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

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April 2014

Abstract

We study the impact of increasing maternity leave benefits on long run outcomes of children, by examining a reform that increased paid and unpaid maternity leave in Norway as of July 1st 1977. Mothers giving birth before this date were eligible only for 12 weeks of unpaid leave, while those giving birth after were entitled to four months of paid leave and 12 months of unpaid leave. This increased time with the child led to a two percentage points decline in high school dropout rates and a five% increase in wages at age 30. These effects are especially large for children of those mothers who, prior to the reform, would take very low levels of unpaid leave.

*We are thankful for comments at numerous seminars, workshops and conferences that have improved 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".

1. Introduction

There are huge disparities in maternity leave entitlements across different countries. On one extreme, countries in Northern Europe, such as Sweden, Norway and 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 after the birth of a child (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 place 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, but 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.

Following the large 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 substantial 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 parental 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 (2012) 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 the impact of a reform in maternity leave benefits in Norway on long term outcomes of children, namely education and earnings at ages 25-33. The reform we analyze increased mandatory paid maternity leave from zero to four months and mandatory unpaid maternity leave from three 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 are able to test for potential manipulation of the date of birth.

We follow children to as late as 2010, when they are 33 years of age. We observe several medium and long term outcomes, such as high school completion, college attendance, and wages up to age 33.

We begin with a very simple look at the data. Using only data on individuals (and their mothers) born in June and July of 1977 (just before and just after the reform was implemented), 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

[1] This is equivalent to moving from the current level of maternity leave entitlements in the US to those of 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.

the large literature on month of birth effects (e.g., Black, Devereux and Salvanes, 2011, present estimates for Norway). Therefore, we use data from nearby years to estimate the difference in outcomes between children born in June and July in years in which no reform took place, and subtract it from the estimate of the effect of being born in July (vs. being born in June) obtainedfrom the 1977 data, as in a difference-in-differences estimator.[3]

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 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 by about two to three percentage points reduction, college attendance increased by three and a half percentage points (only in the double difference specification) and earnings at age 30 increased by five to seven percentage points.

We then 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. In both

[3] 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 from 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, 1978, 1979 and 1977, we can run:

$$Y_i = \alpha + \gamma * D_i^{1977} + \gamma * D_i^{1978} + + \gamma * D_i^{1979} + \phi * D_i^{July} + \beta * D_i^{July} D_i^{1977} + u_i$$

where D_i^{1977} is a dummy indicating whether an individual was born in 1977. As before, β measures the impact of benefiting from the reform on the outcome of interest, among children of eligible mothers. Below we explain why 1976 is excluded from the analysis.

these dimensions, the sets of mothers giving birth in June and in July of 1977, respectively, are similar.[4]

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 birth). This is important because it means that the reform had no impact on the amount of unpaid leave taken by these mothers. It is possible to infer how much unpaid maternity leave, or time off work, is taken by working mothers by examining how much their income falls after giving birth.

We also look at maternal labour supply and income five years after the birth of the child,[5] and see no statistically significant effect of the reform on these variables, using both single (first column) and double (second column) differences. Therefore, the most likely mechanism through which this reform operated was an increase in time with the child, with no short or long run consequences on maternal employment or income.

In the rest of the paper we develop, expand and discuss these results in detail, implementing a regression discontinuity estimator which explores data on date of birth (relative to the date of the reform) linked to data on adult outcomes of these children. The main patterns of Table 1 survive a more sophisticated estimation procedure. We examine the sensitivity of our results to various changes in the specification and samples used.

The paper proceeds as follows. Section 2 provides a short review of the literature. Section 3 gives background information on maternity leave legislation in Norway, while

[4] We also checked whether there were differences in the birthweight of children born in June and July 1977, since we would *not* expect maternity leave benefits to have an impact on birthweight, and we show later that this is indeed the case. This suggests that the differences in outcomes between children born before and after the reform are due to the reform, and not to unobserved differences in child quality that could be manifested in differences in birthweight.

[5] 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.

Section 4 presents the empirical strategy. Section 5 presents data and Sections 6 shows the results. Section 7 discusses evidence on mechanisms by which the reform impacts child outcomes. Section 8 concludes.

2. Short Review of Relevant Literature

There is a very large literature on this topic, 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. However, as in most of the literature, the authors caution against a causal interpretation of their estimates.

Recent papers attempt to address the empirical problems of the previous literature by directly examining 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. There also exists a set of recent papers studying Canadian reforms and focusing on short run outcomes for children, by Baker and Milligan, (2008a, 2008b). These papers find no significant effects of the reform on children's outcomes.

