DISCUSSION PAPER SERIES

IZA DP No. 5793

A Flying Start? Maternity Leave Benefits and Long Run Outcomes of Children

Pedro Carneiro
Katrine V. Løken
Kjell G. Salvanes

June 2011

Forschungsinstitut
zur Zukunft der Arbeit
Institute for the Study
of Labor

A Flying Start? Maternity Leave Benefits and Long Run Outcomes of Children

Pedro Carneiro
University College London,
IFS, CeMMAP and IZA

Katrine V. Løken
University of Bergen

Kjell G. Salvanes
Norwegian School of Economics,
CEE, CESifo and IZA

Discussion Paper No. 5793
June 2011

IZA

P.O. Box 7240
53072 Bonn
Germany

Phone: +49-228-3894-0
Fax: +49-228-3894-180
E-mail: iza@iza.org

IZA Discussion Paper No. 5793
June 2011

ABSTRACT

A Flying Start? Maternity Leave Benefits and Long Run Outcomes of Children*

We study the impact on children of increasing maternity leave benefits using a reform that increased paid and unpaid maternity leave in Norway in July 1977. Mothers giving birth before this date were eligible only for 12 weeks of unpaid leave, while those giving birth after were entitled to 4 months of paid leave and 12 months of unpaid leave. This increased time with the child led to a 2.7 percentage points decline in high school dropout and a 5% increase in wages at age 30. For mothers with low education we find a 5.2 percentage points decline in high school dropout and an 8% increase in wages at age 30. The effect is especially large for children of those mothers who, prior to the reform, would take very low levels of unpaid leave.

JEL Classification: J13, J18

Keywords: maternity leave, children's outcomes

Corresponding author:

Kjell G. Salvanes
Department of Economics
Norwegian School of Economics
and Business Administration
Helleveien 30
N-5035 Bergen-Sandviken
Norway
E-mail: kjell.salvanes@nhh.no

* This is a substantially revised and extended version of IZA DP No. 5362. We thank Gerard van den Berg, Sandy Black, Richard Blundell, Christian Dustmann, Per-Anders Edin, Peter Fredriksson, Kjell-Erik Lommerud, Uta Schoenberg, and seminar participants at Yale, University of Bergen, University of Stavanger, University of Stockholm, Uppsala University, Tinbergen Institute, Norwegian School of Economics, University College London, University of Texas, University of Rome, Universidade Nova de Lisboa, Universidad Carlos III, University of Bologna, Colegio Alberto, ESSLE 2008, EEA 2008, and SOLE 2009 for useful comments on the paper. Løken and Salvanes are thankful to the Research Council of Norway for financial support. Carneiro gratefully acknowledges the financial support from the Economic and Social Research Council for the ESRC Centre for Microdata Methods and Practice (grant reference RES-589-28-0001), the support of the European Research Council through ERC-2009-StG-240910-ROMETA and Orazio Attanasio's ERC-2009 Advanced Grant 249612 "Exiting Long Run Poverty: The Determinants of Asset Accumulation in Developing Countries", and the hospitality of the World Bank Research Group.

When it comes to paid maternity leave, the United States is in the postpartum dark ages. One hundred and seventy-seven nations -- including Djibouti, Haiti and Afghanistan -- have laws on the books requiring that all women, and in some cases men, receive both income and job-protected time off after the birth of a child. But here, the Family and Medical Leave Act of 1993 provides only unpaid leave, and most working mothers don't get to stay home with their newborns for the 12 weeks allowed by the law. Many aren't covered by the FMLA; others can't afford to take unpaid time off. Some go back to work a few weeks after giving birth, and some go back after mere days.

Sharon Lerner, Washington Post, June 13, 2010

Although the evidence on time use within families is limited and needs further study, the increase in work from 1969 to 1996 has produced a reduction in the time available for parents to spend with children. The increase in hours mothers spend in paid work, combined with the shift toward single-parent families, resulted in families on average experiencing a decrease of 22 hours a week (14 percent) in parental time available outside of paid work that they could spend with their children.

Council of Economic Advisors (2009)

1. Introduction

There are huge disparities in maternity leave entitlements across different countries. On one extreme, countries in Northern Europe (such as Sweden, Norway or Germany) mandate very generous paid leave and long periods of job protection after birth. On the other extreme there are a handful of countries such as the United States (US), which have no paid leave mandate and offer little (if any) job protection (ILO, 1998).

These disparities were much smaller 30 to 40 years ago. In several countries, new mothers had benefits similar to the ones currently in existence in the US, where the federal mandate (which is adopted in almost all states) is 12 weeks unpaid leave for women working in firms with 50 or more workers. One striking example, which is the focus of our paper, is Norway. Prior to 1977, working mothers in Norway were entitled to 12 weeks unpaid leave, and to no paid leave. Currently the situation is very different: they are entitled to a full year of paid leave and an additional year of job protection.

With the dramatic growth in female labor force participation, maternity leave benefits have become more generous across the world. In the US, however, they have remained fairly low, in spite of intense debate on this topic. A central question is whether the absence of stronger maternity protection in the US is detrimental to child development, or whether the high levels of benefits in Northern Europe are mainly important for maternal health (and maternal welfare more generally), with little consequence in children's lives. This question is the focus of our paper.

Empirically, this is a notoriously difficult problem, as emphasized (for example) by Bernal (2008) and Dustmann and Schönberg (2008) since mothers who spend more time with their children after birth may have many unobservable attributes that affect child development (or they use child care arrangements which are special in unobservable dimensions). Furthermore, since additional time with children is generally associated with less time at work and lower household income, it is difficult to isolate the two.

In our paper we address these empirical challenges, by studying a reform in maternity leave benefits in Norway on long term outcomes of children, namely education and earnings at age 30. The reform we analyze increased mandatory paid maternity leave from 0 to 4 months and mandatory unpaid maternity leave from 3 to 12 months.[1]

This new set of benefits applied to all eligible mothers having children after July 1st, 1977.[2] We estimate their long term impact on children using regression discontinuity, comparing outcomes of children of eligible mothers born just after and just before this particular date. We assess the importance of month of birth effects, and of any potential

[1] This is equivalent to moving from the current level of maternity leave entitlements in the US to something closer to Holland and several other countries in Southern and Central Europe.

[2] Eligibility criteria, involving work requirements, are discussed below in detail. About 35% of women giving birth in 1977 were ineligible for paid maternity leave benefits.

manipulation of the date of birth. We follow children to as late as 2007, when they are 30 years of age. We observe several long term outcomes, such as high school completion, college attendance, and wages at age 30.[3]

We begin with a very simple look at the data. Take individuals (and their mothers) born only in two months of 1977: June (just before the reform was implemented) and July (just after the reform). We can compare the outcomes of children in these two groups (only for eligible mothers), by running a regression of the outcome of interest on an indicator for being born in July. However, there may be differences in outcomes between children born in these two months of 1977 for reasons unrelated to the reform, as emphasized in a large literature on month of birth effects (Black, Devereux and Salvanes, 2008, present estimates for Norway). Therefore, it is important to use an earlier year, prior to the implementation of the reform, as a comparison. We use data from 1975 to estimate the difference in outcomes between children born in June and July prior to the reform, and subtract it from the estimate of the effect of being born in July (vs. being born in June) that we got from the 1977 data (a difference-in-differences estimator).[4]

Table 1 presents estimates of the impact of the program using the single (first column) and double differences (second column) estimators for a subset of the dependent

---

[3] The appendix, Table A1, also shows IQ, height (males only), and teenage pregnancy (females only).

[4] For the single difference we would run the following regression using data for children born in June and July of 1977:

$$Y_i = \alpha + \beta * D_i^{July} + u_i$$

where Y_i is the outcome of interest and D_i^{July} is a dummy indicating whether an individual was born in July. β measures the impact of benefiting of the reform on the outcome of interest, among children of eligible mothers. For the difference in difference estimator, using data from children born in the months of June of July of 1975 and 1977, we can run:

$$Y_i = \alpha + \gamma * D_i^{1977} + \phi * D_i^{July} + \beta * D_i^{July} D_i^{1977} + u_i$$

where D_i^{1977} is a dummy indicating whether an individual was born in 1977. As before, β measures the impact of benefiting of the reform on the outcome of interest, among children of eligible mothers.

variables we consider in the paper. Child outcomes are shown at the top: indicators for whether a person is a high school dropout, whether she has ever attended college, and log earnings at age 30. The results suggest that the reform had an impact on high school dropout rates, and earnings at age 30, but not on college attendance, both in the single and the double-difference specifications.

Then we examine two pre-birth maternal variables, which should not be affected by the reform: years of education of the mother, and log annual income in 1975 (more variables are shown in the appendix, Table A1). In both these dimensions, the set of mothers giving birth in June of 1977 is similar to the set of mothers giving birth in July of the same year (even when we use the differences in differences estimator).

Finally, we find no impact of the reform on maternal income right around the time the mother gave birth (average log income in the year of birth and the year after b irth), which means that the reform had no impact on unpaid leave.[5] We also look at maternal labour supply and income 5 years after the birth of the child[6], and see no statistically significant effect of the reform on these variables, using both single (first column) or double (second column) differences. This is why we argue that the main mechanism of this reform was an increase in time with the child, with no short or long run consequences on labour market outcomes.

In the rest of the paper we develop, expand and discuss these results in detail, implementing a regression discontinuity estimator that uses information from children

[5] Note that the small significant effect on income at year of birth in the first difference result is purely a month effect, and a consequence of the fact that we only have annual (and not monthly) measures of income for each mother. Mothers giving birth to the child later in the year have more months to work before giving birth and therefore have a higher income during the year of birth. When controlling for this problem using eligible mothers in 1975 there is no effect of the reform on income in the year of birth.

[6] As opposed to more permanent effects of the reform on labour market outcomes of females, after employers and mothers fully adjust their expectations and behaviours.

born in the other months of the year. The main patterns of table 1 survive a more sophisticated estimation procedure. We will also examine a wider set of variables.

The literature on this topic is fairly wide, so we will not review it in detail. Good reviews of the literature on maternal employment and child outcomes are available in Blau and Currie (2006) and Bernal and Keane (2010). The Economic Journal featured a recent symposium on this topic (Gregg and Waldfogel, 2005; Tanaka, 2005; Gregg, Washbrook, Propper and Burgess, 2005). The literature is fairly inconclusive and plagued with empirical problems, as these papers document. The Society for Research in Child Development edited a recent volume on this topic (Brooks-Gunn, Han and Waldfogel, 2010) arguing that, at least for non-hispanic whites in the US, maternal employment in the first year of life does not have particularly detrimental consequences on children because its negative and positive aspects cancel each other out. But, as in most of the literature, the authors caution against a causal interpretation of their estimates.

Recent papers directly examine maternity leave reforms. For the US, Rossin (2011) studies the effect of the 1993 reform on children's birth and infant health. She finds support for some positive effects of the reform on children's health outcomes. We can also find three other empirical analyses of the effect of maternity leave reforms on long term outcomes of children, using registry data with very large sample sizes for Germany (Dustmann and Schönberg,, 2008), Denmark (Rasmussen, 2010), and Sweden (Liu and Skans, 2010).[7] There are two important aspects of these papers relatively to the literature described above: 1) they explore exogenous variation in maternity leave resulting from legislative reforms to these benefits; 2) they are able to look at long run

[7] We should also mention a set of recent papers studying Canadian reforms and focusing on short run outcomes for children, by Baker and Milligan, (2008a, 2008b). These papers also find no significant effects of the reform on children's outcomes.

outcomes of children. Our data challenges the main result of these papers: that there is little or no effect of maternity leave expansions on long run outcomes of children.

This is an important finding. We believe that there are two central aspects of our study that distinguish it from the ones above and may explain our different results. First, we consider a change in maternity leave entitlements when they were at a very low level, similar to the US today. The papers we refer to consider expansions in maternity leave from an already baseline level that is fairly generous. Even in Dustmann and Schönberg, (2008), who study three different reforms in Germany, the earliest reform they consider is an expansion from 2 to 6 months in paid maternity leave entitlements (the long term outcomes considered in the study of that reform are wages at ages 24-26).

Second, we are able to look at education and labour market outcomes as late as age 30. Other papers have examined earlier educational outcomes, or earlier labour market outcomes. One problem with looking to early labour market outcomes is that individuals' careers may only stabilize much later.[8]

In addition, our data lets us link mothers with their children which allows us to do a rich analysis of impacts by subgroups of mothers; and it lets us construct good measures of eligibility for the reform which is important since generally only a fraction of mothers (those who are working a minimum amount of time) is eligible for these benefits.[9]

[8] In fact, we do not find any effect of the reform on earnings at ages 24 and 25.

[9] One drawback of our data is that it does not contain direct measures of labour supply. This information is not essential for estimating effects of the reform but it is useful to understand the mechanisms through which it is operating. We do, however, observe total income in each year. There is no impact of the reform on maternal income in 1977 and 1978. This means that the reform did not change the amount of unpaid leave being taken by mothers giving birth after the reform. We do not consider the case that the reform had no effect at all on leave taking behaviour, since it is highly unlikely. Below we present indirect evidence suggesting that the new paid leave entitlement was fully taken-up by new mothers, and therefore the lack of change in annual income is just a result of unchanged levels of unpaid leave. For example, when we examine later reforms to maternity leave, for which we observe labour supply data, we see close to full uptake of the new benefits. Therefore, we argue that the reform led to an increase in four extra months of

The paper proceeds as follows. Section 2 gives background information on maternity leave legislation in Norway while Section 3 presents the empirical strategy. Section 4 presents data and Sections 5 shows the results. Section 6 discusses (evidence on) mechanisms by which the reform impacts child outcomes. Section 7 concludes.

2. Maternity Leave Reform and Institutional Background

2.1 Maternity Leave Reform

In 1956, maternity leave benefits became available to women in Norway through the introduction of compulsory sickness insurance for all employees. Eligible mothers were entitled to 12 weeks of essentially unpaid maternity leave. This is basically the same level of benefits available for mothers in (nearly all states in) the US in 2011, provided that they work in firms with 50 or more employees.

On July 1st, 1977, Norway saw the introduction of paid maternity leave and an increase in unpaid leave, as illustrated in Figure 1.[10] With this reform, parents were given the universal right to 18 weeks of paid leave with guaranteed job protection before and after the birth of a child.[11] Maternity leave payments were equivalent to 18 weeks of pre-

---

leave actually taken by new mothers, without changing unpaid leave or maternal income. In addition, all of the reforms to either paid or unpaid leave examined in the literature described above had important impacts on the uptake of leave.

[10] These changes were introduced together with a new law increasing workers rights ("Arbeidsmiljøloven") accepted June 3rd, 1977, by the Parliament and introduced July 1st, 1977 (see Prepositions, Ot.prp. nr. 71 and Innst.o. nr. 90). There were additional reforms after 1977. From 1987 and onwards the paid maternity leave was extended almost yearly until 1993. From 1993 and up till now Norway has had the same paid maternity leave of 42 weeks with 100% cover or 52 weeks with 80% cover. We have in this paper decided to focus on the 1977 law for three reasons. It is a change in what we think is a critical period for the child, for instance since breastfeeding is still an issue. It is easier to assess the first change in the law since the latter reforms were anticipated to a larger degree. And, given that available adult data goes only up to 2007, we have a much richer set of available outcomes for children born in 1977 than for those born later. We leave the study of the other reforms for future work.

[11] You could take a maximum of 12 weeks before the birth of the child; however most mothers worked almost until day of birth as they wanted to save leave until after the child was born (Survey on fertility in 1977, Statistics Norway).

birth employment (i.e., 100% replacement rate). Of these 18 weeks, 6 could be taken by the mother alone, while the rest could be shared between both parents. In practice, all leave was almost exclusively taken by the mother (Rønsen and Sundström, 2002). In addition, parents also got entitled to 1 year of unpaid job protection (on top of the 18 paid and job-protected weeks of maternity leave).

Not all mothers were eligible to receive the new benefits, with eligibility depending on their work and income history. Only women working at least 6 of the 10 months immediately prior to giving birth, and having more than 10000 NOK[12] of yearly income, were eligible for leave and coverage.

Because of limitations in our data (we do not observe labour supply directly, and we only have yearly income which includes wage income and benefits) we have to rely on an imperfect measure of eligibility. In particular, we define eligible mothers as those having at least 10000 NOK of salary in the calendar year before giving birth. Our use of 12 rather than 10 months of income to determine eligibility is likely to slightly overstate the number of eligible mothers. We estimate that two thirds of all mothers giving birth in Norway in 1977 were eligible for maternity leave benefits. We tried different alternative definitions of eligibility, without significant changes in our empirical results.

Figure 2 shows the proportion of mothers who were eligible for maternity leave entitlements from 1975–1979, by birth month of the child. Between 1975 and 1979 the proportion of eligible mothers was always between 60% and 70%, and in 1977 it was about 65%. Since we can only focus on eligible mothers in our analysis, this means that our estimates ignore 35% of mothers and children giving birth in that year.

[12] 10000 NOK (USD 1725) refers to the lowest level of income providing pension points in the Norwegian social security system in 1977.

