The Early Development and Health Benefits of Maternity Leave Mandates

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Abstract

We study the impact of an expansion of maternity leave mandates from 6 months to 12 months on the health and development of children. Our advantage relative to previous research lies in a compelling policy episode which improves the causal nature of inferences and a detailed data source which allows the investigation of more subtle health and development indicators. We estimate a substantial impact on time at home for mothers, but only limited impact on breast feeding and measures of infant health. On child development, we estimate a strong impact on the age at which milestones such as feeding and speaking are reached, as well as some indicators of an improved parental environment, but do not find strong evidence for a persistent impact of other measures such as motor-social development.

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Introduction

Maternity leave mandates allow governments to significantly affect a child's environment at the start of life. While some of the arguments for these mandates have other foci—for example, enhancing the continuity of mothers' labour market careers—most are firmly rooted in child welfare. Mandates vary widely in both duration and income replacement across countries. Some of the most generous are found in Europe where leaves can be up to a year long at full salary. In contrast, leaves in Australia and the United States are short and/or unpaid. An explanation of this revealed disagreement about the value of leaves is that much of the evidence of their benefit is based on correlational rather than causal inferences, and in some cases not widely replicated.

In this paper we examine the effects of maternity leave mandates on a wide array of infant health and developmental outcomes, as well as indicators of mother and family well being. The basis of our inference is a dramatic extension in the duration of job protected maternity/parental leave in Canada in 2000/01. Whether purposely or not, Canada's mandates have historically stuck a compromise between the European and Australian/U.S. models; modest leave duration combined with modest income replacement provided through the unemployment insurance system (called Employment Insurance in Canada). In 2000/01 the duration of leave was extended from roughly 6 months to one year. This episode, therefore, can potentially inform both the current constellation of extended leaves in Europe, as well American and Australian policy makers seeking evidence of the value of longer mandates.

Our analysis also speaks directly to research that shows maternal employment in a child's first year of life can have long run detrimental effects on cognitive development (e.g., Baum 2003, Belsky & Eggebeen 1991, Ruhm 2004, Waldfogel et al. 2002 and review in Waldfogel

2006). While this body of research highlights the potential link between early maternal employment and longer-run outcomes, less is known about the mechanisms through which this occurs. With our data, we are able to explore detailed changes in infants' developmental environment in response to an exogenous change in parental care, thereby shedding light on the potential mechanisms.

Knowing more about the mechanisms is important for two reasons. First, it can provide further evidence on the credibility of the link between employment and longer run outcomes, checking that the long-run evidence isn't picking up other determinants of outcomes that vary for employed mothers. Second, knowing through which channels maternity leaves have their impact can provide important information for policy design. For these reasons, understanding the mechanisms is critical.

To examine the Canadian maternity leave mandate change we use data from the National Longitudinal Survey of Children and Youth (NLSCY). This survey is a nationally representative survey of Canada's children that follows their health and their cognitive, emotional, behavioural and motor development. Children exposed to the longer 12 month mandate are aged 0-29 months in data currently available, so we focus our study on this younger age group. Therefore, our results are for child development over the first two and a half years, leaving the study of older children to future work.

Our results focus on three aspects of maternity leave: labour market and childcare, breast feeding and health, and parenting and child development. For the first set of results, we find that, despite the relatively modest income replacement available in the Canadian system, maternal labour supply is quite responsive to the extension of maternity leave. Among mothers returning to work, we estimate an elasticity of the duration of time at home post birth with

respect to the leave mandate of about 0.7. The resulting increase in parental care (3.4 months or almost 50 percent) the children of these mothers receive, primarily between the ages of 7 and 12 months, displaces unlicensed non-parental care in another's home. If unlicensed non-parental care is of lower quality than parental care, then the extension of maternity leave has directly improved the care environment of these children. These results provide a strong "first stage" for the investigation of any impact on breastfeeding, health and development.

We find that mothers' breastfeeding behaviour is much less responsive to the change in leave. We estimate an elasticity of the duration of breastfeeding with respect to the leave mandate of 0.22; the estimate for exclusive breastfeeding is 0.21. Combining the analysis of labour supply and breastfeeding leads to a two sample IV estimate of the elasticity of breastfeeding duration with respect to time at home post birth of 0.45. We are not aware of comparable estimates of these elasticities in the literature. Finally, post reform the proportion of mothers reporting "return to work" as a reason for ending breastfeeding, which is typically the leading reason at longer durations, falls to close to zero. Together these results reveal the limitations of leave policy as a mechanism to achieve aggressive public health goals for breastfeeding.

Increased parental care/longer breastfeeding is thought to be beneficial to both mothers' and children's health. We find few impacts, however, on the measures of health available in the NLSCY. The increased period of breast feeding does not appear to compromise infant health (as measured by weight), but very few of our health indicators indicate an effect – asthma and chronic conditions show the most consistent beneficial results.

Finally, our results for parenting suggest that longer maternity leave leads to improved parenting that persists to older ages. We estimate significant reductions in the age of certain

developmental milestones: when the child first feeds him/her self or speaks a first word. Regarding activities, some of our findings are as expected—increased participation in organized parent/child playgroups and programs over the period of increased parental care. However, other results are less consistent with expectations—no changes in informal parent/child activities such as reading and walks, and a significant reduction in singing. We also do not find any effect on measures of infants' temperament, which are arguably related to the attachment and security they sense in their relationships with their parents. Finally, we find no effect on an index of motor and social development. Analysis of any longer term effects on cognitive ability awaits future waves of data that capture the treated children at older ages.

We tentatively conclude that longer maternity leave mandates do have significant effects on the developmental and care environments of infants under age one. They are not, however, very effective public policy instruments to influence the duration of breastfeeding. Also, we do not find an effect on a mechanism that obviously accounts for a relationship between early maternal employment and cognitive ability documented in previous research.

Maternity Leaves and Child Development

The connection between maternity leave mandates and child development appears to be neither simple nor transparent. Any pathway is predicated, however, on a causal link between the duration of the maternity leave mandate and the amount of time mothers stay at home post birth. In this section, we review the evidence linking maternity leaves and child development.

Both theoretical reasoning and empirical evidence reveal ambiguity in the link between mandates and development. Klerman and Leibowitz (1997) present a model of maternity leave mandates that demonstrates how a mandate can both induce some mothers to stay home longer with their newborns, but other mothers to return to work earlier. Studies of the U.S. Family and

Medical Leave Act (FMLA) (or state specific initiatives that preceded it) report no effect on time at home post birth or estimates that are sensitive to specification (e.g., Baum 2003b, Klerman and Leibowitz 1997, Waldfogel 1999). A recent study of leave mandates in Canada (Baker and Milligan 2005) reports that shorter mandates have little effect on the time mothers spend at home, while longer mandates do. All of this evidence is consistent with a model in which short mandates largely replicate private arrangements mothers have already in place, while longer mandates do lead to an increase in time at home.

If mandates can influence the time at home, how does this translate into better outcomes for babies? An obvious and widely cited channel is breastfeeding. While it is self evident that breastfeeding is challenging when mothers work outside the home, there are few estimates of the impact of maternity leave mandates on the incidence or duration of breastfeeding. Furthermore, almost all evidence is correlational. There is some consensus that the initiation of breastfeeding is independent of the decision to return to work (e.g., Dennis 2002). However, the duration of breastfeeding after initiation does appear to depend on work decisions.

Research on the duration of breastfeeding reveals three key findings. First, women who return to work (especially full time) breastfeed for less time than those who don't (e.g., Berger et al. 2005, Chatterji and Frick 2005, Fein and Roe 1998, Gielen et al. 1991, Kearney and Cronenwett 1991, Kurinij et al. 1989). Second, breastfeeding tends to terminate near or in the month of the return to work (e.g., Lindberg 1996). Finally, mothers report return to work as a reason for ending breastfeeding (e.g., Bick et al. 1998, Schwartz et al. 2002). Very few studies directly examine the relationship between the duration of breastfeeding and the duration of time spent at home post birth (e.g., Visness and Kennedy 1997, Roe et al. 1999), so that the size of the impact of a longer maternity leave mandate on breastfeeding duration is an open question.

Public health goals for breastfeeding are ambitious. The World Health Organization recommends exclusive breastfeeding for 6 months, and breast milk with complementary foods up to age two. Health Canada has adopted the WHO recommendation while the American Academy of Pediatrics counsels at least one year of breastfeeding, the first six months exclusively.[1] The reasons for these recommendations are varied and not uncontroversial. For some the benefits of breastfeeding are expansive and unequivocal (e.g., American Academy of Pediatrics 1997, Dennis 2002, Chatterji and Frick 2005) They include protection against diarrhea, asthma, otitis media, sudden infant death syndrome, insulin-dependent diabetes, mellitus, Crohn's disease, lymphoma and leukemia as well as enhancing cognitive development and neurodevelopment. There are also benefits for mothers through decreased postpartum bleeding, more rapid uterine involution, an earlier return to pre-pregnant weight, delayed resumption of ovulation resulting in increased child spacing and possible reduced risks of ovarian cancer and breast cancer. Almost all this evidence is correlational, however, and others take a much more cautious and narrow position (e.g., Kramer 2001). Surveys of research preceding the WHO recommendation to increase exclusive breastfeeding from 4 to 6 months found compelling evidence only for a reduction in gastrointestinal infection in children and delayed mensus and marginal post birth weight reduction for mothers (Health Canada 2004, Kramer and Kukama 2002). For our empirical work, we use this existing base of evidence to guide our selection of health indicators to study.

