How does job-protected maternity leave affect mothers' employment and infant health?

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Abstract

Unobserved heterogeneity correlated with leave entitlement potentially biases existing evidence of the labour supply effects of job-protected maternity leave. Without firm empirical evidence of the effects of leave on labour market behaviour, a 'causality gap' makes it difficult to credibly answer important policy questions about the effects of leaves on the infants' and mothers' welfare. We study the introduction and expansion of job-protected maternity leave in Canada, where leave entitlement varies sub-nationally and there have been multiple reforms on which to base inferences. We find that modest mandates of 17-18 weeks increase the proportion of mothers on leave but do not increase the time mothers spend at home. Women who previously severed their employment relationship when they gave birth instead take maternity leaves. The physical demands of birth and private arrangements appear to render short mandates redundant. In contrast, we find that expansions of job-protected leaves to lengths up to 70 weeks have large effects on leave-taking, time spent at home, and job continuity. Finally, we study the effects of the increase in time spent at home on measures of infant health, finding no evidence of an effect on the incidence of low birthweight or infant mortality.

1. Introduction

Job-protected maternity leave mandates are arguably the most common public policy directed to the wellbeing of infants and mothers in the developed world. They range from extensive, paid leaves in Europe to the 12 week unpaid leaves available in the United States through the Family and Medical Leave Act of 1993. These mandates are thought to promote infants' and mothers' welfare. The potential benefits for infants are better pre- and post-natal care, a longer period of breastfeeding, more intense parental bonding and lower accident rates in the first years. For the mothers, the potential benefits are better physical and mental health post-birth, and improved long-run labour market outcomes due to increases in employment continuity over the birth event. In fact, maternity leaves are advanced as a key policy response to the "family gap" in earnings between women with and without children (Waldfogel 1998).[1] Based on available evidence, the World Health Organization (2000) concludes that "women need at least 16 weeks of absence from work after delivery" to protect the health of both mother and child.

The basis for these claims is research showing maternity leaves are positively associated with post-birth wages (for example, Shapiro and Mott 1994, Waldfogel 1998a), the employment of females in their childbearing years (for example, Ruhm 1998) and the mental health of mothers (Chatterji and Markowitz 2004). Leaves are also found to be negatively associated with infant mortality (e.g., Ruhm 2000). Other studies find that breastfeeding tends to end in the month the mother returns to work (Lindberg 1996).

These findings, while supportive, must be interpreted with care. In some cases the inference may be biased by heterogeneity between mothers who are eligible for maternity leave or return to work shortly after birth and mothers who don't. This is because the provision of

[1] Phipps, Burton, and Lethbridge (2001) provide evidence of the family gap for Canada.

maternity leave is voluntary, or the mandate does not cover all employers in the labour market. In addition, much of this evidence suffers from a "causality gap": the positive outcomes attributed to maternity leaves rest on first stage relationships between the mandates and mothers' labour supply decisions that are theoretically ambiguous, and also empirically ambiguous in previous research.

There are two fundamental questions. First, how do leave mandates affect the average length of time mothers spend at home with their newborns? This is the necessary first stage to any effect of leaves on infant welfare. Second, how do leaves affect the proportion of mothers who return to employment with the pre-birth employer? This is the necessary first stage to any effect of leaves on females' long run labour market outcomes.

The importance of clear evidence on the impact of maternity leaves is accentuated by growing recognition that the first years are crucial to child development, which in turn is important to adult success. Furthermore, if leave mandates cause working females to stay home longer with their babies, they can serve as an instrument for maternal employment in a child's first year. Therefore, there is also potentially a link from maternity leaves to the important literature on maternal employment and child development.

Our focus is the two first stage relationships between leave mandates and labour supply. We offer answers to the two primary questions to close the gap between the mandates and the benefits they are thought to provide. We next follow the causal trail from leave mandates to infant health. Previous studies have found a link between the mandates and mortality in the first year of life. We revisit this question, exploiting our new evidence of how leaves affect the time mothers are at home with their babies.

The basis of our inference is maternity leave mandates in Canada. This focus offers several advantages. First, job-protected maternity leave mandates are under provincial rather

than federal jurisdiction for most workers. Therefore, mothers' leave eligibility (and how it changes over time) varies geographically, rather than by their choices to match with a particular employer. Second, over our sample period we observe the introduction of modest mandates (17-18 weeks) in several provinces, followed by widespread expansions of leaves to 29-54 weeks. These policy episodes provide perspective on the effects of both limited mandates such as the Family and Medical Leave Act and the longer leaves available in other countries. Finally, using the master files of the Labour Force Survey we can construct a data set that contains monthly observations on the labour supply of mothers in the period surrounding birth. Relative to traditional panel data, any recall bias is minimized and the larger samples allow finer inference of detailed measures of labour market decisions.

Some of these benefits are manifest in figure 1, where we graph the proportions of married females with a child aged less than one who are employed, employed and on leave and employed an at work over the sample period.[2] Overall employment displays the well known positive trend over the period. Its components, however, display very different patterns. The vertical lines mark two reforms that increased the amount of leave available to mothers. Each is associated with an increase in the proportion of mothers on leave and decrease in the proportion at work; most dramatically the reform in 2000. This is prima facie evidence of a relationship between leaves and the time mothers stay at home after birth.

We offer three primary conclusions. First, the introduction of modest mandates increases the proportion of mothers employed and on leave, but has little effect on the length of time they are at home with their infants or on their job continuity with the pre-birth employer. Second, confirming the evidence in figure 1, longer mandate extensions have a significant negative impact on the proportion of mothers employed and at work, and increase job continuity over the

[2] This graph is based on Labour Force Survey data described in Section 5.

birth event. Finally, our results indicate that these mandates have no effect on infant health as measured by infant mortality rates and the incidence of low birth weight.

2. Theory

Klerman and Leibowitz (1997) explore the labour supply effects of maternity leaves in a static framework. With no mandate, employers may voluntarily offer an unpaid (or paid) maternity leave. This is a result of private incentives for both employers and employees to preserve good matches and job-specific human capital. Females choose between this leave and severing the employment relationship to be at home with their child for a longer period. The cost of ending the relationship is the difference between the current wage and the alternative wage. Females make an optimal choice given a reservation wage that declines with each month after giving birth.

A leave mandate that exceeds the employer's voluntary offer will lead some females who previously would have quit their jobs to remain employed and take the mandated leave. Also, some females who had previously taken the shorter leave offered by the employer will now take the additional weeks allowed under the mandate. Therefore the mandate will reduce the number of women quitting their jobs pre-birth to spend time at home, and clearly increase the number of women who are employed and on leave over the birth event. The model has no definitive prediction, however, for the average amount of time women are at home with their child. Some females will take longer leaves under the mandate but others take shorter leaves.

Because our analysis focuses on labour market decisions in specific months around the month of birth (MOB), it is useful to parse this last prediction from a monthly perspective. Women not at home with their child are employed and at work. In months covered both by the employer's voluntary offer and the leave mandate (e.g., the MOB), there should be no change in the proportion employed and at work, as the mandate simply duplicates the already existing

private arrangement. In months the mandate exceeds the voluntary offer the proportion employed and at work should fall. For example, if the voluntary offer is six weeks and the mandate 12 weeks, assuming all leaves start at the point of birth the proportion employed and at work should fall in weeks 7 through 12. Finally, in weeks beyond the mandate the proportion employed and at work may rise if the mandate encourages those who previously quit their jobs to take leave.

Leave mandates may also affect women's wages. There are two points of view. The first is provided by Summers's (1989) analysis of mandated benefits. Mandated benefits act like a tax on the labour of the eligible group, decreasing their wages (received) and employment.[3] Leave mandates increase the cost of employing females in their child bearing years. Therefore, mandates should lower the wages and employment of this group.[4] The second is an informal argument that mandates increase job continuity across the birth event so females with children end up with higher levels of job specific human capital and are able to remain in good matches. In the long run this should increase the wages and economic stature of mothers in the labour market. Waldfogel (1998) argues these effects are important to improving the relative economic stature of mothers.

3. Previous Evidence on Maternity Leaves

Most previous studies of maternity leaves and the labour market are based on U.S. data. While the message of this research is mixed, the variation of leave entitlement across mothers in the U.S. does not typically provide an ideal forum for identification. Historically the provision of

[3] In the special case where employees' valuation of the benefit matches employers' costs, employment is unchanged and the full incidence of the tax falls on wages.
[4] Gruber (1994) uses this framework to investigate the effects of the U.S. Pregnancy Discrimination Act of 1979. He finds that the main (negative) effect of the Act was on the wages of females in their childbearing years.

leave was voluntary.[5] Inference from this period is potentially biased by unobserved differences between mothers who had access to maternity leave and those that didn't. More recently the FMLA (and miscellaneous state-specific initiatives) mandates leave, but only for employers with 50 or more employees. Waldfogel (1999) estimates that more than one-half of private sector workers are uncovered. This means the law affects those who are most likely to have access to leave through agreements with their large employers, and opens the possibility that females sort across firms of different sizes based on their preferences for work around birth.

A number of studies examine how mandates affect labour market outcomes. Using census data, Klerman and Leibowitz (1997) examine the effects of state-specific maternity leave mandates enacted prior to the FMLA on the proportion of new mothers employed, employed on leave and employed at work. The results vary by model specification, but the preferred estimates indicate that these mandates had no statistically significant effect on the labour market outcomes. Baum (2003b) comes to a similar conclusion investigating the effect of the FMLA using the NLSY. Using Current Population Survey data, Waldfogel (1999) reports that the FMLA did increase the proportion of women with a child aged less than one reporting they were employed but on leave, but had no effect on overall employment. Also, Waldfogel (1999) examines the effect of the FMLA on the wages of women with children, reporting no effect. Baum (2003b) reports a similar result for women in their childbearing years using the NLSY.

Other studies examine job continuity across the birth event. Waldfogel (1998a) and Waldfogel, Higuchi and Abe (1999), using NLSY data from the 1980s, show that females who have access to maternity leave at their place of work are more likely to return to their pre-birth employer than those who didn't. The latter study reports that 64.3 percent of new mothers who

[5] A qualification is the Pregnancy Discrimination Act of 1979 by which firms had to treat pregnancy like any other illness in their health plans.

have access to leave returned to the same employer after childbirth, compared to only 42.6 percent of those reporting no access. Baum (2003a) finds that the FMLA increased the proportion of mothers returning to their pre-birth job, but his samples are quite small and the effects vary in statistical significance and are sensitive to model specification.

Klerman and Leibowitz provide perspective on these results. Klerman and Leibowitz (1994) show that in the pre-FMLA era the vast majority of females who work within their child's first year of life remain employed (although on leave) over the birth event. Klerman and Leibowitz (1999) provide direct evidence (from the NLSY) that, pre-FMLA, 60 percent of females working full time before the birth of their child returned to the same employer post-birth. This high percentage leaves limited scope for leave mandates to increase job retention unless they lead to a substantial increase in the incidence of leave.[6]

The sum of this research does not provide conclusive evidence for the first stage relationships between maternity leave and mothers' labour supply. The lack of consensus, and the proliferation of statistically insignificant estimates, may result from the poor experimental design afforded by U.S. policy variation.

Studies based on European data provide more decisive inference, although many do not directly examine the relationship between leave mandates and mothers' labour supply. Waldfogel et al. (1999) find that access to maternity leave increased the job continuity of females in Britain. Winegarden and Bracy (1995) and Ruhm (1998) report that maternity leave increases the employment of females in their childbearing years, exploiting cross country variation in mandates from the 1960s to 1990s. Ruhm also reports that leaves decrease the relative wages of this group at extended durations. Neither study identifies the mechanism whereby the mandates have their effects. Ruhm notes that the employment effects could result from 1) higher

[6] Significantly, Baum (2003a) reports no effect of the FMLA and state specific mandates on the incidence of leave.

proportions of mothers remaining employed over the birth event, 2) greater labour participation of childless females to qualify for leave benefits when they have kids, and/or 3) new mothers returning to work sooner.

For Canada, ten Cate (2003) examines the effect of leave mandates using the public-use files of the Labour Force Survey. She finds the mandates increase the relative employment rate of females with children aged 0-2. The source of this employment effect is not identified. In ten Cate (2000) she examines the effect of these mandates on leave duration and job continuity using data from the Survey of Labour and Income Dynamics. She finds the mandates increase the probability of returning to work within two years of birth. In addition, Phipps (2000) looks at the incentive effects of paid maternity leave through the unemployment insurance system on fertility and hours worked, finding no evidence of changed behaviour.

