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MATERNITY LEAVE AND CHILDREN'S COGNITIVE AND BEHAVIORAL DEVELOPMENT

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ABSTRACT

We investigate the impact of maternity leave on the cognitive and behavioral development of children at ages 4 and 5. The impact is identified by legislated increases in the duration of maternity leave in Canada, which significantly increased the amount of maternal care children received in the second half of their first year. We carefully document how other observable inputs to child development vary across cohorts of children exposed to different maternity leave regimes. Our results indicate that maternity leave changes had no positive effect on indices of children's cognitive and behavioral development. We uncover a small negative impact on PPVT and Who Am I? scores, which suggests the timing of the mother/child separation due to the mother's return to work may be important.

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Introduction

Maternity leave has emerged in many countries as the public policy of choice for improving the lives of mothers and infants after childbirth. In many cases, child development is cited as an important basis for legislative parental leave initiatives. 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…".[1] A new 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). A recent 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).

These statements about the impact of maternity leave on child development are primarily based in indirect evidence. There simply aren't many studies of the direct impact of maternity leave. Instead legislation draws on evidence of the impact of maternal employment or non-parental care on child development projected onto the maternity leave statute. There are a number of reasons to wonder if this projection is appropriate.

First, universal leave statutes typically affect a different and larger group of children than the groups studied in these other literatures. For example, studies that exploit instruments for maternal employment, such as welfare reform, tell us something about the impact of maternal

[1] http://www.dol.gov/esa/whd/fmla/fmlaAmended.htm#SEC_2_FINDINGS_AND_PURPOSES

employment on the children of mothers who respond to this particular treatment. Similarly, some of the best, experimental evidence on the impact of non-parental care is for children "at risk". Whether for welfare or for children at risk, the response observed may not be relevant for other mothers. Second, maternity leaves may affect maternal employment or the use of non-parental care in different ways than those considered in other research. In particular, parental leave can potentially affect child outcomes by 1) changing the amount of time mothers spend at home with a very young child and 2) changing the timing of the return to work. In contrast, instrumental variables strategies pursued in the literature often focus on more permanent effects of whether a mother works at all during the pre-school years, or over broad periods. Third, maternity leaves can affect inputs that are complementary to maternal care and that may have separate effects on child development. For example, there is evidence that maternity leaves, by affecting the duration of maternal care just after birth, can affect the length of time babies are breastfed (e.g., Baker and Milligan 2008a). Finally, whether paid or not, maternity leave policies can affect family income. If maternal care is not a perfect substitute for monetary resources, it is important to consider whether the identifying variation in these other literatures involved comparable impacts on family resources.

These arguments highlight the importance of direct evidence on the impact of maternity leave policy. This is exactly what we offer in this paper. We examine the impact of an expansion of the Canadian paid maternity leave programs on measures of children's cognitive and behavioral development at ages 4 and 5 years. At the end of 2000, Canadian laws were passed that expanded the duration of job-protected, partially compensated maternity/parental leave from approximately 6 months to a full year. Previous research indicates that these changes increased the duration of maternal care children received in the first year of life with an offsetting

reduction in unlicensed non-parental care and maternal full time work (Baker and Milligan 2010, Hanratty and Trzcinski 2009). Among women affected by the reform, the timing of the return to work changed from just under 6 months to almost 9 months post birth, on average. This approximately 50 percent increase in parental time at home in the first year of life provides a very strong basis to evaluate directly the claimed impacts of parental leaves on child development.

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 age-range we examine is the one in which previous evidence suggests the cognitive impact of early maternal care manifests; an age that matters critically for evaluation of development. Third, 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, New Jersey, and Washington and in U.S. states that provide maternity benefits through Temporary Disability Insurance programs (see Brustenev and Vroman 2007).[2] 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 is cited as a rationale for maternity leave reforms.

Our analysis makes use of the National Longitudinal Survey of Children and Youth (NLSCY). This is a survey of Canadian children that provides an array of developmental

[2] The U.S. federal law, the Family and Medical Leave Act, provides up to 12 weeks of unpaid leave.

indicators. We focus on cognitive markers—PPVT, Know Your Numbers, Who Am I?—as well as four behavioral indices.

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; 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 our behavioral indices we can rule out all but very modest improvements. For our cognitive measures the estimated impact of the reform is small, negative and statistically significant for PPVT and Who Am I?. This latter result highlights the relatively neglected issue of how changes in maternity leave laws affect the timing of the mothers' return to work. Specifically, it is consistent with the hypothesis that some ages are better than others for abrupt changes in the parent-child relationship.

Previous Literature

There are relatively few direct evaluations of the impact of maternity/parental leaves on children's outcomes. Baker (2011) provides an overview of research investigating the impacts of changes of the program parameters of leave mandates in Canada and Europe.

Baker and Milligan (2010) examine the effect of the same policy reform analyzed here on behavior, parental inputs and a measure of motor and social development at ages up to 24 months. They find little evidence of an impact at those ages. The present study is distinguished by looking at indicators of cognitive development that are not observable at the younger ages of the previous study. This is important because previous research indicates that the impact of early

maternal employment/childcare does not manifest until older ages, and because cognitive development has been a key focus of previous research.[3]

Most other studies examine the longer term impacts of maternity leave. Dustmann and Schonberg (2008) investigate 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.[4] 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. Finally, and in contrast, Carneiro et al. (2010) 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.

When interpreting this evidence of long term impacts it is important to pay attention to the amount and timing of the increase in maternal/parental care the leave reforms induced. The evidence in Dustmann and Schonberg (2008) indicates that the primary impacts of the German reforms on maternal care are within the bounds of the legislated increases in leave: between 2 and 6 months for the reform in the late 1970s and between 6 and 10 months for the reform in the mid 1980s. In Rasmussen's (2010) study there is an increase in care of 40 days off a pre-reform base of just under 18 weeks. The reform Liu and Skans (2010) investigate led to an increase in

[3] For example, 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.
[4] A third reform considered in the analysis is an increase in unpaid leave from 18 to 36 months in the early 1990s.

maternal care matching the change of the parameters of the law—about 3 additional months at the beginning of the second year. Even in the absence of paid leave women in Norway took 8 months off work post birth, so the change in time input Carneiro et al. (2010) analyze is in the last third of the child's first year. Therefore, Dustmann and Schonberg's (2008) first reform and Rasmussen's (2010) reform speak to changes in maternal care in the first 6 months, Dustmann and Schonberg (2008)'s second reform and Carneiro et al.'s (2010) reform concern care in the second 6 months and Liu and Skans' (2010) reform impacts care in the third 6 months.