Other benefits of extended maternity leave mandates may flow directly from the extended parental care of infants. Ruhm (2000) and Tanaka (2005) report that longer maternity leave

[1] In 2002, U.S. breastfeeding rates at 6 months were 35% and 13% for partial and exclusive breastfeeding respectively (Ruowei 2005). For Canada, 2003 data for births in the previous 5 years reveals rates at 6 months of 39% and 19% respectively (Statistics Canada 2006). The US Happy People 2010 goal is an overall breastfeeding rate of 50% at 6 months.

entitlements are associated with lower infant mortality. The origin of this outcome is not identified, but may involve lower accident rates or better monitoring and maintenance (e.g., checkups and immunizations) of infant health when mothers are at home.

Alternatively, developmental and cognitive outcomes may improve with greater mother/infant interaction. In the economic literature the mechanism for this effect is not well articulated, but one possibility is that non parental care at young ages has detrimental effects. This is consistent with studies that report maternal employment in the first year of life can have adverse consequences. Attachment theory provides a more explicit intermediary: the security and continuity of parent/child relationships. The "Strange Situation" procedure provides a systematic classification of children's attachment security. The consequence of insecure attachment for later outcomes is not empirically clear, however, as the field struggles with more rigorous theoretical predictions and control for confounding factors (e.g., Thompson and Raikes 2003).

A different argument comes from the emerging field of epigenetics. This field studies inheritable marks on genes that convey information such as which parent the gene came from, but also act as switches turning genes on and off. One line of investigation in the field is how environment can affect these switches, potentially triggering them to improperly turn genes on or off. A connection to infant development lies in the argument that maternal/parental care provides the optimal environment for those marks that play a role in the first years. For example, early social environment is thought to influence marks that affect serotonin function and thereby adult depression (e.g., Mustard 2006). Much of the current evidence appears to be animal based and application to humans is by analogy.

Maternity Leave and the Policy Environment in Canada

For most workers in Canada maternity leave provisions that provide a right to return to a pre birth job after a specified period of absence are found in provincial labor standards legislation. All provinces in Canada have maternity leave provisions. The provisions typically protect workers from dismissal due to pregnancy, specify that the leave is unpaid and its maximum duration, specify a minimum period of pre birth employment for eligibility, and finally specify which terms of employment are preserved during the leave and any responsibility of the employer to maintain benefits. Some workers are covered by federal labour standards (e.g., the federal public service, banking, transportation and communications), which contain maternity leave provisions which are similar in principle to their provincial counterparts.

While the legal entitlement is to unpaid leave, since 1971 some workers have access to income support during maternity leave through the Employment (i.e., Unemployment) Insurance (EI) system. Workers who qualify and take a full leave can receive benefits that replace 50-55% of weekly wages up to a cap set to the average wage. The eligibility rules for EI maternity/parental benefits are not the same as for maternal leave entitlements under provincial labor laws, so a worker could qualify for one and not the other. Also, the EI rules do not provide a right of return to the pre-birth job. The proportion of new mothers who receive EI maternity/parental leave benefits is 40-50%.[2]

Over the past 30 years, changes in the duration of EI maternity/parental leave benefit entitlements have influenced provincial maternity leave mandates. This is true as well for the policy change that we examine. Effective December 31, 2000, the duration of EI benefits for maternity/parental leave were increased from 25 to 50 weeks. Correspondingly, the provinces changed labor standards legislation to provide longer leaves.

[2] In National Longitudinal Survey of Children and Youth (NLSCY) data for 1994-2002, roughly 40% of mothers with children aged 3-11 months reported EI income in the previous calendar year.

In table 1 we summarize maternity leave mandates in effect in the different provinces in 2000 and 2001. The first thing to notice is that the mandate in Quebec did not change over this period, as it was already 70 weeks prior to the EI change.[3] Therefore, the policy change in this province is an increase in the proportion of leave that is eligible for income support rather than an increase in the length of leave. While this type of change holds interest in its own right, we omit the province of Quebec to maintain homogeneity in the treatment. Second, by 2001 the mandates in all the other provinces are at least one year with very little variation. There is some small variation in the date in which the increase in the leave comes into effect. Unfortunately the two provinces that delay to 2001 are too small for us to exploit this feature in our analysis. Third, there is some heterogeneity in the starting points in 2000, when leave mandates ranged from a low of 18 weeks in Alberta to a high of 35 weeks in Ontario. Finally, because all provinces save Quebec change their mandate more or less simultaneously, the policy change we seek to analyze is empirically coincident to a time effect starting in 2001.

Because all provinces changed their leave mandates simultaneously, it is important to account for other changes to the social and economic environments of families with young children over this period. Two specific developments stand out. First is the provision of heavily subsidized child care places ($5/day) to children aged 0 and 1 in the province of Quebec in the fall of 2000. This was the last phase of the Quebec Family Plan that provided these subsidized places to all children aged 0-4 in steps over the period 1997-2000. Baker et al. (2005) report that this program had significant effects on the use of non parental care in Quebec as well as on the development of children in this province. This policy provides an additional reason to omit Quebec children and mothers from the current study.

The second development is changes to the federal child tax credit between 1998 and

[3] The mandate in Quebec rose from 52 to 70 weeks in March 1997.

2002. Baker et al. (2005) report the amounts of federal and provincial refundable tax credits available to two child families of different types and income levels over this period. The results show that the resources available to mid to high income two parent families changed very little as a result of these reforms. The credits available to two parent families with lower incomes, however, did gradually rise on the order of $700 annually in total. The credits available to single parent families rose more dramatically especially at low income (up to $2500 in total). The much larger impact on single parent families counsels omitting these mothers and their children from the analysis. The effect on low income two parent families potentially confounds our analysis; a problem we can explore by exploring heterogeneity in any effects by education or other determinants of income.

Data

We use data from the National Longitudinal Study of Children and Youth (NLSCY). The NLSCY is a national longitudinal survey that follows cohorts of Canadian children from birth. The survey is conducted bi-annually and currently the 1994-95, 1996-97, 1998-99, 2000-01 and 2002-03 waves are available. The sampling frame is the same as the Canadian Labour Force Survey, which excludes only residents of the three northern territories, institutions, the military, and Indian Reserves. In 1994-95 the target population for the first wave of the NLSCY was children aged 0-11 in 1994. This cohort has been followed continuously across the following four waves. In addition, in waves 2 through 5 a new cohort of 0 and 1 year olds is added; these children are then followed longitudinally to age 5. This means that waves 1 through 4 provide a cross section snapshot of children aged 0 through the oldest age of the original cohort. In wave 5 a gap opens up at ages 6 and 7.

The NLSCY contains a rich array of information on children's development, health,

parenting, non parental care, test scores and school performance. The information varies both in content and detail by age. For example, there is no cognitive information prior to age 4, and measures of anxiety are constructed differently by age group. This limits both the research questions that can be asked for children at certain ages, as well as the use of children of different ages as control groups.

Our focus is on children up to 24 months in age from the birth cohorts 1998 through 2002 who are living in families with two parents. These observations come from waves 3, 4 and 5 of the survey. Given that 6 months of leave was available in almost all provinces prior to the reform, we exclude children aged 0-6 months in order to focus on the children experiencing the extension of leave. The only children aged 6 months or more who were born after the mandate extension come from wave 5 (2002-03). The data from waves 3 and 4 therefore contribute only untreated controls in the age range 7 to 24 months. There is also a small sample of 25-29 month olds who due to a peculiarity of the survey design were asked the questions for younger children rather than those for aged 3 years and older. A full set of estimates from this group are available on request, and we cite these results throughout the paper where appropriate.

The age structure of the NLSCY is explained in table 2. The table reports the minimum and maximum age in months for children of each birth year cohort in each of the three waves of data that we use. Because the survey is in the field from the fall to the spring, children born on the same day could potentially have different ages when surveyed even within the same wave. For the 1998 birth year, we see children ages 2 to 18 months in wave 3, and then 45 to 63 months by wave 5. For the children born after the policy change, we observe them in wave 5 at ages 9 to 29 months (2001 birth year) and 4 to 17 months (2002 birth year).

The focus on children in two parent families serves several purposes. First, it places

attention on families who have larger financial resources and therefore, are more likely to take advantage of the longer leave mandate. Second, as explained above, it minimizes the influence of policy driven confounding factors such as the revision to federal child benefits. Third, while enhancing the homogeneity of the group studied, this restriction does not severely limit the representativeness of the analysis as births in two parent families represent close to 90% of all births over the period.

We separate the children into two groups: a) ages 7-12 months, and b) ages 13-24 months. The first group isolates the infants that are being contemporaneously treated by the policy reform. The second group allows us to look for any longer term effects of the treatment.

As noted above, we omit children from Quebec. We also omit the small number of children born in Alberta and Saskatchewan in 2001 before the provincial labour law mandate was changed to conform to the change in EI rules. We suspect that some firms in this province may have granted these children's mothers longer leaves when the EI rules changed in anticipation of the change in the provincial law. Finally, we omit cases where the mother is not the "person most knowledgeable" (i.e., the respondent) about the child. This final restriction is to limit the heterogeneity in any response bias. This restriction together with our focus on two parent families yields 75% of all possible observations in each age group. Of the missing 25 percent, roughly 15 percentage points of the loss is due to the requirement that mothers be the PMK and 10 percentage points due to the restriction to two parent families.

For the analysis of breastfeeding we use data from the second and third cycles of the Canadian Community Health Survey (CCHS). These cycles of the CCHS were conducted between January and December of 2003 and 2005 respectively. The target population is individuals aged 12 or older. Females who have given birth in the preceding five years are asked

a series of questions about their breastfeeding practices. We use the data for women who had a child in the years 1998-2001 (second cycle) and 2000-2003 (third cycle), and again focus on mothers in two parent families.[4] While the accuracy of the information from the survey may be affected by recall bias, the questions are quite detailed relative to the breastfeeding information collected in the NLSCY. More importantly, survey difficulties in the fifth wave renders the NLSCY data on breastfeeding unreliable for children born in the 2001 and 2002 birth cohorts. Nevertheless, we refer in footnotes to some selected results from the NLSCY data for comparison.