Finally, there are relatively few studies of the relationship between maternity leaves and health. Winegarden and Bracy (1995) and Ruhm (2000) investigate the effect of European mandates on infant death rates. Both report that these mandates reduced infant mortality. Ruhm's results indicate that the primary effect is after the neonatal period and a result of mandates in excess of 30 weeks. McGovern et al. (1997) examine the effect of maternity leave on maternal health. They report that, starting at 12 weeks, maternity leaves can have positive effects on mental health, vitality and role function. Chatterji and Markowitz (2004) study the effects of maternity leave on maternal mental health in a cross section sample predating the passage of the FMLA. They report that longer leaves reduce depressive symptoms, but do not have an effect on the incidence of clinical depression.[7]

[7] Lero (2003) provides a summary and references to some additional studies of maternal health after childbirth.

4. Maternity Leave Mandates in Canada

Maternity leave defined as a right to return to a pre-birth job after a specified period is established by provincial labour standards legislation (or federal labour standards legislation for employment in the federal public sector or federally regulated industries). British Columbia was the first province to provide maternity leave through the Maternity Protection Act of 1921. This legislation prohibited employers from employing women for 6 weeks following childbirth. New Brunswick was the second province to enact legislation in the 1960s, and the last province to move was Prince Edward Island (P.E.I.) where maternity leaves became law in 1982.

The legislation of different provinces has several common features. First, employees are protected from dismissal due to pregnancy. Second, a maximum period for the leave is always prescribed and the leave is specified as unpaid. In the 1960s and 1970s the laws of several provinces also provided guidance for how the period of leave should be split pre- and post-birth, although current practice is to leave this to the discretion of the mother and employer. Third, the laws specify a minimum period of employment for eligibility. This varies widely: initially 52 weeks of employment was common, although British Columbia effectively had no requirement. The recent trend is to shorter qualification periods. Fourth, most laws specify which terms of employment are preserved during the leave and any responsibility of the employer to maintain benefits. Finally, the laws of some provinces establish rules for extending leaves due to medical complications or pregnancies that continue after term.

The maximum leave provisions of the federal jurisdiction and the provinces in the years 1963 through 2002 are listed in table 1. This same information is graphed in figure 2. In the 1960s, 1970s and early 1980s, the provinces introduced mandates at different points in time,

until the mid 1980s when all provinces mandated 17 or 18 weeks of leave.[8] The next major innovation was in 1990/91 following a change in the treatment of maternity leave under the Unemployment Insurance Act. Eight of ten provinces increased their mandates to between 29 and 52 weeks, six of them moving within a six month period. This change was actually the introduction of a parental leave of between 12 and 34 weeks in addition to the existing maternity leave. In most provinces the additional leave could be taken by either the father or mother, although in practice the vast majority of these leaves are taken by the mother.[9] The final reform is the extension of parental leave at the end of 2000, which brought the total amount of leave available in most provinces to a full 52 weeks. Again this change was induced by a reform of the (now) Employment Insurance Act, and seven of ten provinces changed their mandates simultaneously. Quebec had already extended its mandate in excess of one year in 1997, while British Columbia and Saskatchewan did not change their mandates until early 2001. Note that the early reforms in figure 2 are staggered through time, while the later reforms are clustered in short time-spans. We accommodate these different patterns of variation in mandates in our empirical framework.

While provincial standards provide unpaid maternity leaves, leave benefits are available to some mothers through the Employment Insurance (EI) system. EI in Canada provides protection for "earnings interruptions" from a variety of sources. Starting in 1971 the eligible sources were expanded to include interruptions due to birth. Our analysis of labour supply starts in 1976, so leaves were compensated for some mothers over our sample period.

[8] It is possible that this policy variation *followed* the entry of women into the labour market in different provinces. To test this policy endogeneity hypothesis, we regressed the policy variables on lagged province-year cells of female labour force participation. Using lags of 1 to 10 years, we find that previous labour market participation is a poor predictor of policy values, with coefficients that are both economically and statistically insignificant.

[9] Most provincial laws stipulate that the parental leave must be completed within a set period post-birth (e.g., 52 weeks) and must be taken immediately following any maternity leave.

In the absence of a job-protection mandate, the availability of paid maternity leaves may alter the voluntary leaves offered by employers. This is because the availability of benefits will increase mothers' reservation wages. Within the context of the Klerman/Leibowitz model, this will lower the proportion of females choosing the voluntary leave rather than quitting. In response, employers may lengthen their offered leave to retain attractive employees.

In table 2 we present some features of the EI maternity leave program for the period 1971-2000. In the first column is the maximum duration of benefits available to mothers with sufficient employment in the qualifying period.[10] Benefits are proportional to insurable earnings to a cap set roughly at the average wage. This proportion, the statutory replacement rate, is presented in column 2. Finally, benefits are available after a two week waiting period so the "effective" replacement rate is somewhat lower. In the third column we present the effective replacement rate assuming the individual takes the maximum period of leave. Over our sample period (1976+), females taking the maximum leave and earning less than the average wage could expect to receive 50 to 55 of their pre-birth compensation.[11]

The maternity leave provisions of the EI system do not provide (or require) a right of return to pre-leave employment. Therefore, females planning to leave the labour force with the birth of their child can be eligible to collect EI benefits. Also, the EI eligibility provisions do not demand the qualifying period of employment be with a single employer. It is possible that a woman could qualify for leave under her provincial standards but fail to qualify for EI benefits during the leave, and vice versa.

[10] Initially qualification for benefits required 20 weeks employment in the previous year with earnings greater than 20 percent of maximum weekly insurable earnings in each week, but since 1996 qualification has been based on hours of work. Also, in the 1970s the "magic 10" rule restricted benefits to individuals who could show that 10 of the 20 insurable weeks were from the 20 week period between the 31st and 50th weeks before the expected date of birth. This rule, eliminated in 1984, denied benefits to females who entered the labour force after conception.

[11] Until 2001 benefits were taxed back at a 30% rate for beneficiaries whose annual income exceeded 1.5 times maximum insurable earnings.

5. The Data

Labour Supply

The analysis of mothers' labour supply is based on data from the master files of the Labour Force Survey (LFS). The LFS is a monthly survey designed to provide timely information on Canadians' labour market activity. The data are collected at the individual level, but it is possible to aggregate individuals into families, and associate families with physical dwellings. The survey has a rotating panel design. Individuals belong to a rotation group that is interviewed for six consecutive months. The entrance of rotation groups is staggered so that in any month six groups are interviewed, with one group entering and another group leaving. The survey covers individuals living in the ten provinces, excluding those on Indian Reserves, full-time members of the Armed Forces and inmates of institutions. Information is collected on current labour market status, demographics, job search activities and job characteristics. The microdata are available starting in January 1976.

We create two samples from these data. The first takes advantage of the panel structure of the survey to identify females who experience a birth and to examine their labour market activity in the surrounding period. All adult records include variables reporting the number of own children living at home by the single ages 0 through 24. We identify births through increments in the number of own children less than one year of age between the first and second, second and third, …, fifth and sixth months of a rotation.[12] The month this variable changes is denoted the "month of birth". Assuming a uniform distribution of births within a given month period, the survey information for the MOB is collected when the newborn is two weeks old on

[12] The strategy of identifying births only works for years prior to 1996 when demographic information for other children was collected once in the first month of interview. Starting 1996, this demographic information is updated monthly. Therefore, a change in the number of children less than one could result from a child having his/her first birthday.

average. Depending on when the birth occurs we can observe labour market status up to four months preceding or following the birth. For example, if the birth occurs between the first and second month of the rotation, we observe labour market activity in the month before the MOB and in the four months following. If the birth occurs between the fifth and sixth months, we observe up to four months before the MOB, but no months following.

We next form samples with monthly observations on labour supply for particular months over the birth event. For example, one sample contains monthly observations for the MOB, while another contains the observations for one month after birth. Because births occurring in a given month will come from different rotation groups, women will be captured at different points in the rotation. This means we capture pre- and post-birth labour supply for subsets of the mothers having births in any given calendar month. For example, we observe labour market status three months prior to birth for only for a subset of the women who give birth in March 1985. This is because some of these mothers entered the survey in February (or January), one (two) month(s) prior to birth.

The second sample we create is a time series of cross sections (TSCS). We draw observations from the April and October surveys of each year. This choice of months ensures no rotation group appears twice in the data. Our target group is females with a child aged less than one. The advantages of this sample are that we have much larger sample sizes and that we capture women up to 12 months past the birth month, providing a broader view of any changes in leave incidence and time spend at home.

We focus on "married" (married or cohabitating) adult (aged 20-39) females. An initial analysis revealed that unmarried mothers respond differently to leave mandates, but the number of these women is too small in our data to conduct a full analysis. We also exclude births to married teenage mothers. Our reasoning is that this group often has stronger family ties, and so

may make different decisions than older females whom we expect are more independent. Again sample sizes are too small to allow a separate analysis of teenagers.

Infant Health

Our analysis of infant health is based on vital statistics data on perinatal and infant mortality and the proportion of low weight births, by province. These data are available annually for the period 1955-2001. Our analysis sample is shorter because not all explanatory variables are available for this longer period.

6. Empirical Framework

Labour Supply

We use a variety of empirical strategies to accommodate the different types of mandate variation we observe over the sample period (figure 2). We begin investigating the introduction of 17-18 week mandates in Alberta, Newfoundland, Prince Edward Island and Quebec and the mandate extensions from 12 to 18 weeks in British Columbia and New Brunswick. The analysis uses data from January 1976 through October 1990. The base estimating equation, for either our panel-based or TSCS samples, is

$$y_{ipt} = \alpha \cdot WKSLV_{pt} + X_{ipt}\beta + \varepsilon_{ipt} \qquad (1)$$

where *i* indexes individuals, *p* provinces and *t* months. *WKSLV* is weeks of mandated job-protected maternity leave. For the panel-based sample we code this variable using the statute in effect in the month preceding the MOB. For the TSCS sample we experiment coding *WKSLV* either using the statute effective in the current month or a lagged statute. Since our sample is mothers with children aged less than one, the current statute will only be "correct"[13] for those

[13] If the mandate has not changed recently, the coding will also be correct for mothers with less recent births.

who gave birth very recently. By lagging the statute we ensure correct coding for mothers with less recent births to discover if our results are sensitive on this margin.

The X_{ipt} are control variables: province effects, year effects, calendar month effects, a cubic in age, education (three categories) and a control for any other children aged 1-24.

Our dependent variables are 0/1 indicators that the individual is "employed and at work" or "employed and on leave". For our panel-based data we define these variables for the MOB and for the first, second or third months preceding and following the MOB.[14] In our TSCS data they are defined for the month of observation, and record labour market status over the one year period following the date of birth.

For the panel-based data we also create several indicators of pre/post birth job continuity. First are 0/1 indicators that the individual left a job, or left a job for personal reasons, in the 12 months preceding the MOB. Second is a 0/1 indicator that the individual is employed in the third month following the MOB and has job tenure of three months or longer. This variable is intended to capture the proportion of mothers who are employed post-birth with their pre-birth employer. This interpretation is problematic if individuals who quit their jobs pre-birth, but eventually return to their pre-birth employer, report their tenure post-birth as starting at the time of return to their job. In the appendix we investigate this issue and present evidence that individuals interpret the tenure question to ask when they first started work with their current employer, not when the current employment period with the employer started. More detail on dependent and explanatory variables is also provided in the appendix.

Equation (1) is estimated by ordinary least-squares (OLS). Standard errors are corrected for heteroskedasticity and, because *WKSLV* only varies by province and time, for random effects

[14] While we can also construct a measure of employment for the fourth month following birth, many of the province/year cells for this variable are empty over the period that the mandates were introduced (1976-1982).

at the province/year level.[15] We also estimate variants of (1) which add province-specific linear trends, or add married males aged 20-39 or married childless females aged 20-39 to the sample. In the latter case, we add a full set of interactions between 0/1 indicators that the individual is female or a mom and all the other explanatory variables.

We also investigate the introduction of parental leave in 1990-92, and the extension of parental leave in 2000-2001. In table 3 we present the dates of these reforms by province. Because in both cases many provinces moved almost simultaneously, a conventional cross-section time-series identification strategy is not effective. We therefore use a framework that compares variables of interest immediately before and after the reforms were implemented, using a variety of strategies to control for secular trends.