Aside from any concerns of time, place and other characteristics of each reform, the results of these different studies would be directly comparable if we knew that children's outcomes were monotonically affected by increases in maternal care at different ages over the first years. We are not aware of any research that causally establishes this relationship. This is an important and relatively neglected dimension of maternity leave analysis, because as noted in the Introduction, leave expansions affect both the duration of maternal care and the timing of the mother's return to work.

There are a few studies providing evidence of the developmental impact of the timing of a mother's return to work in the first year.[5] Baydar and Brooks-Gunn (1991) report a statistically significant negative effect for employment by the second quarter of the first year and a statistically significant positive effect for employment in the fourth quarter among children whose mothers worked in the first year. Han et al. (2001) report employment in the first three quarters of the first year has a negative cognitive impact while the impact of returning to work in the fourth quarter is statistically insignificant (although in some cases the point estimates are

[5] Berger et al. (2005) report that children whose mother returns to work within 12 weeks of birth have more externalizing behavior problems than those whose mothers take longer leaves. See Lucas-Thompson et al. (2010) for a review of studies that track the timing of the return to work over the first few years.

comparable). Brooks-Gunn et al. (2002) report a statistically significant negative impact of maternal employment by the 9th month of the first year on cognitive development at age 36 months. Estimates for employment by the 3rd, 6th and 12th months are also negative but smaller in magnitude and not statistically significant. Finally, some of the specifications that Baum (2003) estimates indicate return to work in the first quarter after birth has a negative effect on PPVT scores relative to return in the succeeding three quarters. Therefore, this evidence is mixed, as well as in many cases vulnerable to bias from unobserved differences across the children of mothers who choose to return to work at different points in time.

It is also important to recall that the impact of parental leave depends on there being a change in parental time at home. Baker and Milligan (2008b) find that shorter leaves mostly generate a 'relabeling' of time away from work rather than an expansion of actual time spent with the mother. Most of the action in return to work happens after the first few months. For example, in Brooks-Gunn et al.'s (2002) U.S. sample from the early 1990s 11 percent of mothers report working 1 month after birth, just over 50 percent report working by 3 months and just under 80 percent report working by 12 months. Therefore, the period between ages 6 and 12 months is perhaps the most relevant to policy that seeks to increase parental involvement in early childrearing, as is so often asserted in maternity leave legislation. Our work here focuses directly on this 6 to 12 month age range.

The Reform

Our analysis is based on a reform of Canadian maternity/parental leave (henceforth maternity leave) laws at the end of 2000.[6] Job-protected, uncompensated, maternity leave is provided by provincial labor standards laws, and historically there has been some variation in its

[6] 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. From December 31, 2000 onward, the shared 10 week component of the leave was expanded to 35 weeks, bringing total available leave to 50 weeks. [7]

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.[8] In many instances the change was not announced or enacted until November or December 2000.[9] By June 2001, all provinces offered job protection of sufficient duration to accommodate the new 50 week EI standard.[10] The change by province is presented in table 1.

Expected impact of the reform

[7] 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.

[8] 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.

[9] 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.

[10] 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.

The reform we analyze led to an increase in the maternal care children received in their first year of life. Baker and Milligan (2008a) show this care increased by over 3 months for those affected by the reform, a 50 percent increase over the pre-reform mean of just under 6 months. There was a contemporaneous large decrease in mothers' full time employment of 59 percent (Baker and Milligan 2010). Also the proportion of children in non-parental care fell by 44 percent, almost all of which came out of unlicensed care outside the home. EI benefits available to leave takers replace earnings up to $39,000, at a rate of 55 percent. Baker and Milligan (2010) show that for a woman at median earnings using paid childcare, the after-tax after-childcare replacement rate is close to 100 percent.

The expected impact of the reform flows from any developmental benefits of increased maternal care between ages 6 and 12 months. As noted above there is relatively little previous evidence of the impact of maternal care in this age range. More generally,[11] 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).[12] 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

[11] 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.
[12] 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.

behavioral effects (e.g., O'Brien Caughy et al. 1994, Gregg et al. 2005, Lefebvre et al. 2008, Loeb et al. 2007).[13]

A secondary factor to consider is that increases in maternal time at home post birth facilitate longer periods of breastfeeding. Baker and Milligan (2008a) report that the reform we study here increased the amount of time children were breastfed by one month—a one-half month increase in exclusive breastfeeding. The impact of breastfeeding on cognitive development is generally thought to be positive, however, the evidence is mostly observational and a meta analysis has disputed this claim (Der, Batty, and Deary 2006). Recently Kramer et al. (2008b) have revisited this issue offering evidence of the effect based on a controlled experiment in Belarus; the PROBIT study reported in Kramer et al. (2001). They report that the increase in breastfeeding induced in the PROBIT study is related to an increase in cognitive development (measured at 6.5 years of age) of just over one-third of a standard deviation. The evidence for verbal skills is the strongest. Kramer et al. (2008a) examines the impact of breastfeeding on children's behavior at 6.5 years of age in the same experimental design, finding no effect. We consider breastfeeding as one possible channel for the influence of maternity leave on subsequent development.

The Data

[13] 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) or 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), and the research summarized in Belsky (2006) provide further evidence that non-parental care can have negative behavioral effects.

The National Longitudinal Study of Children and Youth (NLSCY) is a survey of Canada's children. The data we use is a cross section of children aged 0 to 5 years of age available biannually starting in 1994/5. 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 is based on research 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.[14] 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 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

[14] 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).

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 behavioral indices. These are parent-reported measures based on best practices.[15] They measure, respectively, hyperactivity, anxiety, physical aggression and indirect aggression. Each 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 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.[16] 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 were first surveyed in the 4th (2000/2001) cycle when they were less than one year old, while the children

[15] 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).

[16] To keep the sample representative in a given survey, a top up sample is added to replace lost sample members of the longitudinal sample.

of the 2001 birth cohort were first surveyed in the 5th (2002/03) cycle when they were 1 year old.