In order to be able to identify the effects of the reform on children's outcomes it is crucial that mothers are not able to change their eligibility status immediately after the reform is announced. Otherwise, the set of eligible mothers giving birth just before and just after the reform would not be comparable. The maternity leave reform was introduced during a big offensive from the sitting (very radical) parliament at the end of its period. It is unlikely that it was expected since it came along with a lot of other changes (unrelated to the maternity leave reform) and at the end of the legislative period. The Government report became official on April 15th, 1977, and was approved on June 13th, 1977[13]. This means that all women giving birth after the announcement of the law in 1977 were already pregnant when the law was introduced,[14] and because of the rule of working 6 out of 10 months prior to giving birth, it was difficult for women to change their status in the short term. We also checked national newspapers around 1976 and 1977 for news about the reform. We do not find any evidence that newspapers reported anything on the reform before June 1977.[15] Therefore, it is plausible that eligibility status is exogenous for mothers giving birth in 1977.

The 1970s in Norway was the decade of oil discovery, with increasing labour force participation of women, and the implementation of several welfare reforms. We have studied all possible laws and reforms occurring during that period that may have had an impact on maternal and child outcomes. The only one we found was the abortion law

[13] Propositions and regulations from the Government: Ot.prp nr. 61 and Innst.o. nr 61.
[14] Possible effects on fertility will therefore not show up in the data before the beginning of 1978, at the earliest. It is possible that mothers delivering close to July 1st, 1977, were able to delay their delivery. In fact, Gans and Leigh, 2009 estimate that Australian mothers delayed child birth in response to a reform changing incentives to fertility. Nevertheless, for the reform we study there are no significant differences between the number of births occurring just before and just after the reform. This is shown in figure A1 in the Appendix.
[15] Verdens Gang June 30th,1977, Bergens Tiende June 27th,1977, June 30th,1977, Aftenposten June 30th,1977.

implemented January 1st, 1976. This law made it easier for women to have an abortion within 12 weeks of conception. The first cohort to be affected by this reform is born around July 1976. This possibly gives rise to a discontinuity in observed child outcomes between June and July 1976 and hence we do not use 1976 as a comparison to 1977.

2.2 Institutional Background

At the time of the maternity leave reform in 1977, labour force participation for women was relatively high in Norway. Figure 3 shows labour force participation in Norway compared to the US from 1970 to 1990 (distinguishing Norwegian women who are mothers from those who are not). In Norway, the labour force participation rate around 1977 was about 50 percent for married women, which are the most relevant group for our study, and around 70 percent for non-married women. Labour force participation was about the same in Norway and the US during the 1970s, but much higher in the former than in the latter by 1990.[16]

It is also relevant to look at the provision of public child care. In Figure 4 we depict the development of day care coverage in Norway for children aged 0 to 2, in urban and rural areas. In the mid 1970s, very few children aged 0 to 2 were in day care, and there is very little difference in day care attendance between urban and rural areas (1% vs. 0.5%). Although day care centres provided coverage for 15 percent for children aged 0 to 6 in 1977, the coverage for the first two years was very low, only 1–2 percent. This means that the main alternative to maternal care in the early years of the child’s life was informal care by nannies, grandparents or neighbours.

[16] By 2008 the labour force participation rate in the US was around 65 percent (with small race differences). This is comparable to the participation rate around the reform in Norway (OECD, 2008).

3. Empirical Strategy

Let $y_i(1)$ be the outcome for child i in the presence of the reform, and $y_i(0)$ be the outcome for child i in the absence of the reform. Our main goal is to estimate the average impact of the reform on the long term outcomes of children: $\alpha = E\left(y_i(1) - y_i(0)\right)$.

In order to estimate this parameter we compare children born just before and just after the reform, which should be similar except for the fact that mothers of those in the latter group benefit from the change in maternity leave entitlements taking place on July 1st, 1977. In particular, we use a regression discontinuity (RD) estimator to assess the difference in outcomes between children born in June and July of that year.

For those women giving birth in 1977, eligibility for the new maternity leave entitlements ($\mathrm{E_i}$) is a deterministic function of month of birth ($\mathrm{X_i}$):

$$\mathrm{E_i} = 1\{\mathrm{X_i} > \mathrm{c}\}, \tag{1}$$

where c is the cut-off point of July 1st, 1977. Therefore, all mothers giving birth to a child after c potentially receive the treatment defined by new maternity leave entitlements, while those giving birth before c are assigned to the control group. We use only eligible mothers based in our main analysis as defined in Section 2.[17]

The RD estimator for α is given by:

$$\alpha_{RD} = \mathrm{E}[y_i(1) \mid X_i = c] - \mathrm{E}[y_i(0) \mid X_i = c]. \tag{2}$$

As in any RD estimator we are only able to identify a local effect for those born just around the reform. However, this is one case where it is reasonable to conjecture that the

[17] See Appendix, Table A2 for a comparison of results using the total versus the eligible sample.

effects of the reform do not vary substantially with month of birth, in which case α_{RD} would be a consistent estimator of α .

Assuming that $\mathrm{E}[y_i(1) \mid X_i = c]$ and $\mathrm{E}[y_i(0) \mid X_i = c]$ are continuous in x (continuity at x=c is all that is needed) we can estimate them as:

$$\mathrm{E}[y_i(1) \mid X_i = c] = \lim_{x \downarrow c} E[y_i \mid X_i = x]$$
$$\mathrm{E}[y_i(0) \mid X_i = c] = \lim_{x \uparrow c} E[y_i \mid X_i = x]$$

Outcomes of interest for the child include dropping out of high school, college attendance (both measured by age 30), and earnings at age 30 (in the appendix, we examine also the probability of having a child before age 19 for women, and IQ and height for men). Outcomes of interest for the mother include months of unpaid leave, and employment and earnings 5 years after giving birth. These are mainly interesting because we can check for changes in home environments, which can account for the effect of the reform on child outcomes.

We estimate $\alpha_{RD} = \lim_{x \downarrow c} E[y_i \mid X_i = x] - \lim_{x \uparrow c} E[y_i \mid X_i = x]$ by taking the difference between the boundary points of two regression functions of y on x: one for eligibles (x≤c) and one for ineligibles (x>c). We estimate these regression functions with local linear regression (LLR) as in Fan (1992), Hahn, Todd and Van der Klaauw (2001), and Porter (2003). Hahn, et al. (2001) show that LLR outperforms general kernel regression methods in terms of bias. Defining h as the bandwidth, we estimate (α, β, γ, τ):

$$\min_{\alpha,\beta,\tau,\gamma} \sum_{i=1}^{N} K\left(\frac{X_i - c}{h}\right)(y_i - \eta - \beta(X_i - c) - \tau E_i - \gamma(X_i - c)E_i)^2, \tag{3}$$

α_{RD} is estimated as

$$\hat{\alpha}_{RD} = \hat{\tau} \tag{4}$$

We use the triangle kernel which is shown to be boundary optimal (Cheng, Fan and Marron, 1997). We obtain standard errors using the formulas in Porter (2003).[18] The choice of bandwidth is important, as usual. In the main text we present results using a bandwidth of 3, and in the Appendix we present further results using a bandwidth of 5.[19]

It is possible that a simple comparison of outcomes for children born in different months is contaminated by month of birth effects due, for instance, to the fact that the age at which children start school depends on their month of birth and is potentially related to adult education and earnings (see Black, et al., 2008, for evidence for Norway). In this case α_{RD} converges to $\alpha + \lambda_{Bith}$, where λ_{Bith} is a month of birth effect, which does not vary across years. Therefore we combine RD with difference-in-differences (DD) by constructing three types of control groups: one consists of children born in 1975 of eligible mothers; another consists of children born in 1979 of eligible mothers; and another consists of children born in 1977 of ineligible mothers.

We use the first one in our main specification, and the other two in robustness checks (shown in the appendix).[20] We begin by estimating equation (3) for those born in 1975 and those born in 1977. Then we calculate:

$$\hat{\alpha}_{RD,1975} = \hat{\tau}_{1975} = \lambda_{Birth}; \hat{\alpha}_{RD,1977} = \hat{\tau}_{1977} = \alpha + \lambda_{Birth}$$

[18] We verify the results by using the paired-bootstrap percentile-T procedure with 2000 replications. Cameron and Trivedi (2005), show that the bootstrap percentile-T procedure may outperform the analytical standard errors. One reason for this might be the difficulty in estimating parts of the formulas from Porter. From our results we do not see any significant difference between the two methods (if anything there are slightly lower standard errors when using Porter), hence we will use the analytical formulas.

[19] Using cross validation as in Imbens and Lemieux (2008) we get an optimal bandwidth of 3. However, Ludwig and Miller (2007) point to different problems using cross validation. Therefore, we examine the sensitivity of our results to different bandwidths.

[20] As we argued earlier we cannot use 1976 because of a reform in the abortion system. For symmetry we also try 1979 as our second control group and obtain very similar results. We also present figures in the appendix using eligible mothers in 1974 as an additional robustness test.

Since there is no reform in 1975 $\hat{\alpha}_{RD,1975}$ should only capture month of birth effects (June vs. July birth). On the other end, $\hat{\alpha}_{RD,1977}$ confounds effects of the reform with potential month of birth effects. Under the relatively mild assumptions that the two effects do not interact, and that month of birth effects are the same (around July) for those born in 1975 and 1977, we can estimate the effect of the reform as $\hat{\alpha}_{RD-DD} = \hat{\alpha}_{RD,1977} - \hat{\alpha}_{RD,1975}$.[21]

We use the formulas in Porter (2003) for the standard errors of $\hat{\alpha}_{RD,1977}$ and $\hat{\alpha}_{RD,1975}$. In order to get the standard errors for $\hat{\alpha}_{RD-DD}$, we assume that $\hat{\alpha}_{RD,1977}$ and $\hat{\alpha}_{RD,1975}$ are independent (since these are completely different cohorts of children). We obtain similar results if instead we use the bootstrap, which relaxes independence.

Before we proceed to the next section it is important to clarify what questions we can and cannot answer with this empirical strategy. We can answer questions about the outcomes of children benefiting from different amounts of time with the mother early in life, induced by changes in maternity leave entitlements. However, maternity leave reform is about much more than that. For example, it may also affect fertility and labour supply decisions in the medium run, but the full adjustment of these behaviours to the new maternity leave regime is likely to happen slowly.

Therefore, we cannot fully learn about the outcomes of children living under different maternity leave regimes, since this would require waiting for the full adjustment of fertility and labour supply of women (and possibly their spouses). In fact, mothers of children born in both June and July of 1977 are likely to engage in the similar adjustments to fertility and labour supply in the medium run, especially if they are

[21] It is useful to examine graphs comparing outcomes of eligible mothers in 1977 with those of eligible mothers in 1975 and eligible mothers in 1979. The pre and post-reform trends are very similar.

considering having more children. What we can answer is a narrower question about the importance of the time that mothers spend with their children in their first year of life, which is the main difference in the early experiences of children born in June and July, 1977 (in the appendix we show that there are no differences in completed fertility and labour supply between these two groups).

$\alpha = E(y_i(1) - y_i(0))$ is an *intent-to-treat* estimate of the impact of being born in the new maternity leave regime. In addition to this it would be important to estimate the impact of the reform on the amount of time spent at home by mothers, which would give us an idea of the intensity of the treatment. As mentioned before we do not have direct measures of time worked each year in the data, but it is possible to infer this quantity from information on annual income. We discuss this in the next section and in Section 6.

4. Data description

Our data source is the Norwegian Registry data maintained by Statistics Norway. It is a linked administrative dataset that covers the population of Norwegians up to 2007 and is a collection of different administrative registers providing information about month and year of birth, educational attainment, labour market status, earnings, and a set of demographic variables (age, gender) as well as information on families. To ensure that all individuals studied went through the Norwegian educational system, we include only individuals born in Norway. We are able to link individuals to their parents, and it is possible to gather labour market information for both.

The main outcome variables we consider for children are dropout rates from high school, college attendance and earnings at age 30. In terms of educational attainment, we measure education at the oldest age possible for each individual, *i.e.*, in 2007.[22] High School dropouts are defined as all children not obtaining a three year high school diploma, and college attendance is defined from the annual education files identifying whether a person ever started college. Earnings are measured as total gross pension-qualifying earnings reported in the tax registry and are available from 1967 to 2007. These are not top-coded and include labour earnings, taxable sick benefits, unemployment benefits, and parental leave payments.

We also collect data on maternal income 2 and 5 years after taking birth. These are useful to examine possible channels through which the maternity leave may affect child outcomes, namely by promoting attachment of women to the labour market.

In the appendix we discuss the construction of additional outcome variables, which we use in our paper but they are not part of the main analysis. These are IQ and height (for males), teenage pregnancy (for females), place of residence, distance to grandparents, part time work for mothers and completed fertility of mothers.

In order to construct unpaid leave we start by calculating a measure of pre-birth monthly income by dividing 1976 earnings by 12. Then we calculate total earnings in 1977–1980, and divide them by 1976 monthly income, thereby obtaining a measure of number of months of unpaid leave during the first 36 months after birth. For this calculation to work, the assumption is that 1976 earnings are a good approximation for maternal potential post-birth earnings (the earnings she would get had she not gone on

[22] Our measure of child educational attainment is reported by the educational establishment directly to Statistics Norway, thereby minimizing any measurement error due to misreporting. This educational register started in 1970.

unpaid leave), adjusted for inflation.[23] We limit ourselves to a window of 36 months because the further away we move from pre-birth earnings, the more likely earnings may differ because of change of job, part time work, presence of new children, and other factors unrelated to the 1977 reform.[24] We assume that paid leave has a take-up rate of 100% for those giving birth after July 1977. In Section 6 and the appendix we provide evidence for our claim that there was full take-up of paid leave. We also argue that our estimates of unpaid leave are reasonable. Furthermore, in section 6 and the appendix we show that the estimated impact of the reform on unpaid leave is robust to whichever measure of leave we consider.

5. Results

5.1 Descriptive statistics

We focus only on mothers who are eligible for the reform, and therefore it is important to show how they compare to those who are not eligible. We saw from Figure 2 that the proportion of mothers who are eligible for maternity leave entitlements was about 65% in the year of the reform. This means that 35% of mothers and children giving birth in that year are not accounted for in our estimates of the impact of the reform on child outcomes, because the mother is not eligible for maternity leave. Interestingly, current labour force participation rates in OECD countries are generally not much higher than 65%, except in

[23] It is useful to illustrate with a specific example. If the child is born in June 1977 we subtract six months of 1976 monthly earnings from 1977 earnings and compare the remaining earnings in 1977 and 1978 to the 1976 earnings. If the mother earns half of 1976 earnings in the twelve months after birth she has taken six months of unpaid leave. If she earns nothing and takes all twelve months of leave we will continue and use earnings in 1979 and 1980 to construct leave up to 36 months after birth.

[24] However, remember that we will show that all these factors are the same for mothers giving birth before and after the reform, so they will potentially only affect the estimate of the level of unpaid leave and not the difference (effect of the reform).

the Scandinavian countries where they are often above 80%. Furthermore, roughly 25% of working women in the OECD are working only part-time.

Table 2 displays the main characteristics of eligible mothers and their children (born in 1977) as compared to those of ineligible mothers and their children. It is clear that eligible mothers are more highly educated than ineligible mothers. They are also more likely to be employed after birth than ineligible mothers, and as a consequence, their income is higher during that period. Their income 2 years before giving birth is 9 times larger than that of ineligible mothers, presumably because many of the latter do not work. Children of eligible mothers have lower high school dropout rates and higher college attendance rates, however similar earnings at age 30. Eligible and non-eligible mothers and their children are two very different groups. This means that we cannot safely extrapolate our findings to the latter group of mothers and their children.[25]

The average level of unpaid maternity leave taken at the time is quite high, even for those mothers having children before the reform is implemented. For our preferred measure, average unpaid leave is 8 months for those delivering their children before July 1977, and it barely changes for those delivering after this date. The 25th percentile is about 2 months, and the 75th percentile is about 11 months. Any expansion in the time mothers spend with their newborn children resulting from the reform is in addition to this pre-existing level of leave. The fact that unpaid leave did not change in response to the reform is robust to the measure of leave used, and depends solely on the fact that annual income is similar for mothers giving birth before and after the reform date, which means that both groups of mothers are taking the same amount of unpaid leave.

[25] For the narrower question of whether maternity leave is important for children of those mothers affected by the reform (eligible mothers) we have the right population.

Notice that, even if the reform leads to no change in family resources during the initial period of the child's life, it induces a slight change in the timing of these resources. Paid leave allows mothers to receive benefits right after their child is born, whereas unpaid leave does not. However, it is not likely that this change in the timing of benefits dramatically impacts child outcomes, unless we are under an extreme case of credit constraints. In order to investigate this further, in the appendix we present an analysis of the effects of the reform for mothers with different levels of pre-reform income. Poorer mothers are more likely to be credit constrained, so our idea is to use pre-reform income as an indicator of the severity of such constraints (which we find to be unimportant).