The estimating equation is

$$y_{ipt} = \alpha \cdot POST_{pt} + X_{ipt}\beta + \varepsilon_{ipt} \qquad (2)$$

where *POST* is a 0/1 indicator that the province's leave mandate has been extended. It captures the average effect of these mandate extensions. The dependent variables are the same as in (1) with the addition of employment status captured in the fourth month following the MOB. The additional explanatory variables are the same as in (1) with the exception of the year effects. Because there is little temporal variation in *POST* across provinces, year effects will absorb all the variation in the dependent variable due to the mandate reforms. We approach this problem in two ways. In the first we exclude any controls for time, but limit the data to the period immediately surrounding the reforms. For the 1990 introduction of parental leave we use the samples January 1990 through December 1991 and July 1989 through December 1992. For the 2000 extension of parental leave we use the samples January 2000 through December 2001 and

[15] The correction for random effects is conservative as the *WKSLV* variable actually varies at the province month level.

July 1999 through December 2002. In the second approach we use a longer time period and add a polynomial in time. This amounts to a regression discontinuity design, where the discontinuity in leave entitlement occurs in the months reported in table 3. In either approach we also estimate (2) adding married males or married childless females as additional controls for secular trends.

Infant Health

Our analysis of infant health is based on annual data for the period 1961-2001. The estimating equation is

$$y_{pt} = \alpha \cdot WKSLV_{pt} + X_{pt}\beta + \varepsilon_{pt} \qquad (3)$$

where *p* indexes provinces and *t* indexes years. The explanatory variables follow Ruhm (2001) and include province and year effects, the employment population ratio of females aged 15+, the fertility rate defined as the ratio of annual births to the female population aged 15 to 44, real provincial GDP per capita and total provincial health expenditures as a percent of GDP. The dependent variables are the perinatal, neonatal, post-neonatal and infant mortality rates and the proportion of births that are low birth weight (<2500 grams). Again a more detailed description of these variables is available in the appendix.

Equation (3) is estimated by OLS. Standard errors are corrected for heteroskedasticity and random effects at the province/year level. We also estimate variants of (3) that add province-specific linear or quadratic trends.

7. The Results~Labour Supply

In the labour supply analysis we establish the sign and magnitude of the effect of leave mandates on mothers' time spent at home and rates of return to pre-birth employers. We investigate each of the three policy episodes using the empirical strategies as described above.

In table 4 we present descriptive statistics of the labour supply variables from the panel-based sample for the period 1976-2002. The proportion of mothers employed and on leave peaks in the MOB at 42 percent, and then declines to 27 percent by the fourth month following. The proportion employed and at work is very low in the MOB and two months following, at ten percent or less. Note that the complement of being employed and at work variable is being at home, either not in the labour force, unemployed or employed on leave.

In the second panel we present some measures of job continuity. While in the regression analysis we use unconditional measures, here we present the proportion of mothers working three or four months after the MOB with tenure greater than three or four months conditional on employment. Viewed this way, Klerman and Leibowitz's (1994, 1999) point is clear – the vast majority of mothers working at this time have returned to their pre-birth employer.

The Introduction of Mandated Leave: the 1976-1990 Sample

While equation (1) compares provinces changing their mandates to all other provinces, in figure 3 we present an Ontario/Quebec comparison to motivate our identification strategy. These adjacent provinces are of similar size and economic structure. We focus on the month before birth, because in the months other than the MOB the mandate is less likely to duplicate private arrangements. The statistic reported is the (annual) proportion of mothers on leave. Ontario has a mandate of 17 weeks throughout the sample period while Quebec introduces a mandate of 18 weeks in 1978 (indicated by the vertical line). Prior to the Quebec reform the proportions in the two provinces are very similar. Starting the year of the reform, the proportion in Quebec begins a steeper trajectory and a substantial Quebec/Ontario gap emerges.

In the first panel of table 5 we present estimates of the effect of *WKSLV* on the proportion employed and on leave in the months surrounding birth. In the first column the results indicate some statistically significant impacts on the proportion in the three months preceding the MOB

and the third month following. The smaller and statistically insignificant estimates for the MOB and two months following are plausible if the mandates effectively duplicated existing private arrangements. Puzzling, however, is the significant impact three and two months before the MOB. By definition of the MOB,[16] an effect in these months indicates females starting maternity leave 11 weeks before the date of delivery.[17] Data from the 1985 Maternity Leave Survey indicates that this is a very rare event.[18] Maternity leaves in this survey started six weeks before birth on average (four weeks at the median). Eleven weeks pre-birth was the 85th percentile.

One possibility is that *WKSLV* is simply picking up provincial trends in the dependent variable in the absence of any other control. There are strong secular trends in mothers' labour supply over the period (figure 1), and some part of it likely has a provincial characteristic. In the second column we add linear provincial trends. The estimates in the second and third months preceding the MOB are now small and statistically insignificant. We now also see statistically significant effects in all the other months surrounding birth.

These estimates imply economically significant increases in the proportion on leave. To calibrate, we use the averages of the dependent variables calculated for the provinces that introduced mandates, over the years there was no mandate in place.[19] In the MOB, an 18 week mandate implies over a 5.5 percentage point increase in this proportion off a pre-mandate base of 18 percent. In the third month following birth the effect is nine percentage points off a base of six percent. These magnitudes are large.

[16] Recall that in the MOB newborns are two weeks old on average.
[17] Three months is 3*4.333=13 weeks. Since newborns average two weeks old in our MOB, an effect in the third month preceding the MOB should be 13-2=11 weeks before the day of birth (on average).
[18] The Maternity Leave Survey was an addendum to the February 1985 LFS, investigating the circumstances of maternity leaves among females whose last absence from work or last two absences (of two weeks or more) from work included one due to pregnancy.
[19] We use 1976-June 1978 data for Newfoundland, PEI and Quebec, and 1976 data for Alberta.

In column 3 we address the problem of workers in the federal sector who have a different leave mandate (see table 1). The federal sector covers federal public administration and industries such as banking, and some parts of the transportation and communication sectors. Workers in this sector represent four to five percent of employment at the aggregate level. Industrial codes in the LFS (4-digit NAICS) do not allow us to uniquely identify these workers. Therefore, we define a new sample deleting all individuals who, as of the MOB, had current or previous (last 12 months) employment in a 4-digit industry that contained federal workers. This strategy should exclude all federal workers, as well as some workers who were covered by provincial mandates. The resulting estimates are marginally larger than in column 2, although the differences are not statistically significant.

Given that many women were not employed pre-birth or were self-employed, our sample includes women who were not eligible for leave. In column 4 we try to isolate the individuals who are eligible to take mandated maternity leave. The LFS does not have enough work history information to implement the province-specific employment requirements for leave. Instead, we create a new sample that excludes anyone who, as of the MOB, had not worked in the previous year in paid employment. This filter should exclude many ineligible mothers.

The estimates for this sample are larger. This is expected if the sample exclusions isolate those affected by the mandates. In the months following the MOB an 18 week mandate raises the proportion of these mothers on leave between 10 and 14 percentage points. Again, these results are economically significant.

In the last two columns we add, sequentially, married males or childless females to the sample as an additional control for province-specific trends. If the mandates had no independent effect on these control groups we would expect the results to be similar to those in column 2.

This is exactly what we find when adding males (column 5), and to a lesser extent when adding childless females. Certainly these experiments do not overturn our original inference.

The second panel of table 5 contains corresponding results for the proportion employed and at work. Here, the full sample of mothers (that is, not conditional on work in the past 12 months) is of particular interest, because we wish to discover whether the mandates increase time spent at home. The specifications and samples vary across columns as in the upper panel. The estimates provide little evidence that the mandates decreased work in the period surrounding birth. Most are uniformly small and statistically insignificant. The exception is when we restrict the sample to mothers with recent (paid) employment. Here some of the estimates approach economic, although not statistical, significance.

In table 6 we present estimates of the effect of the mandates on job continuity. The dependent variable in the first row is the proportion of females employed in the third month after birth with current tenure greater than three months. We obtain positive estimates for *WKSLV* in all samples from the specification with linear trends, although few reach statistical significance. In the second column, an 18 week mandate is estimated (imprecisely) to raise the proportion by about 6 percentage points off a pre-mandate base of 21 percent.

There is a consistently statistically significant effect on the proportion leaving a job within the 12 months preceding birth, and it is driven by exits for personal or family reasons. An 18 week mandate lowers the proportion by roughly 5.5 percentage points (e.g., column 2) off a pre-mandate base of 35 percent. The estimated change is ten percentage points for the sample who worked in the 12 months preceding birth, which is perhaps the more appropriate sample since those who were not working have no job to leave. These results suggest that the mandates led to a large decrease in the proportion of mothers who severed their employment relationship.

To cross-check these inferences, in table 7 we present estimates from the TSCS sample. Recall the sample is married females with a child aged less than one. The results are consistent with the estimates from the panel data. First, there is consistent evidence of an effect on the proportion of mothers employed and on leave: an 18 week mandate raises the proportion 2.5 percentage points (column 2). Second, estimates for the proportion employed and at work are mostly small and uniformly statistically insignificant. We present results coding *WKSLV*, alternatively, to current month and three month lagged mandates. Coding with a three month lag matches the leave provisions to mothers who have three month olds. Our panel inference indicates that this is the period with the largest behavioural response. In any case, the estimates are not overly sensitive on this margin.

Overall, these results provide strong and robust evidence that the introduction and initial expansions of leave mandates in the 1970s and 1980s led to increased leave taking. However, we find no consistent evidence that women switched to leaves from being employed and at work. Instead, the evidence suggests that women staying at home with their child switched from leaving their jobs to taking leave. Importantly, this finding provides no basis to expect any consequent change in the infants' or the mothers' health, since time at home does not change. We do find a sharp decrease in job separations from the mandates, but no strong evidence of an increase in retention of the pre-birth job. This finding is internally consistent if, in the absence of leave, women were severing then restarting an employment spell with the same employer around the birth event. If so, the effect of the mandate is to re-label the break from the job as a "leave" rather than a separation.

An Extension of Mandated Leave: the 1990 Introduction of Parental Leave

We next investigate the introduction of parental leave starting in 1990 (table 3). These reforms increased the amount of job-protected leave available to new mothers from 17-18 weeks

to 29-52 weeks. To motivate our results, in figures 4 and 5 we present semi-annual estimates of the proportion of mothers employed and on leave in the provinces of British Columbia, New Brunswick, Prince Edward Island and Quebec between 1985 and 1996. These four provinces introduced parental leave over the first six months of 1991, which is indicated by a vertical line in each graph. Figure 4 shows the proportion employed and on leave in the MOB. There is a definite upward trend in this proportion over the period and at best subtle evidence of a break in the first half of 1991. In contrast, the graph for the fourth month following the MOB (figure 5) displays an obvious upward shift starting in 1991.

In table 8 we report estimates of *POST* for the proportions employed and on leave and at work. Given the results in table 5, we focus on months in which mandated leave was most likely unavailable before the extension: two months before the MOB and three and four months following. The results in the first column are from data for 1990 and 1991. There is strong evidence of an increase in leave and offsetting decrease in work at four months after birth, when we expect to see the effect since the earlier 17-18 week leaves likely ended before the fourth month after birth. The estimates indicate offsetting changes of more than ten percentage points. The offsetting effect on being employed and at work is in sharp contrast to the first set of results in table 5 when we found no effect on time at home. In the other months the results are mixed. There is evidence of smaller decreases in work, and small and statistically insignificant changes in the proportion employed and on leave.

In the next column we expand the sample to July 1989 through December 1992. This picks up the reform in Newfoundland and provides longer periods to establish the pre- and post-reform levels. Now there are significant increases in leave in both the third and fourth months following birth and offsetting decreases in work. There is also a very modest decrease in work in the MOB.

To calibrate the results we use the average values of the dependent variables in all provinces in the period just before the reform: July 1989-October 1990. The 15.6 percentage point increase in leave in the fourth month following birth (column 2) is off a pre-reform base of 21 percent. The 12.6 percentage point decrease in the proportion at work is off a base of 30 percent. These are very large effects, suggesting a large increase in the proportion of women who are at home with their children.

In the third column we check for spurious inference. Using data from July 1986 through 1989 we code the changes in the provincial mandates subtracting three years from each date of enactment. Since there were no changes on these dates, the results should provide evidence of the sensitivity of the identification strategy to secular trends. The estimates are almost uniformly statistically insignificant, the exception being some effect in the second month before the MOB.

