Maternal employment, breastfeeding, and health: Evidence from maternity leave mandates

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COMMENTS WELCOME

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Abstract

Public health agencies around the world have renewed efforts to increase the incidence and duration of breastfeeding. Maternity leave mandates present an economic policy that could help achieve these goals. We study their efficacy focusing on a significant increase in maternity leave mandates in Canada. We find very large increases in mothers' time away from work post-birth and in the attainment of critical breastfeeding duration thresholds. However, we find little impact on the self-reported indicators of maternal and child health captured in our data.

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1.0 Introduction

Public health agencies around the world have renewed efforts to increase the incidence and duration of breastfeeding. For example, in recent years the World Health Organization, the US Department of Health and Human Services, and Health Canada have extended exclusive breastfeeding advisories to six months, and recommendations for continued feeding up to 2 years.[1] These revised targets are supported by public health campaigns such as the U.S. Healthy People 2010 initiative, which promote population benchmark breastfeeding rates that well exceed current behavior.[2] In short, the push behind these initiatives is determined and the goals are ambitious.

Public policy must recognize that the obstacles to achieving these aggressive public health goals are varied.[3] In the first weeks after birth there is clearly a rationale for educational interventions. Mothers who do not breastfeed by choice may simply not know the benefits of breastfeeding. Often, those who stop early report difficulty with technique, or worry that their child is not getting enough food. Counseling, support and training might best address these problems. The separate challenge of prolonging breastfeeding duration past the initial weeks brings up a different set of issues. Mothers report that the need to return to work is the leading reason to stop breastfeeding at longer durations. Labor market policies, including job-protected

[1] The World Health Organization recommends six months of exclusive feeding (World Health Organization 2001). The US Department of Health and Human Services recommends six months of exclusive feeding, with continued feeding to one year (Department of Health and Human Services 2000). Health Canada recommends six months of exclusive feeding with continued feeding up to age 2 and beyond (Health Canada 2004).

[2] In the US, the 2010 “Healthy People Goal” is to reach a level of 75 percent feeding in the early postpartum period, 50% at six months, and 25% at one year (Centers for Disease Control 2006). According to the Centers for Disease Control as of 2005, 72% of new mothers breastfeed but fewer than 40% are still breastfeeding at 6 months. In Canada, an initiative to set out public health goals for Canada was set up in 2005, but has not yet followed through with recommendations. In a controversial example of the force of the public health campaign, an advertisement in the United States suggested that not breastfeeding was as bad for one’s child as riding a mechanical bull while pregnant. See Rabin (2006) for details.

[3] See the discussion of the role of policy in attaining breastfeeding goals in Galtry (2003).

leaves from employment after birth and labor standards that facilitate breastfeeding or the expression of breast milk at work, might be the more appropriate response to these challenges.

Underlying these public health campaigns are the benefits attributed to breastfeeding. These benefits are widely believed to be extensive—ranging from higher intelligence for children to reduced risks of breast cancer for mothers—but the supporting research is in some ways problematic. The problem stems from the identification strategy used in most studies. Experimental evidence of the benefits of breastfeeding is rare due to ethical concerns. Therefore, most studies rely on observed variation in breastfeeding behavior across mothers.[4] However, there is good reason to believe that this observed variation is correlated with important unobserved determinants of child outcomes, leading to biased estimates of the impact of breastfeeding on health and development. There are important examples where the estimated benefits of breastfeeding vary with the number of confounding factors accounted for in the analysis (e.g., Der, Batty, and Deary 2006).

Therefore, despite the rising profile of public health goals for breastfeeding, the answers to two fundamental questions remain uncertain. First, what public policies will best attain these goals? Second, what would be the impact of achieving these goals on the health and welfare of children and their mothers? We provide new answers to these questions, focusing on labor market policies that provide mothers job-protected, paid leave post-birth. This is a significant departure from the existing, primarily public health, literature that focuses on educational interventions.

Our contribution is rooted in an increase in maternity/parental (henceforth “maternity”) leave entitlements in Canada. Mothers giving birth before December 31, 2000 were entitled to a

[4] Two recent exceptions to this criticism are Kramer et al. (2001) and Der et al. (2006). We discuss these studies later in the paper.

maximum of approximately six months of job-protected, compensated maternity leave. For children born after that date, both benefit entitlement and job protection were extended to about one year in most provinces. Previous evidence (Baker and Milligan 2005; Hanratty and Trzcinski 2006) indicates that this reform led to a large increase in the amount of time before mothers returned to work post-birth. We pursue the impact of this reform on time spent away from work through to breastfeeding decisions, and finally child and mother health.

Our approach delivers three clear contributions to the literature. First, our policy-based identification strategy makes use of the plausible orthogonality of the increase in maternity leave entitlements to unobserved maternal and child characteristics. This provides variation in the period of time before mothers return to work post-birth, and correspondingly in breastfeeding incidence and duration, which is less likely to be correlated with these unobservables. This is a clear advance in a literature that primarily offers observation-based inference. Also, the labor market policy we evaluate speaks directly to the high proportion of mothers who cite work as an obstacle to breastfeeding. Finally, the specific leave extension we examine affects an age range (six to twelve months) that previous research indicates to be critically important, and also spans important public health goals for breastfeeding.

Our results confirm the previous evidence that extended maternity leave mandates increase the period of time before mothers return to work post-birth. Within the child's first year of life we estimate an increase of 3-3.5 months among those eligible for the leave.[5]

Our primary contribution is evidence of how this increase in time at home affected breastfeeding. We find significant increases in the duration of breastfeeding in the first year—over one month for eligible mothers. For exclusive breastfeeding the increase in duration is

[5] For this and other results reported in this paragraph, we have scaled our estimated coefficients by a factor to correct for the proportion of women in our sample who were (unobservably to us) not eligible for the leave. We explain later in the paper how we derive these 'treatment on the treated' estimates from our reported regression coefficients.

roughly one-half of a month. Importantly, we find that the proportion of women attaining six months of exclusive breastfeeding increased by between 7.7 and 9.1 percentage points, over 39 percent of the pre-reform mean. Connecting the labor supply and breastfeeding results, we provide an instrumental variables estimate of the relationship of time away from work post-birth in the first year of life and the corresponding period breastfeeding. Breastfeeding increases a third of a month with every additional month not at work, which implies an elasticity of 0.458. We are not aware of other estimates of this elasticity in the literature.

Finally, we trace this increase in breastfeeding (and contemporaneous decrease in non-parental care) through to a set of parent-reported health outcomes that previous research has linked to breastfeeding. For no outcome do we find robust evidence that the increase in breastfeeding had a beneficial effect. While this conclusion may originate in some unobserved deficiency in the self-reported measures of health, it does not differ greatly from the more modest benefits of breastfeeding found in studies that exploit a randomized design.

2.0 Previous research on breastfeeding

In the large literature on the determinants and characteristics of breastfeeding durations, the relationship of breastfeeding to work figures prominently. We briefly review this evidence below. Following that, we discuss the voluminous evidence on the consequences of breastfeeding for infant, child, and maternal health.

2.1 Breastfeeding and labor supply

Data on breastfeeding behavior consistently identify return to work as an important reason both for stopping breastfeeding or never starting (e.g., Bick et al. 1998, Lansinoh Laboratories 2005). This reason grows in importance starting at about 6 weeks and emerges as the top reason for stopping at longer durations (Hamlyn et al 2002, Schwartz et al. 2002). The pivotal role of the labor market brings forward the importance of economics in the decision to breastfeed.

There are few theoretical models of the decision to breastfeed in the economic literature. Roe et al. (1999) and Chatterji and Frick (2005) point out that opportunity cost of time spent breastfeeding will rise significantly with the return to work, predicting a clustering of breastfeeding termination just prior. Furthermore, if the expected duration of breastfeeding is short because of planned cessation on return to work, the fixed costs in equipment and learning may not exceed the short flow of benefits, leading to decreased breastfeeding initiation. Implicit here is an assumption that the decision to breastfeed is secondary to the choice of how long to stay away from work, which is also a common assumption in most empirical studies of this topic.

Empirically, the relationship between breastfeeding durations and the return to work is studied in (among many others) Kurinij et al. (1989), Geilen et al. (1991), Lindberg (1996), Visness and Kennedy (1997), Fein and Roe (1998), and surveyed in Dennis (2002). All of these papers provide evidence that maternal employment post-birth is associated with shorter breastfeeding duration, although not with decreased initiation of breastfeeding. Roe et al. (1999) use an instrumental variables strategy to sort out the direction of causality between employment and breastfeeding, reporting that causality flows from return to work to breastfeeding, and not the other way.[6] Chatterji and Frick (2005) use a family fixed effects framework to control for unobserved family background factors, and find that women returning to work earlier have lower breastfeeding durations as well as lower rates of initiation.

Finally, Human Resources and Skills Development Canada (2005) evaluates the same change in maternity leave that we analyze. They use Employment Insurance administrative data

[6] See a critique of this IV strategy in Chatterji and Frick (2005).

on maternity leave beneficiaries combined with follow-up surveys, comparing children born in the year immediately preceding the reform to those born in the year following. Of some concern is high levels of sample attrition in one follow-up survey (>40%) and a low response rate in another (<45%). With these shortcomings in mind, the cross-tabulations in this study report a 10 week increase in the time away from work post-birth and a 3.5 week increase in breastfeeding duration.