Before proceeding to the results, we would like to check whether the treatment and control groups are balanced in terms of the (pre-reform) characteristics we observe. Imbalance may indicate a threat to the validity of our method since it would indicate the possibility that a non-random set of mothers manipulate the date of birth of their children (see Gans and Leigh, 2009). The various panels of Figure 5 show how observable pre-reform characteristics of mothers vary with the month they gave birth in, and allow us to check whether they are identical for mothers having children just before and just after the reform. Maternal years of education, age at birth and income in 1975 are stable across birth months and we see no discontinuity after July 1st, 1977. In addition, there is also no discontinuity in the urban location of the parents in 1976 and the distance to grandparents in 1980 (although this variable is only available in 1980). Moreover, Figure A1 in the Appendix shows very similar numbers of births just before and after the reform was implemented. In sum, selective manipulation of month of birth is not likely to be a serious concern in our data. This is quite reasonable given that in 1977 (and even today) it was not easy to delay childbirth much beyond the due date.

5.2 Children's outcomes

In table 3 we present estimates of the impact of the reform on a set of children's outcomes.[26] The first column shows the RD results while the second column presents the DD results using the cohorts born in 1975 as a control group. In the first column we see a negative effect of the reform of about 2 percentage points in children's dropout rates, however this variable is only significant at the ten percent level. When taking into account potential month of birth effects in the DD specifications in column 2 we see an increase in the effect to 2.7 percentage points (because the month of birth effect is negative in 1975). We see the same pattern for college attendance: an increase of 3.6 percentage points, which is only significant in the DD specification. In addition we see a positive effect on earnings at age 30 of 4.8 % which increases to 5.5 % in the DD specification.[27][28] In Figure 6 we present graphically the results corresponding to the second column in Table 3 (Appendix Figure A2 shows the single difference results.)[29] We clearly see that the reform induced discontinuities (that do not occur in 1975) in

---

[26] See Appendix, Table A3, for additional outcomes, namely IQ for males, and teenage pregnancy for females, for which we have less robust results. See also Table A2, for a comparison of results using the total sample. Those results compare well with the sample of eligible mothers, although they are weaker.

[27] Interestingly, in the appendix, Table A3, there is also a positive effect on IQ. IQ scores are only available for men, but due to the large sample sizes we can still get precise estimates of the effect on the reform on IQ. The RD shows an effect of 0.11, or 5% of a standard deviation. This effect is around 0.24 in column 2 which is 12% of a standard deviation. Using estimates of the effect of IQ on wages from wage regressions estimated on slightly older cohorts of individuals, this translates into more than a 1% in difference in earnings as an adult. We do not see any effect of the reform on teenage pregnancy in any of the specifications. In Table A4 in the Appendix we report results with a bandwidth of 5 corresponding to more smoothing of the data. We see the same patterns in coefficients however the results are weaker, especially for the RD results. This can be a feature of the possible effect of birth month on outcomes hence we will focus on differences-in-differences for the rest of the paper.

[28] See the end of Appendix A for additional robustness tests.

[29] In addition we present DD figures using 1974 and 1979 as control groups in the Appendix, Figure A3 and A4. These figures show that results are very robust, the effects are even slightly higher and more persistent than when using 1975.

dropout rates and earnings at age 30 as a function of month of birth.[30] We also see that there are monthly trends for the different outcomes. The effects on dropout rates are present for all birth months after the reform and for the most part this is also the case for earnings. The effect on college attendance is not as robust. Therefore, most of the impact of the reform seems to be at the low end of the education distribution, with treated children dropping out less from high school and this show up in higher returns on earnings at age 30.[31]

6. Interpretation of empirical results and suggestive mechanisms

In the previous section we established that the maternity leave reform had a substantial impact on schooling and earnings of children. In this section we attempt to understand the mechanisms by which this happened, using limited information from the administrative records we use. The results we present in this section are not individually decisive, but together they tell a consistent story.

6.1 Time with the child

The main problem of our dataset is that it does not have a direct measure of maternal labour supply nor of leave taking behaviour. So how can we be confident that the reform is significantly affecting leave taking behaviour by mothers?

First, Rønsen and Sundström (1996) show that for the 1968-1988 mothers in Norway, almost no one returned to work before 4 months after birth. Secondly, in a survey conducted in 1977 on fertility behavior of women in Norway (Statistics Norway),

[30] We also see less robust patterns for the outcomes relegated to the appendix, Figure A5: IQ, and teenage pregnancy.
[31] It is worthwhile pointing out that if we use earlier measures of earnings we cannot detect this effect. It is important to wait until individuals have reached some maturity in the labour market.

60% of respondents answered that they thought mothers should stay home for the first 2 years after giving birth to a child. In addition, the coverage was 100%, which gives strong incentives for full take up. Third, since we observe days of paid leave after 1992 we are able to check to what extent eligible mothers take up this benefit, and how the take up reacts to subsequent reforms in 1992 and 1993 (see Appendix, Figure A6, showing the following description). Before the April 1992 reform, mothers are able to take 224 days at full coverage or 280 days at 80% coverage. For mothers delivering children in March of 1992, the average take up of paid leave was 250 days. After April 1992 there is an increase in maternity leave entitlements to 245 days of full coverage or 310 days of 80% coverage. We observe that average paid leave taken was 275 days for mothers of those born in April 1992. This figure is slightly higher at 280 in March 1993, just before the 1993 reform which increased paid leave to 266 days of full coverage or 336 days of 80% coverage. By April of 1993 average leave taken was almost 310 days. Given the high levels of leave and strong reactions to reforms, it is reasonable to assume that the take up of paid leave is close to 100%.[32]

Therefore, we are confident that after the 1977 reform all mothers were taking 4 months of paid leave. So the follow-up question is: what was the change in unpaid leave as a result of the reform? One way we can answer this question is by studying what happened to maternal income before and after the reform.[33] An increase in maternal income in the period right after birth may indicate a reduction in unpaid leave taken, and the opposite could be inferred from a decrease in maternal income (perhaps in

[32] We should also point out that the analyzes of other reforms in other countries for which there is data available on labor supply of mothers all indicate a substantial increase in the amount of leave taken after each reform.

[33] Remember that all maternity benefits are part of our measure of income.

substitution of the additional paid leave mothers become entitled to). We examined maternal income in the years surrounding the reform for those delivering children just after and before the reform and we found no impact of the reform on these variables. This is shown in table 4, and it indicates that there was no change in unpaid leave taken by mothers. This is true independently of the measure of earnings we take: income in 1977, average income between 1976 and 1978, or average income between 1975 and 1979.[34] This is true not only of the mean, but of the whole distribution of income.

In addition, as discussed above, using this data it is possible to predict how much unpaid leave was taken by each mother, by comparing her usual earnings in a year with no childbirth to earnings in a year (and subsequent years) with one.

We find no effects of the reform on the amount of unpaid leave taken by mothers as shown in the first column of Table 5. This is not surprising since we emphasized before that there is no change in average annual income for mothers giving birth just before and just after the date of the reform, independently of the measure of earnings we take.

In summary, this means that, whatever the measure of unpaid leave is, there is no change in the amount of unpaid time taken off work for mothers giving birth before or after the reform, otherwise there would be an increase in their income. Therefore, even if our measure of unpaid leave is not exactly right, we can be confident that there is no large change in unpaid leave as a result of the reform. We can rule out any responses that vary more than within one month so this reform was mostly about more paid leave which since it is fully covered means no effect on income. Even with no average response in unpaid

[34] Note that the small significant effect on income at year of birth in the RD result is only a month effect of giving birth to the child later in the year and have more months to work before giving birth. When controlling for birth month using eligible mothers in 1975 there is no effect on income year of birth.

leave it is interesting to see if there are any effects across the distribution of unpaid leave. In the Appendix, Figure A7, we see no such responses. We cannot rule out that not all mothers took 4 months of paid leave, although the earlier evidence provided in this paper shows that this was likely the case (Statistics Norway, fertility survey of 1977).

6.2 Maternal Labour Market Outcomes

It is possible that the reform increased labour market attachment of mothers. This is because of the extensive job protection they became entitled to, which allowed them to come back to their old job long after they gave birth. Therefore, it is conceivable that children born in the post-reform period had better outcomes not only because they spent more time with their mothers, but also because their mothers became more attached to the labour market in the medium and long run, thereby being able to generate more income but also spending more time at work.

Table 5 shows our main results. We do not find any long term effects of the reform on mother's employment two and five years after it took place, or on earnings[35] five years after. This supports the idea that our estimates of the impact of the reform on children's outcomes can be directly related to mother's time investments in the child during its first year of life.

In Figure 7 we present the differences in differences results of Table 5 graphically. The figures confirm the results of the table. There is no discontinuity in long term labour market outcomes.

6.3 Maternal Education

[35] We have also played around with mother's earnings between one and ten years after birth and this gives similar results of no long term effect on income.

We check whether the maternity leave extension had a different effect on mothers with different educational backgrounds.[36] We split the sample in two; mothers with less than 10 years of education versus mothers with 10 years or more of education. We see, from the last two columns of Table 6, that the effects on mothers are very similar for the two groups: there is no effect on unpaid leave and no significant effects on the long term labour market outcomes. For children we see that the fall in dropout rate is 5.2 percentage points for children of mothers with less than 10 years of education while it is around 2 percentage points for children of higher educated mothers. The pattern is similar for earnings at age 30. However, none of these differences across maternal education groups are significantly different. We can still take them as suggestive given their magnitude, and the fact that the effect of the reform is larger at the bottom of the maternal education distribution is consistent with the fact that the most robust effect of the reform is on high school dropout rates, which is a fairly low qualification.

6.2 Results by quartiles of mother's unpaid leave.

Table 7 presents results on mother's and children's outcomes by quartiles of unpaid leave. In principle this variable should be affected by the reform and therefore we should not condition on it. In practice, we saw that the reform has no effect on unpaid leave. Furthermore, if the ranking of mothers in terms of unpaid leave does not depend on the reform, we can interpret these estimates as the effects of the reform for mothers who would take different levels of unpaid leave in the absence of the reform.

We see no effect on mother's outcomes at any quartile.[37] This indicates a substantial increase in mother's time spent at home across the distribution of eligible

[36] In the appendix, Table A5, we present results by distance to grandparents and centralization.
[37] This happens because the distribution of annual income is roughly the same for mothers giving birth just before and after the reform

mothers (since paid leave has increased for all of them). For children we see that the effect on dropout rates is very large for the first and second quartiles, with 9 and 5 percentage points respectively, while we see no effect in the third and fourth quartiles. This is also confirmed by the earnings results which suggest around 10 % higher earnings in the first and second quartile and no effect in the last two quartiles.

Mothers in the first two quartiles have levels of unpaid leave much below the average (0.4 and 5.1 months, respectively). The fact that it is for these mothers that we see the largest effects on dropout rates and earnings (the outcomes for which our results are the most robust) suggests that additional time with the child is mainly important during the earliest months of the child's life. It is possible that these differences do not come entirely from increases in health (say, due to breastfeeding; see also the evidence discussed in Appendix B). There may also be an impact on maternal-child attachment and less stress in the home, leading to changes in personality traits that make these children less likely to drop out of high school.

6.3 Any substantial differences in the impact of the reform according to other criteria?

We have checked and found no differences in the effect of the reform according to pre-reform family income and the state of the local labour market at the time of birth.[38] In contrast to maternal education, these are relatively short run measures of household environments. Additional time with the child does not seem to be especially important for dropout rates of children born in very poor households, unless they are also born in households where mothers have low levels of maternal education.

Above we mentioned that the reform could also have an effect by shifting the availability of income towards those months right after birth, even if there is no change in

[38] See Appendix, Table A6, for results by quartiles of family income.

total income. If some households are severely credit constrained this may make a difference to the child. According to our results, this is unlikely to be the case, if those with low levels of pre-reform income are the most likely to be credit constrained.

In addition we studied completed fertility and marital stability of mothers to the children affected by the reform. We see no effects on any of these outcomes when the children are age 30 (see Appendix, Table A7).

We also analyzed the impact of the reform on older siblings (see Appendix, Table A8). The fact that mothers spend additional time in the home could benefit other siblings as well. However, this is not the case, which suggests that what drives the impact of the reform is specific to the relationship between the mother and the newborn child (perhaps because of a stronger attachment between the two, with benefits for mother and child). In addition, we did not find any difference in dropout rates by gender of the child; although the effect on wages at age 30 is driven by males (see Appendix, Table A9).

6.4 A simple model of the high school dropout decision

Finally, we studied the determinants of the dropout decision. We use it to understand the impact of the reform relatively to that of other variables, and to understand how the impact of other variables changes as a result of the reform.

We started by running a regression of whether an individual is a high school dropout on years of mother's education (measured in 1980), mother's age at birth, whether the mother is married (in 1980), family size, log of the present value of the sum of mother's and father's income between the ages of 0 and 13, and whether the child was born in an urban area. In addition, we included IQ and height, which means that we only estimated this model for males (notice that throughout the paper we did not find differential impacts of the reform by gender). We used a linear probability model on the sample of all males born in 1975 or 1977 to a mother eligible to maternity leave (*i* denotes individual, *t* denotes year of birth):

$$Dropout_{it} = \beta_0 + \beta_1 Ability_{it} + \beta_2 Height_{it} + \beta_3 Mother'sEducation_{it} + \beta_4 AgeatBirth_{it} + \beta_5 Married_{it} + \beta_6 FamilySize_{it} + \beta_7 TotalIncome_{it} + \beta_8 Urban_{it} + \varepsilon_{it}$$

Estimates from this model are shown in the first column of table 8. Dropout rates are lower by: 6.6 percentage points (pp) for each additional ability point; 0.2 pp for each centimetre in height; 1.3 pp for each year of maternal education; 0.3 pp for each year of age at birth of the mother; 12 pp for having a married rather than an unmarried mother; 1.3 pp for a reduction of one in family size; 3.8 pp for a doubling of total maternal and paternal income; and 1.6 pp for being in a rural rather than in an urban area. These are substantial effects, and apart from the urban coefficient, they are largely unsurprising.

In order to understand how the reform affects the dropout decision we start by adapting the empirical strategy laid out in section 4 to this parametric model. We add to the regression of table 8 a parametric function of month of birth (MB, normalizing July = 0, so December = 5 and January = -6), a dummy for being born in 1977 (Y77), and a dummy for being born in July (REFORM), to approximate the nonparametric regression discontinuity estimator of section 4 with a parametric model:

$$\begin{aligned}\mathrm{Dropout}_{it} &= \beta_0 + \beta_1\mathrm{Ability}_{it} + \beta_2\mathrm{Height}_{it} + \beta_3\mathrm{Mother'sEducation}_{it} + \beta_4\mathrm{AgeatBirth}_{it} + \beta_5\mathrm{Married}_{it} \\ &+ \beta_6\mathrm{FamilySize}_{it} + \beta_7\mathrm{TotalIncome}_{it} + \beta_8\mathrm{Urban}_{it} + \gamma_1\mathrm{MB}_{it} + \gamma_2\mathrm{MB}_{it}^2 + \gamma_3\mathrm{REFORM}_{it} + \gamma_4(\mathrm{MB}_{it}*\mathrm{REFORM}_{it}) \\ &+ \gamma_5(\mathrm{MB}_{it}^2*\mathrm{REFORM}_{it}) + \gamma_6\mathrm{Y77}_{it} + \gamma_7(\mathrm{MB}_{it}*\mathrm{Y77}_{it}) + \gamma_8(\mathrm{MB}_{it}^2*\mathrm{Y77}_{it}) + \gamma_9(\mathrm{MB}_{it}*\mathrm{Y77}_{it}*\mathrm{REFORM}_{it}) \\ &+ \gamma_{10}(\mathrm{MB}_{it}^2*\mathrm{Y77}_{it}*\mathrm{REFORM}_{it}) + \eta(\mathrm{Y77}_{it}*\mathrm{REFORM}_{it}) + \varepsilon_{it}\end{aligned}$$

The effect of the reform is given by η.

Estimates of this model are shown in the second column of table 7. The effect of the reform is a bit larger (5.5%) than in our original results, perhaps because we have additional controls, or perhaps because of the parametric method.

Notice that we control for two variables that are possibly affected by the reform: ability and height. Therefore, if anything this parametric model is understating the effect of the reform. However, it is striking that the coefficients on these two variables are essentially unchanged from column 1 to column 2 of this table. This says that even if there is an effect of the reform on ability and height, it is not substantial enough to change the coefficients on these variables. Furthermore, it says that the large effect of the reform on dropout rates does not occur primarily through a change in IQ or height, but through a change in another type of skill, perhaps a non-cognitive skill. It is not surprising that there is no change in the coefficients in the other controls since they are orthogonal to month and year of birth.