In addition, there are 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, 2012), Denmark (Rasmussen, 2010), and Sweden (Liu and Skans, 2010). Just as in our study, these three papers explore exogenous variation in maternity leave resulting from legislative reforms to these benefits, and are able to look at long run outcomes of children. Our data challenges the main conclusion of these papers, which is that there is little or no effect of maternity leave expansions on long run outcomes of children.

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 at a time when they were at a very low level, similar to what is observed in the US today. The three papers we refer to, mostly consider expansions in maternity leave from an already generous baseline level of benefits.

The earliest reform in Dustmann and Schönberg (2012) is the closest to ours, and consists of an expansion from two to six months in paid maternity leave entitlements. Nevertheless, it is much less generous that the reform we consider because the payments women were entitled to in the expansion period (from the third to the sixth month after childbirth) corresponded, on average, only to a third of their average pre-birth income. As a result, there was only a small decrease in maternal labor supply, and a resulting small increase in time with the child. In contrast, in our case, we conjecture that the take-up of the 1977 reform in Norway was 100% for the eligible women (so four months). In sum, even though the 1979 German reform looks at first sight like the 1977 Norwegian reform,

in practice it is much less generous, and it probably lead to smaller impacts on maternal time at home.

The reform studied in Liu and Skans (2010) is quite different from the reform analysed in this paper. They assess an extension of maternity leave in Sweden from 12 to 15 months. In addition, the main alternative to maternity care in Sweden at the time of the reform was subsidized day care. Our study analyses a reform affecting younger children, in a setting where the main source of alternative care is either informal, or a low quality private sector.

The Danish study analyses an extension of six weeks on top of a paid maternity leave entitlement of already three and a half months (Rasmussen, 2010). Hence, when analysing extensions of already generous maternity benefits, these studies found zero or small impacts.

The second important feature that distinguishes our work from that of the papers referred to above, is that we are able to look at education and labour market outcomes as late as age 33. 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.[6] In addition, our data lets us link mothers with their children, allowing us to perform a rich analysis of impacts by subgroups of mothers. It also lets us construct good measures of eligibility for the reform which is important since generally only a fraction of mothers, i.e. those who are working a minimum amount of time, are eligible for these benefits.[7]

[6] In fact, we do not find any effect of the reform on earnings at ages 25 and 26.

[7] 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

3. Maternity Leave Reform and Institutional Background

3.1 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.[8] 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.[9] Maternity leave payments were equivalent to 18 weeks of the pre-birth wage income (i.e., 100% replacement rate for 18 weeks). Of these 18 weeks, six had to be taken by the mother alone, while the rest could be shared between both parents. In practice, all leave

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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 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.

[8] 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 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 believe 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 2010, 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.

[9] 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).

was almost exclusively taken by the mother (Rønsen and Sundström, 2002). In addition, parents also became entitled to one year of unpaid job protection on top of the 18 weeks of paid and job-protected 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 six of the 10 months immediately prior to giving birth, and having more than 10,000 NOK[10] 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 10,000 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 1 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 giving birth and children being born in that year.

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

[10] 10,000 NOK (USD 1725) refers to the lowest level of income providing pension points in the Norwegian social security system in 1977.

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 number of other changes and at the end of the legislative period. The Government report became official on April 15th, 1977, and was approved on June 13th, 1977[11]. This means that all women giving birth after the introduction of the law in 1977 were already pregnant when the law was announced,[12] and because of the rule of working six out of 10 months prior to giving birth, it was difficult for women to change their eligibility status in the short term. We also checked national newspapers around 1976 and 1977 for news about the reform, but find no evidence that newspapers reported on the reform before June 1977.[13] 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 implemented on January 1st, 1976. This law made it easier for women to have an abortion within 12 weeks of conception. The first cohort affected by this reform is born around July 1976. This possibly gives rise to a discontinuity in observed child outcomes between

[11] Propositions and regulations from the Government: Ot.prp nr. 61 and Innst.o. nr 61.
[12] Possible effects on fertility will therefore not show up in the data before the beginning of 1978, at the earliest. We may still worry that mothers delivering close to July 1st, 1977, were able to delay their delivery (although we may also think that this would have been a hard thing to do at the time). Studying a much more recent time period, Gans and Leigh (2009) estimate that Australian mothers delayed child birth (by even as much as a week) in response to a reform changing incentives to fertility (mostly by changing the schedule of inductions and caesarian sections). Since we use daily birth data, we will check whether this is also true in our data by studying if there is any bunching of births occurring right after the reform.
[13] Verdens Gang June 30th, 1977, Bergens Tidende June 27th, 1977, June 30th, 1977, Aftenposten June 30th, 1977.

those born in June and July 1976 and, hence, we do not use 1976 as a comparison to 1977.