2.2 Breastfeeding and health

In 1997, the American Academy of Pediatrics summarized the benefits of breastfeeding, citing 111 research articles, in support of a new set of breastfeeding guidelines (American Academy of Pediatrics 1997). The reported benefits for children include decreases in diarrhea, otitis media (ear infections), gastro-intestinal diseases, asthma, lower respiratory infections, sudden infant death syndrome, lymphoma, and chronic digestive diseases. For mothers the benefits include an earlier return to prepregnant weight, improved bone remineralization, and a reduced risk of ovarian and premenopausal breast cancer.[7]

This evidence, while impressive in volume, may leave a cautious researcher less than fully convinced. The inference in the great majority of the research comes from observation: comparisons of mothers who breastfeed with those who do not; or who breastfeed for shorter or longer periods. Observable maternal characteristics are controlled for in the hope that they span all confounding factors. Kramer et al (2001, p. 414) claim that "*<u>All</u>* of the scientific evidence regarding breastfeeding and morbidity in healthy full-term infants is based on observational studies." (Emphasis added.) Therefore, it is possible that unobservable characteristics drive both the health outcomes and the decision of when and how long to breastfeed.

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[7] In the extreme, Ruhm (2000) and Tanaka (2005) speculate that breastfeeding may be the channel through which maternity leave mandates and infant mortality are linked in their cross-country time series studies.

This concern over causality is not pedantic. Two recent studies that more carefully address the question of causality find that some results from the literature survive a more rigorous test while others do not. Kramer et al. (2001) use an intention-to-treat research design, randomly assigning post-birth lactation support across maternity hospitals and clinics in Belarus. This treatment resulted in very substantial increases in breastfeeding durations, significant reductions in the risks of gastrointestinal tract infections and atopic eczema, but importantly not in the incidence of a variety of respiratory ailments. Der, Batty, and Deary (2006) use matched sibling pairs from the NLSY to revisit the relationship between breastfeeding and cognitive ability. They find that family background confounding variables can explain the previously-documented correlation between breastfeeding and intelligence in children. A further meta-analysis of the literature suggests that previous studies showing stronger relationships between breastfeeding and intelligence control for fewer family background variables.

Another important focus in the literature is the role of threshold effects—the duration of breastfeeding has effects separate to those of incidence. Chantry et al. (2006) find large increases in the odds of contracting pneumonia or otitis media among those breastfed exclusively for only 4 to 5 months rather than six or more. Kramer et al. (2003) find significantly higher rates of gastrointestinal illness for those breastfed exclusively 3 months compared to those breastfed exclusively for at least 6 months (although they find no difference in respiratory diseases or rashes).[8]

3.0 Policy Environment

Income replacement for maternity leaves in Canada is governed by the Employment Insurance (EI) program. Most other terms of the leave, including job protection, are determined

[8] See also Kramer and Kakuma (2002).

by provincial labor market standards. We describe in this section the parameters of maternity leaves, as well as other family policies that changed over the period of interest.

3.1 Maternity leave benefits

Maternity leave benefits have been part of the EI program since 1971. Eligibility and benefit entitlement are determined by the same formulas used to calculate unemployment benefits. Qualification depends on having 600 hours of paid employment over the twelve months prior to the date of the claim. Benefits are based on earnings over the 6 months prior to the cessation of work, with a replacement rate of 55% up to a cap of $39,000. This delivers a maximum weekly benefit of $412.50 per week. However, a two week waiting period with zero benefits lowers the effective replacement rate over the full span of the leave.

The policy change we study affected children born on December 31, 2000 or later. Leave entitlement pre-reform was 15 weeks of paid benefits for mothers, plus a further 10 weeks of benefits that could be split between the mother and the father, yielding a total of 25 weeks of benefits. For children born on December 31, 2000 or later, another 25 weeks was added, making 35 weeks that could be split between the mother and the father—and yielding a total potential entitlement of 50 weeks to new mothers. The reform also included a reduction of the required hours of employment for eligibility from 700 to the current 600.

Because eligibility depends on attachment to the labor force before the birth of a child, not all mothers are eligible. Data from the Survey of Employment Insurance Coverage (Statistics Canada 2006) show that the proportion of mothers with children aged less than one year who had insured employment in the 12 months preceding childbirth was 70 percent in 2000, and has fluctuated between 74 and 75 percent from 2001 through 2005.[9] The proportion of mothers with insurable employment who are eligible for and claim benefits rises from 80 percent in 2000 and 2001 to roughly 85 percent in 2002-2005.

3.2 Job protection for maternity leave

Uncompensated, job-protected maternity leave is provided through provincial labor standards legislation.[10] The standards set thresholds for pre-birth employment to determine eligibility, ban dismissal due to pregnancy, mandate a minimum duration for leave, ensure a return to the same or a similar job upon return, and specify which terms of employment are preserved during the leave.

Contemporaneous with the changes in EI maternity leave benefit entitlements, many provinces increased the mandated duration of job-protected leave to around 52 weeks. We summarize these changes in Table 1. Three differences across jurisdictions are clear. First, Quebec did not change its standards, having extended leave to 70 weeks in March 1997. Second, among the other provinces there is some heterogeneity in starting points, which range from 18 weeks in Alberta to 35 weeks in Ontario. Third, almost all provinces moved on the same date—December 31, 2000—with only Alberta and Saskatchewan having slightly delayed their extensions. We account for each of these aspects of the reform in our empirical strategy.

3.3 Other policy changes

[9] This suggests that the reform led to an increase in the pre-birth employment of mothers, but the match is not exact because each survey runs from April though January of the following year and the reference period is for employment in the 12 months preceding the birth of a child who can be up to 12 months old. Unfortunately there are no data on maternity benefits preceding the 2000 survey.

[10] Workers in some industries are covered by federal labor standards legislation rather than provincial. These industries include federal civil servants, banking, transportation, and communications. Employment in federally regulated industries amounts to around five percent of the labor force.

Because our inferences will lean heavily on differences across time, it is important to account for other changes to the social and economic environments facing families with young children over the period. There are two key developments. The first is a universal, heavily subsidized, childcare program in Quebec. Introduced in 1997, it was extended to children aged 0 to 1 in September 2000. Baker, Gruber, and Milligan (2005) report that the program induced large changes in the use of non-parental care in Quebec, and was associated with substantial changes in measures of family wellbeing. The second change was continued increases to the federal National Child Benefit, with the one-child rate moving from $605 in 1998 to $1,293 by 2002. Milligan and Stabile (2007) report that this benefit had a substantial impact on the employment of single mothers. Because the benefit is targeted to low income families, it had much less impact on the incomes of two-parent families.[11]

4.0 Predicted Effects

In Klerman and Leibowitz's (1997) model of maternity leave mandates the effect of an increase in a leave mandate is ambiguous. Some women will stay away longer from work post-birth while others will return earlier.[12] If mothers do spend more time away from work post-birth, then a simple model of breastfeeding featuring fixed 'startup' costs and a marginal cost of breastfeeding that increases upon the return to work would predict more breastfeeding. There would be higher incidence as more women find the flow of benefits exceed the fixed costs of starting, and longer duration due to the increase in the period of time before women face the higher marginal cost of breastfeeding while working.

[11] In 1998, the National Child Benefit was reduced to zero for family incomes above $25,921. By 2002, this threshold had reached $32,960.

[12] Some of those mothers who took the full mandated leave before the leave extension will now stay away longer. Those who pre-reform quit their jobs permanently to take a leave in excess of the mandate, may now take the new longer mandated leave and thus return earlier.

More time away from work may affect health through increased breastfeeding, but also through other channels. For example, it may crowd out non-parental childcare. If this substitution affects health, then the channel leading from the mandates to health outcomes may not be clear. We attempt to separate the effects of breastfeeding and parental care through our choice of outcomes and the age groups we use for analysis, but the intrinsic duality of the treatment remains a qualification for this part of the analysis.

Finally, family incomes may differ before and after the reform. If so, the change in incomes may present another mechanism through which health outcomes change. In simulations, however, we found that the effective replacement rate changed very little with the reform—although lower earning women with substantial childcare costs did show higher incomes, after tax and childcare costs.[13]

5.0 Data

We use the National Longitudinal Study of Children and Youth (NLSCY) to analyze the effect of the increase in maternity leave mandates on the time mothers stay away from work post-birth and mothers' and children's health. The NLSCY is a national survey of children, excluding only residents of the scarcely populated northern territories, institutions, Indian reserves, and members of the Canadian Forces. It is conducted biannually, and 6 waves of data are currently available: 1994-95, 1996-97,…, 2004-05. The survey content is deep on questions

[13] We calculated replacement rates taking into account taxes and childcare costs, comparing income if on leave and income if at work. For women earning under the median, childcare costs and taxes take a substantial share of earnings, meaning that a paid leave would increase family income if the alternative were work and paid childcare. On the other hand, for women earning at the median, we find replacement rates around 1.0, meaning that the maternity benefit is approximately the same as the after-tax, after childcare wage. At the 90th percentile of earnings, the replacement rate drops to around 60 percent. Importantly, the reform changes the replacement rates very little – the only impact is that the two week initial waiting period for benefits becomes relatively less important when the leave is 50 weeks long, which gives a small (on the order of 0.05) boost to the replacement rates in 2001.

of parental labor supply, childcare, parenting, health and school performance, although at the ages we examine only some of these variables are available.

We start with the sample of all children born in a six year window around the policy reform; those with a year of birth between 1998 and 2003. The NLSCY does not allow us to observe maternal hours worked in the year before the birth, so we cannot condition our sample on eligibility for job protected maternity leave and/or EI benefits. Instead, we simply keep all children—including those born to the roughly 25 percent of mothers who were not eligible.