This sampling design prevents a straightforward application of a regression discontinuity identification strategy to our problem. 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 were age 5. Therefore a strict regression discontinuity estimator would lean very heavily on the age standardization of the developmental scores being correct. Rather than relying on this being true, we sample multiple cohorts before and after the reform so that any age effects average out.[17] This strategy also has the advantage of larger sample sizes, as the number of children in the NLSCY born just before and after the reform is quite small.

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. Therefore, our results are for children in two parent/adult households, who are the majority beneficiaries of universal maternity leave policies.[18]

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.

[17] 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.

[18] 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.

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 12 (Who Am I?) percent of a standard deviation. For the behavioral 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 percent of a standard deviation or less.

We also use data from the Labour Force Survey (LFS) for part of the analysis. 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

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

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) may have unobserved characteristics that lead their children to have better or worse developmental outcomes. We therefore need to find exogenous variation in T to obtain unbiased estimates of β.

Our strategy is to use the variation in time at home induced by the maternity leave reform. Our first stage equation is

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

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,[19] 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. 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

[19] For children surveyed at older ages, the observation may feature censored values of T_i because the mother is still at home at the date of the survey. The use of TFY_i rather than T_i provides a neat solution to observations with censored values of T_i by limiting the time frame to one year. It also ensures that our instrument respects the monotonicity assumption required for IV. As noted by Klerman and Leibowitz (1997), it is possible that some mothers reduce their time at home post birth after an increase in the duration of mandated maternity leave if their optimal leave length is greater than but close to the post reform maximum. Empirically this does not appear to be a significant effect of the reform we investigate (Baker and Milligan 2010). Nevertheless, our instrument should have a monotonic impact on *TFY* as the post reform maximum maternity leave mandate is 12 months.

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 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, single month of child's age at the survey date, province, city size, mothers' and fathers' education (4 categories), age (6 categories), immigrant status, and the presence of up to 2 older or younger siblings.[20]

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.

An 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, this means that any other maternal input (such as breastfeeding) 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

[20] The regressions for age standardized PPVT scores omit the single month of child's age. The results including these age controls are very similar to the ones reported.

effect of extra maternal time, but also any 'downstream' impacts of the extra maternal time on other inputs. In this sense, care must be taken in interpreting the coefficients in 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 some support for this position.

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. 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.[21] 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. The estimate for all women is an increase of 2.17 months.[22] Evidence in Baker and Milligan (2008a) indicates between 65 and 75 percent of women were eligible for the leave over this period, leading to the reported impact for treated women of between (1/0.75) 2.8 and (1/0.65) 3.3 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 come into effect by just over 1 point. Later in the paper, we subject this tentative finding to greater empirical scrutiny.

[21] The polynomials in quarter are not included here given the specification of year of birth effects.

[22] A regression of these year of birth effects on a constant and dummy variable for the reform yields an estimated impact of the reform of 2.173 (0.181). See table 3.

Differences in Observable Developmental Inputs across Birth Cohorts at Ages 1 through 4

A strength of our study is that we can assess the identifying assumption that no other changes in developmental inputs are correlated with our instruments. Bernal and Keane (2009) 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 thoroughly test the validity of our identifying assumption 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. To do this, 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 year of birth effects omitting the intercept. In the second stage 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 3. The first row contains results for TFY_i at different ages between 13 and 71 months. 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 the same when measured at the different points of age. They are in fact very similar ranging from just under two to two and a quarter months. 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.

The final two rows 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. The estimates for all ages are 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 questions about these outcomes do not have a fixed reference point. [23] We therefore turn to the LFS to investigate these and other inputs.

[23] 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.

The results for the LFS are reported in table 4, 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 3 but span the same interval from 13 to 71 months.[24] 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. 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.[25]

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.

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

[24] 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.
[25] 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

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 4 are measures of maternal care that are available in the LFS.[26] 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 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

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

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 3).

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 3 and 4 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 identifying assumption (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.

IV estimates of the impact of Mother's Care on Developmental Outcomes at Age 4 and 5

We now present our main results. We first show our IV estimates, and how they vary with different sets of controls and in different samples. Following the presentation of these results, we proceed in the next section to examine how our results vary with and without controls and investigate the influence of the control variables on our analysis.

In table 5 are IV 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 *F*-statistics for the instrument from the corresponding first stage regressions are reported in the preceding column. The instrument is very significant in each first stage regression.

The estimates for PPVT and Who Am I? are both negative and statistically significant. The estimate for Know Your Numbers is positive and marginally statistically significant. The estimate for PPVT indicates a one month increase in maternal care over the range we study leads to a decrease in this score of 5.7 percent of a standard deviation. The result for Who Am I? is a decrease of 5.1 percent of a standard deviation. Note that the estimate for PPVT echoes the visual inference presented in figures 1 and 2.

The estimates for the behavioral measures are uniformly small—especially for anxiety and hyperactivity—and statistically insignificant. In each case the estimated standard error is 2.8 percent or less of the standard deviation of the corresponding measure. This indicates the power to detect changes in the indicators of at least 5½ 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.[27] The point estimates for the cognitive measures and the indirect aggression measure are robust to these innovations although for the pre/post reform quadratic specification the standard errors are

[27] 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.

much larger. For the remaining behavioral measures, however, the point estimates vary in sign and magnitude with the controls for time.

In column 5 we use our second instrument, weeks of mandated job protected leave, which varies at the provincial level. 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 5 display remarkable stability across the specifications. The estimates for Who Am I? display a bit more sensitivity, although none is positive and all are of the same relative magnitude. The estimates for the other measures vary more significantly across columns, which might be expected given their relatively larger standard errors and lack of statistical significance.

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 6. The estimates for PPVT are very similar to the estimates in the previous table. For Who Am I? the estimates are consistently negative, towards the upper end of the range of estimates in table 5 and statistically insignificant due to much larger standard errors. For the remaining measures the standard errors are also much larger and the point estimates are mostly different than the estimates in table 5 although they remain relatively small.

To try to shed more light on the meaning of the negative impacts on the cognitive scores in table 5, in table 7 we report estimates separately for some sample splits that can be supported by the sample sizes of our data. In the first two columns are estimates separately for male and female children. While the point estimates for many of the developmental measures suggest different impacts by sex, it is the results for PPVT that stand out. The results suggest the impact is borne almost exclusively by males.