Finally, we interact *(Y77*REFORM)* with all the controls (after demeaning, denoted by D), so the reform can change the way the controls affect the dropout decision:

$$
\begin{aligned}
&\mathrm{Dropout}_{it} = \beta_0 + \beta_1 \mathrm{Ability}_{it} + \beta_2 \mathrm{Height}_{it} + \beta_3 \mathrm{Mother'sEducation}_{it} + \beta_4 \mathrm{AgeatBirth}_{it} + \beta_5 \mathrm{Married}_{it} \\
&+ \beta_6 \mathrm{FamilySize}_{it} + \beta_7 \mathrm{TotalIncome}_{it} + \beta_8 \mathrm{Urban}_{it} + \gamma_1 \mathrm{MB}_{it} + \gamma_2 \mathrm{MB}_{it}^2 + \gamma_3 \mathrm{REFORM}_{it} + \gamma_4 (\mathrm{MB}_{it} * \mathrm{REFORM}_{it}) \\
&+ \gamma_5 (\mathrm{MB}_{it}^2 * \mathrm{REFORM}_{it}) + \gamma_6 \mathrm{Y77}_{it} + \gamma_7 (\mathrm{MB}_{it} * \mathrm{Y77}_{it}) + \gamma_8 (\mathrm{MB}_{it}^2 * \mathrm{Y77}_{it}) + \gamma_9 (\mathrm{MB}_{it} * \mathrm{Y77}_{it} * \mathrm{REFORM}_{it}) \\
&+ \gamma_{10} (\mathrm{MB}_{it}^2 * \mathrm{Y77}_{it} * \mathrm{REFORM}_{it}) + \eta_0 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it}) + \eta_1 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DAbility}_{it}) \\
&+ \eta_2 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DHeight}_{it}) + \eta_3 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DMother'sEducation}_{it}) \\
&+ \eta_4 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DAgeatBirth}_{it}) + \eta_5 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DMarried}_{it}) \\
&+ \eta_6 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DFamilySize}_{it}) + \eta_7 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DTotalIncome}_{it}) \\
&+ \eta_8 (\mathrm{Y77}_{it} * \mathrm{REFORM}_{it} * \mathrm{DUrban}_{it}) + \varepsilon_{it}
\end{aligned}
$$

Since we demean the controls before interacting them with *(Y77*REFORM)* we can read the average effect of the reform from the coefficient on *(Y77*REFORM)*. The impact of each control variable on dropping out of high school for those not affected by the reform can be read from the coefficient on the controls. The impact of each variable for those affected by the reform is obtained by adding the coefficient on the variable with the coefficient on the interaction.

Results are displayed in the third column of table 8. Notice that, once again, there is hardly any change in the effects of each of the control variables for those not benefiting from the reform. When we look at those affected by the reform, here is little change on the coefficients on ability, height, maternal education, and log total family income. There are a few changes on the coefficients on maternal age at birth (amplifying its effect) and maternal marital status (dampening the effect), and both remain statistically significant. However, there is substantial dampening of the effects of family size and being born in an urban area, which become insignificant for those benefiting from the reform.

Even though this is a reduced form model for the dropout decision, in interpreting these results it is natural to think of returns and costs to high school graduation. Although we can only speculate about it, we believe that it is unlikely that the reform is changing much the returns to a high school diploma. These returns should be affected by most of the control variables, especially ability and maternal education, and we see no general pattern of interactions of the reform with all variables, let alone one these two in particular. If we think about costs, we see the main impacts of the reform on urban status and family size. Once again, we can speculate that the change in the urban coefficient is another indication that the reform is operating through non-cognitive skills, if the reason why urban children are more likely to drop out of high school is because they are exposed to and tempted to engage in a wider variety of risky behaviours than those living in rural areas. The existence of a family size – and inexistence of a family income - reform interaction may indicate that the effect of family resources on dropout rates is changed by the reform but that it is not financial resources. Instead, it could be time resources, which decrease on a per-capita basis as the number of children increases and cannot be adjusted as easily as financial resources. This makes sense given the nature of the reform, which is essentially increasing time available for activities with children.

7. Concluding remarks

We investigate the long term consequences of time investments in children during their first year of life using a maternity leave reform in Norway, offering up to 4 months of paid leave and an additional 1 year of unpaid leave, which shows substantial positive effects of having mother at home, compared to informal care alternatives. 2.7 percent more children complete high school (and 5 % higher earnings at age 30), going up to 5.2 percent (8 % higher earnings) for those whose mothers have less than 10 years of education.

The alternative for staying home with mothers around the time of the reform is crucial to understand the results. There was almost no available high quality child care for under-two year olds available so the alternative was grandparents or other informal care which is not necessarily a good substitute to mother's time at this period of a child's life. Note that this was different for the two papers from the Nordic countries using registry data. In addition, the Swedish reform for instance was an extension from one year to almost a year and a half, while the Norwegian reform was a reform for much younger children and biting most for mothers taking short leaves. The positive effect of early investments in children on medium to long term outcomes also resembles the relatively large effects found recently from other early investments in children such as the Perry programme and the project STAR (Chetty, Friedman, Hilger, Saez, Schanzenbach and Yagan, 2010; Heckman, Moon, Pinto, Savelyev and Yavitz, 2010).

For policy implications we conclude that fostering policies to increase parents' time with children the first year after birth may have an impact on children's abilities later in life. This effect has been an important part of the goals behind expansions in maternity leave across countries; however this study is the first to show that this may actually be achieved. The situation with maternity leave is remarkably similar in the US today as it was in Norway before the reform. Parental leave is currently under debate in the US[39] and an introduction of 4 months of paid leave and better job protection are typically within feasible policies.[40] Using the rich set of family background variables to address heterogeneity of effects also gives us the advantage of making the study less dependent on institutional settings in Norway. For example by showing that the effects are bigger for children from lower educated households this may be important for policy discussions related to lowering inequalities in general. Many countries, like the US, Britain, and South America have a substantial inequality in education and income. While increasing maternity leave for women and men in these countries will not solve these problems we have shown that it might reduce the existing gap.

[39] USA today July 26th 2005, The New York Times April 16th 2008

[40] http://www.govtrack.us/congress/bill.xpd?bill=h110-3799

References

BAKER, M., and K. MILLIGAN (2008a): "Maternal Employment, Breastfeeding, and Health: Evidence from Maternity Leave Mandates." *Journal of Health Economics*, 27, 871-887.

— (2008b): "Evidence from Maternity Leave Expansions of the Impact of Maternal Care on Early Child Development." NBER Working Paper Series, 13826.

BERNAL, R. (2008): "The Effect of Maternal Employment and Child Care on Children's Cognitive Development." *International Economic Review*, 49, 1173-1209.

BERNAL, R., and M. P. KEANE (2010): "Quasi-Structural Estimation of a Model of Child Care Choices and Child Cognitive Ability Production." *Journal of Econometrics*, 156, 164-189.

BLACK, S., P. J. DEVEREUX, and K. SALVANES (2008): "Too Young to Leave the Nest: The Effects of School Starting Age." NBER Working Paper No. 13969.

BLAU, D., and J. CURRIE (2006): "Pre-School, Day Care, and after-School Care: Who's Minding the Kids?" *Handbook of the economics of education*, 2, 1163-1278.

BROOKS-GUNN, J., W. J. HAN, and J. WALDFOGEL (2010): "First-Year Maternal Employment and Child Development in the First 7 Years." *Monographs of the Society for Research in Child Development*, 75, 147.

CAMERON, A. C., and P. K. TRIVEDI (2005): *Microeconometrics: Methods and Applications*. Cambridge Univiversity Press.

CHENG, M. Y., J. FAN, and J. S. MARRON (1997): "On Automatic Boundary Corrections." *The Annals of Statistics*, 25, 1691-1708.

CHETTY, R., J. N. FRIEDMAN, N. HILGER, E. SAEZ, D. W. SCHANZENBACH, and D. YAGAN (2010): "How Does Your Kindergarten Classroom Affect Your Earnings? Evidence from Project Star." NBER Working Paper No. 16381.

CRONBACH, L., and J. LEE (1964): *Essentials of Psychological Testing, 2nd Edition.*: London, UK: Harper and Row, 1964.

DUSTMANN, C., and U. SCHÖNBERG (2008): "The Effect of Expansions in Maternity Leave Coverage on Children's Long-Term Outcomes." IZA Discussion Papers No. 3605.

FAN, J. (1992): "Design-Adaptive Nonparametric Regression." *Journal of the American Statistical Association*, 87, 998-1004.

GANS, J. S., and A. LEIGH (2009): "Born on the First of July: An (Un) Natural Experiment in Birth Timing." *Journal of Public Economics*, 93, 246-263.

GREGG, P., and J. WALDFOGEL (2005): "Symposium on Parental Leave, Early Maternal Employment and Child Outcomes: Introduction." *The Economic Journal*, 115, 1-6.

GREGG, P., E. WASHBROOK, C. PROPPER, and S. BURGESS (2005): "The Effects of a Mother's Return to Work Decision on Child Development in the Uk*." *The Economic Journal*, 115, 48-80.

HAHN, J. Y., P. TODD, and W. VAN DER KLAAUW (2001): "Identification and Estimation of Treatment Effects with a Regression-Discontinuity Design." *Econometrica*, 69, 201-209.

HECKMAN, J., S. H. MOON, R. PINTO, P. SAVELYEV, and A. YAVITZ (2010): "Analyzing Social Experiments as Implemented: A Reexamination of the Evidence from the Highscope Perry Preschool Program." *Quantitative Economics*, 1, 1-46.

IMBENS, G., and T. LEMIEUX (2008): "Special Issue Editors' Introduction: The Regression Discontinuity Design - Theory and Applications." *Journal of Econometrics*, 142, 611-614.

LIESTØL, K., M. ROSENBERG, and L. WALLØE (1988): "Breast-Feeding Practice in Norway 1860–1984." *Journal of biosocial science*, 20, 45-58.

LIU, Q., and O. N. SKANS (2010): "The Duration of Paid Parental Leave and Children's Scholastic Performance." *The BE Journal of Economic Analysis & Policy*, 10.

LUDWIG, J., and D. L. MILLER (2007): "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design." *Quarterly Journal of Economics*, 122, 159-208.

OECD (2008): "Statistics on Labour Force Participation." OECD, Paris.

PORTER, J. (2003): "Estimation in the Regression Discontinuity Model." Unpublished Manuscript, Department of Economics, University of Wisconsin at Madison.

RASMUSSEN, A. W. (2010): "Increasing the Length of Parents' Birth-Related Leave: The Effect on Children's Long-Term Educational Outcomes." *Labour Economics*, 17, 91-100.

ROSSIN, M. (2011): "The Effects of Maternity Leave on Children's Birth and Infant Health Outcomes in the United States." *Journal of Health Economics*, 30, 221-239

RØNSEN, M., and M. SUNDSTRÖM (1996): "Maternal Employment in Scandinavia: A Comparison of the after-Birth Employment Activity of Norwegian and Swedish Women." *Journal of Population Economics*, 9, 267-285.

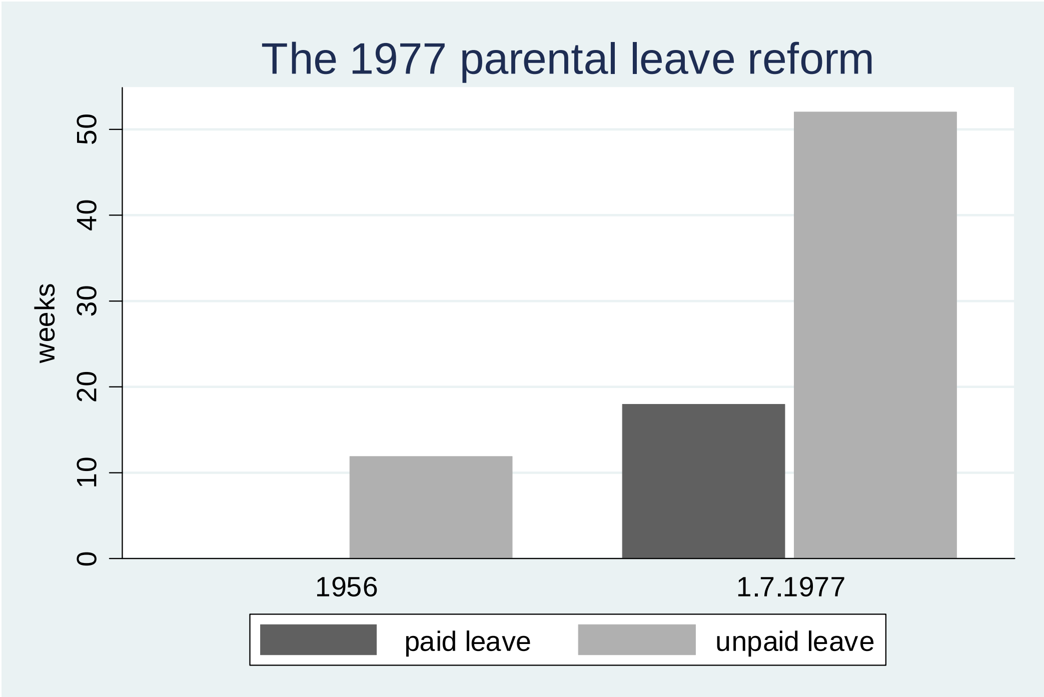
— (2002): "Family Policy and after-Birth Employment among New Mothers–a Comparison of Finland, Norway and Sweden." *European Journal of Population/Revue europeenne de demographie*, 18, 121-152.

SUNDET, J. M., D. G. BARLAUG, and T. M. TORJUSSEN (2004): "The End of the Flynn Effect?:A Study of Secular Trends in Mean Intelligence Test Scores of Norwegian Conscripts During Half a Century." *Intelligence*, 32, 349-362.

SUNDET, J. M., K. TAMBS, J. R. HARRIS, P. MAGNUS, and T. M. TORJUSSEN (2005): "Resolving the Genetic and Environmental Sources of the Correlation between Height and Intelligence: A Study of Nearly 2600 Norwegian Male Twin Pairs." *Twin Research and Human Genetics*, 8, 307-311.

TANAKA, S. (2005): "Parental Leave and Child Health across Oecd Countries." *Economic Journal*, 115, 7-28.