There are three notable omissions from our sample. First, we exclude children from single-parent families; about 10 percent of births over this time period. We are concerned that other policy changes such as the enhancements of the National Child Benefit may influence their behavior. In addition, given the smaller financial resources of single parents, we might expect them to respond to changes in leave mandates differently than their partnered counterparts, but we lack the sample sizes to conduct a separate analysis of this group. This said, we present estimates of our key results from a sample including single parents in the appendix Table A5.

Second, we omit children from Quebec. Not only did Quebec's job protected leave mandate stay constant at 70 weeks through our sample period, but also the expansion of subsidized childcare further confuses the economic environment. These factors render Quebec families unsuitable as either a treatment or a control group for our analysis.[14]

The final excision removes observations when the survey respondent is not the mother, since fathers may have systematically worse knowledge about the breastfeeding habits of mother and child. This decision matches the survey respondents in our breastfeeding data from the Canadian Community Health Survey.

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[14] We 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 changes a few months later.

For the analysis of the time away from work post-birth our focus is the first year of life. We therefore sample children starting at age 13 months, meaning that complete information on the first year is available. We stop our sample at age 29 months, which allows us to maintain a common age span for each birth cohort while maximizing sample size. Our key variable is the number of months in the first year of the child's life that the mother is not at work. This is well-defined for all mothers, including those who have not yet returned to work by the end of the first year. We also examine the proportion of mothers who have not returned to work by 1, 3, 6, 9 and 12 months.

For the analysis of health outcomes we draw two samples, children aged 7-12 and 13-24 months respectively. This allows us to investigate any health impacts contemporaneous with the increase in maternity leave from 6 to 12 months, as well as any persistent affects in the older age group. Available measures of health include birth outcomes (gestation, birth height and length, complications), the mother-reported current health status of the child (and mother), the incidence of specific ailments (nose and ear infections, asthma, allergies, chronic conditions), and injuries. While there is some concern that mother reports may suffer from systematic biases, there is some evidence that parent-reports are more accurate for acute events.[15] For mothers, we observe depression, post-partum depression, and related post-partum problems.

Our analysis of breastfeeding uses the Canadian Community Health Survey (CCHS), a survey of individuals aged 12 years or older. Women who have given birth within five years of the survey date are asked a series of questions about breastfeeding. These questions provide detail on the incidence and duration of breastfeeding, the timing of the introduction of foods, and

[15] Spencer and Coe (1996) find that parent-reported measures of acute events rated 'very good' while parent reports for functional health and life quality status was 'moderate'. The exceptional nature of acute events and the associated contact with medical professionals may underlie these differences. More directly related to the measures we use, Vernacchio et al. (2007) find that parent reporting for otitis media "correlates well with medical records."

the reasons breastfeeding was not initiated and why it ended. We use data from the second (2003) and third (2005) cycles of the survey.[16] We sample the same birth cohorts as in the NLSCY, drawing data on the 1998-2001 cohorts from the second cycle and the 2000-2003 cohorts from the third cycle to ensure breastfeeding activity in the first year of life is fully observable. We again focus on children in two-parent families and exclude observations from Quebec.[17]

Our CCHS variables are chosen to conform to conventions in the breastfeeding literature and to complement the NLSCY variables. They include the proportions of mothers still breastfeeding and still exclusively breastfeeding 1, 3, 4, 6, 9 and 12 months post-birth, the months in the first year of life the child was breastfed, as well as 0/1 indicators for the different reasons breastfeeding ended. Again the duration measures are well-defined for all mothers, including those who breastfeed more than one year or who are currently breastfeeding.

6.0 Empirical Approach

Our empirical strategy compares the outcomes of children born before and after the increase in the maternity leave mandate. We do this through the use of a two-step procedure that respects the fact that our policy variation is not at the level of the individual. To motivate our approach consider the estimating equation

$$(1) \qquad y_i = X_i \beta + \phi POST_i + \varepsilon_i$$

where y is the outcome of interest for individual i, X are conditioning variables and $POST$ equals 1 for cohorts born after the reform (2001 or later). Conditional on the *X's*, *POST* will capture

[16] Data from the first cycle has a different coding of the breastfeeding duration variable and no information on the introduction of food or exclusive breastfeeding.
[17] Because we see only year and not month of birth, we cannot implement the partial exclusion of observations from Alberta and Saskatchewan as we did for the NLSCY. However, in checks with the NLSCY including and excluding these observations makes little difference to the results.

average differences in *y* between children born in pre-and post-reform birth cohorts. While we wish to interpret the estimate of *POST* as only capturing the impact of the policy reform, it is clear it will also capture *any* unobservable, systematic differences across birth cohorts related to *y* and correlated with *POST*. These could be due to other changes in the environment or secular changes in the types of mothers who give birth—it is just these concerns that led us to delete observations on single parents and from the province of Quebec. Because the change in maternity leave was country-wide, there are no obvious control groups within Canada to account for these confounding factors. Also, because past data collection on breastfeeding has been sporadic in the U.S., we cannot construct a U.S. control group.

We address this challenge in three ways. The first is to carefully examine inter-cohort changes in outcomes before and after the reform. We can unpack the *POST* dummy into cohort specific indicators leading to the estimating equation

$$(2) \qquad y_i = X_i\beta + \sum_t \gamma^t YOBt_i + u_i$$

where *YOB* denotes year of birth. The γ^t (t=1999-2003) provide birth cohort specific estimates of the conditional mean of *y*, and therefore can reveal any secular trends in *y* that might undermine the inference. Ideally we would want these estimates to be relatively similar across cohorts before and after the reform. Alternatively, by comparing estimates of γ for adjacent birth cohorts we can construct counterfactual estimates of *POST* assuming the policy reform occurred in 1999/2000, for example, rather than 2000/2001.

Figure 1 demonstrates these points. We graph the estimates of γ when *y* is the number of months the mother is away from work in the child's first year of life. The 1998 cohort is the excluded category, so the estimates are relative to the time at home for mothers of that birth cohort. While there are some differences in the estimates between birth cohorts, the change between the 2000 and 2001 cohorts corresponding to the maternity leave reform is dominant. There is no change of similar scale if we think of counterfactual policy reforms across any other pair of adjacent cohorts. We report estimates of the γ for all outcome variables in the appendix, and draw on this evidence in our interpretation of the estimates of *POST*.

The second approach separates mothers into those who were likely eligible for the leave and those who were not. The NLSCY has only occasionally collected information on pre-birth employment, but has good information on post-birth work. We can use this information to restrict the sample based on post-birth characteristics that should be highly correlated with pre-birth eligibility, such as work post-birth. By comparing our results in this subsample to the results from the full sample, which includes potentially ineligible mothers, we can observe whether the ineligibles are contributing to a result or whether it is the eligibles who drive the estimates in the full sample. Because post-birth work patterns are potentially affected by the reform, this approach can act only as an informal check on our main results.

Finally, a third approach is to construct control groups of older children from pre-reform cohorts. This is feasible for the health outcomes for which we have contemporaneous observations for both our analysis sample and the older age group. The observations on the older children will control for any year-specific environmental factors affecting the health of children of all the ages, or any systematic survey instrument effects: subtle changes in the wording or delivery of the survey across waves that affect the responses to the health questions. We select children aged 25-36 months to serve as a control group for the analysis sample aged 13-24 months.[18] We drop data from the 6th wave of the survey because there are no observations on

[18] For some outcomes the older children span a smaller age interval because some health questions are only asked of specific age groups.

the pre-reform cohorts in the 25-36 month interval. The estimating equation corresponding to (1) for the analysis is

(3) $$y_i = X_i\beta + \alpha YNG_i + \lambda POSTW_i + \eta YNG_i POSTW_i + \mu_i$$

where YNG_{iw}=1 if the observation is for a child aged 13-24 months, and *POSTW*=1 for observations from post-reform survey years (i.e., wave 5 of the data).

Following Donald and Lang (2007), we recognize that our policy effects are effectively identified by variation in the conditional mean of our dependent variables across (typically) six birth cohorts, and so the estimation procedure that we use respects this fact. We first estimate equation (2) with no constant, which provides direct estimates of all the YOB_t effects. The numbers of observations for these first-stage regressions are reported in the appendix tables. We next use these estimates as the dependent variable in a six observation regression analogous to equation (1), but with only a constant and *POST* as explanatory variables and weighting by the sum of the individual weights by year of birth. Because there will be four degrees of freedom in these regressions, the critical value for statistical significance is 2.78 at the five percent level. It is important to note that all the substantial inferences of the paper are robust to different methods of calculating the standard errors.[19]

The control variables for these regressions include dummy variables for male children, single month of child's age, province, city size, mothers' and fathers' education (4 categories),

[19] We also directly estimated equations (1) and (2) comparing robust standard errors to estimates clustering on the year of birth. The latter standard errors 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 54 observation (9 provinces, 6 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 18 observation second stage (9 provinces, 2 values of *POST*). This last method potentially has the advantage of addressing the effects of any autocorrelation in the error term following the advice of Bertrand et al. (2004).

age (6 categories) and immigrant status, and the presence of up to 2 older or younger siblings.[20] We also report a specification that adds the year of birth provincial unemployment rate as a control for local labor market conditions.[21]

7.0 Results

We now discuss the empirical results, looking sequentially at the time a mother spends at home, breastfeeding, and finally health. Most of the estimates are for samples that include mothers who were not eligible for job-protected maternity leave or Employment Insurance benefits. We can translate these intention-to-treat estimates to an impact of the treatment on the treated using information from the Survey of Employment Insurance Coverage discussed earlier. If we assume that all mothers with pre-birth insurable employment were eligible (75 percent), then we should scale our estimates by (1/0.75) 1.33. However, only 85 percent of these mothers actually qualified and applied for benefits, suggesting a scale factor of [1/(0.75*(0.85)] = 1.57. We use these two scale factors as bounds to translate our intention-to-treat estimates to estimates of treatment on the treated in the discussion of results that follows.