The remaining four columns present the results when we delete the federal sector, focus on those who had a paid job within 12 months of the MOB, and when we add males or childless females as an additional control. The estimates confirm the inferences of column 2. Some notable differences are a much larger effect in the third month following the MOB when we focus on those with recent employment, and the statistical insignificance of some estimates for employed and at work when we add males or childless females to the sample. The latter result is likely a result of the 1990/92 recession, an issue we take up below.

In table 9 we examine the effect of the mandate extension on our measures of job continuity. The results indicate significant increases in the proportion of mothers employed with their pre-birth employer in the fourth month following birth. The nine percentage point increase in column 2 can be compared to a pre-reform base of 41 percent. There is also a statistically significant, although modest, decrease in the proportion leaving a job in the 12 months preceding the MOB for family reasons.

In table 10 we further investigate the estimated increase in job continuity. As explained in Section 2, its expected source is females, who previously would have quit their job and taken long periods off with their new baby, now taking the mandated leave and remaining with their employers. If this were the only effect at work, we would expect the change in overall employment at four months to equal the change in employment at four months with tenure greater than 4 months. The estimates in table 8, however, show that the change in overall employment is relatively small because the increase in the proportion on leave is almost offset by the change in the proportion work.

To be concrete, consider the estimated increase in job continuity at four months after birth from column 2 of table 9: 9.11 percentage points. This is repeated in the first row of table 10. The increase in overall employment from this specification is the sum (table 8) of a 15.6 point increase in the proportion employed and on leave and a 12.6 point decrease in the proportion employed and at work. The net effect is almost +3 percentage points. We provide a direct estimate of this effect in the second row of table 10. Therefore, the change in overall employment is not consistent with the increase in job continuity being solely more females taking the mandated leave. The missing detail is provided in the third through sixth rows. The increase in job continuity is associated with a substantial decrease in employment with tenure less than five months. The estimates indicate that a strong plurality of the shift in the distribution of tenure is a reduction in employment with tenure equal to two months.

An explanation of the decrease in employment at tenures less than five months is females who previously quit their job and returned to work shortly after the MOB, taking the longer leave provided by mandate. Therefore, the introduction of parental leave caused a shift of employment from tenures under five months to longer tenures. This might occur if these mothers preferred a gradual return to the labour market after birth to immediate full time employment, but could not

negotiate this arrangement with their employer. The introduction of parental leave allows these females to put off full time employment to a more acceptable time. Also consistent with this story, almost two-thirds of the 6.1 point decline in employment at tenures less than five months is in part time work. The estimated change in part time work at these tenures is -0.03921 with a standard error of 0.0128.

In table 11 we again cross-validate our inference by estimating the effect of the mandate extensions using the TSCS sample. Here we code the mandate using, alternatively, the current month and a six month lag. There is consistent evidence of a four to six percentage point increase in the proportion employed and on leave. There is also fairly consistent evidence of a corresponding decline in the proportion employed and at work, except when we add males or childless females as a control group. The source of this discrepancy, also seen in the estimates for MOB+3 in table 8, is seen in figure 6. The 1990-92 recession had a differential impact on the overall employment of these different groups. Males and childless females experienced declines in both work and in overall employment. While the mothers experienced a decline in work (figure 1), their employment held steady. Figure 6 shows the different paths of employment for these groups in this period. Therefore, while the reductions in work for males or childless females net out the reduction in work for mothers in these estimates, the former is likely a recession effect while the latter is likely a mandate effect.

As a further check on inference we have re-estimated our models for both the panel-based and TSCS samples using still another identification strategy. We add a polynomial in time to equation (2) and expand the sample to 1988-1994. This is like a regression discontinuity design in which all effects of time, except the mandate reforms, are assumed to be smooth functions. For the panel-based data these estimated effects are very similar for the fourth month following the MOB, but generally smaller for other months. For the TSCS data the results indicate a two to

three percentage point increase in the proportion on leave, and a modestly smaller decrease in the proportion at work. In either case the results are very robust to specification of the time effects as linear, a quadratic or a cubic. These results are available from the authors on request.

An Extension of Mandated Leave: the 2000 Extension of Parental Leave

The final section of our labour supply analysis examines the extension of parental leave starting in 2000. As indicated in table 3, this reform raised the amount of leave available to one year in all provinces except Quebec, where leave had been extended to 70 weeks in 1997. The motivation for our inference is clear in figure 1. In 2001 we see a dramatic increase in the proportion of mothers with a child aged less who are employed and on leave, and an offsetting decrease in the proportion employed and at work.

The extension of leave from roughly six to twelve months in most provinces is outside the span of observation of our panel-based data, which only extend to four months after the MOB. We therefore focus on our TSCS sample. Estimates of the proportion of mothers with a child aged less than one on leave or at work are presented in table 12. The progression of samples and specifications is the same as in table 11. In the first panel there is very strong evidence of roughly a 9 percentage point increase in the proportion on leave and a 10-11 point decrease in the proportion at work. The results are uniformly larger in the second panel when we lag the coding of the mandate by 9 months. The estimated increase in leave is 12-13 percentage points and the estimated decrease in work is 12-14 points. The difference across panels is expected, as figure 1 shows that the effect of the mandate reform grows over time. Given that we sample mothers who had a birth in the previous 12 months, it is not until 2002 data that all of this group would be eligible to take the longer leave.

To calibrate these results we use the average of the dependent variables between July 1999 and November 2000 in all provinces save Quebec.[20] The 12-13 percentage point increase in leave is off a pre-reform base of 31 percent. The 12-14 percentage point decrease in work is off the same base. These are large changes. As figure 1 makes clear, by 2002 the proportion at work was below its level in 1976. Over same period overall employment more than doubled.

As a check on our inference we again have re-estimated the model adding polynomials in time and expanding the sample (to July 1997 through 2002). The results (not shown) support the inference from table 12, although the estimated effect of the mandate extension is somewhat smaller when we add a cubic in time.[21]

The estimates for both the 1990 and the 2000 mandate expansions reveal large effects on leave-taking, time at home (the complement of employed at work) and job continuity. These results contrast sharply with the estimates for mandates introductions in the 1970s and 1980s, for which we found no evidence of increased time at home or job continuity. It may be that private arrangements and the physical demands of birth render modest mandates redundant. The longer mandates exceed the private arrangements and thus have a measurable, large effect on behaviour.

These conclusions have implications for researchers investigating the effect of leaves on health, child development, or long-run female labour market outcomes. Short leaves, such as those mandated by the FMLA in the U.S., may have no effect on time spent at home or job continuity. If there is no effect on labour supply, there is no basis for the second-stage benefits.

8. The Results~Infant Health

[20] We exclude Quebec because was extended here to 70 weeks in March 1997.
[21] These results are available on request. The sensitivity of the results to the specification of time effects might be expected. Figure 1 shows that the mandate reform does not cause a sharp discontinuity because there is a lag until all of the sampled population is subject to the new provisions.

Maternity leave can have a positive impact on infant welfare if it leads mothers to spend more time at home with their newborns. While the benefits may take a variety of forms—increased periods of breastfeeding, better cognitive and emotional development—previous studies have focused on infant mortality rates and the incidence of low birth weight. The argument is that mothers provide better supervision of newborns, decreasing accidents and better monitoring of any deterioration in infant health. We focus on these same measures of infant health to provide a link to this literature.

In table 13 we present estimates of equation (3) for the period 1961-2001. This period spans the mandate reforms examined in our analysis of labour supply, and adds the period 1961-1975 when five provinces introduced leave mandates. Therefore, in these regressions we take advantage of all the cross-province variation in the timing of mandate reforms.

In the first column there is fairly consistent evidence that *WKSLV* has an effect on the measures of mortality. For each measure the estimate is negative—as expected—and in most cases statistically significant. Somewhat puzzling, however, is the only insignificant estimate is for post-neonatal mortality, the period in which Ruhm (2000) reports leave mandates have the largest effect. The estimate for low birth weight is also negative but also not significant.

In the next column we add province-specific linear trends to the specification. Infant mortality rates have been declining over the period in all provinces for a variety of reasons, and some of these may be province-specific. The effect on inference is dramatic. The estimates of *WKSLV* are uniformly much smaller, and now positive and statistically insignificant. In the third column we add quadratic provincial trends that may be more appropriate given the length of the sample period. The conclusions offered in the previous column remain. We have also experimented with a quadratic specification of the *WKSLV* variable to allow the effect on infant

health to vary across different durations.[22] The original inference remains: there is no evidence that leave mandates affect these measures of infant health.

The 1961-2001 sample period pools together different types of mandate reforms. This may be inappropriate because our labour supply analysis indicates that different types of mandate extensions had different effects. In particular, we find little evidence that the introduction of mandates had an effect on the proportion of mothers employed and at work.

We therefore refine our analysis focusing on the introduction of parental leave starting in 1990.[23] A priori, we expect this extension of leave from 17-18 to 29-52 weeks to have its primary effect in post-neonatal period. Figures 5 and 6 provide an overview. We again compare Quebec and Ontario. Each of these provinces introduced parental leave near the end of 1990, although the Quebec mandate was far more generous. Therefore, we would expect any effect of the differential reform to turn up in the 1991+ data. There is no obvious trend break for infant mortality (figure 5) in 1991 for either province. One might argue that the gap that opens up between the Quebec and Ontario rates in the 1990s is a result of Quebec's more generous mandate, but this gap emerges in 1990 before the reforms take place. The evidence for post-neonatal mortality (figure 6) is even less supportive. There is no trend break in 1991 and the rates in the two provinces remain very similar.

Our ability to formally test for effects of this reform is limited because our mortality data are annual, which prevents us from exploiting inter year differences in the timing of the reform across provinces. Also, there are few obvious additional jurisdictional controls for secular trends. Our strategy is to estimate a variant of (3) replacing the year effects with smooth

[22] Our experiments focus on neonatal and post-neonatal mortality, periods that correspond to the mandates under study. While the parameters of the quadratic in *WKSLV* are sometimes jointly significant, the inference is not robust to the changes in the specification of the province-specific trends. Also, the estimates often imply effects of implausible magnitude.

[23] Unfortunately vital statistics data are not yet available to allow an analysis of the 2000 reforms.

functions of time, and replacing *WKSLV* with the variable *POST* from equation (2). We code *POST* for each province on an annual basis. It captures any change in the within province time series variation of infant health with the introduction of parental leave, in addition to what is captured by the time effects. We use data for 1988 through 1994 for this analysis.

The results are reported in table 14 for different specifications of the time trend. While the estimates for each mortality rate are uniformly negative and reasonably robust to specification, they are always statistically insignificant. Also, taken at face value the estimates indicate that the largest proportional effects are for perinatal and neonatal mortality. It is not clear why the introduction of the extended parental leave would reduce mortality in the perinatal period, since the perinatal period is covered by the basic maternity leave. We have experimented with other empirical specifications and consistently find that the introduction of parental leave has a negative, statistically insignificant, association with mortality in the perinatal through post-neonatal periods.[24]

9. Conclusions

We investigate the relationship between mandated job-protected maternity leave and the labour force behaviour of mothers with newborns. Most significantly, our results provide convincing evidence of the first stage labour market relationships that must lie behind any effect of leaves on infants' or mothers' welfare. The analysis reveals that mandates can increase the time mothers spend at home with their infants and increase job continuity over the birth event. Importantly, these relationships are not found for the introduction of modest mandates (17-18

[24] We investigated two alternative specifications. In the first we simply interact *POST* with the time trend, and test the significance of the interactions. In the second we pool data for the separate infant mortality rates with data on the rate at age one, as a control for jurisdiction-specific trends. We add (all) interactions of the time trend with a 0/1 indicator for the infant rate as well as with *POST*. In a linear specification the time*Infant**POST* interaction is negative and significant for all infant mortality rates. In the quadratic specification this interaction it is only (jointly) significant for the post neonatal rate, but indicates a positive trend break after parental leave was introduced.

weeks) that most likely duplicate existing private arrangements. Instead it is in the extension of leaves to longer durations that the connection to labour supply is found.

These findings have important implications for previous research. First, they may explain the lack of consistent evidence of an impact of short mandates, such as the FMLA, on labour supply. Second, they provide some validation for the benefits ascribed to the longer leaves available in European countries.