Figure 1
The 1977 reform



Source: regjeringen.no, lovdata.no

Figure 2
Proportion of mothers eligible for maternity leave

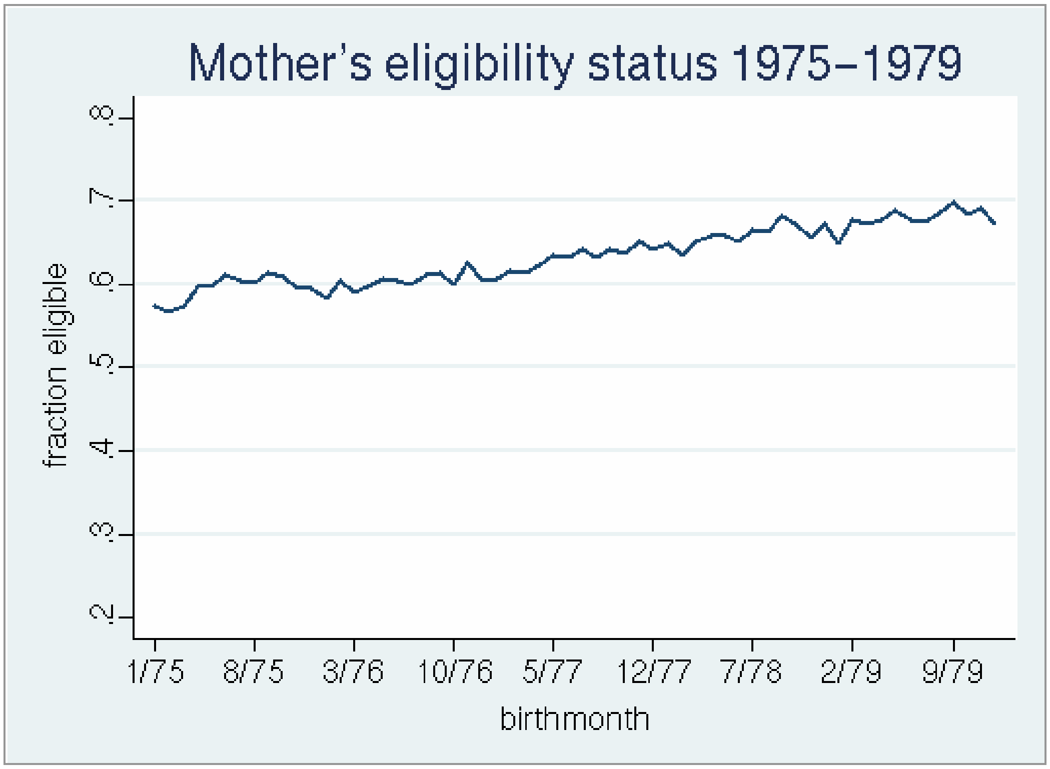
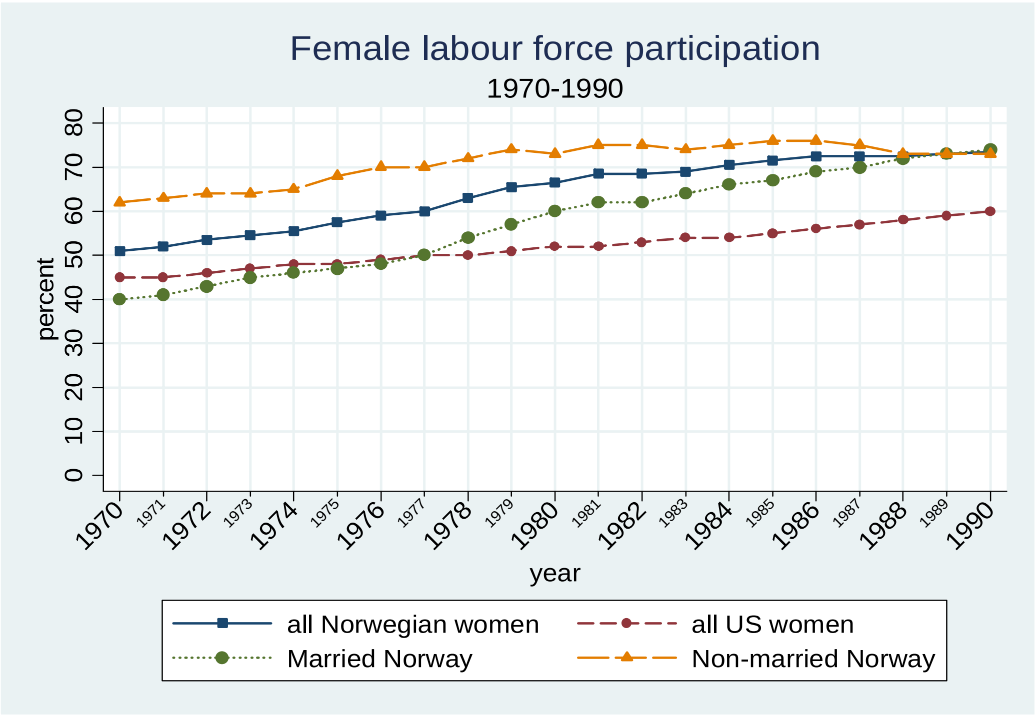
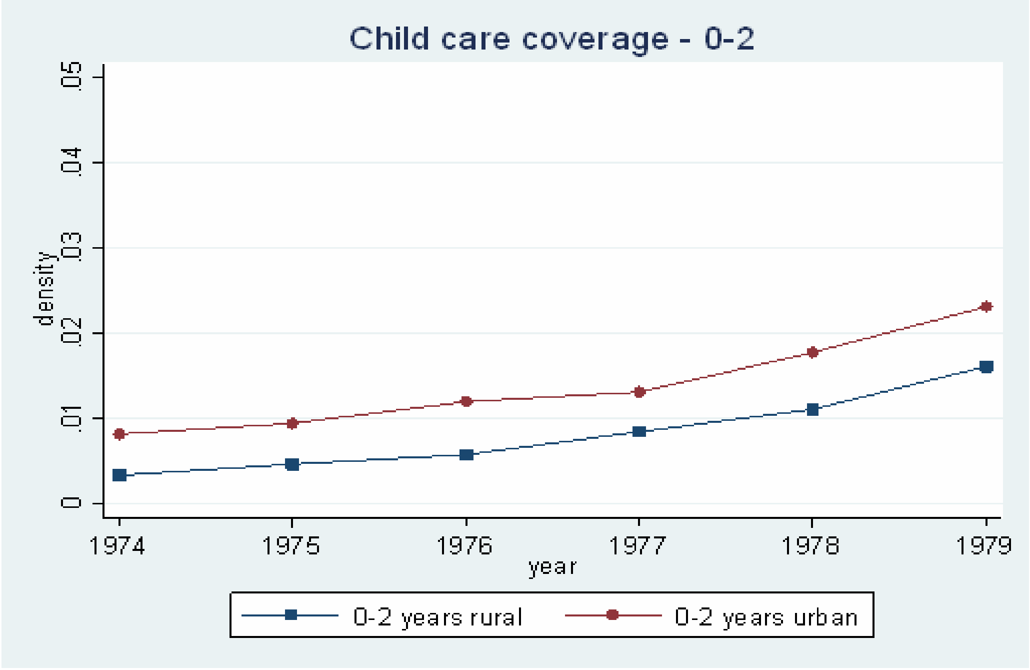


Figure 3
Female employment in Norway and the US 1970-1990



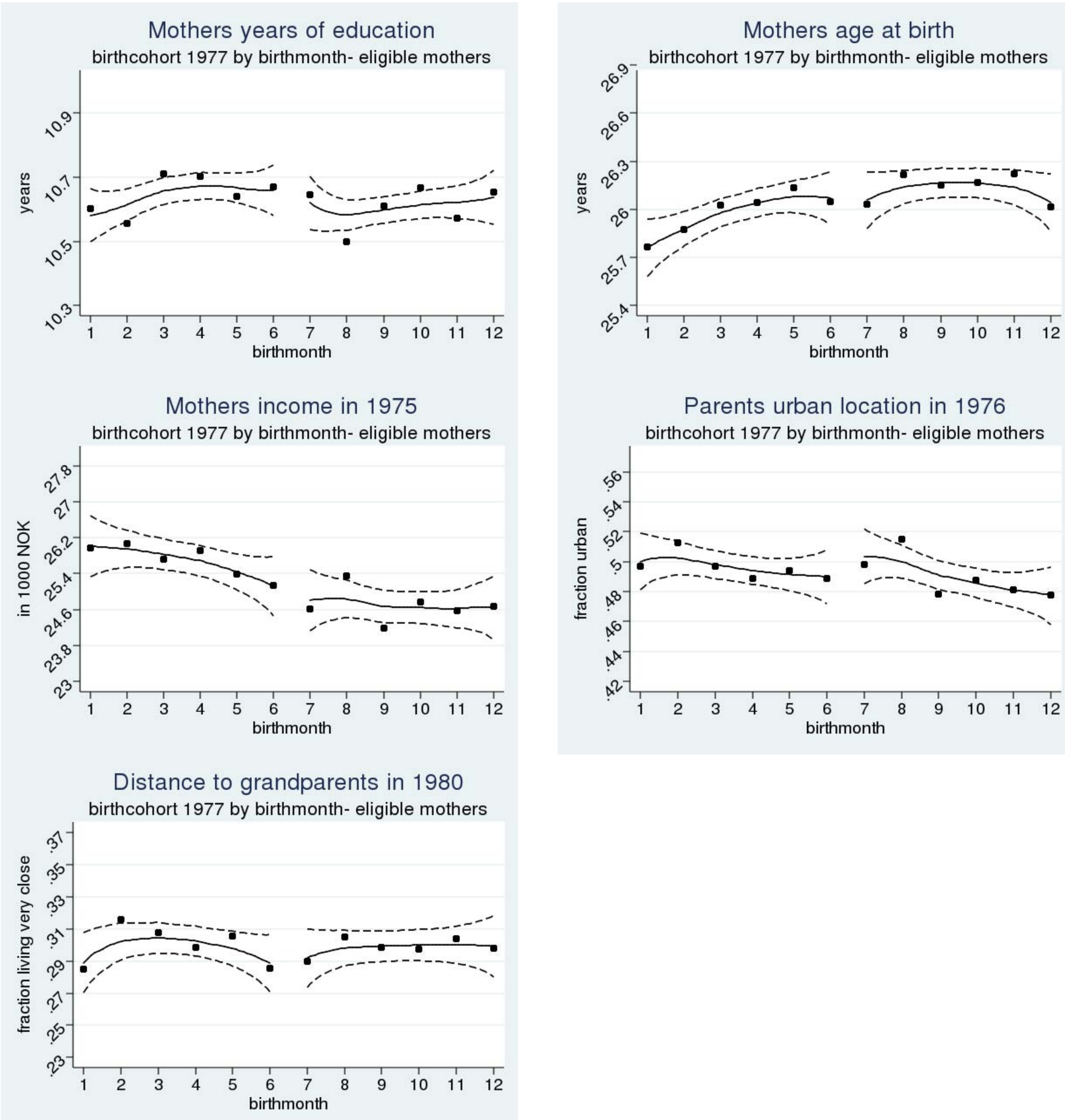
Source: Statistics Norway, Bureau of Labor Statistics (projected from Population Bulletin, Vol 63 (2008), OECD

Figure 4
Day-care coverage in Norway split by age and urban-rural areas



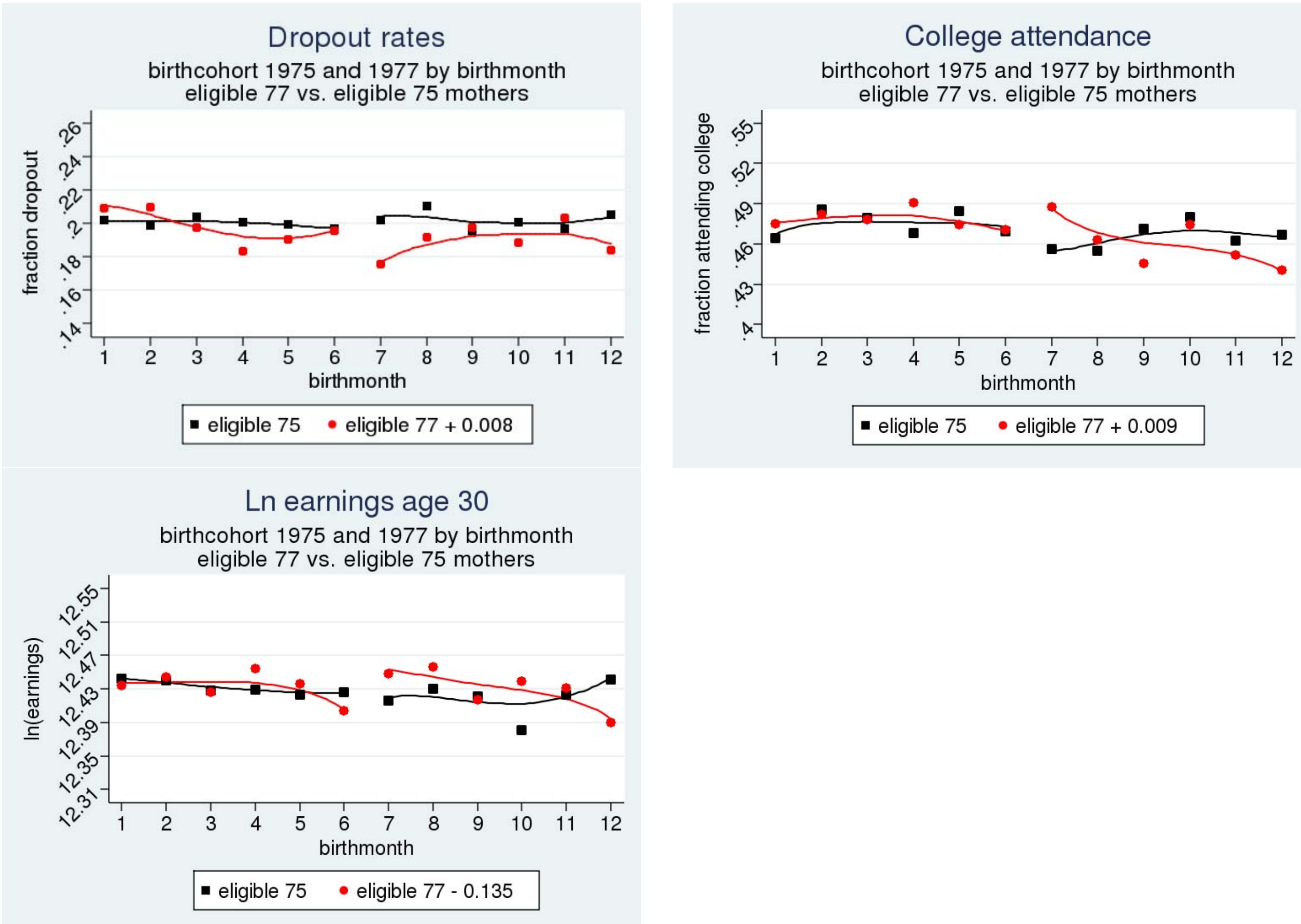
Data source: NSD municipality

Figure 5
Pre-reform characteristics



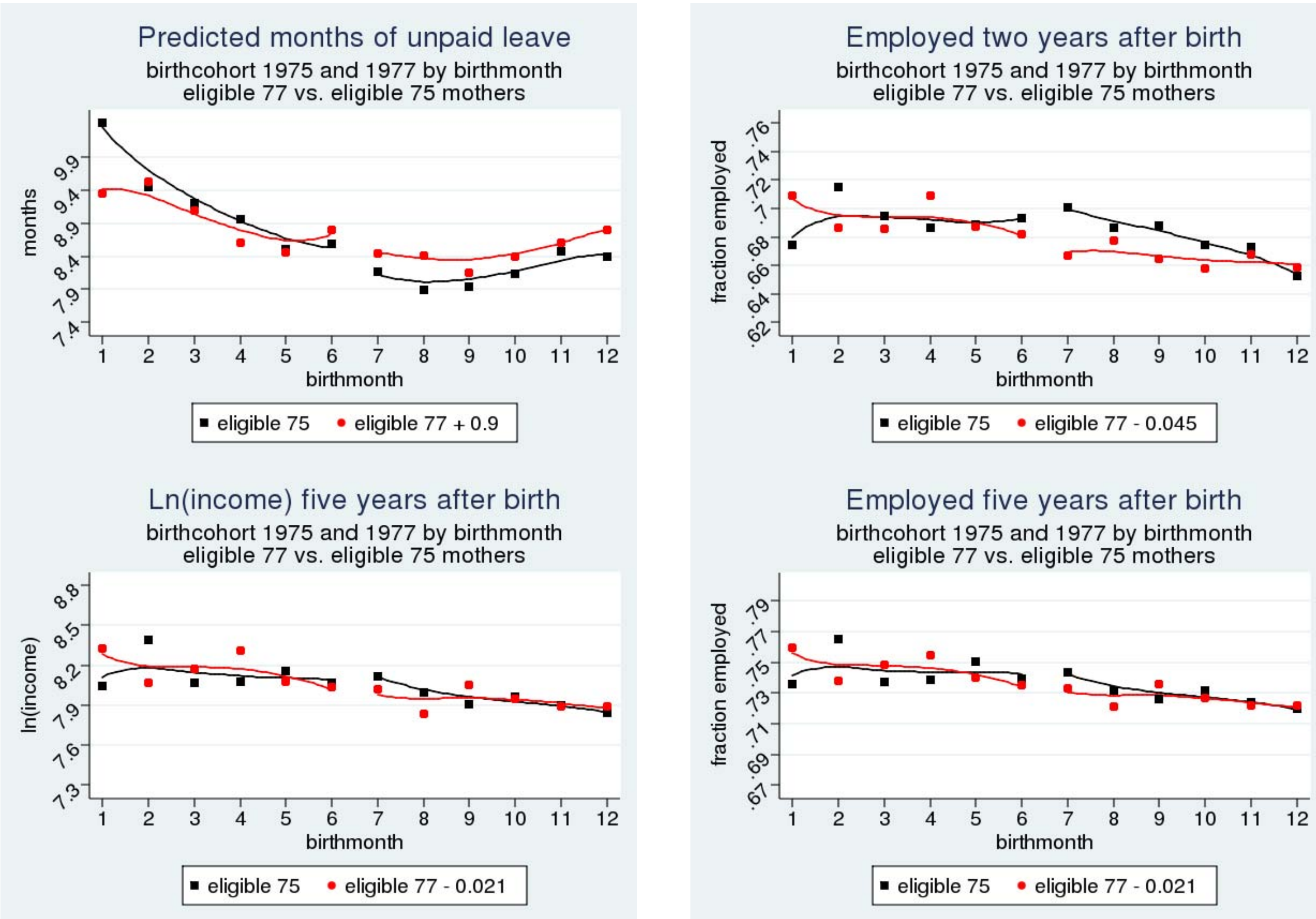
Note: Each graph shows the estimated mean for mother's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three and the dashed lines are the corresponding 95 % confidence intervals. The y-axis includes outcomes within +/- .15 of a standard deviation around the mean.

Figure 6
Children's outcomes by birth month, eligible mothers 1977 versus 1975



Note: Each graph shows the estimated mean for children's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three. The y-axis includes outcomes within +/- .15 of a standard deviation around the mean.

Figure 7
Mother's outcomes by birth month, eligible mothers 1977 versus 1975



Note: Each graph shows the estimated mean for mother's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three. The y-axis includes outcomes within +/- .15 of a standard deviation around the mean.

Table 1
Parametric regressions – using only children born in June and July

Birth month	Single Difference	Differences-in-differences using 1975 as controls
Children		
High School Dropout	-.020*	-.025*
	(.011)	(.016)
College Attendance	.094	.131
	(.069)	(.098)
Log Earnings at Age 30	.045**	.055*
	(.022)	(.031)
Mothers		
Pre-Reform Characteristics		
Years of Education	-.023	-.013
	(.063)	(.088)
Log Income Two Years Prior to the Birth of the Child	-.014	.027
	(.031)	(.040)
Outcomes		
Average Log Income in the Year of Birth and the Year After Birth	.148*	-.030
	(.080)	(.116)
Employed 5 Years After the Birth of the Child	-.002	-.006
	(.012)	(.017)
Log Income 5 Years after the Birth of the Child	-.018	-.068
	(.138)	(.194)

The second column of this table shows coefficients of a regression of each of the variables in the first column on an indicator for being born in July 1977. The sample includes only individuals born in June and July of 1977. For the third column of the table we add to the sample those born in June and July of 1975, and we regress each of the variables in the first column on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter.