7.1 Time at home

Estimates of the impact of the maternity leave expansion on mothers' time not at work in the first year of life are presented in Table 2. In the first row are the results for the summary measure "total months away from work." There is a significant increase of almost 2.3 months, which represents a 28 percent increase over the pre-reform average of 8.2 months. As already seen in Figure 1, the estimates of the individual year of birth (YOB) effects, reported in Table A1

[20] 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.
[21] Because interviews process spans several months, we use the average unemployment rate for the months September through May in the relevant years.

of the appendix, are consistent with a causal interpretation of this result. Adjusted to an estimate of treatment on the treated, the intention-to-treat coefficient of 2.3 months is scaled up to between 3.05 and 3.59 months.

In the next column is the estimate from a specification that excludes all explanatory variables except the YOB effects from the first-stage regression. This estimate provides a simple check on our implicit assumption that births are randomly distributed across birth years. While this specification does not guard against possible correlation of unobserved mother or child characteristics with the *POST* dummy, it would certainly be harder to maintain this assumption if we found significant correlation of observable characteristics with *POST*. As it turns out this estimate is very similar to its predecessor, indicating a 2.16 increase in months away from work in the first year.

The third column adds the provincial unemployment rate as an additional control in the first-stage regression. It is natural to suspect that labor market conditions might affect the time before a mother returns to work post-birth. The estimate, however, is almost identical to the one in the first column.

In the next rows of Table 2 we map out the distribution of the impact of the reform by presenting separate estimates for the proportion of mothers still away from work at 1, 3, 6, 9 and 12 months. A comparison of the estimates across columns reveals that in each case the specification of the conditioning variables matters little to the inference. As a result we can efficiently summarize both the estimates of *POST* and the individual *YOB* effects (Table A1) for these variables in Figure 2 where we graph the empirical survival function by year of birth at 1, 3, 6, 9 and 12 months from the data.[22]

[22] These are simple weighted means of the proportions still not at work at the indicated month by birth cohort.

In Figure 2, pre-reform cohorts are denoted with broken lines; post-reform with smooth lines. There are two important conclusions. First, the estimates are relatively stable within the pre- and post-reform birth cohort groupings. While there is some variation across cohorts at longer durations pre-reform, this does not represent a secular trend upwards: the highest line is for the 1999 cohort and the function drops back down with the 2000 cohort. The overall pattern of the lines across the reform supports a causal interpretation of the *POST* estimates. Second, the largest impact of the reform is exactly where we expect: at durations between the old and new mandates. The effect here is substantial. As reported in Table 2 (first column) the increase at 9 months is 79 percent of the pre-reform mean, and at 12 months is 71 percent. It is clear that the reform led to significant increase in the length of time mothers were away from work.

We obtain an alternative summary of these results by directly estimating the relationship between months not at work in the first year and the provincial job-protected leave mandates presented in Table 1. This specification takes account of differences in the pre-reform mandates across provinces. We estimate this relationship in one step, substituting the legislated leave mandate (in weeks) for *POST* in (1), and estimating the standard errors clustering on province/*POST* (18 groups). In the base specification of the conditioning variables the estimate is 0.092 (0.016), or approximately (0.092*4.333) 0.4 of a week at home for each additional week of mandated leave. After scaling for treatment, the estimate is between 0.123 and 0.143.

As a final check, we compare these results to those from the restricted sample of 'likely eligibles': mothers who return to work in their child's first year of life.[23] In this sample, the estimate of *POST* is 3.22 (0.196), which sits at the midpoint of the estimated bounds from the scaling method. This suggests that our estimates in the full sample are driven by the eligibles,

[23] As mentioned earlier, this selection is potentially endogenous to the reform. In unreported results, we estimate a modest decrease in the proportion of mothers returning in the first year as a result of the reform which is not statistically significant. Moreover, the individual *YOB* effects do not clearly indicate a causal effect.

with the ineligibles implicitly taking the role of control group. The mean time away from work for this sample 5.9 months, which is less than the 8.2 months in the full sample. It then follows that the estimated percentage increase in time not at work for the treated from this sample at 55 per cent (3.22/5.9) is much higher than our previous estimate of 28 percent from the full sample. We can also use this sample to directly estimate an elasticity of time away from work with respect to weeks of provincially-varying job-protected leave for the treated, evaluated at the sample means, which is 0.742.

7.2 Breastfeeding

In Table 3 we present the breastfeeding results based on data from the CCHS. We begin with incidence and duration of breastfeeding, and close with an investigation of why breastfeeding stopped. The incidence question in the CCHS asks women if they breastfed or even tried breastfeeding their baby.[24] In the first row of the table, the reform seems to have had relatively little effect on incidence. The results are not large relative to the mean, and only in one specification does the coefficient attain statistical significance. This is largely consistent with the literature, which has found little impact of return to work on incidence.

The next 7 rows of Table 3 contain the results for months of breastfeeding in the child's first year. Months of breastfeeding is estimated to have increased by 0.75, which is 14 percent of the pre-reform mean of 5.34 months. A check across columns reveals this result does not depend on the specification of the explanatory variables, and the individual's year of birth effect estimates in Table A2 support a causal interpretation. We summarize the results at specific duration thresholds by graphing the empirical survival functions by year of birth in Figure 3. Again the functions are relatively stable within the pre- and post-reform cohorts, especially

[24] The exact wording is "(For your last baby,) did you breastfeed or try to breastfeed your baby, even if only for a short time?"

between 3 and 9 months. It is also at these months where we see the main impact of the reform in a significant shift upwards in the functions. Finally, scaling the results to obtain a treatment on the treated, we estimate an increase in months in the first year ranging from 0.995 to 1.17. We have no information on employment pre- or post-birth in the CHHS, so we cannot attempt to estimate a treatment on the treated through a restricted sample as we did for the NLSCY.

Recommendations for breastfeeding in US and Canada range from one to two years. While breastfeeding rates at long durations remain modest post-reform, the proportional increases over the pre-reform means resulting from the increase in the mandate are impressive. Scaling the estimates for treatment (1.33) they range from 23 percent at 6 months to 32 percent at 12 months. Furthermore, these estimates are lower bounds, as the pre-reform means for the treated are likely lower than for the full sample.[25]

We also estimate the relationship between months of breastfeeding in the first year and weeks of mandated leave. The estimated parameter is 0.031 (0.008). For the treated using the 1.33 scale factor the estimate is 0.041 months, or 0.18 of a week with each week of mandated leave. To calculate an elasticity, we need the sample averages of months of breastfeeding in the first year and weeks of mandated leave for the treated. While we cannot directly estimate these means in the CCHS, we can do so using information from the NLSCY.[26] The resulting elasticity for eligible women is 0.318.

[25] The NLSCY also contains some limited information on breastfeeding. We do not use these data due to reported problems in the application of this part of the survey in wave 5, which, importantly, spans the reform we analyze. Deleting the wave 5 data (which still leaves some pre- and post-reform observations), however, we can calculate the ratios of means of breastfeeding variables between the sample of births to mothers who returned to work in the first year (our sample of the "likely treated") and the full sample of births. For example, in the NLSCY the mean proportion of mothers, who return to work in the first year and are still breastfeeding at 6 months is 92 percent of the full sample mean. Applying this proportion to the CCHS full sample mean we obtain a 25 percent increase in the proportion still breastfeeding at 6 months for the treated as a result of the reform.

[26] See footnote 25. Using the NLSCY we calculate the ratios of the mean months breastfeeding in the first year and weeks of mandated leave between the full sample of births and the sample of births to mothers who returned to work in the first year. We then apply these ratios to the full sample means in the CCHS to obtain a mean for the treated.

In the next 4 rows are results for exclusive breastfeeding.[27] Total months of exclusive breastfeeding are estimated to increase 0.38 in the full sample, or over 12 percent of the pre-reform average of 3 months. After scaling up, the estimate for the treated is between 0.51 and 0.59 months. Looking at the attainment of thresholds, the primary effect is between 3 and 6 months, with the estimated change increasing as a percent of the pre-reform mean. At six months exclusive breastfeeding—the public health target—the estimated impact of the reform is 5.8 percentage points in the full sample. Scaling for treatment, we get 7.7 to 9.1 percentage points or 38.7 to 45.5 percent of the pre-reform full sample mean of 0.20. These proportional increases are likely lower bounds as the pre-reform mean for the treated is likely smaller.[28]

The final rows of Table 3 investigate the reasons mothers report ending breastfeeding. There is a relatively large impact on the proportion that reports introducing food due to work relative to the pre-reform mean. There is also a significant and quite large impact on stopping due to work, approaching 50 percent of the pre-reform mean. As work falls in importance, the proportions reporting other reasons must necessarily rise. The largest increase comes from mothers reporting that the baby was ready for solid food. In the final row of the table we report a significant effect on the proportion who never initiated due to work that is large relative to the pre-reform mean.