The outcomes we investigate vary by age group. This is because either their interest is age specific, or because they are not available for all age groups. A first set measure direct changes in infants' environment and care as a result of the policy change. It includes how long the mother stayed at home post-birth, mothers' employment and work characteristics post birth, the type and amount of any non-parental care the child received, parenting practices and family functioning, and the duration of breastfeeding, including a measure of exclusive breastfeeding.

A second set of variables cover some health outcomes of the child and mother. A fairly complete birth report is available including birth weight and length, gestation, complications of the delivery and post natal complications. Measures of the current health of the child include height and weight, diagnoses of conditions such as asthma and bronchitis, injuries, the experience of nose and ear infections and overall self (parent) reported health indices. For mothers' current health we have self reported health, a depression index, and a record of post partum depression and complications.

[4] We also drop observations from Quebec. We unable to drop observations from Alberta and Saskatchewan in 2001 before the provincial labour law mandate was changed because month of birth is not provided. In regressions using mandated months of leave as an explanatory variable we use a calendar weighted average of the mandate for these provinces in 2001.

A final set of variables cover the emotional, motor and social development of the child. For social and motor development we have an aggregate index, as well as responses to the individual questions on which it is based. There are also a series of questions that capture the age at which milestones, such as first steps, are achieved. For emotions there is no aggregate index, only answers to individual questions about security and temperament.

Empirical Framework

A challenge to the empirical analysis is that the change in maternity leave mandates in 2001 was national and almost simultaneous. This means that in a direct analysis of the reform any effect will be observationally equivalent to a year effect. Consider the estimating equation

$$(1) \qquad y_{iw} = X_{iw}\beta + \sum_t \gamma^t YOBt_{iw} + \varepsilon_{iw}$$

where y is the outcome of child *i* in wave *w*, *X* are a set of control variables and *YOBt* is a year of birth indicator. A child is treated if $YOBt \geq 2001$. Because the interval of children's ages is the same for each birth cohort in each sample, the *YOB* indicators are perfectly co-linear with a set of year indicators, or more properly wave indicators for the wave of the survey in which the outcome is measured for children in a given birth cohort. Therefore, the γ^t's for the years of birth 2001 and later capture an effect of the change in maternity leave mandates on the outcome only if there are no other changes over time (e.g., wave effects) on other determinants of the dependent variable.

We adopt a number of approaches to this challenge. We start with equation (1). Estimates of the full set of *YOBt* effects for each dependent variable (reported in the appendix) provide a visual evaluation of whether the inter cohort variation in the mean of the dependent variable is consistent with the timing of the change in policy. This information is then summarized in the specification

(2) $$y_{iw} = X_{iw}\beta + \phi POST_{iw} + \varepsilon_{iw}$$

where we replace the *YOB* indicators with the dummy variable *POST*, which equals 1 if $YOB \geq 2001$, and 0 otherwise. While the results from this second regression, which are reported in the tables, provide an estimate of an average effect of the policy change, they are only interpretable causally in concert with the estimates of the individual *YOBt* effects.

Figures 1 and 2 demonstrate the issues. In figure 1 we graph the individual estimates of the year of birth effects (taken from the appendix) for the dependent variable "child's' age in months when the mother returned to work" using the age 13-24 month old sample. The omitted birth cohort is 1998, and the horizontal lines above and below the 0 line indicate the average standard error of the estimates. Clear in Figure 1 is a distinct and dramatic change in the estimates starting with the 2001 cohort. This cohort is the first eligible for the longer leaves. In this case we case we interpret the estimate of the *POST* causally with some confidence. In figure 2 we present similar information for an index of the temper of a child's reaction to changes in routine using the 7-12 month sample (only available for cohorts 1999-2002). In this case we obtain a statistically significant (and positive) estimate of *POST* in some specifications. This graph reveals, however, variability in the *YOBt*, and estimates of different signs for the two "treatment" cohorts (2001 & 2002), so that a causal interpretation would be aggressive.

For children aged 13-24 months we have an additional strategy that exploits the fact that some outcomes are measured for older children (aged 25-33) to construct a group of untreated children in each wave of the data.[5] This allows us to estimate a difference-in-difference equation which controls for any survey wave effects on outcomes. The equation estimated is

[5] We cannot construct this estimator for the 6-12 month age group as all children of these ages are treated in the 5th wave of data.

$$(3) \qquad y_{iw} = X_{iw}\beta + \alpha YNG_{iw} + \sum_{w} \lambda^{w} WAVEw + \eta YNG_{1w} WAVE5 + \varepsilon_{iw}$$

where YNG_{iw}=1 if the observation is for a child aged 13-24 months, and *WAVE5* is an indicator of an observation from wave 5 of the data, the only wave in which children aged 13-24 months were exposed to the longer maternity leave mandate.

Finally, as mentioned above we also have a 25-29 month age sample, for which we do not report estimates for the sake of brevity. Nevertheless, in this sample the outcomes of both treated (*YOB*=2001) and untreated (*YOB*=2000) children were measured in the same wave of the survey (wave 5). While these children were, of course, the same age in the same wave of the survey and not at the exact same point in time, estimates from this sample can be viewed as identifying an effect off of children born just before and after the change in policy, and controlling for any systematic survey wave effect on measurement. As we note in the course of presenting our results the inference from this sample is very consistent with the results we present for the younger samples.

None of these approaches replicates a randomized experiment. However, consistency across the different estimators gives us confidence we estimate a causal effect. We believe our empirical strategy offers advances on correlational strategies used in many previous studies.

The control variables in the base specification include dummy variables for males, single month of child's age, province, city size, mothers' and fathers' education (4 categories), age (6 categories) and immigrant status, and the presence of up to 2 older or younger siblings.[6] We also report a specification that adds wave of interview provincial unemployment rate as a control.[7]

An important issue for our inference is the correct calculation of the standard errors.

[6] For the CCHS regressions the control variables are dummy variables for province, city residence, mothers' education (4 categories), age (single year) and immigrant status. The other controls are not available in the CCHS.
[7] Because interviews process spans several months, we use the average unemployment rate for the months September through May in the relevant years.

Following Donald and Lang (2004), we recognize that our policy effects are identified by variation in the conditional mean of our dependent variables across (typically) 5 birth cohorts. We have calculated our standard errors a number of ways that recognize this finiteness of the inference, and no one approach is consistently the more conservative. The estimates we report are from a two step procedure. We first estimate equation (1) with no constant, which provides direct estimates of all the *YOBt* effects. We next use these estimates as the dependent variable in a 5 (or 4) observation regression analogous to equation (2), but with only a constant and *POST* as explanatory variables and weighting by the sum of the individual weights by year of birth. Note that the values of the dependent variables for the second step are just the estimates of the *YOBt* effects, reported in the appendix, rescaled. Because there will be 3 (or 2) degrees of freedom in these regressions, the critical values for statistical significance are much higher then normal (i.e., 3.2 or 4.3 at the 5 percent level). We indicate estimates significant at the 5 percent level using the appropriate values from the *t*-distribution. It is important to note that all the substantial inferences of the paper are robust to the different methods of calculating the standard errors.[8]

Time at home Post Work

Any benefits of longer maternity leave mandates are predicated on mothers staying at home longer post birth. Baker and Milligan (2005) analyze the impact of the 2000 maternity leave extensions on the decision to work post birth. They report significant changes in the proportions of mothers with a child aged less than one who were employed and on leave (up by

[8] We also directly estimated equations (1) and (2) comparing robust standard errors to estimates clustering the on year of birth. These latter estimates were implausibly *small* for some parameters of interest. Another approach involved a first step of estimating equation (1) with a full set of province/year of birth interactions. The estimates of POST were then estimated from a, e.g., 45 observation (9 provinces, 5 years of birth), second step regression using the province/year of birth interactions as the dependent variable. A third approach substituted a full set of province/*POST* interactions in the first step resulting in a, e.g., 18 observation second stage (9 provinces, 2 values of *POST*). This last method potentially has the advantage of addressing the effects of any auto correlation in the error term following the advice of Bertrand et al. (2004). For some results, while these alternatives produce larger standard errors, critical values are lower than for the method reported, so conclusions regarding statistical significance remain unchanged, and confidence intervals are very similar.

over 33%) and employed and at work (down by over 33%) as a result of this reform. This suggests that this policy led mothers to spend substantially more time a home post birth.

Our NLSCY data allow us to refine this inference by directly estimating the number of additional months mothers stayed at home. Each mother is asked the age of her child in months when she returned to work. We therefore can construct measures of the incidence of return to work, the point in time post birth a return takes place and other characteristics of this employment.

Estimates of equation (1) for measures of post birth work for our various samples are presented in table 3. The estimates for children aged 7-12 months are in the first 3 columns. Estimates both excluding and including the survey wave unemployment rate are reported. There is a large significant reduction in the proportion of mothers of these children who have returned to work of almost 30 percentage points, which is 54% of the pre-reform baseline rate of 0.55. The estimates of the individual *YOBt* effects, reported in the appendix, show that, conditional on the control variables, there is a discrete shift downward in the proportion employed, starting with the 2001 birth cohort. There is a much smaller, but significant impact on the proportion currently employed. In this case employed includes both those currently at work and those on leave from a job. The inference is that post reform more mothers terminated their jobs to give birth. The appendix table shows this effect was particularly strong for the 2001 birth cohort. The final two variables measure characteristics—hours and continuity—of any employment post birth. These estimates do not indicate any effect of the change in policy. While one of the estimates for the continuity or post birth work is statistically significant, and indicates a modest up tick in the proportion, the estimates of the individual *YOBt* effects in the appendix indicate that this is a spurious result of inter cohort variation in the proportion rather than a systematic

effect of the change in policy.