Table 2
Characteristics of eligible and non-eligible mothers

Eligibility status	Eligible 1977	Non-eligible 1977
Children		
High School Dropout	.186	.276
	(.388)	(.447)
College attendance	.46	.35
	(.50)	(.48)
Log Earnings at Age 30	12.6	12.5
	(.74)	(.76)
Mothers		
Years of Education	10.63	9.61
	(2.18)	(1.72)
Age at Birth (in years)	26.1	26.5
	(.028)	(.041)
Income in 1975 (in NOK)	25216	2831
	(18390)	(7080)
Employed 2 years After Birth	.725	.362
	(.447)	(.481)
Employed 5 years After Birth	.758	.534
	(.428)	(.499)
Income in 1982 (in NOK)	71216	29434
	(73324)	(48202)

Table 3
Children's outcomes

Variables		**Nonparametric Regression discontinuity**	**Nonparametric Differences-in-differences using 1975 as controls**
	Bandwidth	**3**	**3**
	Mean		
Dropout rate	.19	-.019*	-.027**
		(.010)	(.014)
College attendance	.46	.018	.036**
		(.013)	(.018)
Ln(earnings) at age 30	12.6	.048**	.055*
		(.020)	(.029)
N		29163	59564

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table 4
Mother's income around time of birth

Variables	**Nonparametric Regression discontinuity**	**Nonparametric Differences-in-differences using 1975 as controls**
Bandwidth	**3**	**3**
Ln(income) Year of birth	.191** (.083)	-.067 (.120)
Ln(income) +/- one year around year of birth	.036 (.025)	-.001 (.035)
Ln(income) +/-two years around year of birth	.020 (.024)	.005 (.034)
N	29163	59564

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table 5
Mother's labor supply

Variables		**Nonparametric Regression discontinuity**	**Nonparametric Differences-in-differences using 1975 as controls**
	Bandwidth Mean	**3**	**3**
Predicted months of unpaid leave	7.81	-.276 (.198)	.121 (.291)
Employed 2 years after birth	.73	-.014 (.012)	-.018 (.017)
Employed 5 years after birth	.76	-.004 (.011)	-.004 (.016)
Ln(Income) 5 years after birth	8.31	-.039 (.126)	-.068 (.178)
N		29163	59564

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table 6
Differences-in-differences using eligible mothers in 1975 as control group; Results by mother's education

Variables	**Nonparametric differences-in-differences**	
Bandwidth	**3**	
	Mother's education	
subgroups	Less than 10 years	10 years or more
Children		
Dropout rate	-.052**	-.019
	(.026)	(.016)
College attendance	.068**	.026
	(.028)	(.023)
Ln(earnings) at age 30	.089**	.033
	(.045)	(.037)
Mothers		
Predicted months of unpaid leave	-.259	.157
	(.524)	(.337)
Employed 2 years after birth	-.008	-.018
	(.029)	(.020)
Employed 5 years after birth	.004	-.004
	(.028)	(.019)
Ln(Income) 5 years after birth	.098	-.093
	(.305)	(.216)
N	22067	37497

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table 7
Differences-in-differences using eligible mothers in 1975 as control group; Results by quartiles of mother's months of unpaid leave.

Variables	**Nonparametric differences-in-differences**			
Bandwidth	**3**			
	Quartiles of mothers months of unpaid leave			
Quartiles	**1 (lowest)**	**2**	**3**	**4 (highest)**
Average levels of unpaid leave	.40	5.14	9.46	18.02
(Std.Dev)	(.67)	(1.67)	(.92)	(10.2)
N	14894	14894	14889	14887
Children				
Dropout rate	-.090***	-.050*	.008	.015
	(.026)	(.027)	(.029)	(.032)
College attendance	.077**	.001	.018	.054
	(.036)	(.036)	(.036)	(.035)
Ln(earnings) at age 30	.107*	.123**	.005	-.006
	(.060)	(.056)	(.056)	(.057)
Mothers				
Predicted months of unpaid leave	.008	-.059	-.018	.031
	(.043)	(.118)	(.057)	(.725)
Employed 2 years after birth	-.004	-.018	-.027	-.010
	(.012)	(.022)	(.036)	(.035)
Employed 5 years after birth	.040*	-.035	-.024	.011
	(.021)	(.027)	(.035)	(.036)
Ln(Income) 5 years after birth	.473*	-.505*	-.279	.168
	(.251)	(.304)	(.371)	(.381)

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table 8
The high school dropout decision for boys

High school dropout	**Model 1**	**Model 2**	**Model 3**
	Parametric Differences-in-differences using 1975 as controls		
Ability	-.066*** (.001)	-.066*** (.001)	-.065*** (.003)
Height	-.002*** (.000)	-.002*** (.001)	-.001* (.001)
Mothers education	-.013*** (.001)	-.013*** (.001)	-.012*** (.003)
Mothers age at birth	-.003*** (.001)	-.003*** (.001)	-.004*** (.001)
Parents married in 1980	-.121*** (.008)	-.120*** (.008)	-.102*** (.019)
Family size	.013*** (.002)	.013*** (.002)	.007 (.005)
Family income	-.038*** (.005)	-.038*** (.005)	-.038*** (.011)
Urban location	.016*** (.005)	.016*** (.005)	.010 (.010)
Reform*year77	-	-.055* (.031)	-.055* (.031)
Include interactions of reform, year and month controls	no	yes	yes
Interact reform effect with all control variables	No	no	yes
N	26378	26378	26378

***significant at 1 %, **significant at 5%, *significant at 10%

Appendix A

Construction of Additional Variables

The IQ data is taken from the Norwegian military records for the relevant cohorts, tested at the age of 18-19. Military service is compulsory for every able young man. IQ at these ages is particularly interesting as it is about the time of entry into higher education (or into the labour market for those who decide not to go to university).

The IQ measure is a composite score from three speed IQ tests, arithmetic, word similarities, and figures (see Sundet, Barlaug and Torjussen, 2004, for details). The figures test is similar to the Raven Progressive Matrix test (Cronbach and Lee, 1964) the arithmetic test is quite similar to the arithmetic test in the Wechsler Adult Intelligence Scale (WAIS) (Sundet, Tambs, Harris, Magnus and Torjussen, 2005, Cronbach and Lee, 1964) and the word test is similar to the vocabulary test in WAIS. The composite IQ test score is an un-weighted mean of the three subtests. The IQ score is reported in stanine (Standard Nine) units, a method of standardizing raw scores into a nine point standard scale that has a discrete approximation to a normal distribution, a mean of 5, and a standard deviation of 2. Height (in cm) is obtained from the same military records as IQ.

Teenage pregnancy is constructed as a dummy equal to one if the girl has given birth to a child before she turns 20 years old, and zero otherwise.

Distance to grandparents is created by tracking the postcode information for the parents of each child in the study with the postcode information for both sets of respective grandparents in 1980. Living in the same postcode area means that you live within maximum a few blocks from each other which means it is possible to have daily contact. We have postcode information for about 80% of the sample. We create a distance dummy equal to one if the couple lives in the same postcode area as at least one set of grandparents, and 0 otherwise. The rural-urban variable is constructed using information from Statistics Norway on the degree of centralization of municipalities in Norway. Urban municipalities include all municipalities with a large city centre or close to a large city centre while rural municipalities have small or almost non-existing city centres.

The working part time variable is constructed using information from the 1980 census on whether mother work full time, part time or not work at all. We define working part time in 1980 as working between 10 and 1300 hours per year, versus the alternative of not working or working more than 1300 hours per year. The completed fertility of mothers is constructed by using the population files in 2007 with information on total number of children. As we measure total number of children 30 years after the reform, this should capture completed fertility for all mothers, even teenage mothers in 1977.

We would like to have direct information on months of leave, but this is only available in Norway from 1992 and onwards. Even then we only have information on paid leave. Therefore, in order to compute total leave taken by each mother we proceed in the following way. First, we assume that the take-up of paid leave was 100% when it was first introduced in 1977, which is a plausible assumption.[41] This was in fact what happened in response to the 1992 and 1993 reforms to paid leave. . Before the April 1992 reform, mothers are able to take 224 days at full coverage or 280 days at 80% coverage.

[41] Firstly, Rønsen and Sundström, 1996 show that for the 1968-1988 mothers in Norway almost no one returned to work before 4 months after the birth. Secondly, from a survey conducted in 1977 on fertility behavior of women in Norway (Statistics Norway), 60% answered that they thought mothers should stay home for the first 2 years after giving birth to a child. In addition, the coverage was 100% which gives strong incentives for full take up. Third, since we observe days of paid leave after 1992 we are able to check to what extent eligible mothers take up this benefit, and how the take up reacts to subsequent reforms.

For mothers delivering children in March of 1992, the average take up of paid leave was 250 days. After April 1992 there is an increase in maternity leave entitlements to 245 days of full coverage or 310 days of 80% coverage. We observe that average paid leave taken was 275 days for mothers of those born in April 1992. This figure is slightly higher at 280 in March 1993, just before the 1993 reform which increased paid leave to 266 days of full coverage or 336 days of 80% coverage. By April of 1993 average leave taken was almost 310 days. Given the high levels of leave and strong reactions to reforms, it is reasonable to assume that the take up of paid leave is close to 100%.

In order to construct unpaid leave we start by calculating a measure of pre-birth monthly income by dividing 1976 earnings by 12. Then we calculate total earnings in 1977–1980, and divide them by 1976 monthly income, thereby obtaining a measure of number of months of unpaid leave during the first 36 months after birth. For this calculation to work, the assumption is that 1976 earnings are a good approximation for maternal potential post-birth earnings (the earnings she would get had she not gone on unpaid leave), adjusted for inflation.[42] We limit ourselves to a window of 36 months because the further away we move from pre-birth earnings, the more likely earnings may differ because of change of job, part time work, presence of new children, and other factors unrelated to the 1977 reform.[43]

---

[42] It is useful to illustrate with a specific example. If the child is born in June 1977 we subtract six months of 1976 monthly earnings from 1977 earnings and compare the remaining earnings in 1977 and 1978 to the 1976 earnings. If the mother earns half of 1976 earnings in the twelve months after birth she has taken six months of unpaid leave. If she earns nothing and takes all twelve months of leave we will continue and use earnings in 1979 and 1980 to construct leave up to 36 months after birth.

[43] One problem with our approach can be that mothers may return to part time work and hence some of our estimated leave is not absence from work but rather lower earnings due to part time work. This is not a problem as long as the reform in itself does not effect this transition, as it will only affect levels and not the change. As we see no effects on earnings five years later, this is not likely to be of large concern.

Figure A1
Number of children born to eligible mothers, by birth month, 1975-1979.

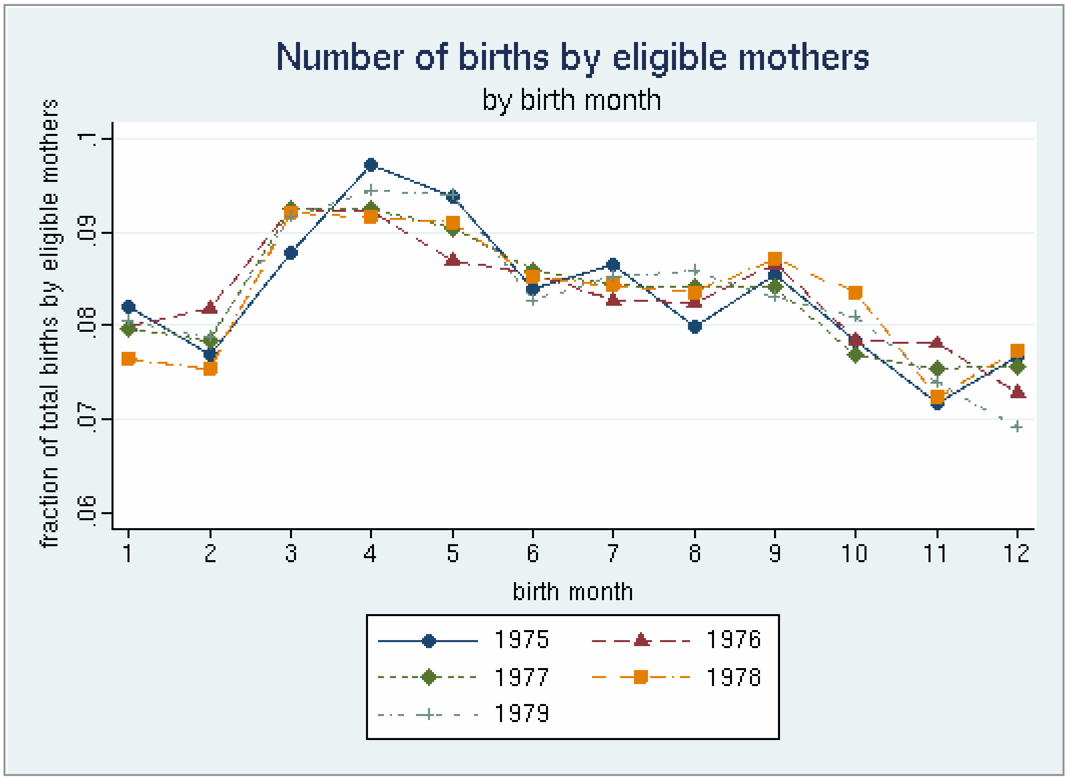
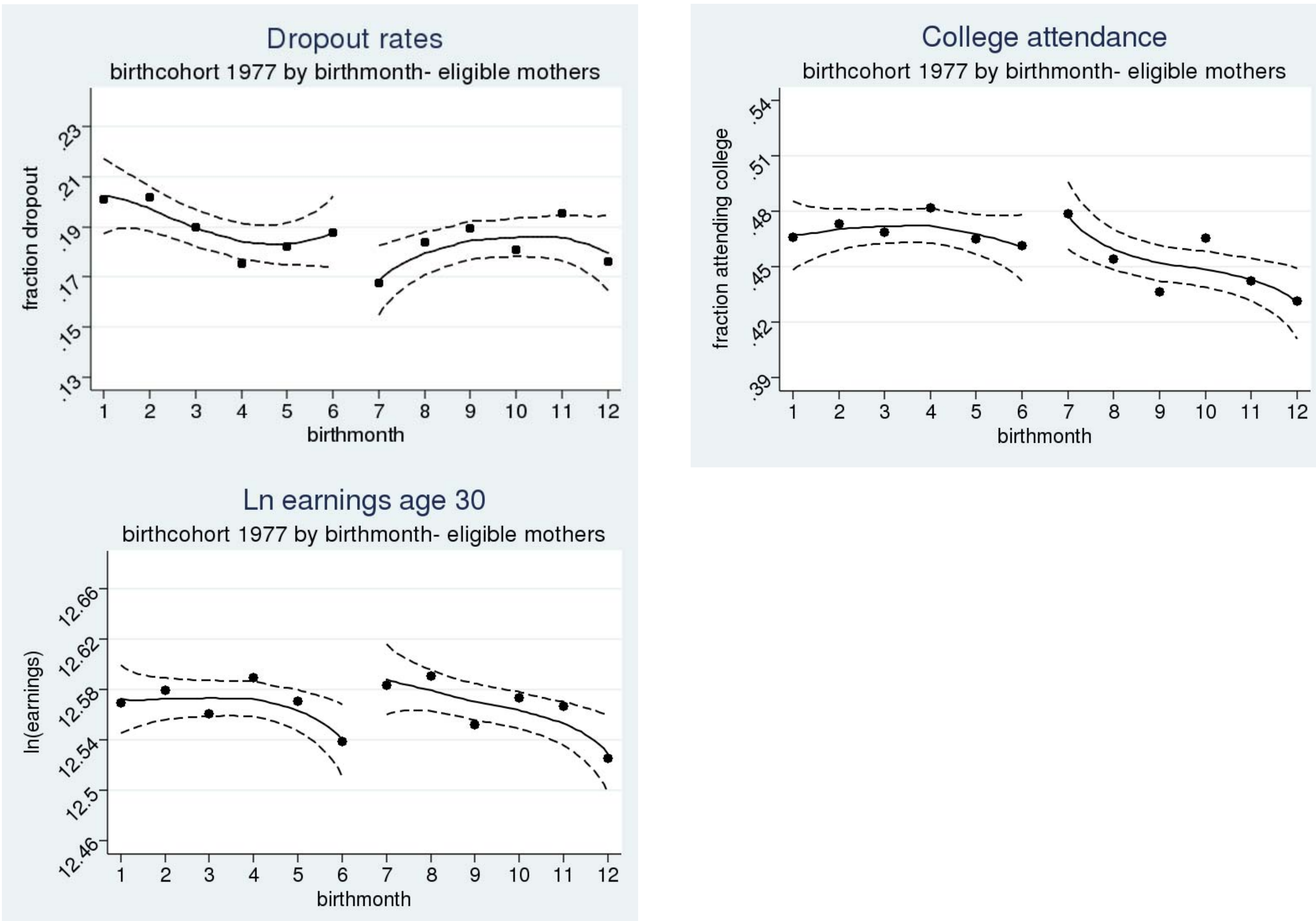
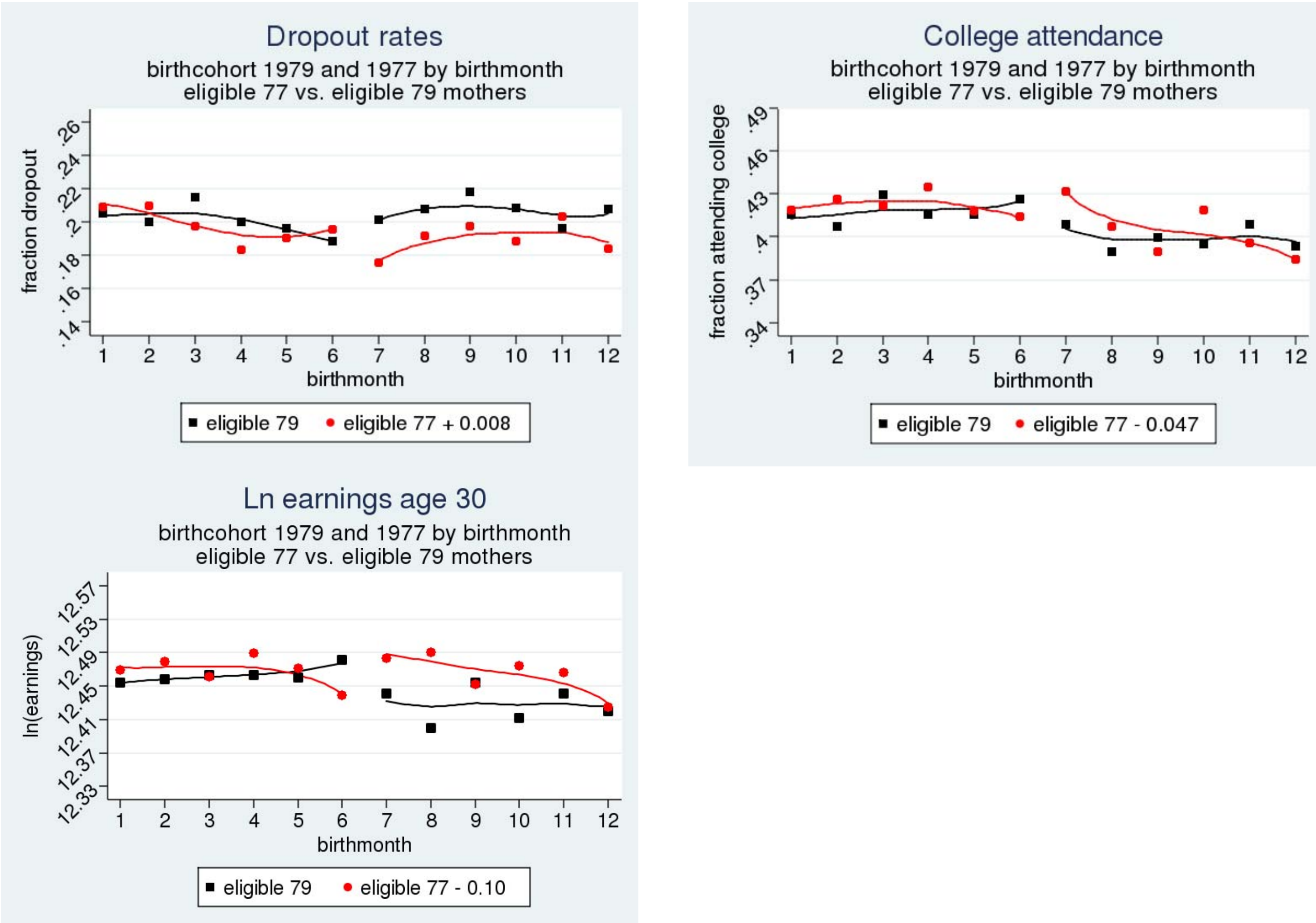