We close the breastfeeding analysis with a two-sample instrumental variables (Angrist and Krueger 1992) estimate of the impact of months away from work on months breastfeeding. Informally, one can use ratios of results from Tables 2 and 3 to obtain TSIV estimates – they are informal because the conditioning variables differ in the NLSCY and CCHS. For example, the

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[27] The CCHS questionnaire asks, "How old was your (last) baby when you first added any other liquids (e.g. milk, formula, water, teas, herbal mixtures) or solid foods to the baby's feeds?"
[28] We cannot use the NLSCY here to estimate the pre-reform means for the treated because this survey does not collect information on exclusive breastfeeding.

ratio of the estimated reduced form relationship between *POST* and months breastfeeding in the first year to the estimated first-stage relationship between *POST* and months not at work, is (0.746/2.284) 0.327. Alternatively we could use the ratio of estimated relationships of these outcomes to weeks of mandated leave: (0.031/0.092) 0.337.[29]

In Table 4 we present a formal TSIV estimate using conditioning variables that are common to the two data sets, and using weeks of mandated leave rather than *POST* as the instrument.[30] The estimated first-stage and reduced form relationships are very similar to those reported in the discussion of Tables 2 and 3; further evidence that the conditioning variables are not important to the inference. The TSIV estimate of 0.334 is very similar to its informal counterparts calculated above. Using our estimates of the sample means of the two variables for the treated, the TSIV result implies an elasticity of months of breastfeeding in the first year with respect to months not at work in the same period of 0.458. We are not aware of another estimate of this elasticity in the literature, and it indicates that breastfeeding is fairly responsive to time not at work, at least among women whose behavior is responsive to maternity leave mandates.

7.3 Health

Few previous studies of the health benefits of breastfeeding are based on clearly exogenous variation in breastfeeding behavior. The changes in breastfeeding induced by the expansion of maternity leave, therefore, lead us to extend our analysis to the health and wellbeing variables available in the NLSCY. While the NLSCY is not designed specifically as a health survey, it does ask a series of questions on birth outcomes and infant and mother's health.

[29] Using our scaled results for the treated yields the same TSIV estimate, as the scale factor simply divides out.
[30] The conditioning variables are province, mother's age (single year), mother's education (4 categories) urban residence and mother's immigration status.

Importantly the questions span many of the outcomes that have been linked to breastfeeding behavior, including the incidence of respiratory ailments (asthma, allergies and ear infections).

Before discussing the results, two important qualifications should be mentioned. First, we cannot separate the impact of increased breastfeeding on these health outcomes from any impact of the increase in maternal care, since both happened simultaneously. Therefore, strictly speaking, our analysis is of the health impact of maternity leave mandates rather than breastfeeding per se.[31] That said, we focus on health indicators that are more plausibly related to breastfeeding and also by examining health outcomes in the child's second year of life when parental care is not very different pre- and post-reform.[32] It is also important to note that in most cases an increase in breastfeeding and an increase in parental care should have complementary effects on health outcomes. This clearly complicates the interpretation of any corresponding changes in health outcomes, It does not pose a problem, however, if we find no effects on health. The second qualification concerns the impact of the increase in maternity leave on breastfeeding at short and long breastfeeding durations. While the effect was very strong for longer durations, it was almost non-existent for breastfeeding initiation. This implies that our results will not be informative about the health impact of the decision to breastfeed or breastfeeding over the first months. However, given the already high rates of breastfeeding initiation in developed countries, and the public health focus on attaining durations of six months or more, our analysis is well aligned with policy debate in this area.

The estimates are presented in Table 5. On the left side of the table are results for children aged 7-12 months, with results for children aged 13-24 months on the right. For both

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[31] A policy to isolate the impact of breastfeeding duration would either facilitate breastfeeding at work or prolong the breastfeeding of mothers at home, without changing the decision of when to return to work post birth.

[32] The impact of the parental leave reforms on parental and non-parental care is studied in detail in Baker and Milligan (2007). They find a large substitution away from unlicensed non-parental care in someone else's home toward parental care in their own home for ages 7-12 months, but no impact at 13-24 months.

age groups, we implement the same two-stage estimation method as previously, looking for an impact of the reform on health outcomes.[33] For the 13-24 month age group, we can also implement a difference-in-differences estimator using children age 25-36 months as the control group. The results for the 13-24 age group allow us to distinguish whether results for the age 7-12 month group are prophylactic or persistent. As well, they can help to distinguish between the impact of breastfeeding and of direct maternal care, because there is very little change in care arrangements of children aged more than 12 months after the reform.[34]

The first set of results is for the mothers. There are four variables—a 5 level indicator of mothers self-reported health, an index of depression, a 0/1 indicator of no post-partum depression, and a count of post-partum problems.[35] In none of the three specifications across the table do any of the mother's health variables indicate an impact of the reform. All estimates are statistically insignificant and small relative to the pre-reform mean.

The remaining rows in Table 5 contain the results for child's health. There is no evidence of an impact on current weight, either during the time of expanded leave from months 7 to 12 or following the leave in months 13 to 24. The 95 percent confidence interval around our point estimate precludes large effects. This is a potentially important result because while formula feeding is sometimes linked with obesity, there are also concerns that campaigns for longer breastfeeding—especially exclusive breastfeeding—might compromise the early growth

[33] We also ran some two-sample IV regressions, using the provincially-varying weeks of mandated leave as an instrument for breastfeeding, with health outcomes as the dependent variable. The results were very similar to the reduced form approach we report. The IV regressions effectively force the maternity leave reform to affect health only through breastfeeding and not other potential channels. Because we have no *a priori* reason to assume other channels are not operative, we choose to focus on the reduced form results in the paper.

[34] See Baker and Milligan (2007) for details on the effect of the maternity leave expansions on the mode of childcare.

[35] The self-reported health index ranges from 1 (excellent) to 5 (poor). The depression index is based on a 0 to 3 score for twelve separate questions, giving a range from 0 to 36. Higher numbers indicate more depression. Post-partum problems include post-partum haemorrhage, post-partum infection, post-partum depression for more than 14 days and post-partum hypertension.

of infants. There is also no impact on the child's overall health, as self-reported by the mother on a five point scale. Nor is there any suggested impact on the number of injuries or the incidence of nose/throat infections, both of which may have been expected to fall with the increased in maternal care.

We now turn to the results for ear infections, which have been linked to breastfeeding in past research. The estimated reduction in the incidence of ear infections for the younger children is 0.07. which while sizeable relative to the mean, does not quite meet our heightened bar for statistical significance at the five percent level.[36] The estimated reduction for the 13-24 month olds, however, is both sizable and statistically significant. In the next row, the effect on the number of infections is negative, large and statistically significant for both age groups. The individual estimates of *YOB* in Tables A3 and A4 of the appendix suggest a causal interpretation is possible, although not as clear cut as we observed for the measures of labor supply and breastfeeding. Also, it is possible that the decrease in ear infections comes not from breastfeeding but from the substitution from non-parental to maternal care. For example, in non-parental care there may be higher exposure to viruses and bacteria from other children. However, we did not find a similar impact for nose and throat infections, which we might expect if this non-parental care channel were operative. This may lend some support for breastfeeding as the causal mechanism.

However, the difference-in-differences results for ear infections in the last column are small and insignificant. This points to some environmental factor that lowered the incidence of this ailment for all children aged 13-36 months in wave 5 of the data. To further investigate this hypothesis we estimated *POST* separately for children from the samples of mothers who returned

[36] As a further check on the validity of parental reports, we note that Auinger et al. (2003) find that 68.2% of children under age 6 had ever had an episode of otitis media. Furthermore, 43.5% of children had at least one episode before age 12 months. We view these incidence rates as consistent with those found in the NLSCY.

to work within one year of birth (our sample of the “likely treated” based on post-birth employment) and of mothers who didn’t. The estimated reductions in the incidence and number of ear infections from these different samples (not reported) are very similar: negative, large and statistically significant. This is further evidence that the effect is related to environmental factors rather than increased breastfeeding or maternal care.

For the remaining health indicators, there are some significant estimates, but none appears to be robust. Moreover, the standard errors for some of the estimates are small enough to rule out (at the 95 percent level of confidence) large effects.[37] The impacts on asthma and allergies, which have been linked to breastfeeding in past research, are large and significant for children aged 7 to 12 months. In these cases, however, the *YOB* estimates in Table A3 vary substantially across cohorts, especially for asthma. The estimates for children age 13-24 do not indicate any persistence of these effects. Bronchitis shows a significant difference-in-difference estimate, but the *YOB* estimates for this variable suggest this is being driven by variation in the outcome in the control group. The estimate for chronic conditions is also statistically significant for 7 to 12 month olds, and the *YOB* estimates suggest a more systematic relationship to the reform. While the NLSCY tracks a number of specific chronic conditions, the subcategory of “other” chronic conditions seems to be driving the observed result, rendering this finding opaque. Again, this result does not appear to persist.

Our choice of health variables from the NLSCY has been guided by the links between breastfeeding and health reported in past research. The experimental study of Kramer et al. (2001) finds no effects on respiratory ailments; nor do we in the NLSCY. In (unreported)

[37] For instance, the 95% confidence interval for our wheezing estimate for the 13-24 month old sample is -0.053 to +0.036. The mean incidence is 0.27, so we can rule out an impact of 20% of the mean at a 95% level of confidence. While the standard errors on our other health measures are typically in the same range as for wheezing, the means vary much more. So, for bronchitis with a mean incidence of only 0.02, the confidence interval spans from an impact of close to -100% to +63% of the mean.

falsification tests we find no impact for outcomes such as birth weight, gestation, c-sections that no previous research has linked to breastfeeding. Finally, our data do not allow us to investigate effects on gastrointestinal problems or skin rashes – two ailments commonly linked to breastfeeding.[38] Overall, we find little evidence that the extra breastfeeding induced by the expanded parental leave policies led to improvements *in the health outcomes captured by the NLSCY for children aged up to 24 months*.