In the second two columns we report estimates separately by mothers' education, defining one group by up to a high school diploma and the other by any post secondary education or training. While the split by sex in the sample is roughly 50/50, the split by mothers' education is roughly 25/75. The only statistically significant result by this split is for the children of more educated mothers, but the standard errors of the estimates for children of less educated mothers are quite large.

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 3 and 4 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 result for Who Am I? is in line with the preceding estimates, while the estimate for PPVT is about half the size

of the previous estimates. The result for Know Your Numbers contrasts with most of the preceding estimates underlying the sensitivity of the results for this outcome to sample, and specification.

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 in the range of estimates from the previous tables and at most marginally statistically significant.

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 a change in the questions about education starting in cycle 7. In this cycle 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. 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 is also a change 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 report 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 change in the educational questions in the NLSCY, or some other sample issue.[28] 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.

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. An exception is hyperactivity—in this case the estimate converges to the estimates in table 5 when a cubic in time is specified (result not reported). This means that the statistically significant estimates for these outcomes in column 1 of table 5 cannot be distinguished from a simple trend in the data. More generally the estimates

[28] 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.

for these outcomes are sensitive to the specification of controls for secular trends. However, this is not the case for PPVT or 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 5 provide little evidence that the Canadian maternity leave reforms had a measurable *positive* impact on the cognitive and behavioral outcomes of children. We believe this is a significant conclusion. Firstly, as noted in the introduction, a positive impact is assumed in legislation enacting maternity leave in many developed countries. Secondly, 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.

This conclusion is consistent with most evidence of the impact of maternity leave on developmental outcomes at older ages (Dustmann and Schonberg 2008, Liu and Skans 2009, Rasmussen 2010). Each of these studies has a credible identification strategy based on sharp changes in maternity leave laws. What makes the results here of particular interest is the large change in maternal care, the measurement of children's outcomes at ages 4 and 5 and the detailed evidence on developmental inputs in the years between the change in maternal care and the behavioral and cognitive measurement. 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 results therefore provide a bridge to the inference on long term outcomes provided in other studies.

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.

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 5.7 percent of a standard deviation, while the

impact on Who Am I? is 5.1 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. It is certainly true that some observational studies find early childhood education has positive impacts on cognition for which maternal care is often a substitute. However, it is not obvious what sort of critical early childhood instruction is provided to children of these very early ages when in non parental care.

We think a more 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.[29] 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.

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

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

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. 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 of 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.

Our results 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	9958	101.87	102.27	101.40
		(15.37)	(15.15)	(15.62)
Know Your	9977	1.37	1.39	1.34
Numbers		(0.58)	(0.59)	(0.58)
Who Am I?	9485	24.77	25.16	24.34
		(6.29)	(6.04)	(6.53)
Aggression	10994	1.64	1.68	1.60
		(1.88)	(1.91)	(1.85)
Indirect	10788	0.52	0.55	0.48
Aggression		(1.08)	(1.09)	(1.07)
Anxiety	10995	1.98	1.95	2.00
		(1.93)	(1.90)	(1.97)
Hyperactivity	10969	3.93	3.87	3.98
		(2.65)	(2.66)	(2.64)

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.

Table 3: 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	1.979** (0.090)	2.197** (0.153)	2.246** (0.181)
Mother returned to work post-Birth	-0.019 (0.017)	0.000 (0.024)	0.004 (0.015)
Child has younger sibling(s)	-0.021 (0.014)	-0.001 (0.021)	-0.008 (0.017)
Child has older sibling(s)	-0.014 (0.015)	-0.010 (0.010)	-0.006 (0.023)

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. * and ** indicate statistical significance at the 10% and 5% levels respectively.

Table 4: 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 5: Two Stage Least Squares Estimates of the Impact of Maternal Care on Developmental Outcomes, 1997-2004 sample

	F-stat 1st Stage	1	2	3	4	5
PPVT	103.63	-0.883**	-0.821**	-0.808***	-0.779*	-1.203*
		(0.392)	(0.388)	(0.300)	(0.423)	(0.630)
Know Your Numbers	78.85	0.028*	0.022	0.021	0.022	-0.007
		(0.017)	(0.017)	(0.013)	(0.019)	(0.027)
Who Am I?	78.15	-0.322**	-0.254*	-0.502***	-0.203	-0.529*
		(0.162)	(0.154)	(0.130)	(0.176)	(0.277)
Aggression	91.20	-0.018	-0.002	-0.023	0.005	-0.037
		(0.053)	(0.052)	(0.043)	(0.059)	(0.087)
Indirect Aggression	87.42	0.020	0.025	0.053**	0.021	0.041
		(0.028)	(0.27)	(0.023)	(0.031)	(0.046)
Anxiety	90.25	-0.002	0.014	0.018	0.021	0.081
		(0.055)	(0.054)	(0.045)	(0.061)	(0.092)
Hyperactivity	90.06	-0.002	-0.001	0.074	-0.021	0.170
		(0.076)	(0.076)	(0.063)	(0.086)	(0.130)
Instrument		Treatment	Treatment	Treatment	Treatment	Leave
Period		1997-2004	1997-2004	1997-2004	1997-2004	1997-2004
Demographic Controls		Yes	Yes	Yes	Yes	Yes
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 the indicated specification of demographic characteristics and time effects. The reported *F* statistics are for the TSLS estimates in column (1). *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

Table 6: Two Stage Least Squares Estimates of the Impact of Maternal Care on Developmental Outcomes, 1999-2002 sample

	1	2	3
PPVT	-0.870**	-0.879**	-0.903**
	(0.377)	(0.378)	(0.381)
Know Your	-0.022	-0.021	-0.022
Numbers	(0.081)	(0.080)	(0.081)
Who Am I?	-0.483	-0.586	-0.550
	(0.722)	(0.726)	(0.726)
Aggression	0.131	0.124	0.132
	(0.271)	(0.264)	(0.271)
Indirect	0.094	0.092	0.093
Aggression	(0.147)	(0.144)	(0.148)
Anxiety	0.121	0.120	0.121
	(0.282)	(0.275)	(0.282)
Hyperactivity	-0.412	-0.420	0.411
	(0.460)	(0.451)	(0.460)
Instrument	Treatment	Treatment	Treatment
Period	1999-2002	1999-2002	1999-2002
Demographic Controls	Yes	Yes	No
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 the indicated specification of demographic characteristics and time effects. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