There are also implications for future research. There is growing recognition that the first years are crucial to child development (e.g., Carnegie Task Force on Meeting the Needs of Young Children 1994), and in turn that childhood development is a strong predictor of success in adulthood (e.g., Heckman 1999). Maternity leave is a prominent government policy directed to the welfare of infants. It is also important to make the connection between studies of maternity leave mandates and the much larger literature on maternal employment and child development. The dramatic increase in female employment of the last 100 years has precipitated equally dramatic changes in the way children are brought up, with a substantial and increasing proportion of mothers working. Researchers have investigated many effects of this social transformation on children's cognitive, emotional and physical development.[25] A central challenge in this literature is finding an instrument for maternal employment.[26] Our findings for leave extensions suggest that the maternity leave mandates may serve as an instrument for maternal employment in the first year, which will assist in answering these important questions.

An outstanding issue is how the behavioural effect is influenced by income replacement over the maternity leave. The compensation of leaves in Canada is modest compared to European standards, but clearly exceeds the norm in the U.S. If income replacement is

[25] See the review contained in Ruhm (2000).
[26] Recent advances involve richer controls for observable differences between working and non working mothers (Baum 2002, Ruhm 2001), but typically there is no account of unobserved differences.

important, our estimates provide a lower bound for the effects of leaves in Europe and likely overestimate the effect of a simple extension of the FMLA. That said, in the latter case the estimates possess additional value because income replacement for maternity leave is gaining a constituency in the U.S. Importantly, the Department of Labor's "Baby-UI" rule, in effect from 2000 to 2003, allowed states to compensate maternity leaves through their Unemployment Insurance funds. While no states took advantage of this policy, the spirit of this compensation scheme is very similar to the Canadian system. One plan currently in effect is California's Paid Family Leave Insurance Program, with six weeks of income replacement for family leaves and a replacement ratio similar to Canada's.[27] Therefore, our estimates of mandated job-protected leave expansions in Canada may have relevance for any reforms in California, or adoption of similar plans in other states.

[27] Program details are available at http://www.edd.ca.gov/direp/pflfaq1.asp#RELATION%20OF%20PAID%20FAMILY%20LEAVE (accessed June 30, 2004).

Appendix

On the Suitability of the Tenure Variable to Capture Job Continuity Pre/Post Birth

The tenure question in the LFS asks "When did ... start working [at name of employer]?". The responses to this question indicate that individuals interpret this question to mean first start working at this employer rather than the start of the current job or employment period.

Table A-1: Distribution of Tenure among Mothers Employed in the Third and Fourth Month Following the MOB

Mothers Employed in the Third Month After the MOB				
Tenure	1 month	2 months	3 months	4+ months
All	4.2	4.5	2.2	89.1
Those Not Employed in MOB	19.4	19.4	10.2	51.0
Those Employed in MOB	1.1	1.4	0.5	97.0

Mothers Employed in the Fourth Month After the MOB					
Tenure	1 month	2 months	3 months	4 months	5+ months
All	4.6	5.4	3.9	2.4	83.7
Those Not Employed in MOB	15.1	20.6	13.8	9.7	40.8
Those Employed in MOB	2.1	1.8	1.5	0.6	94.0

Notes: Source is the LFS. MOB=month of birth. The reported statistics are the proportion of mothers working in the indicated month following the MOB who report the indicated current job tenure.

Some evidence is provided in table A-1. We report the distribution of current job tenure for mothers employed in the indicated month following the MOB for our 1976-1990 panel-based sample. For example, 4.2 percent of mothers working in the third month following the MOB report current job tenure of one month.

There are at least two striking results here. First, the overwhelming majority of mothers working in the third or fourth month following birth report a job tenure indicating they are with their pre-birth employer. Second, a substantial fraction of those who were not employed in their MOB also report a job tenure that indicates they are at their pre-birth employer: 51 percent of those working in the third month following birth and 41 percent of those working in the fourth. This suggests that these individuals interpret the tenure question to ask when they first started work with their current employer, not when the current employment period with the employer started.

Variable Definitions and Sources

Labour Supply Analysis

WKSLV:	Weeks of mandated job-protected maternity/parental leave. Source is provincial statues and Labour Canada (Various Issues).
Education:	0/1 indicator that the individual has completed some post-secondary education but not a degree; 0/1 indicator that the individual has completed a university degree. Source is LFS.
Other Child:	0/1 indicator of the presence of another child aged one of greater living at home. Source is LFS.

Infant Health Analysis

Perinatal Mortality Rate:	The number of perinatal deaths (stillbirths (gestational age of 28 or more weeks) and early neonatal deaths (deaths in the first week of life)) per 1,000 total births (includes stillbirths). Unknown gestational age is excluded in both numerator and denominator. Source is Statistics Canada (1993, 1999) and CANSIM.
Neonatal Mortality Rate:	The number of neonatal deaths (under 28 days of age) per

1,000 live births. Source is Statistics Canada (1993, 1999) and CANSIM.

Post-Neonatal Mortality Rate: The number of post-neonatal deaths (between 28 days and one year of age) per 1,000 live births. Source is Statistics Canada (1993, 1999) and CANSIM.

Infant Mortality Rate: Number of infants who die in the first year of life per 1,000 live births. Source is Statistics Canada (1993, 1999) and CANSIM.

Low Birth weight Rate: Number of low birth weight (<2500 grams) births per 1000 live births. Source is Statistics Canada (1993, 1999) and CANSIM.

Employment/Population Ratio: The employment population ratio for females aged 15 and older. Source is CANSIM.

Real GDP: Provincial GDP deflated by the national Consumer Price Index (CPI). Each variable combines information from two series that span a different parts of the full time period (1961-1996). The series were spliced by projecting missing values based on growth rates observed in the other series. Source is CANSIM.

Population: Total provincial population. Source CANSIM.

Fertility Rate: The ratio of the number of live births to the population of females aged 15-44.

Total Health Spending: Total (public and private) spending on health. Data is available for 1960, 1965, and 1970-2002. Missing values in the 1960s were imputed by linear interpolation. Source is the Canadian Institute of Health Information.

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Table 1: Weeks of Mandated Job-protected Maternity/Parental Leave by Province

	Fed.	AB	BC	MB	NB	NF	NS	ON	PEI	QU	SA
1963	0	0	12	0	0	0	0	0	0	0	0
1964	0	0	12	0	12	0	0	0	0	0	0
1965	0	0	12	0	12	0	0	0	0	0	0
1966	0	0	12	0	12	0	0	0	0	0	0
1967	0	0	12	0	12	0	0	0	0	0	0
1968	0	0	12	0	12	0	0	0	0	0	0
1969	0	0	12	0	12	0	0	0	0	0	0
1970	0	0	12	0	12	0	0	12	0	0	0
1971	15	0	12	0	12	0	0	12	0	0	0
1972	15	0	12	0	12	0	0	12	0	0	0
1973	15	0	12	0	12	0	17	12	0	0	18
1974	15	0	12	17	12	0	17	12	0	0	18
1975	15	0	12	17	12	0	17	17	0	0	18
1976	15	0	12	17	17	0	17	17	0	0	18
1977	15	18	12	17	17	0	17	17	0	0	18
1978	15	18	12	17	17	17	17	17	0	18	18
1979	15	18	12	17	17	17	17	17	0	18	18
1980	15	18	12	17	17	17	17	17	0	18	18
1981	15	18	18	17	17	17	17	17	0	18	18
1982	15	18	18	17	17	17	17	17	17	18	18
1983	15	18	18	17	17	17	17	17	17	18	18
1984	15	18	18	17	17	17	17	17	17	18	18
1985	41	18	18	17	17	17	17	17	17	18	18
1986	41	18	18	17	17	17	17	17	17	18	18
1987	41	18	18	17	17	17	17	17	17	18	18
1988	41	18	18	17	17	17	17	17	17	18	18
1989	41	18	18	17	17	17	17	17	17	18	18
1990	41	18	18	17	17	17	17	17	17	18	18
1991	41	18	30	34	29	17	34	35	34	52	18
1992	41	18	30	34	29	29	34	35	34	52	18
1993	41	18	30	34	29	29	34	35	34	52	18
1994	41	18	30	34	29	29	34	35	34	52	18
1995	41	18	30	34	29	29	34	35	34	52	18
1996	41	18	30	34	29	29	34	35	34	52	30
1997	41	18	30	34	29	29	34	35	34	70	30
1998	41	18	30	34	29	29	34	35	34	70	30
1999	41	18	30	34	29	29	34	35	34	70	30
2000	54	18	52	54	54	52	52	52	52	70	30
2001	54	52	52	54	54	52	52	52	52	70	52
2002	54	52	52	54	54	52	52	52	52	70	52

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

Table 2: Some Parameters of the UI/EI System

	Maximum Duration of Maternity/Parental Leave Benefits	Maximum Weekly Insurable Earnings	Statutory Replacement Rate	Effective Replacement Rate
1971	15	150	0.75	0.65
1972	15	150	0.75	0.65
1973	15	160	0.75	0.65
1974	15	170	0.75	0.65
1975	15	185	0.75	0.65
1976	15	200	0.67	0.58
1977	15	220	0.67	0.58
1978	15	240	0.67	0.58
1979	15	265	0.60	0.52
1980	15	290	0.60	0.52
1981	15	315	0.60	0.52
1982	15	350	0.60	0.52
1983	15	385	0.60	0.52
1984	15	425	0.60	0.52
1985	15	460	0.60	0.52
1986	15	495	0.60	0.52
1987	15	530	0.60	0.52
1988	15	565	0.60	0.52
1989	15	605	0.60	0.52
1990	15	640	0.60	0.52
1991	25	680	0.60	0.55
1992	25	710	0.60	0.55
1993	25	745	0.57	0.52
1994	25	780	0.55	0.51
1995	25	815	0.55	0.51
1996	25	750	0.55	0.51
1997	25	750	0.55	0.51
1998	25	750	0.55	0.51
1999	25	750	0.55	0.51
2000	25	750	0.55	0.51

Notes: Source is Statistics Canada (Various Issues).

Table 3: Dates of the Introduction of Mandated Parental Leave by Province

	Introduction of Parental Leave Starting in 1990			Extension of Parental Leave Starting in 2000		
	Weeks of Leave in 1989	Date of Introduction	Total Weeks of Mandated Leave Post Reform	Weeks of Leave in 2000	Date of Extension	Total Weeks of Mandated Leave Post Reform
Alberta	18	N.A.	18	18	February 7, 2001.	52
British Columbia	18	March 22, 1991	30	30	December 31, 2000.	52
Manitoba	17	December 14, 1990.	34	34	December 31, 2000.	54
New Brunswick	17	May 9, 1991	29	29	December 31, 2000.	54
Newfoundland	17	June 11, 1992	29	29	December 31, 2000.	52
Nova Scotia	17	July 11, 1991	34	34	December 31, 2000.	52
Ontario	17	November 18, 1990	35	35	December 31, 2000.	52
Prince Edward Island	17	April 9, 1991	34	34	December 31, 2000.	52
Quebec	18	January 1, 1991	52	70	N.A.	70
Saskatchewan	18	February 3, 1995	30	30	June 14, 2001.	52
UI	15	November 18, 1990	25	25	December 31, 2000.	50

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

Table 4: Descriptive Statistics of the Labour Supply of Females Around the MOB 1976-2002 from the Panel-based Sample.

Month	Employed and on Leave	Employed and At Work
MOB-1	0.26	0.25
	(0.44)	(0.43)
MOB	0.42	0.05
	(0.49)	(0.22)
MOB+1	0.40	0.07
	(0.49)	(0.25)
MOB+2	0.37	0.10
	(0.48)	(0.30)
MOB+3	0.33	0.16
	(0.47)	(0.36)
MOB+4	0.27	0.22
	(0.45)	(0.41)
(Employed MOB+3 & Tenure>3) | Employed MOB+3		0.93
		(0.26)
(Employed MOB+4 & Tenure>4) | Employed MOB+4		0.89
		(0.31)
Left Job within 12 months of MOB		0.26
		(0.44)
Left Job within 12 months of MOB due to personal/family reasons		0.15
		(0.15)

Notes: Source is the LFS. MOB is month of birth. Standard deviations in parentheses.