Figure A2
Children's outcomes by birth month, eligible mothers 1977



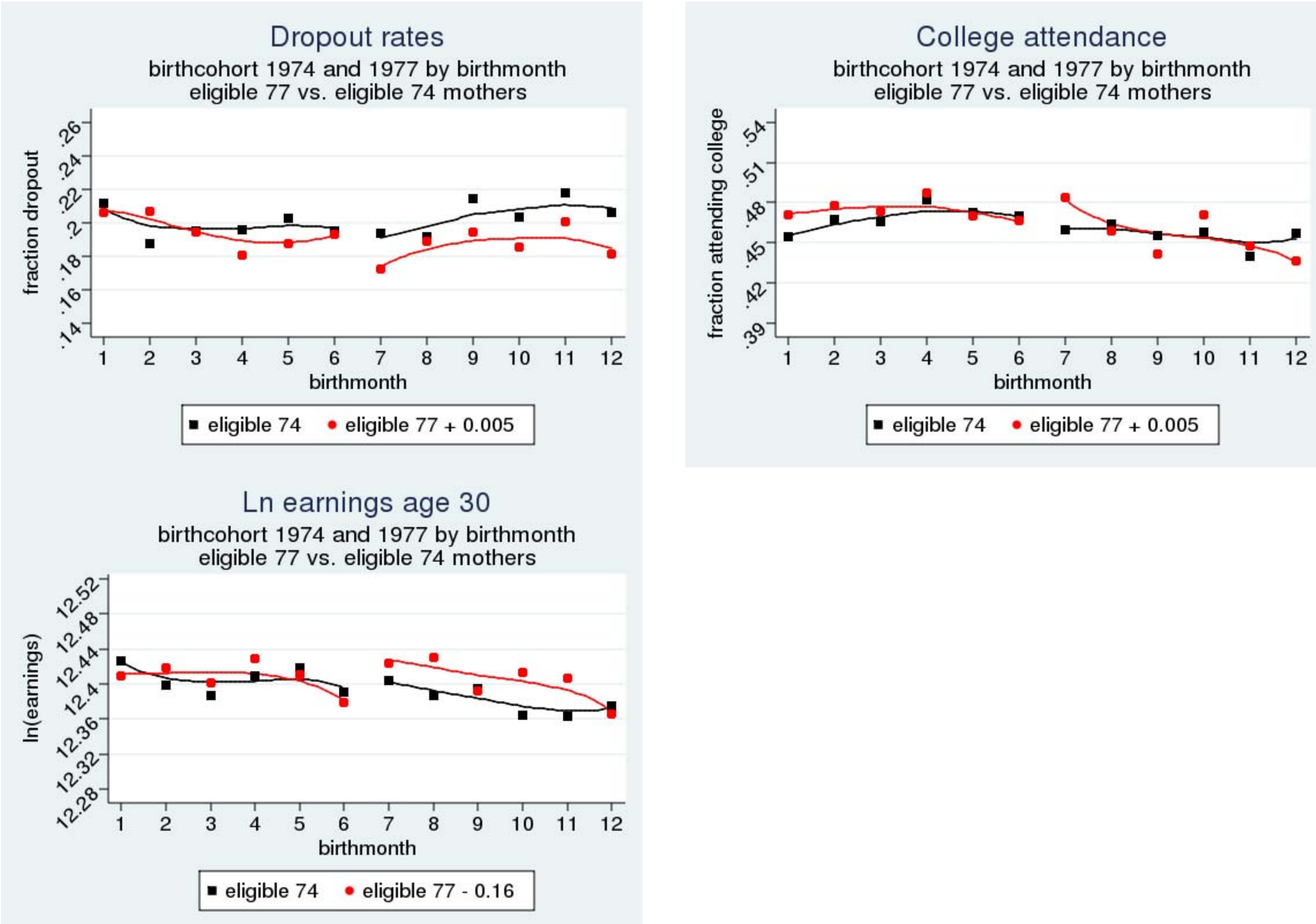
Note: Each graph shows the estimated mean for children's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three and the dashed lines are the corresponding 95 % confidence intervals. The y-axis includes outcomes within +/- .15 of a standard deviation around the mean.

Figure A3
Children's outcomes by birth month, eligible mothers 1977 versus 1979



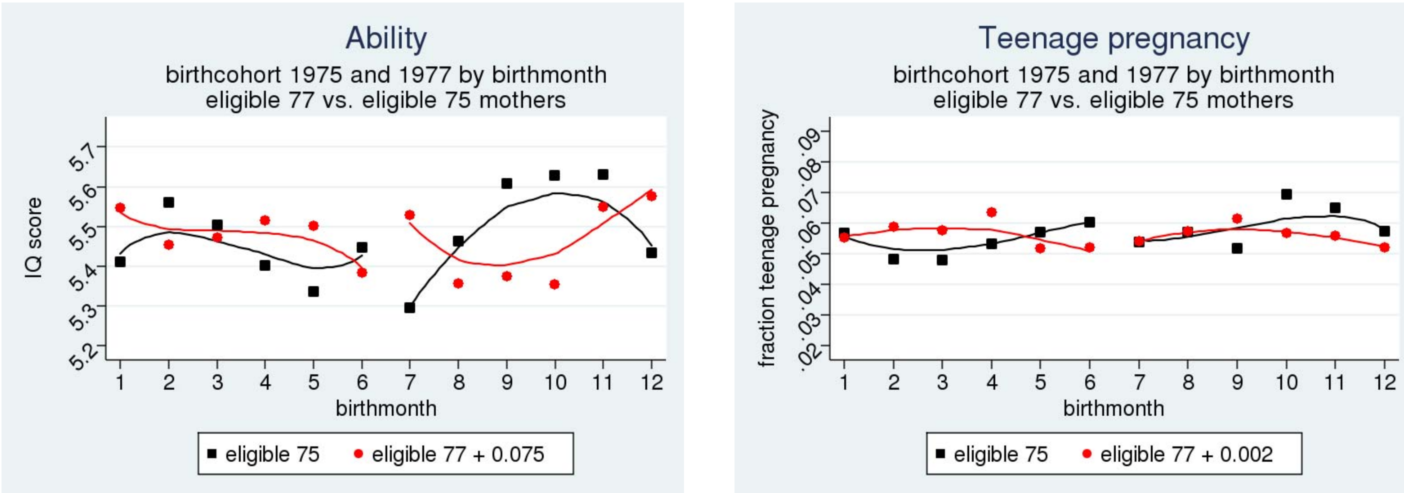
Note: Each graph shows the estimated mean for children's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of 3. The y-axis includes outcomes within +/- .15 of a standard deviation around the mean.

Figure A4
Children's outcomes by birth month, eligible mothers 1977 versus 1974



Note: Each graph shows the estimated mean for children's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of 3. The y-axis includes outcomes within +/- .15 of a standard deviation around the mean.

Figure A5
Children's outcomes by birth month, eligible mothers 1977 versus 1975: IQ and Teenage pregnancy



Note: Each graph shows the estimated mean for mother's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three. The y-axis includes outcomes within +/- .15 of a standard deviation around the mean.

Figure A6:
Days of paid leave in 1992 and 1993

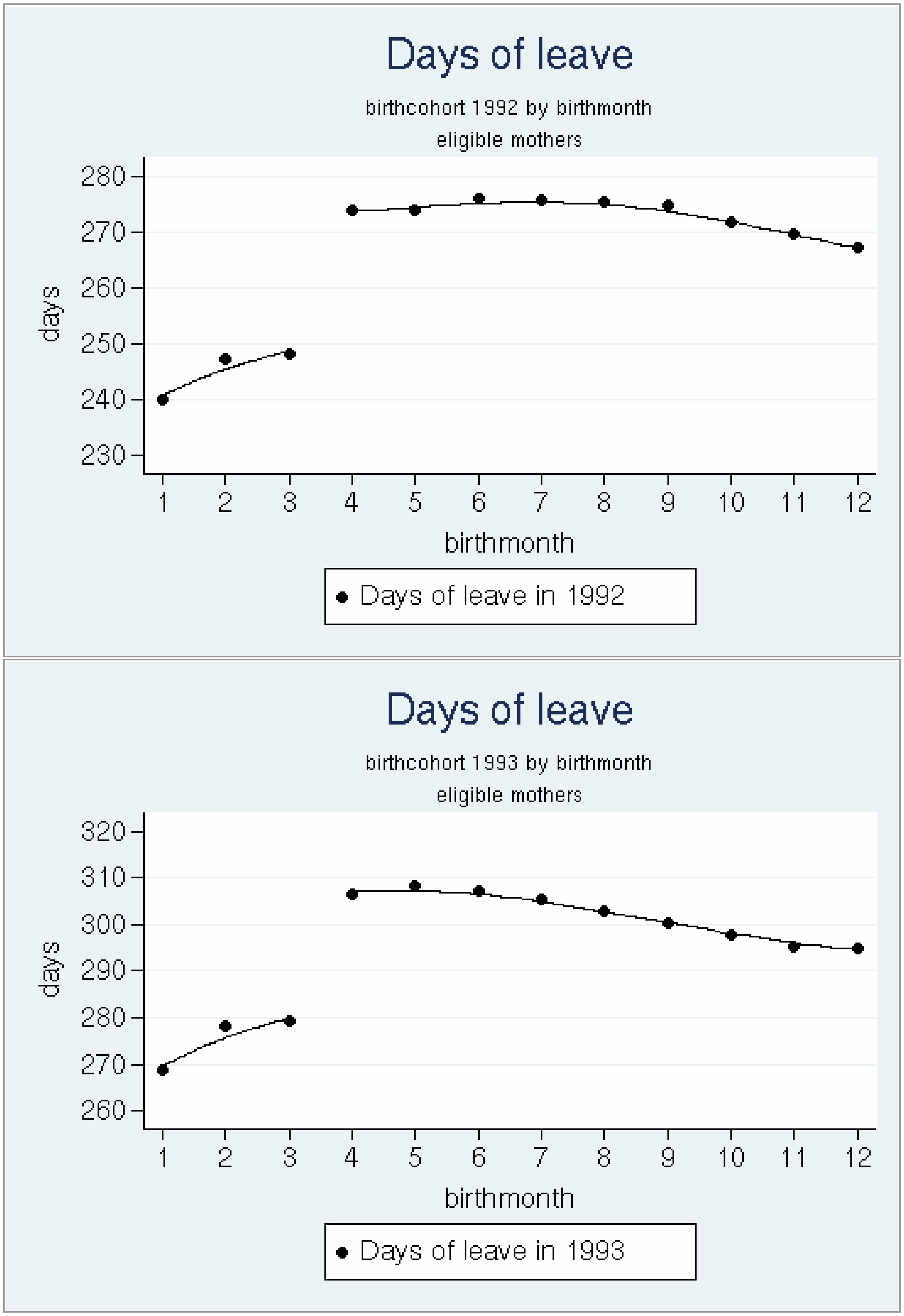


Figure A7
Quantiles of unpaid leave: show no action in unpaid leave across any quantiles of unpaid leave.

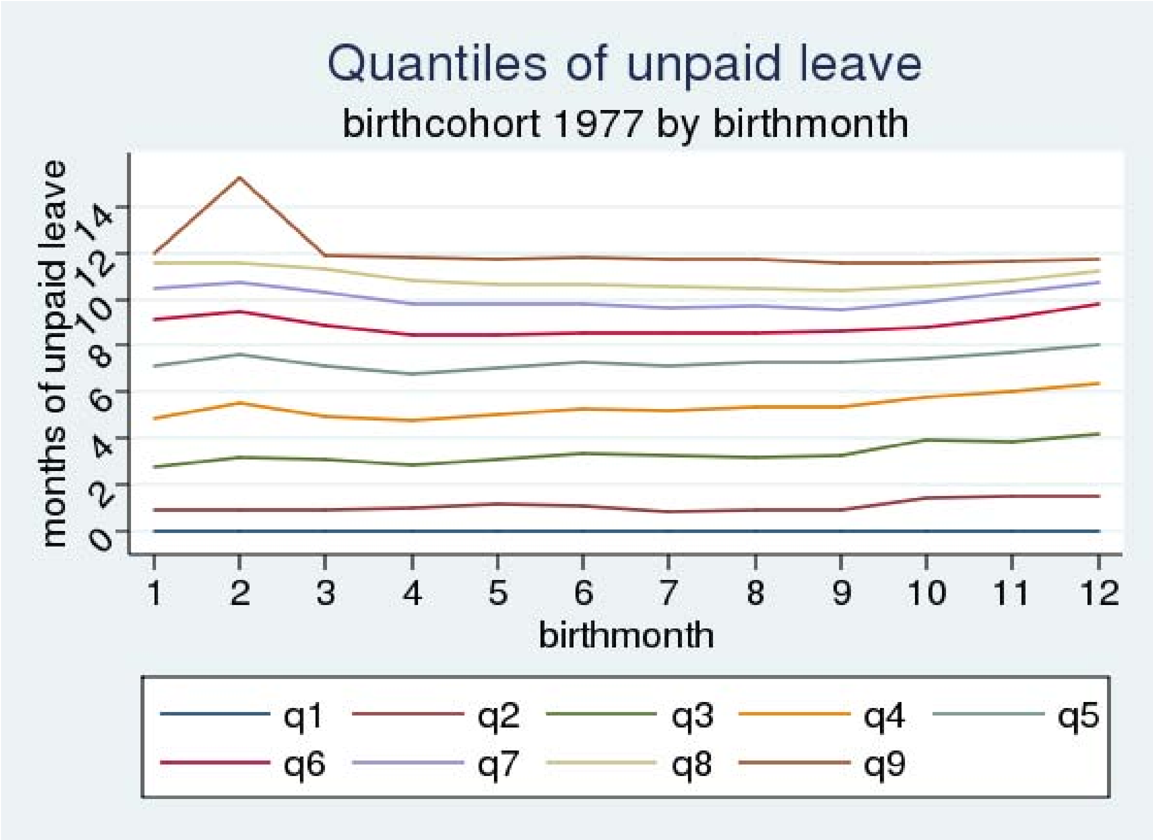


Table A1
Parametric regressions – using only children born in June and July – additional outcomes

Birth month	**Single Difference**	**Differences-in-differences using 1975 as controls**
Children		
Teenage pregnancy	.002	.009
	(.009)	(.013)
IQ (males)	.142*	.295***
	(.074)	(.102)
Height (males)	.499*	.503
	(.281)	(.384)
Mothers Pre-characteristics		
Age at birth	-.096	.051
(in years)	(.134)	(.187)
Urban location in 1976	.009	.009
	(.014)	(.020)
Distance to grandparents in 1980	.004	-.019
	(.014)	(.020)

The second column of this table shows coefficients of a regression of each of the variables in the first column on an indicator for being born in July 1977. The sample includes only individuals born in June and July of 1977. For the third column of the table we add to the sample those born in June and July of 1975, and we regress each of the variables in the first column on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter.