Table 7: Two Stage Least Squares Estimates of the Impact of Maternal Care on Developmental Outcomes for Selected Subsamples

	Sample			
	Male Child	Female Child	Mother Low Education	Mother High Education
PPVT	-1.632***	-0.004	-1.102	-0.795*
	(0.552)	(0.547)	(0.939)	(0.433)
Know Your	0.014	0.045*	0.014	0.034*
Numbers	(0.024)	(0.025)	(0.039)	(0.019)
Who Am I?	-0.407*	-0.246	0.011	-0.463**
	(0.234)	(0.223)	(0.369)	(0.185)
Aggression	0.075	-0.113	-0.133	0.013
	(0.075)	(0.078)	(0.148)	(0.057)
Indirect	-0.009	0.045	-0.023	0.025
Aggression	(0.038)	(0.042)	(0.085)	(0.029)
Anxiety	0.059	-0.061	-0.181	0.052
	(0.076)	(0.082)	(0.149)	(0.061)
Hyperactivity	0.049	-0.079	-0.207	0.044
	(0.109)	(0.107)	(0.218)	(0.081)
Instrument	Treatment	Treatment	Treatment	Treatment
Period	1997-2004	1997-2004	1997-2004	1997-2004
Demographic Controls	Yes	Yes	Yes	Yes
Control for Time	Quartic	Quartic	Quartic	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. "Low Education" is defined by schooling up to a high school diploma. "High Education" is defined as the receipt of any post secondary instruction. * 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

	1	2	3
PPVT	-0.406***	-0.785***	-1.190**
	(0.135)	(0.130)	(0.500)
Know Your	-0.022***	-0.031***	0.025*
Numbers	(0.005)	(0.005)	(0.014)
Who Am I?	-0.223***	-0.336***	-0.405***
	(0.052)	(0.050)	(0.137)
Aggression	-0.062***	-0.052***	-0.041
	(0.017)	(0.017)	(0.044)
Indirect	-0.039***	-0.033***	0.054**
Aggression	(0.009)	(0.008)	(0.024)
Anxiety	0.029*	0.017	0.003
	(0.018)	(0.017)	(0.044)
Hyperactivity	0.031	0.057**	0.054
	(0.024)	(0.024)	(0.063)
Instrument	Treatment	Treatment	Treatment
Period	1997-2004	1997-2004	1997-2004
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 specification of demographic characteristics and time effects. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

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)
2003	0.084***	0.109***	0.048**	0.032**
	(0.013)	(0.017)	(0.015)	(0.016)

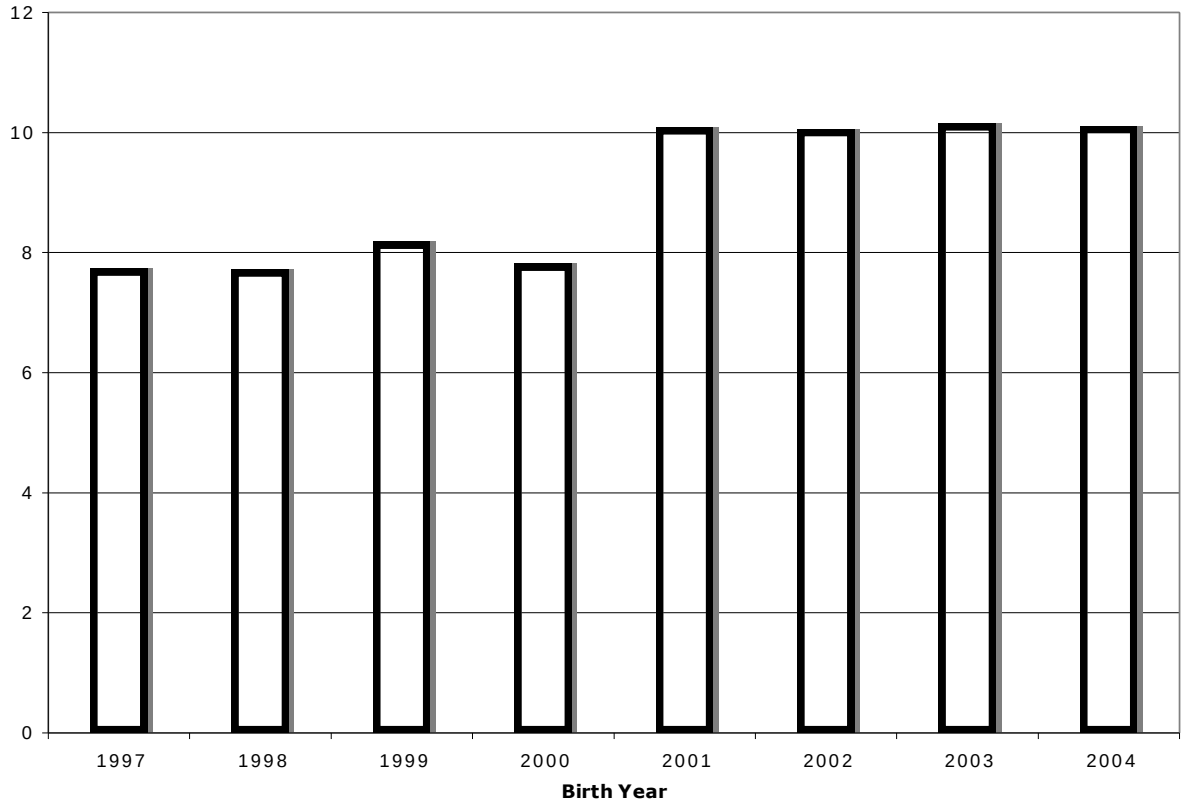
Notes: The data are from the NLSCY. All estimates are from regressions of the indicated dummy variable for educational attainment on dummy variables for the child's year of birth. 1997 is the omitted birth cohort. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

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)