Table 5: Impact of the Introduction of Mandated Leave on Employed on Leave and Employed at Work from Panel-based Sample

Employed and On Leave						
MOB-3	0.0022	0.0012	0.0012	0.0020	0.0011	0.0020
	(0.0006)	(0.0008)	(0.0007)	(0.0015)	(0.0008)	(0.0013)
MOB-2	0.0015	0.0006	0.0008	0.0010	-0.0002	-0.0004
	(0.0005)	(0.0006)	(0.0006)	(0.0012)	(0.0008)	(0.0009)
MOB-1	0.0030	0.0032	0.0029	0.0054	0.0032	0.0049
	(0.0009)	(0.0007)	(0.0008)	(0.0012)	(0.0008)	(0.0009)
MOB	0.0016	0.0031	0.0032	0.0057	0.0028	0.0051
	(0.0009)	(0.0009)	(0.0008)	(0.0020)	(0.0010)	(0.0012)
MOB+1	0.0021	0.0047	0.0051	0.0077	0.0036	0.0038
	(0.0011)	(0.0011)	(0.0010)	(0.0017)	(0.0011)	(0.0015)
MOB+2	-0.0002	0.0037	0.0046	0.0068	0.0026	0.0032
	(0.0011)	(0.0011)	(0.0011)	(0.0020)	(0.0014)	(0.0017)
MOB+3	0.0024	0.0050	0.0059	0.0071	0.0045	0.0035
	(0.0014)	(0.0017)	(0.0017)	(0.0027)	(0.0020)	(0.0020)
Employed and At Work						
MOB-3	-0.0015	-0.0009	-0.0011	-0.0000	0.0001	-0.0013
	(0.0015)	(0.0019)	(0.0017)	(0.0028)	(0.0020)	(0.0021)
MOB-2	0.0006	0.0019	0.0017	0.0032	0.0028	0.0026
	(0.0011)	(0.0013)	(0.0012)	(0.0022)	(0.0015)	(0.0016)
MOB-1	-0.0014	-0.0001	-0.0003	0.0002	0.0001	-0.0023
	(0.0008)	(0.0008)	(0.0007)	(0.0012)	(0.0011)	(0.0018)
MOB	-0.0001	0.0002	0.0002	0.0003	-0.0002	-0.0014
	(0.0004)	(0.0005)	(0.0005)	(0.0005)	(0.0011)	(0.0014)
MOB+1	0.0001	-0.0011	-0.0012	-0.0014	0.0004	-0.0007
	(0.0005)	(0.0005)	(0.0006)	(0.0008)	(0.0009)	(0.0016)
MOB+2	0.0009	-0.0010	-0.0013	-0.0023	0.0007	-0.0004
	(0.0010)	(0.0009)	(0.0011)	(0.0015)	(0.0012)	(0.0019)
MOB+3	-0.0003	-0.0008	-0.0011	-0.0030	0.0004	0.0010
	(0.0013)	(0.0019)	(0.0019)	(0.0027)	(0.0022)	(0.0026)
Provincial Trends	No	Yes	Yes	Yes	Yes	Yes
Federal. Sector			Deleted			
Recent Work				Yes		
Control Group					Males	Childless Females

Notes: Reported statistics are the parameter on weeks of mandated leave from a regression of the indicated variable on weeks of leave, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period is 1976 through October 1990. MOB is month of birth.

Table 6: Impact of Introduction of Mandated Leave on Job Continuity from Panel-based Sample

Employed MOB+3 Tenure>3	0.0016	0.0034	0.0040	0.0042	0.0034	0.0051
	(0.0016)	(0.0021)	(0.0021)	(0.0028)	(0.0025)	(0.0034)
Left Last Job	0.0000	-0.0031	-0.0031	-0.0060	-0.0025	-0.0036
	(0.0010)	(0.0010)	(0.0010)	(0.0016)	(0.0015)	(0.0014)
Left Last Job-Personal	-0.0018	-0.0031	-0.0034	-0.0058	-0.0030	-0.0027
	(0.0008)	(0.0011)	(0.0012)	(0.0019)	(0.0011)	(0.0012)
Provincial Trends	No	Yes	Yes	Yes	Yes	Yes
Federal Sector			Delete			
Recent Work				Yes		
Control Group					Males	Childless Females

Notes: Reported statistics are the parameter on weeks of mandated leave from a regression of the indicated variable on weeks of leave, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period is 1976 through October 1990. MOB is month of birth.

Table 7: Impact of Introduction of Mandated Leave on Employed on Leave and Employed at Work from Time Series of Cross Sections Sample

Current Mandate						
Employed and on Leave	0.0011	0.0014	0.0016	0.0027	0.0014	0.0017
	(0.0004)	(0.0005)	(0.0005)	(0.0010)	(0.0005)	(0.0003)
Employed and at Work	-0.0004	0.0000	0.0000	-0.0002	-0.0001	-0.0004
	(0.0004)	(0.0005)	(0.0006)	(0.0009)	(0.0006)	(0.0007)
Three Month Lagged Mandate						
Employed and on Leave	0.0010	0.0014	0.0016	0.0027	0.0014	0.0019
	(0.0004)	(0.0005)	(0.0005)	(0.0010)	(0.0005)	(0.0005)
Employed and at Work	-0.0007	-0.0004	-0.0004	-0.0007	-0.0003	-0.0008
	(0.0004)	(0.0005)	(0.0006)	(0.0008)	(0.0006)	(0.0007)
Provincial Trends	No	Yes	Yes	Yes	Yes	Yes
Federal Sector			Delete			
Recent Work				Yes		
Control Group					Males	Childless Females

Notes: Reported statistics are the parameter on weeks of mandated leave from a regression of the indicated variable on weeks of leave, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period is 1976 through October 1990. MOB is month of birth.

Table 8: Impact of the 1990 Extension of Mandated Leave on Employed on Leave and Employed at Work from Panel-based Sample.

Employed and On Leave							
MOB-2	-0.0121	-0.0026	-0.0170	-0.0095	0.0114	-0.0090	-0.0156
	(0.0114)	(0.0089)	(0.0091)	(0.0096)	(0.0118)	(0.0114)	(0.0096)
MOB	-0.0101	0.0120	-0.0298	0.0085	0.0292	0.0110	0.0226
	(0.0120)	(0.0167)	(0.0192)	(0.0182)	(0.0182)	(0.0173)	(0.0172)
MOB+3	0.0221	0.0610	0.0142	0.0405	0.0958	0.0728	0.0499
	(0.0236)	(0.0291)	(0.0277)	(0.0330)	(0.0343)	(0.0302)	(0.0333)
MOB+4	0.1128	0.1558	0.0121	0.1520	0.2268	0.1674	0.1922
	(0.0228)	(0.0339)	(0.0204)	(0.0345)	(0.0519)	(0.0391)	(0.0414)
Employed and at Work							
MOB-2	-0.0344	-0.0202	0.0448	-0.0139	-0.0248	0.0268	-0.0001
	(0.0096)	(0.0165)	(0.0206)	(0.0170)	(0.0222)	(0.0163)	(0.0228)
MOB	-0.0184	-0.0217	-0.0041	-0.0197	-0.0241	0.0121	-0.0217
	(0.0071)	(0.0050)	(0.0079)	(0.0060)	(0.0061)	(0.0072)	(0.0141)
MOB+3	-0.0484	-0.0564	-0.0175	-0.0472	-0.0809	-0.0198	-0.0187
	(0.0198)	(0.0166)	(0.0154)	(0.0184)	(0.0232)	(0.0195)	(0.0322)
MOB+4	-0.1010	-0.1259	-0.0399	-0.1345	-0.1343	-0.1121	-0.1897
	(0.0217)	(0.0285)	(0.0324)	(0.0306)	(0.0432)	(0.0319)	(0.0568)
Years	1990-1991	1989-1992	1986-1989	1989-1992	1989-1992	1989-1992	1989-1992
Federal				Delete			
Recent Work					Yes		
Control Group						Males	Childless Females

Notes: Reported statistics are the parameter on a 0/1 indicator of the introduction of parental leave from a regression of the indicated variable on leave indicator, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period as indicated. MOB is month of birth.

Table 9: Impact of the 1990 Extension of Mandated Leave on Job Continuity from Panel-based Sample

Employed MOB+3 Tenure>3	-0.0075	0.0286	-0.0176	0.0078	0.0435	0.0808	0.0477
	(0.0223)	(0.0260)	(0.0334)	(0.0287)	(0.0304)	(0.0304)	(0.0349)
Employed MOB+4 Tenure>4	0.0918	0.0911	-0.0572	0.0764	0.1454	0.1221	0.0414
	(0.0335)	(0.0277)	(0.0320)	(0.0286)	(0.0363)	(0.0349)	(0.0492)
Left Last Job	0.0065	-0.0164	0.0529	-0.0143	-0.0051	-0.0414	-0.0143
	(0.0094)	(0.0149)	(0.0156)	(0.0142)	(0.0179)	(0.0149)	(0.0187)
Left Last Job-Personal	-0.0223	-0.0233	0.0432	-0.0208	-0.0241	-0.0226	-0.0193
	(0.0100)	(0.0128)	(0.0121)	(0.0116)	(0.0166)	(0.0126)	(0.0130)
Years	1990-1991	1989-1992	1986-1989	1989-1992	1989-1992	1989-1992	1989-1992
Federal Recent Work				Delete	Yes		
Control Group						Males	Childless Females

Notes: Reported statistics are the parameter on a 0/1 indicator of the introduction of parental leave from a regression of the indicated variable on leave indicator, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period as indicated. MOB is month of birth.

Table 10: An Account of the Change in the Proportion of Mothers Employed MOB+4 with Tenure>4

Change in the Proportion of Mothers Employed MOB+4 with Tenure>4	0.0911 (0.0277)
Change in Total Employment at MOB+4	0.0299 (0.0230)
Change in the Proportion of Mothers Employed MOB+4 with Tenure=1	-0.0041 (0.0094)
Change in the Proportion of Mothers Employed MOB+4 with Tenure=2	-0.0290 (0.0106)
Change in the Proportion of Mothers Employed MOB+4 with Tenure=3	-0.0136 (0.0074)
Change in the Proportion of Mothers Employed MOB+4 with Tenure=4	-0.0145 (0.0064)
Total Change in Employment at Tenures<5 (Sum of Rows 3-6)	0.0612

Notes: Reported statistics are the parameter on a 0/1 indicator of the introduction of parental leave from a regression of the indicated variable on leave indicator, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period is 1989-1992. MOB is month of birth.

Table 11: Impact of the 1990 Extension of Mandated Leave on Employed on Leave and Employed at Work from Time Series of Cross Sections Sample.

Current Mandate							
Employed and on Leave	0.0289 (0.0042)	0.0400 (0.0068)	-0.0122 (0.007)	0.0424 (0.0071)	0.0610 (0.0103)	0.0405 (0.0069)	0.0438 (0.0069)
Employed and at Work	-0.0282 (0.0084)	-0.0333 (0.0075)	0.016 (0.008)	-0.0359 (0.0074)	-0.0387 (0.0111)	0.0044 (0.0072)	-0.0153 (0.0073)
Six Month Lagged Mandate							
Employed and on Leave	0.0333 (0.012)	0.0449 (0.008)	N.A.	0.00480 (0.0072)	0.0688 (0.0109)	0.0443 (0.0084)	0.0443 (0.0083)
Employed and at Work	-0.0282 (0.0106)	-0.0328 (0.0072)	N.A	-0.0334 (0.0078)	-0.0429 (0.0097)	0.0052 (0.0060)	-0.0119 (0.0080)
Years	1990-1991	1989-1992	1986-1989	1989-1992	1989-1992	1989-1992	1989-1992
Federal				Delete			
Recent Work					Yes		
Control Group						Males	Childless Females

Notes: Reported statistics are the parameter on a 0/1 indicator of the introduction of parental leave from a regression of the indicated variable on leave indicator, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period as indicated. MOB is month of birth. N.A is not applicable.

Table 12: Impact of the 2000 Extension of Mandated Leave on Employed on Leave and Employed at Work from Time Series of Cross Sections Sample.