8.0 Conclusions

We study the impact of an increase in maternity leave entitlements on mothers' time away from work, breastfeeding, and mothers' and children's health. Our primary finding is that this policy change had significant impacts on mothers' time away from work post birth and the length of time they breastfed. Time away from work increased more than three months for those eligible for leave. Correspondingly, breastfeeding duration increased sharply, by over a month, and the proportion of mothers attaining the public health benchmark of six months exclusive breastfeeding increased by nearly 40 percent.

We also track the effect of the increase in breastfeeding (and parental care) on a set of self-reported indicators of the mother and child (in the first 24 months) health captured in the NLSCY. We do not find any consistent, robust effect on any of these variables.

These findings should be of interest to two groups of policymakers. For public health officials aiming to increase breastfeeding duration it appears the labor market policy may prove an effective way of achieving breastfeeding goals. More generally, the findings should prove useful for those interested in the impacts of compensated maternity leaves, such as the short term

[38] The NLSCY does ask about any overnight hospital admissions due to gastrointestinal illness. The pre-reform incidence of this ailment is less that two percent in the 7-12 month age group. The estimated impact of the reform on this variable, while negative, is statistically insignificant.

disability entitlements available around birth in some states, or the paid family leave program introduced in California in 2004.[39]

In the future, more research on these cohorts of Canadian children may prove fruitful. Age and data constraints limit our study to children up to a few years of age and to a limited set of parent-reported measures of health. As time unfolds longer-run health outcomes may be observed, Medical records would provide another window on health. Rigorous study of these data as, and if, they become available will allow a more complete picture of the health impacts of parental leave and breastfeeding to be formed.

[39] See Progressive States Network (2006) for details on legislative initiatives.

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Table 1: The Extension of Mandated Parental Leave by Jurisdiction

	Weeks of leave in 2000	Date of Extension	Weeks of leave post-reform
Newfoundland	29	31-Dec-00	52
Prince Edward Island	34	31-Dec-00	52
Nova Scotia	34	31-Dec-00	52
New Brunswick	29	31-Dec-00	54
Quebec	70	no reform	70
Ontario	35	31-Dec-00	52
Manitoba	34	31-Dec-00	54
Saskatchewan	30	14-Jun-01	52
Alberta	18	7-Feb-01	52
British Columbia	30	31-Dec-00	52
Federal	41	31-Dec-00	54
Employment Insurance	25	31-Dec-00	50

Notes: The source is provincial statutes and federal labor standards publications.

Table 2: Estimated impact of longer maternity leave mandates on time spent not at work post-birth (NLSCY data)

	Pre-Reform Mean	POST	POST No Controls	POST UR Control
Months not at work in the first year	8.23	2.284* (0.299)	2.158* (0.258)	2.273* (0.331)
Months not at work ≥ one month	0.96	0.022 (0.009)	0.019* (0.006)	0.021 (0.013)
Months not at work ≥ three months	0.92	0.039* (0.012)	0.035* (0.011)	0.038* (0.015)
Months not at work ≥ six months	0.81	0.091* (0.024)	0.079* (0.018)	0.090* (0.025)
Months not at work ≥ nine months	0.47	0.371* (0.040)	0.353* (0.035)	0.369* (0.046)
Months not at work ≥ twelve months	0.41	0.291* (0.041)	0.277* (0.040)	0.291* (0.041)

Notes: N=6. Standard errors in parentheses. *~statistically significant at the 5% level. UR~conditional on month of birth unemployment rate. The column labeled 'POST' contains regression coefficients for the 'POST' variable in the second stage of our two-stage procedure. The column labeled 'POST No Controls' contains regression coefficients for the 'POST' variable in the second stage when the first-stage contained only year dummies an no other control variables. The column labeled 'POST UR Control' reports the regression coefficient for the 'POST' variable in the second stage when the first-stage included a province-year varying unemployment rate.

Table 3: Estimated impact of longer maternity leave mandates on breastfeeding in the (CCHS data)

	Pre-Reform Mean	POST	POST No Controls	POST UR Control
Incidence of breastfeeding	0.86	0.026	0.034*	0.027
		(0.011)	(0.011)	(0.012)
Months of breastfeeding in first year	5.34	0.746*	0.781*	0.735*
		(0.107)	(0.138)	(0.127)
Months of breastfeeding ≥ one month	0.72	0.051*	0.059*	0.051*
		(0.015)	(0.014)	(0.014)
Months of breastfeeding ≥ three months	0.63	0.067*	0.074*	0.064*
		(0.008)	(0.009)	(0.009)
Months of breastfeeding ≥ four months	0.57	0.056*	0.060*	0.055*
		(0.008)	(0.007)	(0.010)
Months of breastfeeding ≥ six months	0.47	0.081*	0.084*	0.080*
		(0.013)	(0.016)	(0.015)
Months of breastfeeding ≥ nine months	0.26	0.060*	0.058*	0.058*
		(0.019)	(0.021)	(0.022)
Months of breastfeeding ≥ twelve months	0.14	0.034	0.028	0.033
		(0.012)	(0.015)	(0.013)
Months of exclusive breastfeeding in first year	3.07	0.379*	0.371*	0.370*
		(0.074)	(0.084)	(0.085)
Months of exclusive breastfeeding ≥ three months	0.54	0.055*	0.056*	0.054*
		(0.012)	(0.012)	(0.014)
Months of exclusive breastfeeding ≥ four months	0.43	0.058	0.053	0.058
		(0.022)	(0.020)	(0.022)
Months of exclusive breastfeeding ≥ six months	0.20	0.058*	0.051*	0.057*
		(0.014)	(0.015)	(0.018)
Introduced food due to work	0.06	-0.036*	-0.033*	-0.037*
		(0.010)	(0.008)	(0.010)
Stopped breastfeeding due to work	0.17	-0.082*	-0.079*	-0.084*
		(0.022)	(0.021)	(0.022)
Stopped breastfeeding baby ready for solid food	0.04	0.106*	0.100*	0.106*
		(0.032)	(0.032)	(0.032)
No breastfeeding due to work	0.05	-0.038*	-0.029	-0.040*
		(0.017)	(0.012)	(0.015)

Notes: N=6. Standard errors in parentheses. *~statistically significant at the 5% level. The regression specifications are the same as described in Table 2.

Table 4: Two Sample Instrumental Variables Estimate of the Relationship between Months spent at home in first year and Months of Breastfeeding in first year

First-stage Relationship between months spent at home in first year and weeks of mandated leave (NLSCY)	0.092* (0.014)
Reduced Form Relationship between months breastfeeding in first year and weeks of mandated leave (CCHS)	0.031* (0.008)
TSIV estimate of the Relationship between Months spent at home in first year and Months of Breastfeeding in first year	0.334* (0.103)

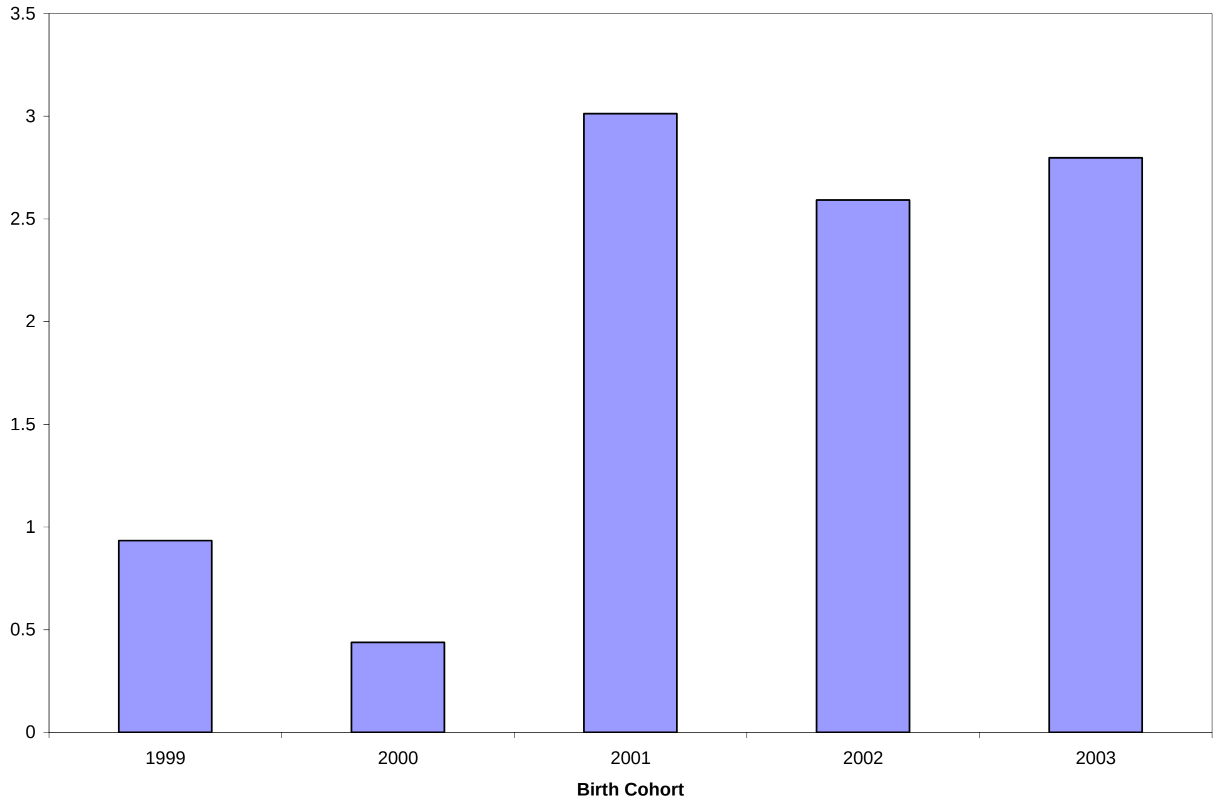
Notes: Clustered standard errors in parentheses. Standard error for TSIV estimate following Jappelli et al. (1998). *~statistically significant at the 5% level.