In the next three columns are results for children aged 13-24 months. Because the maximum leave duration is 12 months post reform, this age range is nominally not affected by the mandate extension. That said, the first row indicates a modest reduction in the proportion of mothers who have returned to work of almost 6 percentage points. The next row suggests that this reduction is not due to some individuals on very long maternity leaves as there is an almost 5 percentage point reduction in the proportion currently employed (i.e., at work or on leave), although these estimates are statistically significant only at the 10 percent level. Also, for both these variables there is some variability in the estimates of the individual *YOBt* effects (see the appendix) suggesting these results may overstate any reduction in employment. Consistent with the results for the 7-12 month sample, the next two rows indicate no effect on the continuity or hours of post birth work.

The final two rows investigate the duration of time at home for those mothers who have returned to work. There is an 8 to 9 percentage point reduction in the proportion of these mothers who had returned to work after 12 months—the length of the post 2000 maternity/parental leave mandates. In the next row is a key result: a 2.8 to 3.4 month increase in the amount of time these mothers stayed at home before returning, a 44 to 54 percent increase over the baseline rate of 6.3 months. This is the basis for any effects on the breastfeeding, health and development of their children. If we replace the POST dummy variable in equation (2) with a variable representing the leave entitlement by province, by year, at the time of birth, we can calculate an elasticity of time at home with respect to the leave mandate for these mothers. The

estimate of the parameter on the leave variables is 0.141 (0.032),[9] which at sample means implies an elasticity of 0.724. Together with a perhaps modest reduction in the proportion of mothers returning to work, this result indicates a significant increase in parental care primarily at age 7-12 months.

The key result of this section is also found in the 25-29 month old sample; this sample yields an estimated 3.2 month increase in the age at which mothers returned to work (conditional on return).[10] Other results for the sample are supportive of the preceding inference, but not typically statistically significant due to the smaller sample sizes.

Non Parental Care

Investigating changes in non parental care with the change in maternity leave mandates provides a simple consistency test of our employment results. More importantly, it also provides insight to changes in the care environments of infants when mothers are at home. The NLSCY provides a rich set of variables capturing the various types of non parental care.

In the three columns of table 4 are the results for children aged 7-12 months. There is a large, significant 16-17 percentage point reduction in the proportion of these children in non parental care which is 39-41% of the pre-reform mean. The estimates of the individual *YOBt* effects in the appendix suggest this change is a result of the policy reform. Note that this decrease is only (17/29) 59 % of the reduction in employment of these mothers. This could happen because some mothers taking longer maternity leave still used non parental care, or the childcare question is erroneously picking up programs attended by the mother and child, or

[9] Standard error in parentheses. In this case the leave variable varies by province and *POST*. We estimate equation (2) directly, substituting the leave entitlement for *POST*, and calculating the standard errors clustering at the province/*POST* level (18 groups).
[10] The estimate is 3.238 with a standard error of 0.425.

missing the use of very informal non-parental care.[11]

To explore this puzzle we split the mothers into four groups: working with care, working without care, not working with care and not working without care. Separate regressions for each of these categories (not reported) reveal 23 of the 29 percentage point reduction in mothers who have returned to work is in the category 'working with care', while the remainder is in the category 'working without care'. The reason we do not see a 23 point decrease in the use of non-parental care, however, is due to a 5-6 point increase in the proportion not working and using care. Therefore, the translation of the effect of the drop in employment to the *reported* use of non-parental care is moderated by both the use of care in the absence of work and work without care, which may simply reflect the underreporting of care in the survey.

The next rows reveal a fall in hours conditional on being in care and a greater than 50% reduction in unlicensed care in someone else's home. The latter is likely babysitters or family childcare providers working out of their homes. While some parents may prefer this type of care, the quality of unlicensed care may have higher variance.

In the next 3 columns we explore if any of these results persist for older children aged 13-24. All the estimates are small and statistically insignificant, indicating no deviation from the pre-reform status quo. This inference is reinforced by the estimates of the individual *YOBt*'s reported in the appendix, which do not indicate any discrete change in any variable starting with the 2001 cohort. This finding provides an additional reason to interpret the estimates in table 3, indicating a modest reduction in the employment of the mothers of these children, with caution.

Breastfeeding

[11] The gateway question to the childcare section of the survey asks "Do you currently use childcare such as daycare, babysitting, care by a relative or other caregiver, or a nursery school while you (and your spouse/partner) are at work or studying?" If the answer to this question is no, then the rest of the childcare section is skipped. It may be that some respondents who use more informal types of care answer 'no' to this gateway question.

Longer maternity leave may significantly extend the period infants are breastfed. This is important because breastfeeding rates significantly lag public policy goals. In the US the proportion of children breastfed in the early post partum period is close to 70%, which is just shy of the Happy People goal of 75%.[12] But the rate at 6 months of age is just one-third, and at one year under 20%, which are below the Happy People targets of 50% and 25% respectively. Exclusive breastfeeding (2002) is much lower at 43% at 3 months and 13% at 6 months, well below WHO recommendations. Rates in Canada prior to the extension of leave considered here are quite similar. In 1997 about 73% of mothers initiated breastfeeding and 31% of infants were breastfed 6 months or longer.[13]

The reason to expect longer maternity leaves to increase these rates is that work is a leading reason to stop breastfeeding. In our CCHS data "return to work or school" is the third most common reason to end breastfeeding after "not enough milk" and "child weaned him/her self". Similarly in the US, a survey conducted by breastfeeding product company Lansinoh Laboratories and released in 2005 reveals work related issues and return to work were major reasons for stopping after "not enough milk" and "problems latching" (non mutually exclusive categories).[14] Also in the UK (Hamlyn et al 2002), return to work or college is a leading reason to stop breastfeeding. The UK data are revealing because the reasons are reported by duration of completed breastfeeding. Return to work grows in importance starting at 6 weeks, and is the top reason after 4 months (non mutually exclusive categories).

In table 5 are estimates of the effect of longer leaves on breastfeeding from the CCHS data. The first row reveals little effect on the incidence of breastfeeding. Because the CCHS

[12] US breastfeeding rates are from Centers for Disease Control (2006) and cover the period 1998-2003.
[13] These rates are from Health Canada (1998) as cited in Chalmers and Wen (2004).
[14] Survey results accessed at http://www.corporatenews.net/cgi-bin/pc201v3.php?source=pc200v3.php&pr=11&pccl=24610 on June 6, 2006.

data are retrospective from a point in time (2003), children from more recent birth cohorts are more likely to be still breastfed. We only use birth cohorts 1998-2001, and for the analysis of duration we focus on children breastfed one year of less. From the data available we can determine whether the children in each of these birth cohorts have been breastfed this long.[15] The second row reveals a modest drop in the proportion of children who satisfy our sample selection criteria with the extension of leave. This indicates that a small part of the effect of the mandate on breastfeeding duration occurs outside our sample window, which both before and after the reform contains the vast majority of mothers.

In the next row we report the effect on completed duration, conditional on it being one year or less. It rises by almost 2.6 weeks, which is almost 13% of the pre-reform mean of just over 4.5 months. Again, replacing the *POST* dummy variable with leave entitlement by province/year permits calculation of an elasticity of breastfeeding duration with respect to the leave mandate. The estimated parameter on the leave variable is 0.110 (0.036).[16] At the sample means this implies an elasticity of 0.217.[17] This inelastic response reflects the fact that the leave mandate rose about 20 weeks on average while breastfeeding duration rose by just over 2.5 weeks.

The next 4 rows break down the effect for specific durations. The results indicate that much of the effect is extension of breastfeeding beyond six months. While we obtain significant estimates at the other durations, the appendix table shows that it is the result for greater than 6 months that has the clearest causal interpretation.

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[15] Breastfeeding duration in the CCHS is recorded in discrete categories. A continuous measure is constructed by coding each category at its midpoint duration. This restriction to breastfeeding one year or less gets around having to code the final open ended category "more than 1 year".

[16] We again estimate a version of (2) directly substituting leave entitlement for POST, and calculating the standard errors clustering on province/*POST*.

[17] The estimated increase in duration (conditional on one year of less) from the NLSCY data is 2.2 weeks. The estimated parameter on leave duration is 0.088 (0.045) which implies an elasticity at sample means of 0.162.

While the relationships between breastfeeding duration or time at home post birth and leave entitlements are of policy interest, from a more general perspective we might want to know the direct relationship between breastfeeding duration and time at home. A simple OLS estimate of this relationship is likely to be biased as mothers who stay at home longer will be different in unobserved ways from those who don't. Our policy change provides an opportunity to estimate this relationship, however, using a two sample instrumental variables (`) procedure. This involves combining information on time at home post birth from the NLSCY with the information on breastfeeding duration in the CCHS and using the variation in maternity leave entitlement as a result of the reform as an instrument. To construct the TSIV estimate we use data from the NLSCY combined with data from the first and second waves of the CCHS. The first wave of the CCHS is used to capture children from the 1998 and 1999 cohorts at a younger age than they appear in the second wave. This allows use to select children of roughly similar ages (ages 1-3 years) from each birth cohort. Not all covariates are available in both data sets so our estimates are based on a selected set which we describe in a footnote.[18] That said, the estimated first stage relationship between the child's age at the mother's return to work and leave entitlement, 0.147 (0.028), matches well the estimate for the 13-24 month sample reported above. The TSIV estimate of the relationship between breastfeeding duration and child's age at the return to work is 1.024 (0.370),[19] which implies a roughly one week increase in breastfeeding for each additional month at home. At sample means this implies an elasticity of 0.445. We are not aware of another estimate of this elasticity in the literature.[20]

[18] The common covariates are province, child's age in years, urban area, mother's age, and whether the mother is an immigrant. In the CCHS data we proxy for the mother's return to work post birth with evidence of work in the preceding year.
[19] Standard errors are calculated following Jappelli et al. (1998) clustering on province/*POST* (18 groups).
[20] In the NLSCY data we can calculate a IS estimate directly using the surveys information on breastfeeding. This estimate is 0.857 (0.273) which implies an elasticity 0.302.