3.2 Institutional Background

At the time of the maternity leave reform in 1977, labor force participation for women was relatively high in Norway. Figure 2 shows labor 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 labor force participation rate around 1977 was about 50% for married women, who are the most relevant group for our study, and around 70% for non-married women. Labor force participation for women was about the same in Norway and the US during the 1970s, but much higher in the former than in the latter by 1990.

It is also relevant to look at the provision of public childcare. In the mid 1970s, very few children aged zero to two were in day care. Although day care centres provided coverage for 15% of children aged three to six in 1977, the coverage for the first two years was very low, at only 1–2%. 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.

4. 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 of mothers satisfying income and employment eligibility criteria: $\alpha = E(y_i(1) - y_i(0) \mid I_i = 1)$, where I_i is an indicator that takes value 1 if the mother satisfied the income and employment eligibility criteria for maternity leave benefits discussed in section 3.[14]

In order to estimate this parameter we compare children born just before and just after the reform, who should be similar except for the fact that mothers of those in the latter group benefited from the change in maternity leave entitlements taking place on July 1st, 1977.

For those women satisfying income and employment eligibility criteria and giving birth in 1977, eligibility for the new maternity leave entitlements (E_i) is further determined by a deterministic function of date of birth (X_i):

$$E_i = 1\{X_i > c\}, \quad (1)$$

where c is the cut-off point of July 1st, 1977. Therefore, all mothers giving birth after c potentially receive the treatment defined by new maternity leave entitlements, while those giving birth before c are assigned to the control group.

The regression discontinuity (RD) estimator for α is given by:

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

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

[14] This answers the following question: what are the consequences of a maternity leave reform on the long term outcomes of children whose eligible mothers were exposed to it (what is usually called, intent to treat). This is different from the question: what is the impact of taking up maternity leave for the long term outcomes of children, which corresponds to a different parameter. We can answer the former question with our data, but not the latter.

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

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

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

Outcomes of interest for the child include dropping out of high school, college attendance (both measured by age 30) , earnings at age 30, completed years of education at age 30, the probability of having a child before age 19 for women, IQ for men and earnings between ages 25-33. Outcomes of interest for the mother include months of unpaid leave, and employment and earnings five years after giving birth. These are mainly interesting because we can examine whether the reform induced changes in home environments, which can account for the effect of the reform on child outcomes.

We estimate $\alpha_{RD} = \lim_{x \downarrow c} E[y_i \mid I_i = 1, X_i = x] - \lim_{x \uparrow c} E[y_i \mid I_i = 1, X_i = x]$ 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; Fan, 1992), as in Hahn, Todd and Van der Klaauw (2001), and Porter (2003). Defining h as the bandwidth, we estimate (α, β, γ, τ):

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

α_{RD} is estimated as

$$\hat{\alpha}_{RD} = \hat{\tau} \qquad (4)$$

We use the triangle kernel which is shown to be boundary optimal (Cheng, Fan and Marron, 1997). We obtain standard errors as recommended in Lee and Lemieux (2009) using heteroskedastic-robust standard errors (White, 1980). The choice of bandwidth is important, as usual. We present main results using a bandwidth of 90 days, however we also present further results using both smaller and larger bandwidths.

We also check for the existence of date of birth manipulations by any mothers delivering in the neighbourhood of the date of the reform, which could potentially affect our results. We document that the number of births did not change in the days and weeks immediately preceding and following the date of the reform. We also document that the characteristics of mothers giving birth just before and just after the reform are virtually identical.

Finally, we examine what happens to our estimates when we drop from our sample children being born right in the vicinity of the reform (within one, two, four and six weeks of the reform). This allows us to examine the sensitivity of our results to observations close to the discontinuity. When we start dropping observations close to the discontinuity, we also start moving away from the original RD design. It is possible that date of birth affects children's outcomes because, for example, the age at which children start school depends on their day and month of birth, and this is potentially related to their adult education and earnings (see Black, *et al.*, 2008, for evidence for Norway). In this case α_{RD} estimates $\alpha + \lambda_{Birth}$, where α is the impact of the reform, and λ_{Birth} is a date of birth effect. If we assume that the date of birth effect does not vary across years, we can combine RD with difference-in-differences (DD) by constructing two types of control

groups: one consisting of children born in 1975, 1978 and 1979 of eligible mothers (our main specification); and another consisting of children born in 1977 of ineligible mothers.