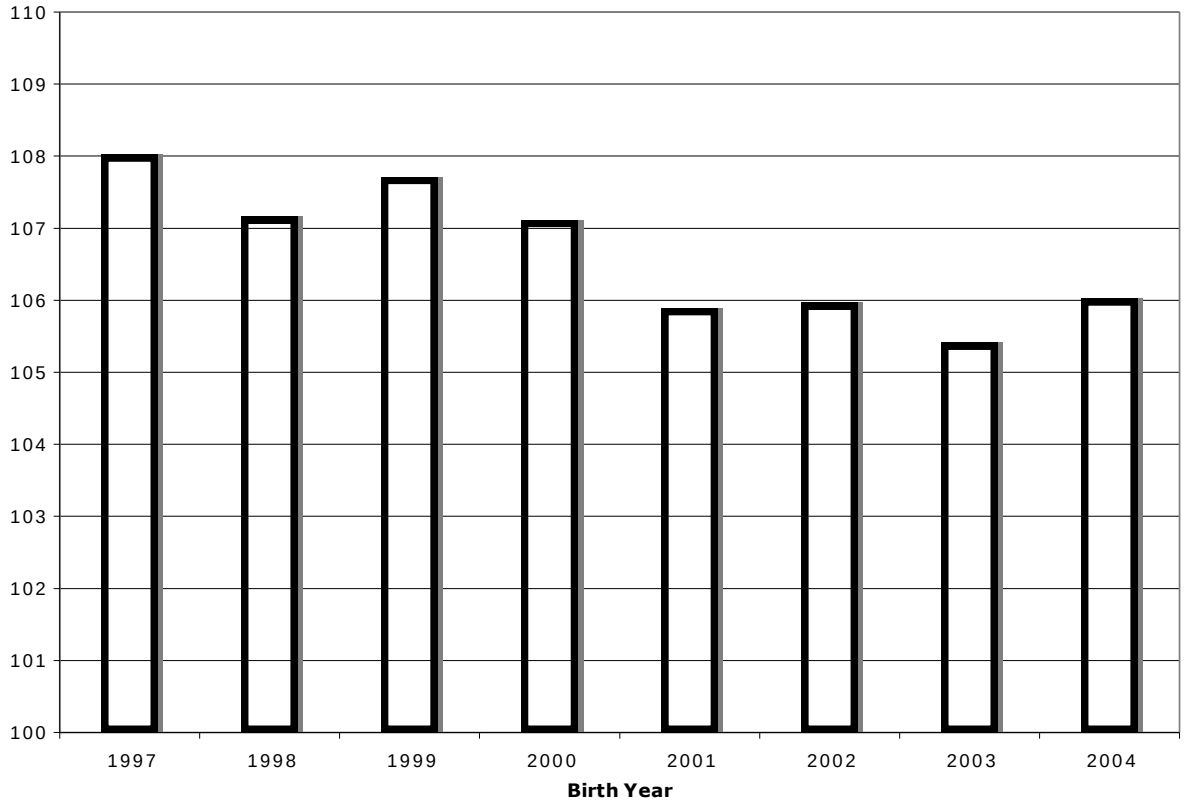
Notes: The data are from the LFS. All estimates are from regressions of the indicated dummy variable for educational attainment on dummy variables for the child's year of birth. 1997 is the omitted birth cohort. *, ** and *** indicate statistical significance at the 10%, 5% and 1% levels respectively.

Figure 1: Time Mother at Home in First Year: Estimated Year of Birth Effects Relative to the 1997 Birth Cohort



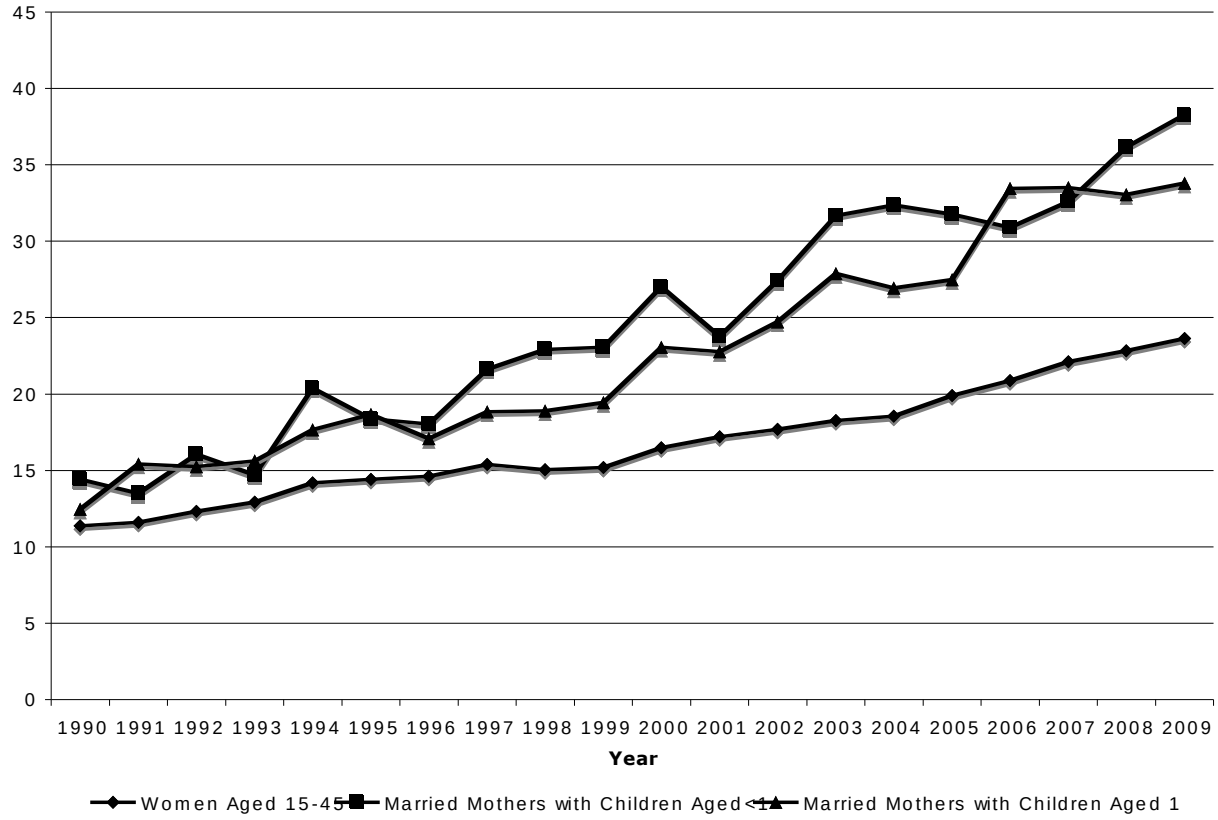
Notes: Estimates are from a regression of mothers' time at home in the first year on demographic controls and year of birth effects (no constant). The data are from the NLSCY.