In the eighth row we report the effect on exclusive breastfeeding.[21] The estimate is almost 1.5 weeks. Again we can estimate an elasticity using the estimated parameter on the variable for leave entitlement (0.065 (0.019)). At the sample means, the elasticity is 0.208. The appendix table reveals more volatility in the *YOBt* estimates before the policy reform so this inference is not as strong as for total duration.

The final two rows investigate effects on the relationship between work and the introduction of food or the cessation of breastfeeding. The estimates indicate no effect on the proportion who end exclusive breastfeeding due to work. The effect on the cessation of breastfeeding due to work is dramatic, however. Post reform, the proportion reporting work as a reason to end breastfeeding falls to close to zero.

Health

We next examine the impact of leave on broader measures of health. Longer periods of parental care in infancy may have both direct and indirect effects on children's health. A relationship between maternity leave and child mortality in the post neonatal period (e.g., Ruhm 1998) may result from either better accident prevention or better monitoring of children's health. As noted above, many health effects are attributed to a longer period of breastfeeding. Longer maternity leave may also lead to better maternal health, for example reducing post partum complications or depression.

In table 6 are estimates of effects on mothers' and infants health. The results for 7-12 month olds reveal few statistically significant results. A close call is the estimates for the child's health, which indicate an increase in wellbeing (a 5 point scale, 1=excellent, 5=poor). In addition to the imprecision of the estimate, however, the pattern of the *YOBt* effects for this

[21] The variable is constructed from a measure of the age at which any other liquids or solids were added to the child's feed. Consistency with the breastfeeding variable was required (age at which other foods introduced <= age at which breastfeeding ended).

variable (in the appendix) casts some doubt on a causal interpretation. Of course the absence of an effect is of interest in some instances. For example, the estimate for the child's current weight (including a conditioning variable for the child's birth weight) is small and statistically insignificant, which lessens concerns that longer periods of exclusive breastfeeding might compromise infant growth. As a summary of more serious illness, we report an estimate for the incidence of chronic conditions.[22] It indicates a significant reduction in chronic conditions and the individual *YOBt* estimates suggest that it's causal. The result is not robust to the addition of the wave unemployment rate, although it's not exactly clear why economic conditions would affect the incidence of the conditions captured by this measure.

The results for the 13-24 month old sample offer more for discussion. We see significant reductions in mothers' depression (a 36 point scale, higher values representing more depression), the incidence of ear infections and the incidence of asthma. In each case the estimates of *YOBt* reported in the appendix support a causal interpretation, and the estimates are similar conditioning on the wave unemployment rate. The result for asthma is intriguing because previous research has argued longer periods of breastfeeding reduce the incidence of this ailment. This result is also present and very similar for ages 25-29—the estimate -0.027 (0.006)—although in this sample alternative methods of computing the standard error render the estimate statistically insignificant. Gone, however, is any indication of improvement the child's overall health and the reduction in chronic conditions (the latter result re-emerges in the 25-29 month sample). Finally, the inference of no effect on child's weight finds further support in this age group.

For the age group 13-24 month there is another identification strategy that adds older

[22] These conditions include allergies, bronchitis, heart conditions, epilepsy, cerebral palsy, kidney condition, mental handicap, learning disability, attention deficit disorder, emotional, psychological or nervous difficulties, and a open category "other".

untreated children in each wave of data as an additional control. These untreated children would help control, for example, for a secular decline in ear infections after 2000, coincident with the policy reform. Estimates of equation (2) using children aged 25-33 months of age as a control group are reported in table 7.[23] The interaction of the *YNG* and *WAVE5* dummy variables indicate any additional effect for the treated 13-24 month olds. The impact on mother's depression identified in table 6 appears to be the result of a wave effect that also affected older children who did not experience longer leaves. The effect on ear infections is much smaller here, as there is a large negative wave effect for this ailment, and the estimate is not statistically significant when the standard errors are estimated by other methods. In contrast the effect on the incidence of asthma is very similar to the one in table 6, although again the standard error is larger by different approaches.

The analysis of health has uncovered few contemporaneous or persistent effects of longer parental care/breastfeeding on the measures of health available in the NLSCY. While the dependent variables reported were chosen on the guidance of previous research, a broader group of outcomes including infants' nose infections and bronchitis and mother's post partum depression were investigated. In each case the estimates do not indicate an effect of longer leave/breastfeeding duration on health. Additionally, the inference is that extended maternity leaves had no effect on outcomes such as period of gestation, the incidence of C-sections and birth height and weight. A "positive" result is no evidence that longer periods of exclusive and any breastfeeding compromise infant growth as measured by weight. That said, our estimates of the increase in the duration of breastfeeding are quite modest. There is also weaker evidence of a

[23] In this application we cannot use our two step estimation procedure because there is only one post treatment wave and so two post treatment observations to identify two parameters. While this problem highlights the finiteness of the identification, we can obtain standard error estimates by instead estimating (2) in one step, clustering the standard errors by wave**YNG*. We note that in many instances these estimates are smaller than their robust counterparts.

persistent effect on the incidence of asthma. It could be that there are other benefits but they result from initial rather than extended periods of breastfeeding and/or manifest at older ages. Also, we have no measure of gastrointestinal problems one of the more widely acknowledged benefits of breastfeeding.[24]

Development and Environment

Previous studies have revealed longer term effects of early maternal employment on cognitive abilities. Given the ages of children in our sample, we are not able to investigate their cognitive development with the waves of the survey currently available. The NLSCY does, however, provide a wide array of behavioural measures that allow us to investigate temperament, motor control and pre cursors to more comprehensive cognitive measures such as the ability to count or to speak simple phrases. These measures also provide information on the infants' exposure to organized children's programs and activities such as stories, singing and walks.

Table 8 contains the results. In the first three rows are measures of parenting styles and the overall family environment. The strongest result here is for positive parenting, a measure is built up from a series of questions about the interaction between parent and child. for both age groups we see an increase in positive parenting, although we lose precision once the control fro the unemployment rate is added. This result is consistent with other research using NLSCY data that shows a negative association between good parenting practices and the use of non parental care/maternal employment (Baker et al. 2005).

The next four variables measure various dimensions of the child's temperament and ability to adapt to new situations. In each case the variable is a 5 point scale with higher values indicating less desirable behaviour. While some of the point estimates, particularly for the 1 year

[24] There is a variable capturing hospital admissions for gastrointestinal reasons. An analysis of this variable reveals no effect of the policy.

olds. suggest that increased parental care leads to better temperament, few are statistically significant and the estimates of *YOBt* in the appendix show that for each variable there is fairly regular cohort to cohort variation in these measures rather than a systematic effect of the reform. A much wider array of these variables was investigated, including measures of reactions to new places and how easily and often the child got upset. Consistent with the reported results there is little evidence of a contemporaneous or persistent effect of increased parental care on these measures.

The NLSCY records the age at which developmental milestones are achieved. The two that are appropriate for this age interval are reported in the next two rows.[25] The age these children first feed themselves is estimated to drop by 8/10's of a month off a pre-reform base of 8.25 months for 7-12 month olds and one and a third months off a pre reform base of 10 month for 13-24 month olds. While the absolute effects are somewhat larger than for the 7-12 month olds, the proportionate effects are quite similar.[26] In each case the estimates of the *YOBt* support a causal interpretation. The age for first words drops similarly for both age groups at just over 6/10's of a month. The result for the older children conditional on the unemployment rate just fails to be significant at the 5 percent level (critical value is 4.3). Overall these estimates suggest parental care accelerates these two markers of motor and cognitive development.

The next three variables are measures of activities the child participates in. The reading and singing variables are measured on a 5 point scale with higher values indicating higher frequency. There is no evidence of an increase in the frequency the child is read to, but a strong, significant and persistent reduction in the exposure to singing after the reform, which suggests

[25] The other milestones recorded are sitting up, first solid food and first words.
[26] This might be expected. In results not reported the policy change also has a significant negative effect on the proportion in these age groups who have not achieved these milestones. At older ages more children achieve the milestone and are added to the calculation of the mean, but this effect will be larger for the pre reform period.

that whatever type of care the children received before the reform encouraged more singing. There is also a significant increase of almost 8 percentage points in a 0/1 indicator of participation in parent/child groups for the younger age group, as there is for other organized activities such as drop-in centres and playgroups (results not reported). Therefore, the drop in non parental care shown in table 4 does not imply a reduction in organized activity.

The NLSCY offers an index of motor/social development built up from a battery of questions about the child's capabilities. In the final row is the estimate for this index, which indicates no systematic change as a result of the reform. Of course, the index could mask offsetting changes as increased parental care might promote those skills best learned through one-on-one interaction, but impede those gained through exposure to larger social environments. However, separate estimates for the individual questions reveal that there is no systematic effect of the policy change on any of these dimensions of development.

