Real Weekly Earnings	10309	440.903 (34.576)	471.176 (33.938)	469.782 (35.223)	468.963 (35.140)	N.A.	479.446 (34.980)	489.800 (35.519)	508.334 (36.408)
Mother working	12260	-0.074	-0.068	-0.091	-0.094	N.A.	-0.093	-0.097	-0.118

PT to care for own children		(0.016)	(0.016)	(0.017)	(0.017)		(0.017)	(0.017)	(0.017)
Mother not available for work-caring for own children	12260	-0.0008 (0.0013)	0.0003 (0.0016)	0.0008 (0.0015)	0.0003 (0.0014)	N.A.	0.0014 (0.0017)	0.0015 (0.0020)	0.0013 (0.0012)
Mother not looking for work-caring for own children	12260	0.048 (0.010)	0.047 (0.010)	0.041 (0.010)	0.049 (0.010)	N.A.	0.050 (0.010)	0.049 (0.010)	0.047 (0.010)
Stay at Home Mother (YBB)	12260	0.250 (0.025)	0.233 (0.024)	0.239 (0.025)	0.248 (0.025)	N.A.	0.260 (0.025)	0.264 (0.025)	0.251 (0.025)
Stay at Home Mother (YOB)	12260	0.312 (0.027)	0.299 (0.026)	0.307 (0.027)	0.310 (0.027)	N.A.	0.338 (0.027)	0.319 (0.027)	0.321 (0.027)
Mother has younger children	12260	0.091 (0.019)	0.105 (0.019)	0.095 (0.019)	0.111 (0.019)	N.A.	0.113 (0.019)	0.109 (0.019)	0.097 (0.019)
Mother has older children	12260	0.454 (0.029)	0.452 (0.029)	0.458 (0.030)	0.455 (0.029)	N.A.	0.455 (0.029)	0.470 (0.030)	0.458 (0.030)
Age 36-47 months									
Mother Employed	7212	0.296 (0.038)	0.281 (0.038)	0.322 (0.038)	0.245 (0.039)	0.300 (0.038)	0.338 (0.039)	0.319 (0.038)	0.309 (0.039)
Mother Employed Full Time	7212	0.296 (0.039)	0.279 (0.039)	0.309 (0.039)	0.265 (0.039)	0.300 (0.039)	0.340 (0.039)	0.335 (0.039)	0.346 (0.040)
Mother's Usual Weekly Hours	4660	36.442 (1.367)	36.644 (1.437)	36.681 (1.384)	37.039 (1.410)	36.904 (1.396)	37.397 (1.381)	37.940 (1.392)	38.258 (1.394)
Mother Not in the Labor Force	7212	0.590 (0.037)	0.593 (0.037)	0.559 (0.037)	0.628 (0.037)	0.588 (0.037)	0.554 (0.037)	0.567 (0.037)	0.591 (0.038)
Mother's Real Weekly Earnings	3956	237.859 (28.527)	205.605 (28.603)	217.505 (28.994)	232.668 (29.559)	191.999 (28.562)	233.719 (29.400)	259.744 (29.113)	295.058 (30.961)
Mother's Real Hourly Earnings	3956	5.760 (0.681)	5.254 (0.678)	5.482 (0.685)	5.657 (0.700)	4.768 (0.682)	5.570 (0.697)	6.234 (0.693)	6.548 (0.737)
Economic	6105	494.583	460.088	474.153	441.590	492.074	487.521	552.507	597.828