Current Mandate							
Employed and on Leave	0.0435 (0.0117)	0.0939 (0.0175)	0.0039 (0.0079)	0.0868 (0.0183)	0.1404 (0.0217)	0.0972 (0.0180)	0.0851 (0.0196)
Employed and at Work	-0.0538 (0.0084)	-0.1107 (0.0188)	0.0006 (0.0107)	-0.1045 (0.0186)	-0.1672 (0.0244)	-0.1131 (0.0187)	-0.0899 (0.0214)
9 Month Lagged Mandate							
Employed and on Leave	0.1112 (0.0175)	0.1283 (0.0108)	N.A.	0.1244 (0.0106)	0.1818 (0.0154)	0.1300 (0.0110)	0.1223 (0.0125)
Employed and at Work	-0.0985 (0.0156)	-0.1422 (0.0122)	N.A.	-0.1355 (0.0120)	-0.1982 (0.0160)	-0.1425 (0.0127)	-0.1201 (0.0166)
Years	2000-2001	1999-2002	1997-1999	1999-2002	1999-2002	1999-2002	1999-2002
Federal Recent Work				Delete	Yes		
Control Group						Males	Childless Females

Notes: Reported statistics are the parameter on a 0/1 indicator of the extension of parental leave from a regression of the indicated variable on leave indicator, province, year and calendar month effects, a cubic in age, education (three categories) and a control for siblings aged 1-24. Robust standard errors are in parenthesis. Sample period as indicated. MOB is month of birth. N.A is not applicable.

Table 13: Impact of Weeks of Mandated Leave on Various Measure of Infant Mortality and the Incidence of Low Birth weight, 1961-2001

Infant Mortality	-0.038	0.024	0.011
	(0.013)	(0.016)	(0.020)
Perinatal Mortality	-0.061	0.011	-0.011
	(0.014)	(0.018)	(0.023)
Neonatal Mortality	-0.029	0.014	0.003
	(0.010)	(0.013)	(0.016)
Post-Neonatal Mortality	-0.008	0.010	0.008
	(0.006)	(0.008)	(0.010)
Low Birth Weight	-0.005	0.002	0.002
	(0.003)	(0.004)	(0.005)
Provincial Trends	No	Linear	Quadratic

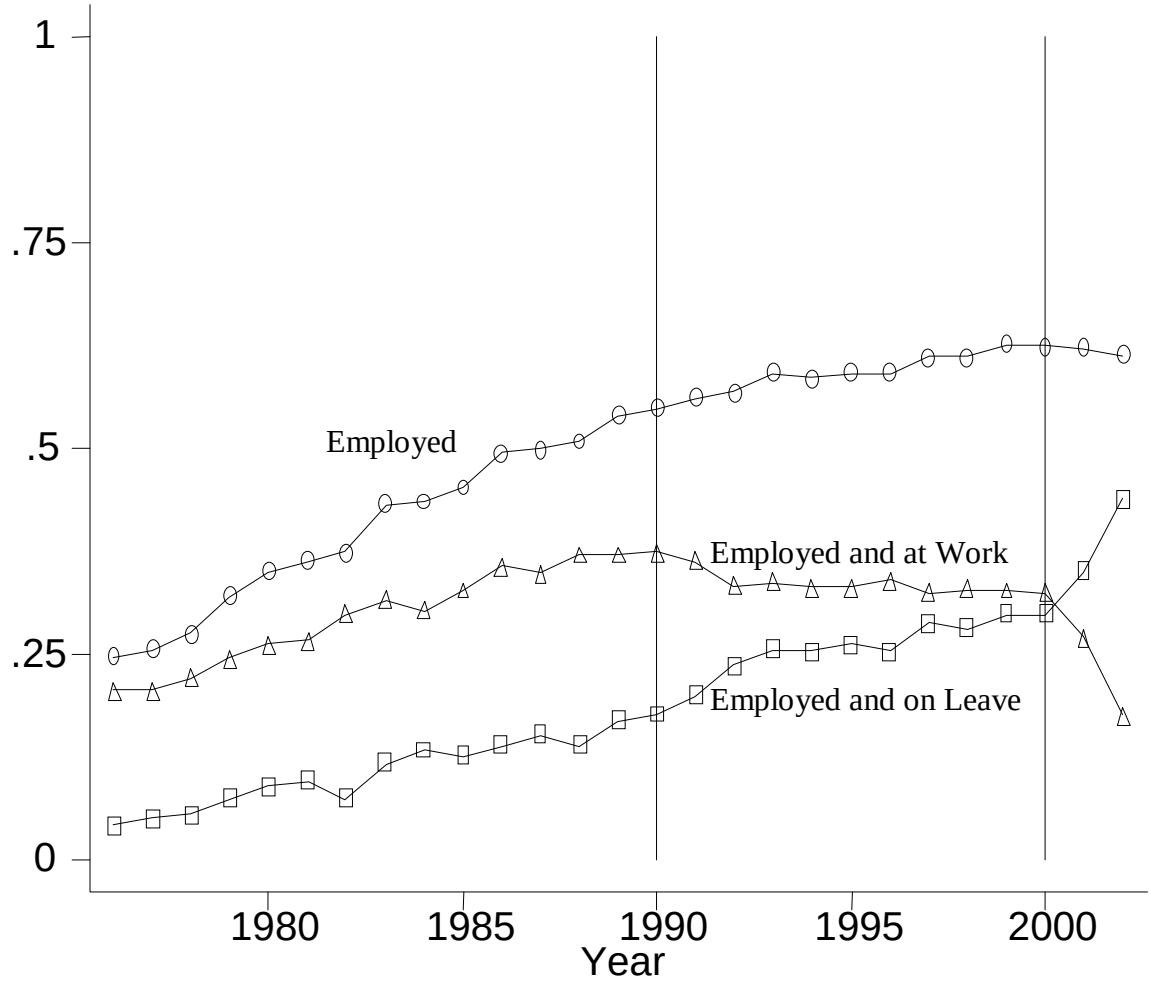
Notes: Reported statistics are the parameter on weeks of mandated leave from a regression of the indicated variable on weeks of leave, province and year effects, the employment population ratio of females aged 15+, the ratio of annual births to the female population aged 15 to 44, real provincial GDP per capita, total provincial health expenditures as a percent of GDP and the indicated provincial trends. Robust standard errors are in parenthesis.

Table 14: Impact of the 1990 Extension of Parental Leave

Infant Mortality	-0.427	-0.495	-0.407
	(0.369)	(0.426)	(0.360)
Perinatal Mortality	-0.527	-0.581	-0.554
	(0.519)	(0.585)	(0.607)
Neonatal Mortality	-0.335	-0.397	-0.347
	(0.305)	(0.337)	(0.319)
Post-Neonatal Mortality	-0.115	-0.123	-0.087
	(0.249)	(0.283)	(0.0280)
Time Trend	Linear	Quadratic	Cubic

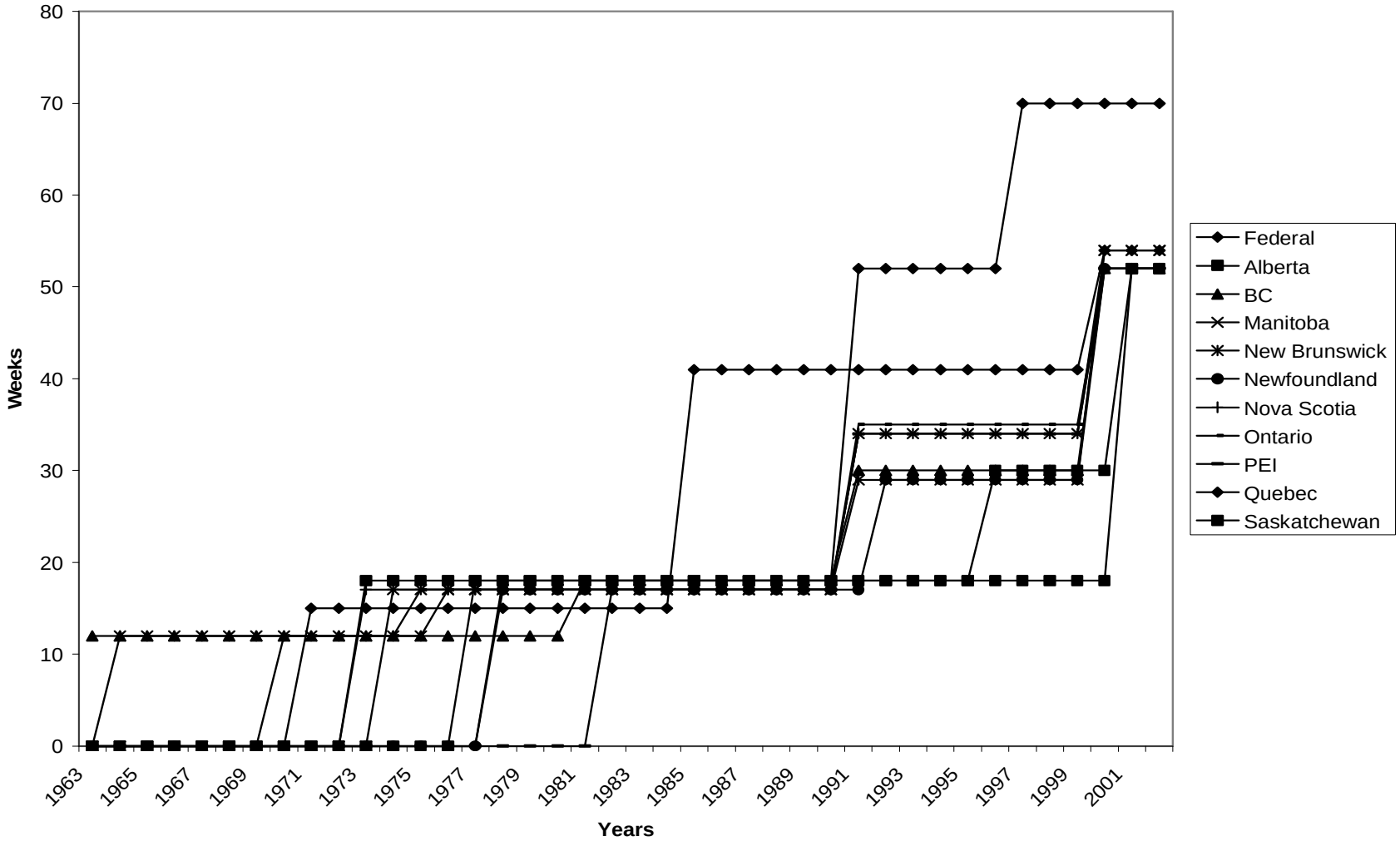
Notes: Reported statistics are the parameter on a 0/1 indicator of the introduction of parental leave from a regression of the indicated variable of this indicator, province effects, the employment population ratio of females aged 15+, the ratio of annual births to the female population aged 15 to 44, real provincial GDP per capita, total provincial health expenditures as a percent of GDP and the indicated time trends. Robust standard errors are in parenthesis

Figure 1: The Employment Rates of Married Females Aged 30-39 with a Child Aged less than One: 1976-2002



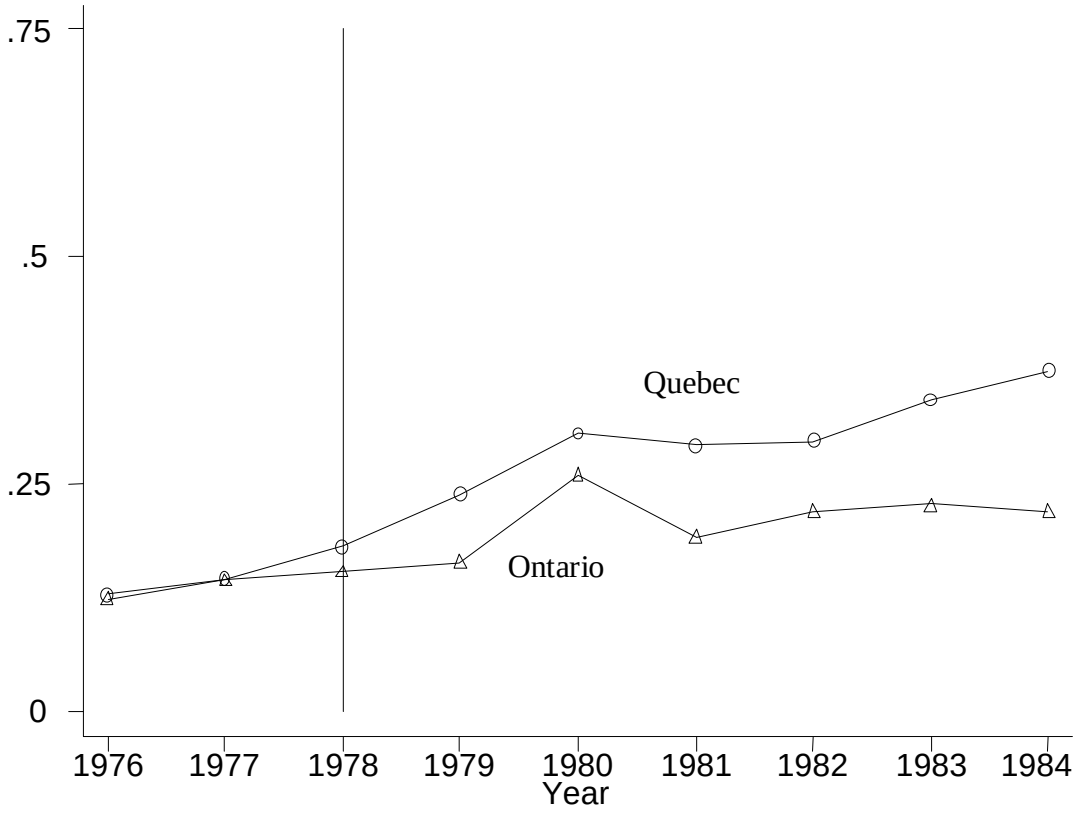
Notes: Source is the April and October files of the LFS.