Table A2
Mother's labor supply and children's outcomes, total sample of all mothers and children in 1977 with control groups in 1975

Variables	**Nonparametric regression discontinuity**	
Bandwidth	**3**	**3**
Control group	RD	1975
Children		
Dropout rate	-.013 (.009)	-.012 (.012)
College attendance	.009 (.010)	.016 (.014)
Ln(earnings) at age 30	.024 (.016)	.028 (.023)
Mothers		
Predicted months of unpaid leave	-.288* (.158)	-.004 (.227)
Employed 2 years after birth	-.006 (.010)	-.010 (.014)
Employed 5 years after birth	-.005 (.010)	-.009 (.014)
Ln(Income) 5 years after birth	-.057 (.108)	-.125 (.149)
N	46245	97312

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A3
Children's outcomes – teenage pregnancy and IQ

Variables		**Nonparametric Regression discontinuity**	**Nonparametric Differences-in-differences using 1975 as controls**
	Bandwidth Mean	**3**	**3**
Teenage pregnancy	.052	.002 (.008)	.008 (.012)
IQ (males)	5.39	.110* (.067)	.240*** (.094)
N		14070 (TP-girls) 13150 (IQ-boys)	29042 (TP-girls) 27304 (IQ-boys)

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A4
Children's outcomes using bandwidth 5

Variables		Nonparametric Regression discontinuity	Nonparametric Differences-in-differences using 1975 as controls
	Bandwidth Mean	**5**	**5**
Dropout rate	.19	-.012 (.008)	-.019* (.012)
College attendance	.46	.008 (.011)	.025* (.015)
Ln(earnings) age 30	12.6	.036* (.020)	.037 (.028)
N		29163	59564

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A5

Differences-in-differences using eligible mothers in 1975 as control group; Results by urbanization and distance to grandparents

Variables	**Nonparametric differences-in-differences**			
Bandwidth	**3**		**3**	
	Distance to grandparents		**Centralization**	
subgroups	Close	Not-close	Urban	Rural
Children				
Dropout rate	-.050*	-.003	-.025	-.028
	(.029)	(.019)	(.020)	(.021)
College attendance	.039	.033	.050**	.019
	(.038)	(.024)	(.026)	(.026)
Ln(earnings) at age 30	.054	.054	.052	.058
	(.056)	(.039)	(.041)	(.040)
Mothers				
Predicted months of unpaid leave	1.12*	.083	-.036	.344
	(.604)	(.387)	(.399)	(.425)
Employed 2 years after birth	-.048	-.014	-.012	-.025
	(.035)	(.022)	(.023)	(.024)
Employed 5 years after birth	.002	-.006	-.023	.015
	(.034)	(.021)	(.22)	(.23)
Ln(Income) 5 years after birth	.037	-.136	-.246	.100
	(.371)	(.239)	(.248)	(.254)
N	13824	33704	30314	29250

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A6

Differences-in-differences using eligible mothers in 1975 as control group; Results by quartiles of family income two years prior to birth.

Variables	**Nonparametric differences-in-differences**			
Bandwidth	**3**			
	Quartiles of ln(family income) two years prior to birth			
Quartiles	**1 (lowest)**	**2**	**3**	**4 (highest)**
Mean ln(family income) two years before (Std.Dev)	6.6 (3.2)	9.7 (.17)	10.0 (.09)	10.4 (.20)
N	14894	14898	14886	14847
Children				
Dropout rate	.012 (.030)	-.014 (.033)	-.077*** (.029)	-.022 (.029)
College attendance	.011 (.037)	.005 (.039)	.111*** (.036)	.029 (.042)
Ln(earnings) at age 30	.072 (.063)	.033 (.062)	.074 (.063)	.051 (.067)
Mothers				
Predicted months of unpaid leave	-.112 (.589)	.852 (.621)	.558 (.567)	-.319 (.720)
Employed 2 years after birth	.006 (.033)	-.062* (.036)	-.023 (.033)	-.050 (.039)
Employed 5 years after birth	-.005 (.032)	-.000 (.034)	.012 (.032)	-.046 (.036)
Ln(Income) 5 years after birth	-.065 (.364)	-.049 (.383)	-.043 (.355)	-.357 (.403)

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A7
Mother's outcomes – part time in 1980, completed fertility (number of children in 2007) and marital stability in 2007.

Variables		**Nonparametric Regression discontinuity**	**Nonparametric Differences-in-differences using 1975 as controls**
	Bandwidth	**3**	**3**
	Mean		
Working part time in 1980	.42	-.000 (.013)	-.007 (.018)
Completed fertility in 2007	2.5	-.022 (.026)	-.034 (.036)
Parents are married in 2007	.73	-.001 (.012)	-.013 (.016)
N		29163	59564

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A8
Older sibling's outcomes

Variables		**Nonparametric Regression discontinuity**	**Nonparametric Differences-in-differences using 1975 as controls**
	Bandwidth Mean	**3**	**3**
Dropout rates older siblings	.20	-.011 (.016)	-.010 (.023)
College attendance older siblings	.49	.007 (.020)	.006 (.029)
Ln(earnings) in 2007 older siblings	12.7	-.045 (.031)	-.034 (.043)
N		12046	23875

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A9
Differences-in-differences using eligible mothers in 1975 as control group; Results by gender

Variables	**Nonparametric differences-in-differences**	
Bandwidth	**3**	
	Gender	
subgroups	Females	Males
Children		
Dropout rate	-.027	-.026
	(.020)	(.021)
College attendance	.029	.041*
	(.026)	(.025)
Ln(earnings) at age 30	-.001	.106***
	(.042)	(.037)
Mothers		
Predicted months of unpaid leave	-.161	.417
	(.401)	(.423)
Employed 2 years after birth	-.021	-.015
	(.023)	(.024)
Employed 5 years after birth	-.014	.006
	(.022)	(.023)
Ln(Income) 5 years after birth	-.019	-.119
	(.257)	(.246)
N	29042	30522

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Further checks to the validity of the procedure

Table A10 in the Appendix shows an analysis of two populations that should not be affected by the reform: eligible mothers in 1975 and non-eligible mothers in 1977. Therefore, if we estimate the RD model of this section on these two populations we should not find any effects. This is certainly the case for mother's outcomes in these years. The results are not statistically significant and the effect on children has the opposite sign of the reform effect. This is as we have seen earlier due to the month effect in child outcomes, children born earlier in the year have better outcomes than children born later in the year.

Table A11 and A12 runs the parametric regressions of Table 1 in the paper using placebo months (April versus May and August versus September) as discontinuities. There should be no effect on children's outcomes as there are no reforms between these birth months. And this is indeed the case, as we see from the tables there is no effect on the outcomes. The outcomes for children are of the opposite sign as the reform effect again reflecting the effect of birth of months on outcomes.

Table A10
Placebo results: Mother's labor supply and children's outcomes
Eligible mothers 1975 and non-eligible mothers 1977

Variables	**Nonparametric regression discontinuity**	
Bandwidth	**3**	**3**
Control group	Eligible 1975	Non-eligible 1977
Children		
	.007	.001
Dropout rate	(.010)	(.015)
College attendance	-.018	-.009
	(.013)	(.016)
Ln(earnings) age 30	-.007	-.020
	(.020)	(.027)
Mothers		
Predicted months of unpaid leave	-.318	-
	(.214)	
Employed 2 years after birth	.007	-.002
	(.012)	(.016)
Employed 5 years after birth	.001	-.010
	(.011)	(.017)
Ln(Income) 5 years after birth	.029	-.121
	(.125)	(.180)
N	30401	17082

Each cell presents the estimated discontinuity in the outcomes. The second column shows results for July 1st 1975 and the third column for July 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%

Table A11

Placebo: Parametric regressions – using only children born in April and May

Birth month	Single Difference	Differences-in-differences using 1975 as controls
Children		
Dropout rates	.007	.009
	(.011)	(.016)
College attendance	-.017	-.031
	(.014)	(.019)
Ln(earnings) at age 30	-.019	-.012
	(.020)	(.028)
Mothers		
Pre-Reform Characteristics		
Years of education	-.063	-.060
	(.061)	(.084)
Ln(Income) two years prior to birth	-.019	.010
	(.087)	(.130)
Outcomes		
Average Ln(Income) year of birth and year after birth	.162	-.228
	(.100)	(.146)
Employed 5 years after	-.015	-.026
	(.012)	(.016)
Ln(Income) 5 years after birth	-.132	-.308
	(.131)	(.184)

The second column of this table shows coefficients of a regression of each of the variables in the first column on an indicator for being born in May 1977. The sample includes only individuals born in April and May of 1977. For the third column of the table we add to the sample those born in April and May of 1975, and we regress each of the variables in the first column on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter.

Table A12
Placebo: Parametric regressions – using only children born in August and September

Birth month	Single Difference	Differences-in-differences using 1975 as controls
Children		
Dropout rates	.006	.020
	(.011)	(.016)
College attendance	-.018	-.033
	(.014)	(.020)
Ln(earnings) at age 30	-.029	-.030
	(.021)	(.031)
Mothers		
Pre-Reform Characteristics		
Years of education	.001	.081
	(.061)	(.087)
Ln(Income) two years prior to birth	-.120	-.133
	(.097)	(.143)
Outcomes		
Average Ln(Income) year of birth and year after birth	.165	.047
	(.094)	(.138)
Employed 5 years after	.015	.021
	(.012)	(.018)
Ln(Income) 5 years after birth	.219	.313
	(.139)	(.198)

The second column of this table shows coefficients of a regression of each of the variables in the first column on an indicator for being born in September 1977. The sample includes only individuals born in August and September of 1977. For the third column of the table we add to the sample those born in August and September of 1975, and we regress each of the variables in the first column on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter.

Appendix B

Breastfeeding

Using a survey from mainly one maternity hospital in Norway over time (Liestøl, Rosenberg and Walløe, 1988) show the pattern of breastfeeding for about 150 years in Norway. They show that breastfeeding in Norway started to decline around 1920 and reached its lowest point around 1967 when only 30 percent of women breastfed for 3 months and as few as 5 percent for 9 months. In the late 1970s, the level of breastfeeding in Norway was back to the level of around 1940 after a decline from the 1920s onwards. Around the period of the maternity leave reform we are using, about 75 percent breastfed for 3 months, 50 percent for 6 months and 25 percent of mothers where breastfeeding for 9 months or more. Clearly there is an increase in breastfeeding in this period if we only study this data set.

We use survey data for mothers being asked about their breastfeeding for all of their children, and create average months of breastfeeding. The survey is from a health data set covering all 40 year olds in the early 1990s ("The 40 year old survey"). We are able to match about 5% of the children in our sample. However, we have the whole population of children so we still have more than 100 observations in each month cell. This is too little data to establish a convincing regression design as with our other results, but in Figure B1 we show the average months of breastfeeding across months of birth for eligible mothers in 1977 and 1975. Firstly this shows that breastfeeding has increased from 1975 to 1977 as is consistent with the data from Bernal and Keane, 2010. However there is no increase in breastfeeding after the reform in 1977.[44] If anything there is a small decline in average months of breastfeeding across birth months in 1977. This indicates that breastfeeding is not the most important mechanism to explain the positive results on children's outcomes.

We present the results for the effect on maternity leave on the height of men at the age of 18–19, which is an outcome linked to better health. In Table B1 we present the results both from the RD design and the DD results using eligible mothers from 1975 as comparison group. The results suggest that there is a positive effect of about 0.5 centimetres for men born post-reform. The increase per decade in height among men measured at 18 was about one centimetre for cohorts born from 1950 to 1990 in Norway, so the 0.5 centimetre is quite substantial. This clearly indicates that there is a positive effect of the reform through better health. Given that we do not see an increase in breastfeeding around the reform this is likely to come from the mother investing more time at home the first year of the child's life, providing a more stable and less stressful environment.

[44] We have also tried different measures as an indicator variable for breastfeeding at least 6, 8 and 9 months and we obtain similar results. There is no clear pattern across birth months for eligible mothers in 1977 (or on our control groups of eligible mothers in 1975 and non-eligible mothers in 1977).

Figure B1
Breast Feeding in Norway – eligible mothers 1977

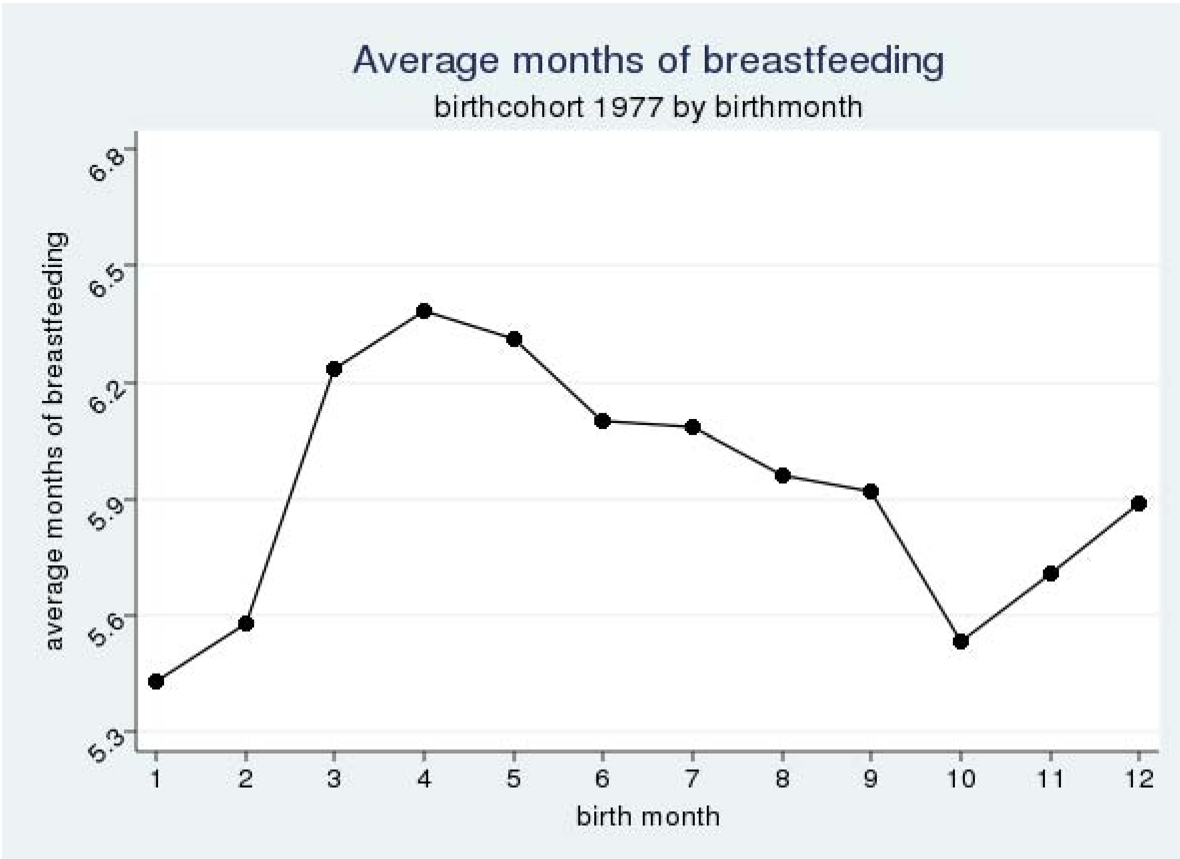


Table B1
Height (males only)

Variables		**Nonparametric Regression discontinuity**	**Nonparametric Differences-in-differences using 1975 as controls**
	Bandwidth Mean	**3**	**3**
Height (male)	180 cm	.48* (.27)	.63* (.37)
N		13541	28371

Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform July 1st 1977. We estimate regressions using local linear regression as in Hahn et al. (2003) and derive analytic standard errors based on formulas in Porter (2003) using a triangle kernel. ***significant at 1 %, **significant at 5%, *significant at 10%