Family's Real Weekly Earnings		(45.345)	(44.620)	(45.616)	(46.533)	(45.408)	(46.799)	(46.419)	(50.744)
Mother working PT to care for own children	7212	-0.062	-0.070	-0.061	-0.088	-0.079	-0.073	-0.085	-0.084
		(0.024)	(0.023)	(0.024)	(0.023)	(0.023)	(0.024)	(0.023)	(0.024)
Mother not available for work-caring for own children	7212	0.0018	0.0016	0.0018	0.0028	0.0038	0.0030	0.0040	0.0029
		(0.0039)	(0.0038)	(0.0037)	(0.0047)	(0.0044)	(0.0039)	(0.0043)	(0.0039)
Mother not looking for work-caring for own children	7212	0.049	0.049	0.043	0.045	0.047	0.046	0.045	0.049
		(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.013)	(0.015)
Stay at Home Mother (YBB)	7212	0.140	0.146	0.138	0.165	0.137	0.125	0.133	0.145
		(0.029)	(0.029)	(0.028)	(0.029)	(0.029)	(0.028)	(0.028)	(0.029)
Stay at Home Mother (YOB)	7212	0.151	0.159	0.157	0.187	0.163	0.133	0.156	0.161
		(0.031)	(0.031)	(0.031)	(0.032)	(0.031)	(0.031)	(0.031)	(0.032)
Mother has younger children	7212	0.378	0.401	0.391	0.404	0.420	0.424	0.396	0.421
		(0.038)	(0.038)	(0.038)	(0.038)	(0.038)	(0.039)	(0.039)	(0.039)
Mother has older children	7212	0.393	0.387	0.376	0.376	0.392	0.338	0.381	0.339
		(0.041)	(0.040)	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)	(0.042)
Age 48-59 months									
Mother Employed	7390	0.263	0.275	0.235	0.245	0.249	0.266	0.253	0.237
		(0.039)	(0.039)	(0.039)	(0.040)	(0.040)	(0.040)	(0.040)	(0.039)
Mother Employed Full Time	7390	0.335	0.295	0.315	0.333	0.333	0.360	0.357	0.356
		(0.040)	(0.040)	(0.040)	(0.040)	(0.41)	(0.041)	(0.041)	(0.040)
Mother's Usual Weekly Hours	4881	38.227	36.779	37.812	38.630	38.560	38.658	39.361	39.179
		(1.449)	(1.463)	(1.417)	(1.448)	(1.463)	(1.450)	(1.460)	(1.440)
Mother Not in the Labor Force	7390	0.618	0.604	0.651	0.641	0.628	0.623	0.639	0.637
		(0.038)	(0.039)	(0.039)	(0.039)	(0.039)	(0.039)	(0.040)	(0.039)
Mother's Real Weekly Earnings	4090	288.641	298.639	305.781	312.933	317.327	339.985	358.829	361.668
		(31.054)	(31.896)	(31.659)	(32.278)	(33.181)	(32.415)	(33.198)	(32.728)

Mother's Real Hourly Earnings	4090	6.067 (0.737)	6.644 (0.743)	6.349 (0.746)	6.300 (0.763)	6.497 (0.768)	7.073 (0.763)	7.265 (0.786)	7.471 (0.784)
Economic Family's Real Weekly Earnings	6253	581.289 (47.916)	613.982 (48.551)	590.096 (48.701)	603.777 (49.431.)	623.027 (49.311)	637.142 (50.440)	690.544 (50.771)	686.527 (50.571)
Mother working PT to care for own children	7390	-0.085 (0.023)	-0.048 (0.025)	-0.092 (0.024)	-0.093 (0.024)	-0.091 (0.024)	-0.091 (0.024)	-0.091 (0.024)	-0.105 (0.024)
Mother not available for work-caring for own children	7390	0.0083 (0.0048)	0.0086 (0.0047)	0.0076 (0.0046)	0.0086 (0.0052)	0.0062 (0.0046)	0.0076 (0.0047)	0.0076 (0.0052)	0.0108 (0.0059)
Mother not looking for work-caring for own children	7390	0.044 (0.011)	0.050 (0.011)	0.045 (0.011)	0.042 (0.010)	0.042 (0.011)	0.046 (0.010)	0.052 (0.011)	0.045 (0.011)
Stay at Home Mother (YBB)	7390	0.129 (0.027)	0.145 (0.027)	0.170 (0.028)	0.127 (0.027)	0.153 (0.028)	0.154 (0.028)	0.147 (0.028)	0.126 (0.027)
Stay at Home Mother (YOB)	7390	0.150 (0.029)	0.166 (0.030)	0.191 (0.030)	0.144 (0.030)	0.173 (0.030)	0.160 (0.030)	0.165 (0.031)	0.152 (0.030)
Mother has younger children	7390	0.485 (0.040)	0.496 (0.040)	0.487 (0.040)	0.499 (0.041)	0.483 (0.041)	0.504 (0.041)	0.530 (0.041)	0.514 (0.040)
Mother has older children Age 60-71 months	7390	0.358 (0.039)	0.372 (0.040)	0.373 (0.040)	0.358 (0.041)	0.358 (0.041)	0.343 (0.041)	0.337 (0.041)	0.352 (0.041)
Mother Employed	7298	0.263 (0.040)	0.244 (0.040)	0.253 (0.041)	0.256 (0.040)	0.246 (0.040)	0.270 (0.040)	0.242 (0.041)	0.251 (0.040)
Mother Employed Full Time	7298	0.280 (0.041)	0.276 (0.040)	0.287 (0.041)	0.287 (0.041)	0.276 (0.041)	0.293 (0.041)	0.279 (0.042)	0.300 (0.041)
Mother's Usual Weekly Hours	4883	38.156 (1.560)	38.717 (1.621)	38.758 (1.589)	38.524 (1.581)	38.789 (1.606)	39.541 (1.613)	38.126 (1.613)	39.410 (1.635)
Mother Not in the	7298	0.612	0.631	0.624	0.632	0.640	0.618	0.641	0.635