Figure 2: Maximum Job-protected Maternity/Parental Leave Mandates by Province, 1963-2002



Notes: See Table 1.

Figure 3: The Proportion of Married Mothers, Aged 30-39, Employed and On Leave in the Month Before Birth

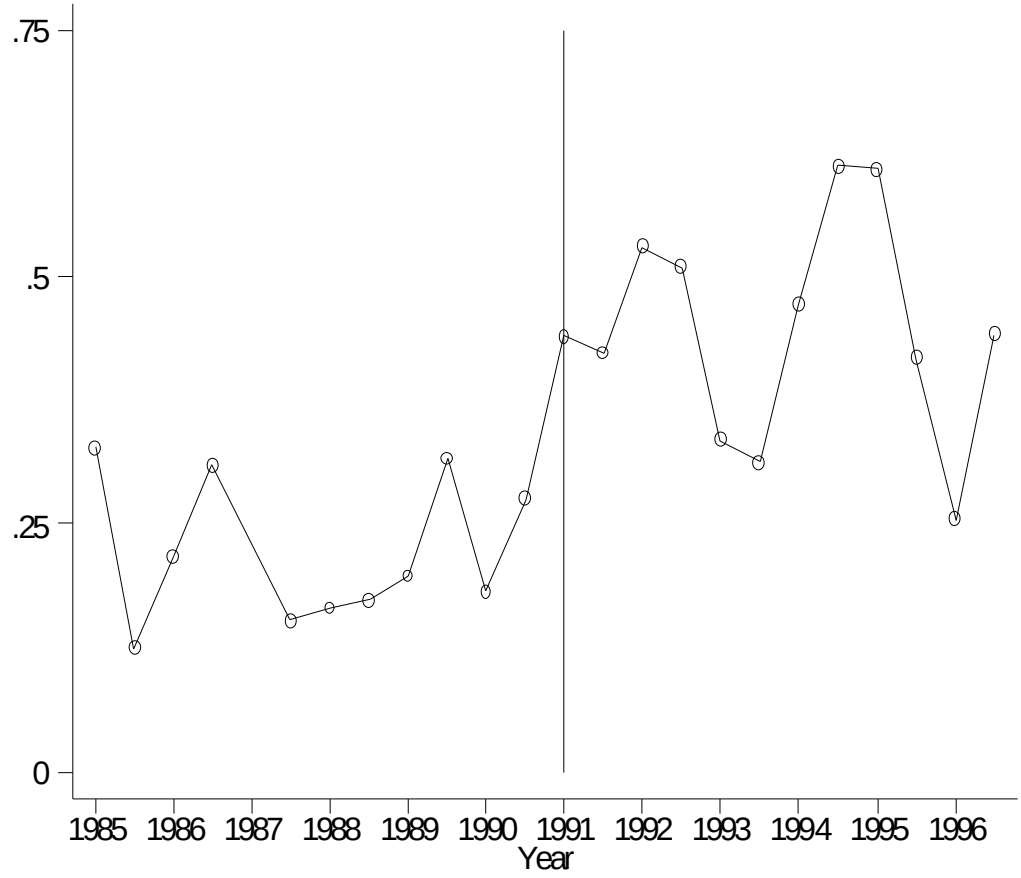


Notes: Source is the panel-based sample from the LFS.

Figure 4: The Proportion of Married Mothers, Aged 30-39, Employed and On Leave in the "Month of Birth"

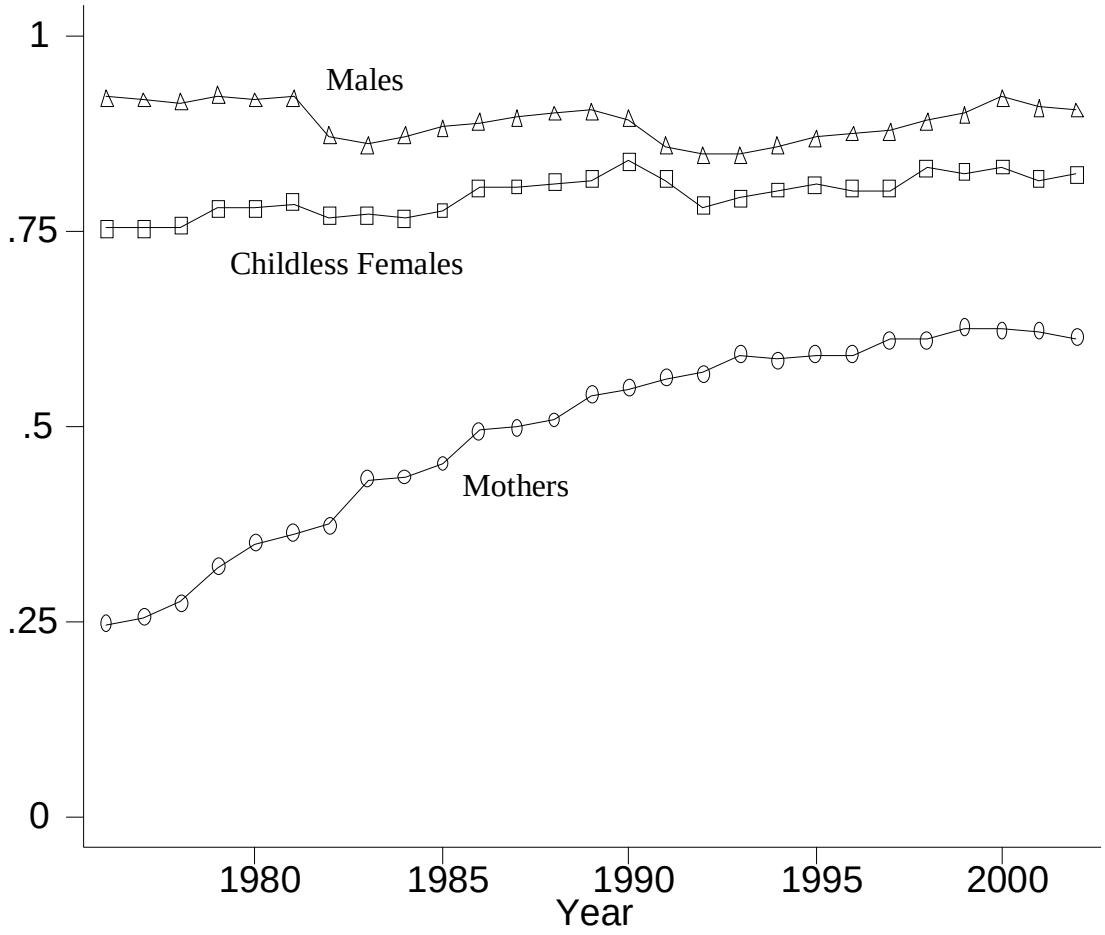
Notes: Source is the panel-based sample from the LFS. The proportion reported is for the provinces of British Columbia, New Brunswick, Prince Edward Island and Quebec.

Figure 5: The Proportion of Married Mothers, Aged 30-39, Employed and On Leave Four Months after the "Month of Birth"



Notes: Source is the panel-based sample from the LFS. The proportion reported is for the provinces of British Columbia, New Brunswick, Prince Edward Island and Quebec.

Figure 6: The Employment Rate of Married Males, Childless Females and Females with a Child Aged less than One, Aged 30-39: 1976-2002



Notes: Source is the April and October files of the LFS.

Figure 7: The Infant Mortality Rates in Ontario and Quebec 1985-1996

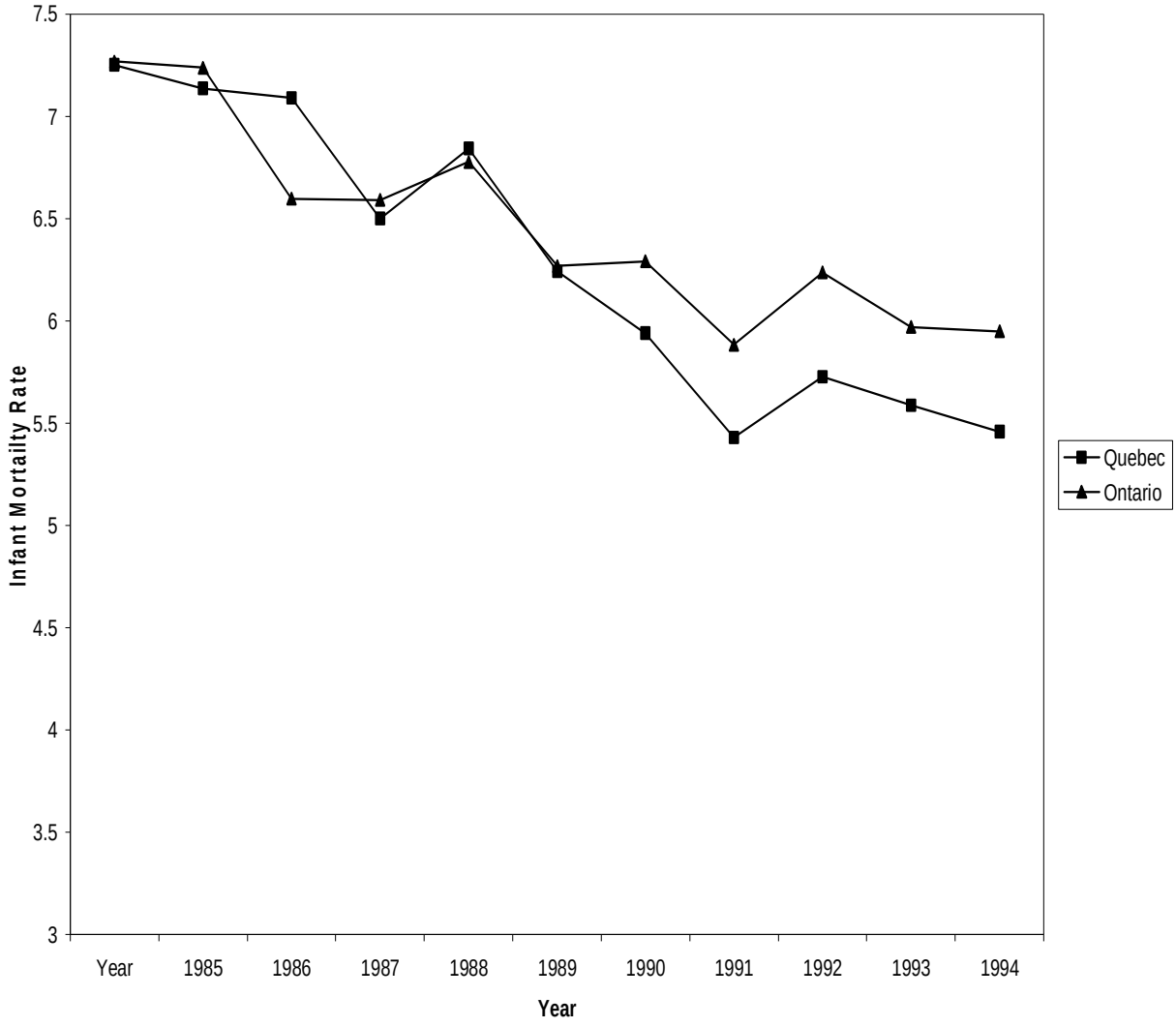


Figure 8: The Post-Neonatal Mortality Rates in Ontario and Quebec 1985-1996