Figure 2: PPVT: Estimated Year of Birth Effects Relative to the 1997 Birth Cohort



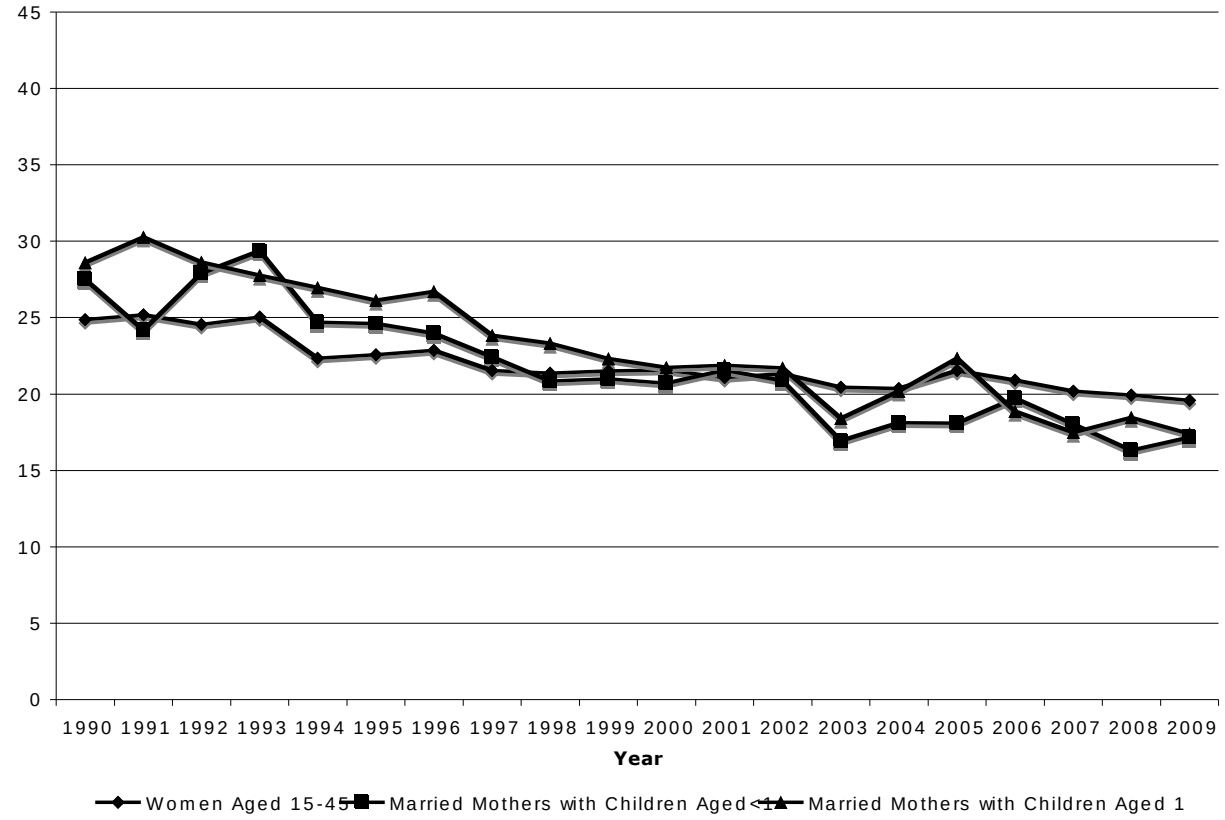
Notes: Estimates are from a regression of children's age standardized PPVT score on demographic controls and year of birth effects (no constant). The data are from the NLSCY.

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.

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.
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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.[30] 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).

[30] 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 Development al Inputs by Year of Birth: NLSCY Data

	N	1997	1998	1999	2000	2001	2002	2003	2004
13-24 months									
Time mother	8307	8.108	7.915	8.297	7.786	10.134	10.082	10.037	9.969
home in first year		(0.192)	(0.249)	(0.218)	(0.271)	(0.215)	(0.257)	(0.214)	(0.270)
Mother returned	8307	0.843	0.890	0.814	0.859	0.813	0.836	0.836	0.847
to work post-		(0.025)	(0.033)	(0.033)	(0.035)	(0.029)	(0.035)	(0.028)	(0.035)
Birth									
Child has younger	8307	0.108	0.111	0.061	0.084	0.048	0.087	0.076	0.068
sibling		(0.015)	(0.018)	(0.016)	(0.019)	(0.017)	(0.020)	(0.017)	(0.020)
Child has older	8307	0.549	0.566	0.572	0.579	0.586	0.526	0.565	0.536
sibling		(0.026)	(0.034)	(0.029)	(0.036)	(0.029)	(0.036)	(0.029)	(0.037)
25-39 months									
Time mother	8520	8.173	8.009	8.691	8.242	10.412	10.451	10.615	10.424
home in first year		(0.207)	(0.243)	(0.239)	(0.254)	(0.227)	(0.243)	(0.220)	(0.250)
Mother returned	8520	0.939	0.933	0.868	0.965	0.943	0.953	0.904	0.905
to work post-		(0.024)	(0.029)	(0.029)	(0.031)	(0.027)	(0.030)	(0.027)	(0.030)
Birth									
Child has younger	8520	0.397	0.343	0.308	0.347	0.339	0.326	0.360	0.368
sibling		(0.025)	(0.030)	(0.029)	(0.031)	(0.028)	(0.030)	(0.028)	(0.031)
Child has older	8520	0.489	0.509	0.503	0.484	0.508	0.473	0.486	0.478
sibling		(0.027)	(0.032)	(0.031)	(0.034)	(0.030)	(0.033)	(0.030)	(0.034)
48-71 months									
Time mother	11165	7.862	7.839	8.271	7.952	10.182	10.209	10.253	10.263
home in first year		(0.199)	(0.205)	(0.211)	(0.233)	(0.208)	(0.229)	(0.209)	(0.234)
Mother returned	11193	0.936	0.960	0.910	0.962	0.950	0.970	0.941	0.926
to work post-		(0.021)	(0.022)	(0.023)	(0.024)	(0.022)	(0.024)	(0.022)	(0.025)
Birth									
Child has younger	11193	0.505	0.482	0.463	0.487	0.453	0.453	0.486	0.515
sibling		(0.025)	(0.026)	(0.027)	(0.030)	(0.028)	(0.030)	(0.030)	(0.030)
Child has older	11193	0.589	0.593	0.586	0.546	0.621	0.546	0.592	0.532
sibling		(0.035)	(0.026)	(0.027)	(0.030)	(0.027)	(0.030)	(0.027)	(0.030)