Table 5: Estimated impact of longer maternity leave mandates/breastfeeding duration on health outcomes (NLSCY data)

	7-12 Months		13-24 Months		
	Pre-Reform Mean	POST	Pre-Reform Mean	POST	Difference in Difference
Mother's health	1.84	0.039 (0.044)	1.86	0.017 (0.038)	-0.016 (0.074)
Mother's depression	4.09	-0.171 (0.238)	3.92	-0.271 (0.191)	0.272 (0.254)
Absence of post-partum depression	0.88	0.012 (0.057)	N.A.	N.A	N.A
Mother post-partum problem	0.22	0.032 (0.016)	N.A	N.A	N.A
Child's current weight	9.39	-0.103 (0.154)	11.59	-0.023 (0.083)	0.044 (0.174)
Child's health	1.34	-0.082 (0.041)	1.44	-0.053 (0.056)	0.122 (0.074)
Child injured in past 12 months	0.03	0.008 (0.004)	0.07	0.003 (0.011)	-0.001 (0.019)
Child nose/throat infections	0.32	0.006 (0.041)	0.47	-0.012 (0.023)	-0.005 (0.056)
Child ear infections	0.25	-0.070 (0.030)	0.50	-0.125* (0.019)	-0.052 (0.037)
Child number of ear infections	0.42	-0.159* (0.069)	1.02	-0.289* (0.060)	-0.011 (0.138)
Child asthma	0.03	-0.014* (0.003)	0.06	-0.018 (0.007)	-0.024 (0.013)
Child wheeze	0.23	-0.000 (0.033)	0.27	-0.009 (0.016)	-0.009 (0.016)
Child allergies	0.07	-0.028* (0.008)	0.07	-0.006 (0.011)	-0.030 (0.019)
Child bronchitis	0.02	-0.005 (0.007)	0.02	-0.004 (0.006)	-0.015* (0.004)
Child chronic condition	0.12	-0.051* (0.012)	0.13	-0.020 (0.014)	-0.027 (0.028)

Notes: N=6 (N=. Standard errors in parentheses. *~statistically significant at the 5% level. N.A.= not available. The 'POST' columns report the regression coefficient on the 'POST' variable in the second stage of our two stage procedure. The 'Difference in Difference' column reports the regression coefficient on the 'POSTW-YNG' interaction variable described in equation (3) in the text.

Figure 1: Estimates of months before the mother returned to work by birth cohort, relative to 1998 births



Notes: Displayed are the coefficients on year of birth dummy variables using NLSCY data.

Figure 2: Proportion of mothers not at work by month post-birth, by birth cohort

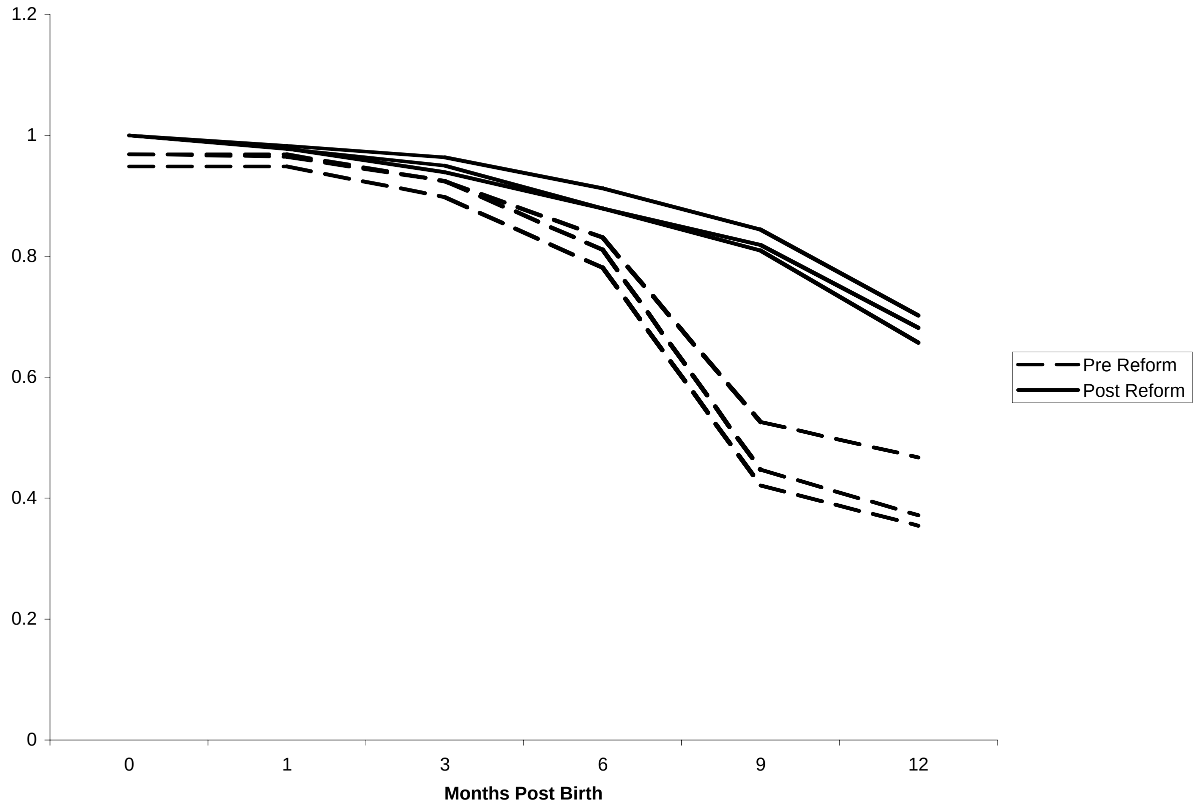


Figure 3: Proportion of mothers still breastfeeding by month post-birth, by birth cohort

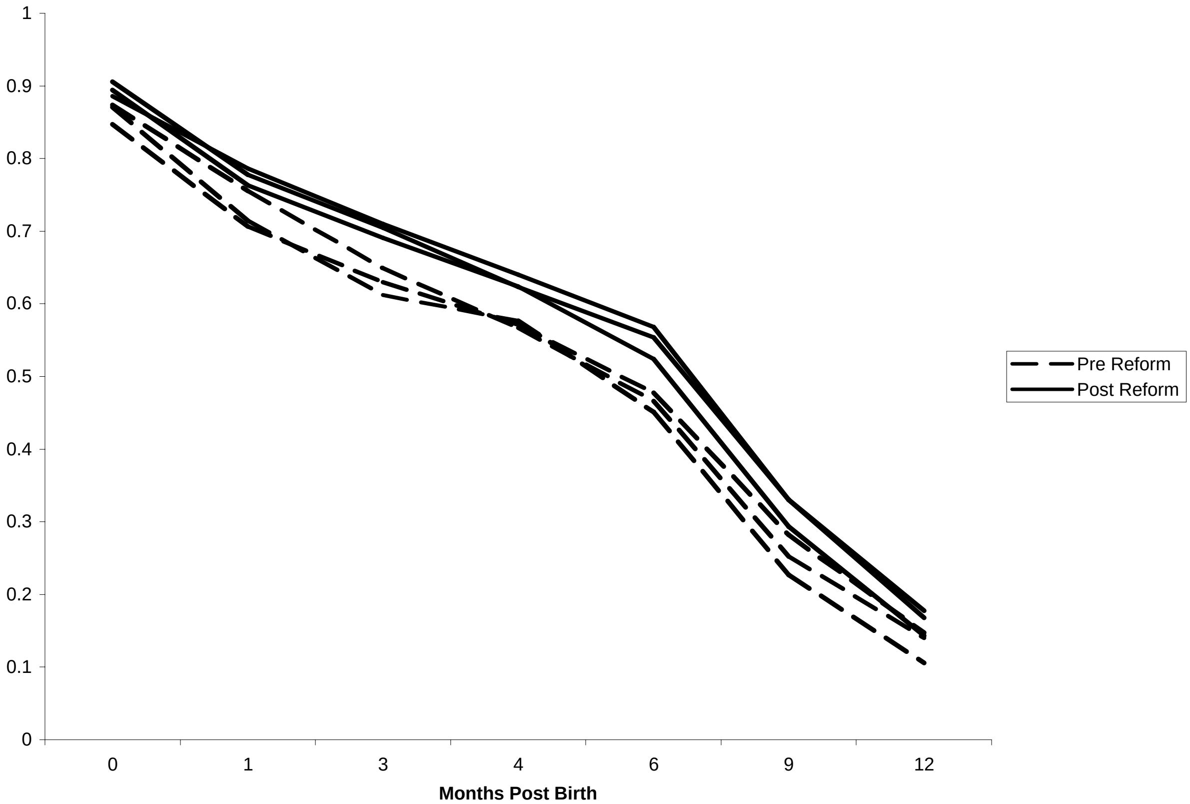


Table A1: Estimated impact of longer maternity leave mandates on time not at work

	1999	2000	2001	2002	2003
Months not at work in	0.934	0.438	3.013	2.591	2.798
the first year	(0.230)	(0.244)	(0.228)	(0.228)	(0.228)
Months not at work ≥	0.028	0.020	0.045	0.032	0.042
one month	(0.013)	(0.014)	(0.013)	(0.012)	(0.012)
Months not at work ≥	0.033	0.033	0.077	0.057	0.055
three months	(0.018)	(0.019)	(0.017)	(0.017)	(0.018)
Months not at work ≥	0.067	0.044	0.159	0.109	0.127
six months	(0.024)	(0.027)	(0.024)	(0.024)	(0.024)
Months not at work ≥	0.125	0.041	0.457	0.414	0.435
nine months	(0.030)	(0.033)	(0.029)	(0.029)	(0.029)
Months not at work ≥	0.122	0.027	0.368	0.324	0.356
twelve months	(0.031)	(0.032)	(0.032)	(0.032)	(0.031)

Notes: N=5758. Robust standard errors in parentheses.