Difference-in-difference estimates from the 13-24 month old sample are reported in table 9 for variables that are collected for both 13-24 month olds and 25-33 month olds.[27] Some of their results from table 8 gain a second life here because the standard error estimates are much smaller. As noted above, however, the standard errors for the difference-in-differences estimates are sensitive to method and larger by the alternatives. Furthermore, with the exception mood changes few of the estimates bear a resemblance to their table 8 counterparts. Finally, in almost all cases the inter cohort variation in the conditional mean is significant (see the appendix)

Conclusions

In this paper, we explore the causal impact of extended maternity leave mandates on the health and development of children and their mothers. We have both a compelling policy

[27] Estimates of the parenting measures are not possible because they are different for children whose "survey age" is 25+ months. Some mothers of children aged 25+ months are asked the younger parenting questions, but in wave 5 they are all eligible for longer maternity leave and therefore are not a legitimate control group.

episode and detailed data on health and development. While the strength of the causal inferences varies across the outcomes we study, our work presents a marked improvement on the purely correlational inferences in most existing studies.

Overall, our findings are mixed. We estimate a substantial impact on time at home, but only limited impact on breast feeding and measures of infant health. On child development, we estimate a strong impact on the age at which milestones such as feeding and speaking are reached, as well as some indicators of a positive parental environment, but do not find strong evidence for persistent impact of other measures such as motor-social development.

The findings are qualified in several important ways. First, we study only two-parent families. Single-parent families may respond differently to extended leaves. Second, our results can only be applied to a mandate extension from 6 to 12 months and may not be informative for shorter leaves. In particular, the health impact of an expansion from 6 to 12 months of leave on breast-feeding may be different from the impact of the introduction of a short leave which might induce breastfeeding initiation. Finally, we are not yet able to study the longer-run impact of the leave expansion on important indicators such as cognitive development. We anticipate studying these outcomes as data become available.

With those caveats in mind, our findings have important implications. In the context of existing research, other researchers have investigated links between leave mandates and outcomes such as infant mortality. In the data we study, we find few intermediate indicators that would link maternity leave mandates to large changes in future health outcomes. For policy makers, our estimates indicate that an extension of mandates will have a substantial impact in time at home, but a smaller impact on breastfeeding. If expanded durations for breastfeeding is a policy goal, maternity leave mandates alone may not be sufficient to attain that goal.

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Table 1: Dates of the Introduction of Mandated Parental Leave by Province

	Extension of Parental Leave Starting in 2000		
	Weeks of Leave in 2000	Date of Extension	Total Weeks of Mandated Leave Post Reform
Alberta	18	February 7, 2001.	52
British Columbia	30	December 31, 2000.	52
Manitoba	34	December 31, 2000.	54
New Brunswick	29	December 31, 2000.	54
Newfoundland	29	December 31, 2000.	52
Nova Scotia	34	December 31, 2000.	52
Ontario	35	December 31, 2000.	52
Prince Edward Island	34	December 31, 2000.	52
Quebec	70	N.A.	70
Saskatchewan	30	June 14, 2001.	52
Federal	41	December 31, 2000	54
EI	25	December 31, 2000.	50

Notes: Sources are provincial statues and Labor Canada (Various Issues).

Table 2: Age structure in the NLSCY

		Wave 3: 1998-99	Wave 4: 2000-01	Wave 5: 2002-03
1998	min	2	25	45
	max	4	43	63
1999	min	2	9	37
	max	16	31	46
2000	min		0	25
	max		19	33
2001	min			9
	max			29
2002	min			4
	max			17

Notes: Reported are the minimum and maximum ages in months for each birth cohort in the NLSCY.

Table 3: Estimated impact of longer maternity leave mandates on mothers' labour supply in the NLSCY

	7-12 Months			13-24 months		
	Pre-Reform Mean	POST	POST UR Control	Pre-Reform Mean	POST	POST UR Control
Mother has returned to work	0.55	-0.298*	-0.294*	0.63	-0.058*	-0.060*
		(0.032)	(0.039)		(0.018)	(0.017)
Mother currently employed	0.60	-0.086*	-0.089*	0.63	-0.048	-0.050
		(0.019)	(0.023)		(0.019)	(0.018)
Hours when returned to work	25.82	1.109	0.897	30.65	1.682	1.517
		(0.713)	(0.433)		(1.080)	(1.057)
Continuous work since return	0.86	0.042	0.041*	0.84	0.027	0.028
		(0.015)	(0.012)		(0.019)	(0.020)
Mother returned in 12 months				0.61	-0.090*	-0.079*
					(0.013)	(0.019)
Child's age at return to work				6.32	3.393*	2.817*
					(0.303)	(0.306)

Notes: Standard errors in parentheses. *~statistically significant at the 5% level.

Table 4: Estimated impact of longer maternity leave mandates on use of non-parental care in the NLSCY

	7-12 months			13-24 months		
	Pre-Reform Mean	POST	POST UR Control	Pre-Reform Mean	POST	POST UR Control
In care	0.41	-0.174*	-0.161*	0.47	0.011	-0.013
		(0.025)	(0.007)		(0.033)	(0.033)
Hours if in care	28.51	-6.632*	-7.289*	29.1	0.612	0.429
		(0.561)	(2.311)		(1.413)	(1.272)
Care in centre	0.03	-0.009	-0.009	0.06	0.007	0.007
		(0.011)	(0.010)		(0.017)	(0.017)
Care at own home	0.12	-0.035*	-0.038	0.15	-0.023	-0.030
		(0.009)	(0.014)		(0.017)	(0.018)
Care at other's home	0.26	-0.130*	-0.115*	0.26	0.030	0.010
		(0.013)	(0.026)		(0.033)	(0.031)
Care in other's home unlicensed	0.22	-0.118*	-0.104*			
		(0.011)	(0.027)			

Notes: Standard errors in parentheses. *~statistically significant at the 5% level.

Table 5: Estimated impact of longer maternity leave mandates on breastfeeding in the CCHS

	Pre-Reform Mean	POST	POST UR Control
Incidence	0.86	0.025	0.027
		(0.011)	(0.011)
One year or less	0.86	-0.057*	-0.061*
		(0.015)	(0.012)
Duration if one year or less	19.76	2.574*	2.473*
		(0.254)	(0.169)
Duration > one month	0.67	0.047*	0.041*
		(0.014)	(0.015)
Duration > three months	0.51	0.043*	0.040
		(0.014)	(0.015)
Duration > six months	0.27	0.067*	0.071*
		(0.010)	(0.008)
Duration > nine months	0.15	0.038*	0.037
		(0.012)	(0.011)
Duration exclusive if total duration one year or less	12.34	1.425*	1.429*
		(0.281)	(0.295)
Introduced food due to work	0.08	-0.013	-0.012
		(0.010)	(0.015)
Stopped Breastfeeding due to work	0.16	-0.151*	-0.164*
		(0.047)	(0.039)

Notes: Standard errors in parentheses. *~statistically significant at the 5% level.

Table 6: Estimated impact of longer maternity leave mandates on health

	7-12 months			13-24 months		
	Pre-Reform Mean	POST	POST UR Control	Pre-Reform Mean	POST	POST UR Control
Mother's Depression	4.24	-0.372	-0.289	4.06	-0.733*	-0.499
		(0.260)	(0.106)		(0.094)	(0.176)
Mother's health	1.88	-0.000	-0.012	1.89	-0.005	0.020
		(0.044)	(0.064)		(0.062)	(0.057)
Child's health	1.37	-0.094	-0.100	1.45	-0.033	-0.058
		(0.056)	(0.047)		(0.076)	(0.071)
Child's weight	9.38	-0.092	-0.124	11.63	0.038	-0.059
		(0.152)	(0.085)		(0.111)	(0.070)
Ear infections	0.26	-0.064	-0.060	0.50	-0.096*	-0.091*
		(0.033)	(0.038)		(0.027)	(0.028)
Chronic Condition	0.12	-0.041*	-0.026	0.13	-0.013	0.004
		(0.009)	(0.027)		(0.012)	(0.021)
Child injured in last 12 months	0.02	0.007	0.006			
		(0.003)	(0.003)			
Asthma				0.05	-0.026*	-0.019*
					(0.003)	(0.002)
Allergies				0.08	-0.003	0.011
					(0.017)	(0.021)

Notes: Standard errors in parentheses. *~statistically significant at the 5% level.

Table 7: Difference in differences estimates of impact of longer maternity leave mandates on health in NLSCY

	WAVE5	*YNG*WAVE5*	Obs.
13-24 months			
Mother's	-0.581*	-0.075	5750
Depression	(0.039)	(0.066)	
Mother's health	0.079*	-0.033	6015
	(0.027)	(0.016)	
Child's Health	0.210*	0.118	6066
	(0.003)	(0.011)	
Ear infections	-0.101*	-0.024*	6061
	(0.006)	(0.006)	
Asthma	-0.016*	-0.024*	6059
	(0.002)	(0.004)	
Allergies	0.036*	-0.024	6065
	(0.014)	(0.012)	
Chronic	0.015	-0.006	6065
condition	(0.018)	(0.018)	

Notes: Standard errors in parentheses. *~statistically significant at the 5% level. Obs=# of observations.