Labor Force		(0.039)	(0.039)	(0.040)	(0.039)	(0.039)	(0.039)	(0.040)	(0.039)
Mother's Real Weekly Earnings	4059	254.137 (37.917)	262.654 (37.683)	270.864 (38.266)	272.714 (37.485)	285.921 (38.723)	284.383 (38.617)	276.456 (39.055)	332.030 (41.693)
Mother's Real Hourly Earnings	4059	6.243 (0.820)	6.186 (0.807)	6.517 (0.831)	6.549 (0.825)	7.004 (0.834)	6.603 (0.834)	6.637 (0.837)	7.834 (0.888)
Economic Family's Real Weekly Earnings	6136	515.362 (53.217)	498.161 (52.856)	527.541 (53.975)	531.564 (53.241)	574.278 (54.202)	576.359 (54.263)	575.050 (55.103)	602.437 (55.738)
Mother working PT to care for own children	7298	-0.080 (0.024)	-0.095 (0.024)	-0.103 (0.025)	-0.087 (0.024)	-0.078 (0.025)	-0.078 (0.025)	-0.078 (0.025)	-0.098 (0.024)
Mother not available for work-caring for own children	7298	0.0009 (0.0039)	0.0020 (0.0047)	0.0057 (0.0041)	0.0029 (0.0035)	0.0031 (0.0044)	0.0010 (0.0038)	0.0045 (0.0045)	0.0010 (0.0038)
Mother not looking for work-caring for own children	7298	0.048 (0.012)	0.046 (0.012)	0.045 (0.011)	0.042 (0.011)	0.048 (0.012)	0.051 (0.012)	0.043 (0.012)	0.057 (0.013)
Stay at Home Mother (YBB)	7298	0.117 (0.026)	0.112 (0.026)	0.121 (0.026)	0.106 (0.026)	0.098 (0.026)	0.102 (0.026)	0.093 (0.026)	0.090 (0.026)
Stay at Home Mother (YOB)	7298	0.154 (0.030)	0.160 (0.030)	0.157 (0.030)	0.139 (0.029)	0.153 (0.030)	0.144 (0.030)	0.138 (0.030)	0.132 (0.030)
Mother has younger children	7298	0.629 (0.041)	0.676 (0.040)	0.644 (0.041)	0.657 (0.041)	0.650 (0.041)	0.657 (0.041)	0.657 (0.041)	0.661 (0.041)
Mother has older children	7298	0.264 (0.041)	0.318 (0.041)	0.307 (0.042)	0.296 (0.041)	0.280 (0.042)	0.241 (0.042)	0.318 (0.042)	0.291 (0.019)

Notes: Each row presents analysis of the indicated dependent variable. Reported are the regression coefficients on year of birth effects for the indicated cohort. Estimates are conditional on the other demographic controls described in the text. N is sample size. Robust standard errors are in parentheses. N.A. not applicable.

Table A3: Estimated impact of longer maternity leave mandates on family real earnings from the LFS - Estimates based on a wage deflator

	Age 13-35 Months	Age 36-47 Months	Age 48-59 Months	Age 60-71 Months
Mother's Real Weekly Earnings	27.476**	7.920	20.464**	4.465
	(10.139)	(17.152)	(7.377)	(9.963)
Mother's Real Hourly Earnings	0.368*	-0.173	0.049	-0.121
	(0.172)	(0.259)	(0.160)	(0.243)
Economic Family's Real Weekly Earnings	38.037**	37.723*	17.429	13.395
	(12.737)	(18.407)	(10.435)	(7.776)

Notes: The reported statistics are from an 8 observation regression of the indicated input by year of birth on a constant and a dummy variable for birth cohorts exposed to the new maternity leave provisions. The wage deflator is according to Industrial Aggregate Wage from Statistics Canada's *Survey of Employment Payrolls and Hours* (catalogue 72-002-XIB). * and ** indicate statistical significance at the 10% and 5% levels respectively.