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 Development al 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	0.257	0.251	0.245	N.A.	0.249	0.260	0.244
		(0.028)	(0.028)	(0.029)	(0.029)		(0.029)	(0.029)	(0.029)
Mother Employed Full Time	12260	0.263	0.269	0.277	0.275	N.A.	0.282	0.299	0.309
		(0.028)	(0.028)	(0.029)	(0.029)		(0.029)	(0.029)	(0.029)
Mother's Usual Weekly Hours	7801	35.139	35.447	35.814	36.027	N.A.	36.096	36.224	36.796
		(1.056)	(1.066)	(1.062)	(1.097)		(1.071)	(1.065)	(1.072)
Mother Not in the Labor Force	12260	0.610	0.608	0.617	0.619	N.A.	0.614	0.606	0.621
		(0.028)	(0.028)	(0.029)	(0.028)		(0.028)	(0.029)	(0.029)
Mother's Real Weekly Earnings	6656	230.796	233.557	240.716	244.111	N.A.	249.198	248.631	284.904
		(21.776)	(21.809)	(22.121)	(22.208)		(22.298)	(22.618)	(22.831)
Mother's Real Hourly Earnings	6656	6.027	5.903	6.228	6.086	N.A.	6.086	6.100	6.711
		(0.516)	(0.516)	(0.528)	(0.530)		(0.534)	(0.543)	(0.545)
Economic Family's Real Weekly Earnings	10309	440.903	471.176	469.782	468.963	N.A.	479.446	489.800	508.334
		(34.576)	(33.938)	(35.223)	(35.140)		(34.980)	(35.519)	(36.408)
Mother working PT to care for own children	12260	-0.074	-0.068	-0.091	-0.094	N.A.	-0.093	-0.097	-0.118
		(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	0.0003	0.0008	0.0003	N.A.	0.0014	0.0015	0.0013
		(0.0013)	(0.0016)	(0.0015)	(0.0014)		(0.0017)	(0.0020)	(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 Family's Real Weekly Earnings	6105	494.583 (45.345)	460.088 (44.620)	474.153 (45.616)	441.590 (46.533)	492.074 (45.408)	487.521 (46.799)	552.507 (46.419)	597.828 (50.744)
Mother working PT to care for own children	7212	-0.062 (0.024)	-0.070 (0.023)	-0.061 (0.024)	-0.088 (0.023)	-0.079 (0.023)	-0.073 (0.024)	-0.085 (0.023)	-0.084 (0.024)
Mother not	7212	0.0018	0.0016	0.0018	0.0028	0.0038	0.0030	0.0040	0.0029

available for work-caring for own children		(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 (0.014)	0.049 (0.014)	0.043 (0.014)	0.045 (0.014)	0.047 (0.014)	0.046 (0.014)	0.045 (0.013)	0.049 (0.015)
Stay at Home Mother (YBB)	7212	0.140 (0.029)	0.146 (0.029)	0.138 (0.028)	0.165 (0.029)	0.137 (0.029)	0.125 (0.028)	0.133 (0.028)	0.145 (0.029)
Stay at Home Mother (YOB)	7212	0.151 (0.031)	0.159 (0.031)	0.157 (0.031)	0.187 (0.032)	0.163 (0.031)	0.133 (0.031)	0.156 (0.031)	0.161 (0.032)
Mother has younger children	7212	0.378 (0.038)	0.401 (0.038)	0.391 (0.038)	0.404 (0.038)	0.420 (0.038)	0.424 (0.039)	0.396 (0.039)	0.421 (0.039)
Mother has older children	7212	0.393 (0.041)	0.387 (0.040)	0.376 (0.041)	0.376 (0.041)	0.392 (0.041)	0.338 (0.041)	0.381 (0.041)	0.339 (0.042)
Age 48-59 months									
Mother Employed	7390	0.263 (0.039)	0.275 (0.039)	0.235 (0.039)	0.245 (0.040)	0.249 (0.040)	0.266 (0.040)	0.253 (0.040)	0.237 (0.039)
Mother Employed Full Time	7390	0.335 (0.040)	0.295 (0.040)	0.315 (0.040)	0.333 (0.040)	0.333 (0.41)	0.360 (0.041)	0.357 (0.041)	0.356 (0.040)
Mother's Usual Weekly Hours	4881	38.227 (1.449)	36.779 (1.463)	37.812 (1.417)	38.630 (1.448)	38.560 (1.463)	38.658 (1.450)	39.361 (1.460)	39.179 (1.440)
Mother Not in the Labor Force	7390	0.618 (0.038)	0.604 (0.039)	0.651 (0.039)	0.641 (0.039)	0.628 (0.039)	0.623 (0.039)	0.639 (0.040)	0.637 (0.039)
Mother's Real Weekly Earnings	4090	288.641 (31.054)	298.639 (31.896)	305.781 (31.659)	312.933 (32.278)	317.327 (33.181)	339.985 (32.415)	358.829 (33.198)	361.668 (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	7390	-0.085	-0.048	-0.092	-0.093	-0.091	-0.091	-0.091	-0.105

PT to care for own children		(0.023)	(0.025)	(0.024)	(0.024)	(0.024)	(0.024)	(0.024)	(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	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)
Age 60-71 months									
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 Labor Force	7298	0.612 (0.039)	0.631 (0.039)	0.624 (0.040)	0.632 (0.039)	0.640 (0.039)	0.618 (0.039)	0.641 (0.040)	0.635 (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	6136	515.362	498.161	527.541	531.564	574.278	576.359	575.050	602.437

Family's Real Weekly Earnings		(53.217)	(52.856)	(53.975)	(53.241)	(54.202)	(54.263)	(55.103)	(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.