Table 8: Estimated impact of longer maternity leave mandates on development and environment

	7-12 months			13-24 months		
	Pre-Reform Mean	POST	POST UR Control	Pre-Reform Mean	POST	POST UR Control
Positive Parenting	18.19	0.290*	0.217	17.92	0.219*	0.187
		(0.034)	(0.182)		(0.057)	(0.069)
Hostile Parenting	1.49	0.114	0.0105	2.64	0.102	0.004
		(0.061)	(0.071)		(0.136)	(0.134)
Family Function	8.59	0.464	0.383	9.05	-0.478	-0.318
		(0.493)	(0.665)		(0.310)	(0.306)
Difficult Child	1.94	-0.033	-0.033	2.25	-0.297	-0.273
		(0.060)	(0.059)		(0.131)	(0.135)
Upset to change in routine	1.88	0.292	0.337	2.24	-0.112*	-0.104*
		(0.104)	(0.104)		(0.022)	(0.020)
Mood changes of child	2.19	0.179	0.183	2.74	-0.288	-0.175
		(0.105)	(0.113)		(0.126)	(0.124)
Attention child requires	3.33	0.019	-0.001	3.42	-0.205	-0.204
		(0.061)	(0.031)		(0.079)	(0.078)
Response to new person	2.81	0.014	0.020	3.32	-0.231	-0.100
		(0.071)	(0.062)		(0.112)	(0.113)
Age: feed self	8.28	-0.827*	-0.857*	10.02	-1.366*	-1.371*
		(0.131)	0.130)		(0.201)	(0.201)
Age: first words	8.32	-0.630*	-0.757*	10.17	-0.715*	-0.680
		(0.162)	(0.158)		(0.160)	(0.160)
Frequency child is read to	4.32	0.103	0.086	4.68	-0.037	-0.021
		(0.091)	(0.112)		(0.038)	(0.030)
Frequency child is sung to	4.75	-0.604*	-0.681*	4.71	-0.365*	-0.372*
		(0.045)	(0.044)		(0.048)	(0.047)
Parent/Child group	0.05	0.080*	0.074*	0.09	0.035	0.032
		(0.014)	(0.026)		(0.016)	(0.017)
Standardized Motor/Social Score	99.44	-1.112	-1.246	102.43	0.708	0.316
		(1.240)	(0.955)		(1.038)	(1.159)

Notes Standard errors in parentheses. *~statistically significant at the 5% level.

Table 9: Difference in difference estimates of the impact of longer maternity leave mandates on development and environment in NLSCY

	WAVE5	*YNG*WAVE5*	Obs.
13-24 months			
Family	0.362*	-0.521*	5858
Function	(0.101)	(0.059)	
Difficult Child	-0.325*	-0.037	6017
	(0.019)	(0.012)	
Upset to change	-0.085*	-0.025	4372
in routine	(0.011)	(0.015)	
Mood changes	0.007	-0.252*	6014
of child	(0.032)	(0.027)	
Attention child	-0.086*	-0.046	6017
requires	(0.033)	(0.021)	
Response to	0.258*	-0.255*	6016
new person	(0.037)	(0.019)	
Frequency child	0.152*	-0.111*	5278
is read to	(0.025)	(0.016)	
Parent/Child	0.138*	-0.042*	6060
group	(0.010)	(0.009)	
Standardized	-0.001	1.498*	5890
Motor/Social	(0.489)	(0.319)	
Score			

Notes Standard errors in parentheses. *~statistically significant at the 5% level. Obs=# of observations.

Figure 1: Estimates of child's age (months) the mother returned to work by birth cohort, relative to 1998 births, age13-24 month old sample.

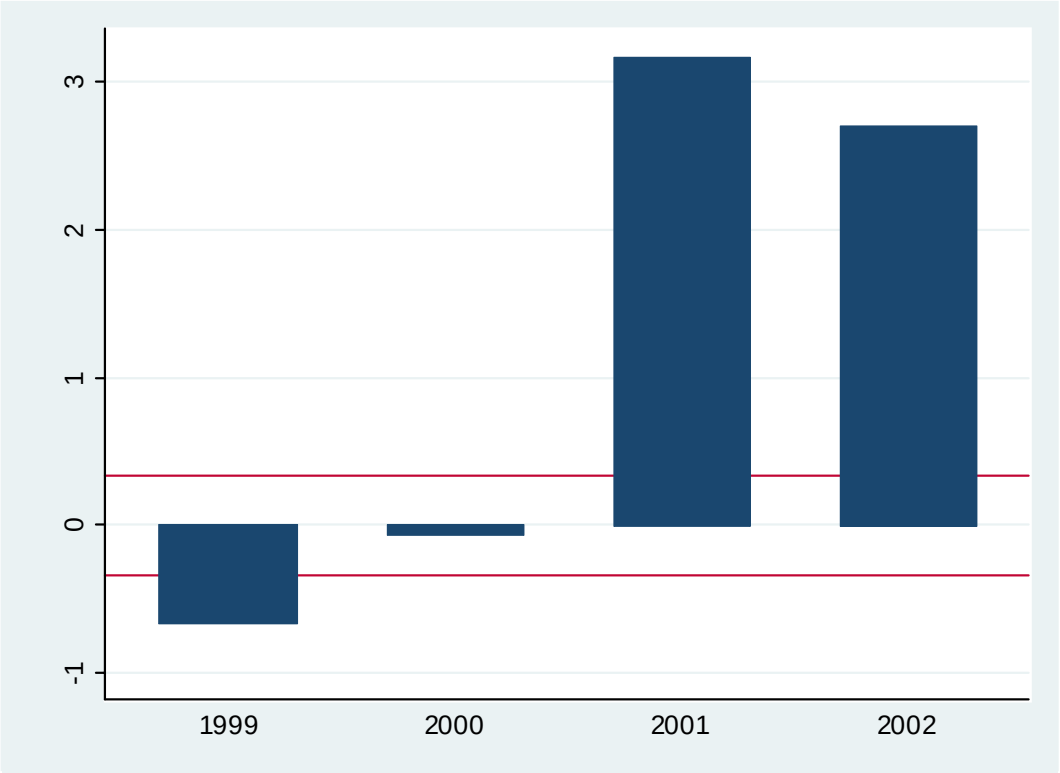
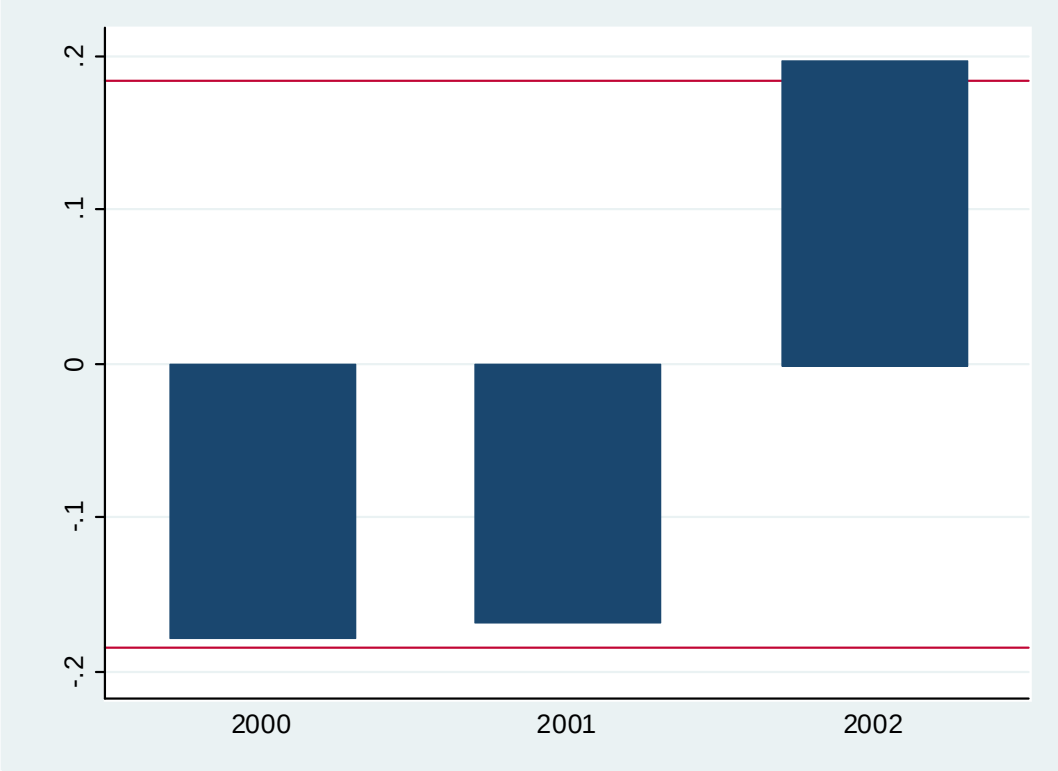


Figure 2: Estimates of an Index of Child's Reaction to a Change in Routine relative to 1999 births, age 7-12 month sample.