Table A2: Estimated impact of longer maternity leave mandates on breastfeeding by year of birth

	1999	2000	2001	2002	2003
Incidence of breastfeeding	-0.003	-0.028	0.005	0.018	0.013
	(0.024)	(0.024)	(0.021)	(0.021)	(0.022)
Months of breastfeeding in first year	-0.148	0.083	0.854	0.728	0.592
	(0.336)	(0.032)	(0.297)	(0.332)	(0.316)
Months of breastfeeding ≥ one month	-0.032	-0.050	0.024	0.009	0.006
	(0.033)	(0.030)	(0.027)	(0.030)	(0.030)
Months of breastfeeding ≥ three months	-0.030	-0.022	0.054	0.040	0.040
	(0.038)	(0.033)	(0.031)	(0.035)	(0.033)
Months of breastfeeding ≥ four months	0.019	0.003	0.071	0.046	0.061
	(0.040)	(0.036)	(0.033)	(0.038)	(0.036)
Months of breastfeeding ≥ six months	-0.008	0.009	0.098	0.077	0.064
	(0.040)	(0.036)	(0.035)	(0.039)	(0.089)
Months of breastfeeding ≥ nine months	-0.021	0.032	0.081	0.073	0.053
	(0.035)	(0.033)	(0.032)	(0.036)	(0.034)
Months of breastfeeding ≥ twelve months	-0.027	0.009	0.035	0.039	0.019
	(0.027)	(0.026)	(0.026)	(0.029)	(0.027)
Months of exclusive breastfeeding in first year	0.238	0.129	0.578	0.499	0.393
	(0.217)	(0.198)	(0.191)	(0.211)	(0.203)
Months of exclusive breastfeeding ≥ three months	0.044	0.025	0.091	0.074	0.065
	(0.041)	(0.037)	(0.035)	(0.039)	(0.038)
Months of exclusive breastfeeding ≥ four months	0.091	0.081	0.131	0.102	0.135
	(0.041)	(0.036)	(0.034)	(0.038)	(0.037)
Months of exclusive breastfeeding ≥ six months	0.054	0.049	0.108	0.099	0.084
	(0.030)	(0.027)	(0.026)	(0.031)	(0.028)
Introduced food due to work	0.014	-0.008	-0.028	-0.039	0.051
	(0.021)	(0.018)	(0.016)	(0.017)	(0.017)
Stopped breastfeeding due to work	0.054	-0.009	-0.056	-0.087	-0.092
	(0.033)	(0.029)	(0.026)	(0.028)	(0.026)
Stopped breastfeeding baby ready for solid food	0.002	0.056	0.105	0.162	0.174
	(0.007)	(0.011)	(0.014)	(0.019)	(0.019)
No breastfeeding due to work	-0.006	-0.038	-0.046	-0.080	-0.069
	(0.054)	(0.045)	(0.040)	(0.040)	(0.046)

Notes: N=5708 for all regressions except those for those in last four rows for which N=4778, 4709, 4709, 818, respectively. Robust standard errors in parentheses.

Table A3 Estimated impact of longer maternity leave mandates/breastfeeding duration on Health Outcomes in the NLSCY, 7-12 months by year of birth

	1999	2000	2001	2002	2003
Mother's health	0.435	0.043	0.026	0.091	0.114
	(0.145)	(0.065)	(0.122)	(0.070)	(0.132)
Mother's depression	0.250	0.556	0.266	0.004	0.359
	(0.763)	(0.362)	(0.594)	(0.334)	(0.619)
Absence of post-partum		-0.110	-0.031	-0.017	-0.183
depression		(0.030)	(0.048)	(0.025)	(0.095)
Mother post-partum		0.007	-0.009	0.030	0.125
problem		(0.043)	(0.065)	(0.043)	(0.092)
Child's current weight	0.106	-0.354	-0.251	0.075	0.829
	(0.163)	(0.097)	(0.220)	(0.188)	(0.074)
Child's health	-0.104	-0.082	-0.185	-0.104	-0.186
	(0.101)	(0.053)	(0.078)	(0.058)	(0.084)
Child injured in past 12	0.009	-0.001	0.014	0.006	0.022
months	(0.025)	(0.011)	(0.020)	(0.012)	(0.026)
Child nose/throat	-0.076	-0.102	-0.036	-0.039	-0.058
infections	(0.072)	(0.037)	(0.068)	(0.039)	(0.073)
Child ear infections	-0.080	-0.041	-0.174	-0.071	-0.139
	(0.069)	(0.032)	(0.052)	(0.033)	(0.069)
Child number of ear	-0.217	-0.105	-0.371	-0.173	-0.346
infections	(0.116)	(0.066)	(0.087)	(0.067)	(0.097)
Child asthma	-0.025	-0.001	-0.022	-0.012	-0.013
	(0.016)	(0.014)	(0.012)	(0.012)	(0.017)
Child wheez	-0.115	0.072	0.004	0.032	-0.021
	(0.042)	(0.036)	(0.048)	(0.034)	(0.051)
Child allergies	0.000	0.018	-0.015	-0.019	-0.034
	(0.041)	(0.023)	(0.027)	(0.020)	(0.027)
Child bronchitis	-0.014	-0.015	-0.004	0.003	-0.010
	(0.010)	(0.015)	(0.011)	(0.013)	(0.009)
Child chronic condition	0.024	0.026	-0.051	-0.034	-0.062
	(0.063)	(0.029)	(0.034)	(0.027)	(0.034)

Notes: N=1674 for all regressions, except mother's post-partum depression 1379 and mother's post-partum problems 1263. Robust standard errors in parentheses.

Table A4 Estimated impact of longer maternity leave mandates/breastfeeding duration on Health Outcomes in the NLSCY, 13-24 months by year of birth

	1999	2000	2001	2002	2003
Mother's health	0.083	0.114	0.134	0.036	0.071
	(0.073)	(0.082)	(0.072)	(0.074)	(0.066)
Mother's depression	-0.237	-0.072	-0.730	-0.178	-0.250
	(0.370)	(0.371)	(0.364)	(0.332)	(0.364)
Child's current weight	-0.195	-0.324	-0.203	-0.189	-0.091
	(0.131)	(0.125)	(0.140)	(0.151)	(0.132)
Child's health	-0.128	0.003	-0.148	-0.030	-0.133
	(0.057)	(0.064)	(0.056)	(0.061)	(0.053)
Child injured in past 12	-0.035	-0.035	-0.016	-0.020	-0.029
months	(0.022)	(0.020)	(0.024)	(0.023)	(0.019)
Child nose infections	-0.040	-0.090	-0.034	-0.086	-0.053
	(0.043)	(0.044)	(0.043)	(0.043)	(0.040)
Child ear infections	-0.018	-0.012	-0.106	-0.183	-0.138
	(0.043)	(0.044)	(0.042)	(0.038)	(0.039)
Child number of ear	-0.104	-0.124	-0.278	-0.441	-0.414
infections	(0.106)	(0.101)	(0.101)	(0.090)	(0.098)
Child asthma	-0.005	0.010	-0.028	-0.005	-0.016
	(0.018)	(0.021)	(0.018)	(0.019)	(0.019)
Child wheeze	-0.001	-0.019	-0.048	0.008	0.003
	(0.038)	(0.041)	(0.037)	(0.041)	(0.035)
Child allergies	-0.006	-0.028	-0.002	-0.021	-0.027
	(0.021)	(0.021)	(0.022)	(0.019)	(0.019)
Child bronchitis	-0.013	-0.010	-0.020	-0.001	-0.014
	(0.009)	(0.014)	(0.010)	(0.011)	(0.010)
Child chronic condition	-0.032	-0.042	-0.043	-0.026	-0.059
	(0.026)	(0.027)	(0.027)	(0.025)	(0.024)

Notes: N=3418 for all regressions Robust standard errors in parentheses.

Table A5 Full Sample Results

Time spent not at work post-birth	POST
Months spent at home in first year	2.092* (0.229)
Months at home ≥ one month	0.017 (0.006)
Months at home ≥ three months	0.031* (0.009)
Months at home ≥ six months	0.080* (0.021)
Months at home ≥ nine months	0.343* (0.031)
Months at home ≥ twelve months	0.271* (0.029)
Breastfeeding	
Incidence	0.028* (0.006)
Months of Breastfeeding in first year	0.665* (0.128)
Months of Exclusive Breastfeeding in first year	0.349* (0.071)
Introduced food due to work	-0.034* (0.009)
Stopped breastfeeding due to work	-0.079* (0.019)
Stopped breastfeeding baby ready for solid food	0.105* (0.034)
No breastfeeding due to work	-0.034 (0.017)

Notes: N=6. Standard errors in parentheses. *~statistically significant at the 5% level.