Appendix

Table A1: Estimated impact of longer maternity leave mandates on mothers' labour supply by year of birth

	1999	2000	2001	2002	Obs
7-12 months					
Mother has returned to work	-0.123	-0.053	-0.401	-0.318	1799
	(0.067)	(0.038)	(0.057)	(0.035)	
Mother currently employed	-0.009	0.028	-0.125	-0.065	1803
	(0.063)	(0.036)	(0.068)	(0.038)	
Hours when returned to work	1.037	-1.434	1.799	0.343	860
	(2.276)	(1.321)	(2.992)	(1.753)	
Continuous work since return	0.001	-0.036	0.036	0.025	860
	(0.042)	(0.052)	(0.052)	(0.037)	
13-24 months					
Mother has returned to work	-0.057	-0.032	-0.092	-0.107	2948
	(0.044)	(0.041)	(0.043)	(0.045)	
Mother currently employed	-0.050	-0.005	-0.073	-0.080	2969
	(0.043)	(0.040)	(0.042)	(0.044)	
Mother returned in 12 months	-0.040	-0.018	-0.119	-0.105	2948
	(0.045)	(0.041)	(0.044)	(0.045)	
Child's age at return to work	-0.657	-0.054	3.174	2.700	1892
	(0.362)	(0.301)	(0.359)	(0.360)	
Hours when returned to work	-1.681	0.772	0.528	2.338	1901
	(1.326)	(1.186)	(1.267)	(1.419)	
Continuous work since return	-0.042	-0.025	-0.012	0.029	1901
	(0.044)	(0.033)	(0.037)	(0.030)	

Table A2: Estimated impact of longer maternity leave mandates on non parental care by year of birth

	1999	2000	2001	2002	Obs.
7-12 months					
In care	0.004	0.059	-0.151	-0.148	1808
	(0.068)	(0.039)	(0.063)	(0.036)	
Hours if in Care	-0.171	1.076	-5.004	-6.390	860
	(3.083)	(2.225)	(3.450)	(2.247)	
Care in centre	0.012	0.025	0.004	0.002	1808
	(0.022)	(0.013)	(0.019)	(0.010)	
Care at own home	-0.022	0.013	-0.005	0.035	1808
	(0.045)	(0.026)	(0.047)	(0.023)	
Care at other's home	0.016	0.023	-0.151	-0.113	1808
	(0.061)	(0.035)	(0.050)	(0.031)	
Care in other's home unlicensed	0.033	0.015	-0.139	-0.104	1808
	(0.061)	(0.034)	(0.046)	(0.029)	
13-24 months					
In care	-0.037	0.009	-0.027	0.051	2960
	(0.044)	(0.042)	(0.044)	(0.044)	
Hours if in Care	-1.535	-0.446	-0.751	-2.430	1512
	(1.837)	(1.763)	(1.727)	(2.002)	
Care in centre	-0.010	0.036	0.004	0.030	2960
	(0.022)	(0.022)	(0.024)	(0.023)	
Care at own home	0.020	-0.028	-0.014	-0.036	2960
	(0.033)	(0.031)	(0.034)	(0.029)	
Care at other's home	-0.048	-0.005	-0.017	-0.058	2960
	(0.037)	(0.037)	(0.037)	(0.045)	

Table A3: Estimated impact of longer maternity leave mandates on breastfeeding by year of birth

	1999	2000	2001	2002	2003	Obs.
Incidence	-0.005	-0.080	0.003	0.020	0.015	5826
	(0.024)	(0.037)	(0.021)	(0.021)	(0.021)	
One year or less	0.017	-0.013	-0.034	-0.063	-0.068	5832
	(0.029)	(0.026)	(0.026)	(0.030)	(0.028)	
Duration if one year or less	0.082	0.138	3.153	2.477	2.388	4955
	(1.312)	(1.233)	(1.168)	(1.327)	(1.255)	
Duration > one month	-0.021	-0.051	0.026	0.011	0.011	4955
	(0.036)	(0.033)	(0.030)	(0.034)	(0.033)	
Duration > three months	0.032	0.003	0.065	0.038	0.063	4955
	(0.042)	(0.038)	(0.036)	(0.041)	(0.039)	
Duration > six months	0.020	0.038	0.099	0.095	0.083	4955
	(0.035)	(0.032)	(0.032)	(0.037)	(0.035)	
Duration > nine months	-0.002	0.025	0.057	0.046	0.040	4955
	(0.028)	(0.027)	(0.027)	(0.030)	(0.029)	
Duration exclusive if total duration one year or less	1.114	0.712	2.495	2.025	2.047	4955
	(0.851)	(0.807)	(0.781)	(0.873)	(0.855)	
Introduced food due to work	0.031	0.025	-0.002	0.009	0.023	4064
	(0.024)	(0.023)	(0.020)	(0.023)	(0.022)	
Stopped Breastfeeding due to work	0.054	-0.057	-0.105	-0.176	-0.178	4075
	(0.033)	(0.033)	(0.030)	(0.029)	(0.028)	

Table A4: Estimated impact of longer maternity leave mandates on health by year of birth

	1999	2000	2001	2002	Obs.
7-12 months					
Mother's	0.293	0.578	0.226	-0.157	1725
Depression	(0.696)	(0.357)	(0.357)	(0.323)	
Mother's	0.386	0.043	0.014	0.053	1805
health	(0.124)	(0.063)	(0.112)	(0.065)	
Child's Health	-0.048	-0.102	-0.273	-0.118	1805
	(0.0)	(0.0)	(0.0)	(0.0)	
Child's	0.085	-0.354	-0.168	-0.252	1725
weight	(0.161)	(0.094)	(0.207)	(0.105)	
Child injured	0.018	-0.004	0.010	0.006	1805
in last 12	(0.023)	(0.010)	(0.017)	(0.011)	
months					
Ear infections	-0.093	-0.038	-0.186	-0.069	1804
	(0.061)	(0.032)	(0.051)	(0.033)	
Chronic	0.009	0.016	-0.054	-0.029	1805
condition	(0.051)	(0.027)	(0.031)	(0.025)	
13-24 months					
Mother's	-0.072	-0.040	-0.849	-0.598	2796
Depression	(0.402)	(0.346)	(0.390)	(0.320)	
Mother's	0.157	0.172	0.164	0.045	1941
health	(0.079)	(0.072)	(0.076)	(0.078)	
Child's Health	-0.151	-0.034	-0.161	-0.012	2968
	(0.0)	(0.0)	(0.0)	(0.0)	
Child's	-0.320	-0.469	-0.273	0.726	2735
weight	(0.143)	(0.125)	(0.140)	(0.081)	
Ear infections	-0.026	-0.034	-0.099	-0.170	2967
	(0.046)	(0.041)	(0.044)	(0.040)	
Asthma	0.010	0.005	-0.020	-0.018	2965
	(0.019)	(0.017)	(0.018)	(0.018)	
Allergies	-0.008	-0.012	0.003	-0.045	2968
	(0.022)	(0.022)	(0.022)	(0.017)	
Chronic	-0.039	-0.034	-0.039	-0.051	2968
condition	(0.026)	(0.027)	(0.027)	(0.025)	

Table A5: Estimated impact of longer maternity leave mandates on behaviour and development by year of birth

	1999	2000	2001	2002	Obs
7-12 months					
Positive Parenting	-0.236 (0.289)	0.046 (0.149)	0.286 (0.219)	0.293 (0.148)	1774
Hostile Parenting	-0.019 (0.200)	0.049 (0.119)	0.342 (0.235)	0.096 (0.110)	1796
Family Function	-0.547 (0.732)	1.110 (0.369)	1.354 (0.533)	0.832 (0.421)	1755
Difficult Child	-0.040 (0.153)	-0.138 (0.100)	-0.148 (0.169)	-0.086 (0.098)	1795
Upset to change in routine	NA	-0.176 (0.173)	-0.167 (0.207)	0.197 (0.172)	1196
Mood changes of child	-0.072 (0.189)	-0.196 (0.112)	-0.143 (0.172)	0.130 (0.123)	1793
Attention child requires	-0.394 (0.196)	-0.113 (0.132)	-0.065 (0.198)	-0.058 (0.130)	1792
Response to new person	0.209 (0.221))	0.121 (0.142)	-0.081 (0.227)	0.118 (0.138)	1795
Age: feed self	NA	0.461 (0.251)	-0.731 (0.284)	-0.426 (0.241)	718
Age: first words	NA	0.265 (0.667)	-0.889 (0.846)	-0.317 (0.682)	176
Frequency child is read to	-0.046 (0.149)	0.093 (0.089)	-0.152 (0.140)	0.192 (0.074)	1800
Frequency child is sung to	NA	0.088 (0.139)	-0.658 (0.212)	-0.506 (0.143)	1202
Parent/Child group	0.031 (0.032)	0.031 (0.019)	0.083 (0.040)	0.098 (0.022)	1804
Standardized Motor/Social Score	-2.511 (1.781)	-2.773 (1.131)	-1.118 (2.123)	-2.750 (1.119)	1697
13-24 months					
Positive Parenting	0.120 (0.177)	0.152 (0.168)	0.354 (0.172)	0.234 (0.152)	2954
Hostile Parenting	-0.122 (0.134)	-0.222 (0.132)	-0.114 (0.128)	0.233 (0.133)	2954
Family Function	0.054 (0.423)	1.029 (0.401)	-0.199 (0.426)	-0.128 (0.445)	2861
Difficult Child	-0.158 (0.109)	-0.073 (0.109)	-0.489 (0.103)	-0.156 (0.096)	2950

Upset to change in routine	NA	-0.020 (0.156)	-0.129 (0.092)	-0.098 (0.150)	2077
Mood changes of child	0.071 (0.140)	0.271 (0.139)	-0.261 (0.127)	0.030 (0.128)	2948
Attention child requires	0.090 (0.160)	0.033 (0.147)	-0.205 (0.150)	-0.005 (0.145)	2949
Response to new person	0.070 (0.160)	0.323 (0.158)	-0.167 (0.156)	0.037 (0.136)	2949
Age: feed self	NA	-0.449 (0.263)	-1.523 (0.197)	-1.503 (0.249)	2020
Age: first words	NA	-0.347 (0.285)	-0.808 (0.222)	-0.898 (0.297)	1985
Frequency child is read to	0.119 (0.069)	0.065 (0.066)	0.032 (0.072)	0.065 (0.068)	2959
Frequency child is sung to	NA	0.033 (0.076)	-0.329 (0.068)	-0.419 (0.110)	2088
Parent/Child group	0.029 (0.026)	0.050 (0.029)	0.054 (0.029)	0.087 (0.033)	2968
Standardized Motor/Social Score	2.106 (1.377)	0.656 (1.209)	2.533 (1.432)	0.440 (1.298)	2848