We begin by estimating equation (3) for those in either of the control groups, and for those born of eligible mothers in 1977. Then we calculate:

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

Since there is no reform for the control groups, $\hat{\alpha}_{con}$ (the RD estimate for those in a control group) should only capture date of birth effects. On the other end, $\hat{\alpha}_{RD,1977}$ confounds effects of the reform with potential date of birth effects. Under the relatively mild assumptions that the two effects do not interact, and that date of birth effects are the same (around July) for those born in control years, for those born of ineligible mothers in 1977, and for those born of eligible mothers in 1977, we can estimate the effect of the reform as $\hat{\alpha}_{RD-DD} = \hat{\alpha}_{RD,1977} - \hat{\alpha}_{RD,con}$.

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, both mothers of children born in June and children born in July of 1977 are likely to engage in similar adjustments to fertility and labour

supply in the medium run, especially if they are considering having more children (note that we will show that there are no differences in completed fertility and labour supply between mothers with children born in June or July 1977). What we can answer is the question: how important is the time that mothers spend with their children in their first year of life?

5. 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 2010 and is a collection of different administrative registers providing information about day 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 in the sample 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.[15] 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 2010. These are not top-coded and include labour earnings, taxable sick benefits, unemployment benefits, and parental leave payments.

[15] 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.

We also collect data on maternal income measured two and five years after the birth of the child. This is useful for examining potential channels through which maternity leave affects child outcomes, namely by promoting attachment of women to the labour market.

In order to construct a measure of unpaid leave we start by calculating a measure of the mothers' pre-birth monthly income by dividing 1976 earnings by 12. We then calculate total earnings in 1977–1980, and divide them by our estimate of monthly income in 1976, thereby obtaining a measure of the 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 potential post-birth earnings (the earnings that the mother would get had she not gone on unpaid leave), adjusted for inflation.[16] We limit ourselves to a window of 36 months because the further away we move from pre-birth earnings, the more likely it is that earnings may differ because of change of job, part time work, presence of new children, and other factors unrelated to the 1977 reform.[17] We assume that paid leave has a take-up rate of 100% for those giving birth after July 1977. In Section 7.1 we will give more information on the plausibility of this assumption.

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

[16] 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 earnings in 1976. If the mother earns half of her 1976 earnings in the eighteen months after birth (corresponding to 6 months of full time work) then she has taken twelve months of unpaid leave. If she earns the full amount of 1976 earnings in the eighteen months following birth, then she takes six months of leave. If the mother was able to take four months of paid leave (by giving birth after the reform), then we take that into account by subtracting four months of wages from the post birth income. However, we count this as paid, not unpaid, leave.

[17] 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).

this age 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.

Teenage pregnancy is constructed as an indicator variable taking a value equal to one if the girl has given birth to a child before turning20 years old, and zero otherwise.

Distance to grandparents is created using postcode information for the parents of each child in the study, and postcode information for both sets of respective grandparents in 1980. Living in the same postcode area means living within a maximum of 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 family lives in the same postcode area as at least one set of grandparents, and zero 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 or town centres.

The working part time variable is constructed using information from the 1980 census on whether mother works full time, part time or not 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 2010 with information on total number of children. Since we measure total number of children 33 years after the reform, this should capture completed fertility for all mothers giving birth in 1977.

6. Results

6.1 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 1 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 children and mothers 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 labor force participation rates in OECD countries are generally not much higher than 65%, except in the Scandinavian countries where they are often above 80%.

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 have more education 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 two years before giving birth is nine times higher than that of ineligible mothers, presumably because many in the latter group do not work. Children of eligible mothers have lower high school dropout rates and higher college attendance rates, but similar earnings at age 30 when compared to children of ineligible mothers. In summary, 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.

The average level of unpaid maternity leave taken at the time is quite high, even for those mothers giving birth before the reform is implemented. For our preferred measure, average unpaid leave is eight months for those delivering their children before July 1977 according to our estimates, and it barely changes for those delivering after this date. The 25th percentile is about two months, and the 75th percentile is about 11 months. Any expansion in the time mothers spend with their newborns resulting from the reform is in addition to this pre-existing level of leave.

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. An imbalance could indicate a threat to the validity of our method, suggesting the possibility that a non-random set of mothers manipulated the date of birth of their children (see Gans and Leigh, 2009). The various panels of Figure 3 show how observable pre-reform characteristics of mothers vary with the day on which they gave birth, 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), and in birth weight of the child.

Moreover, in Figure 4 we display the number of children born to eligible mothers in 1975, 1977, 1978 and 1979, by week of birth. This figure shows very similar numbers of births in the days just before and just after the reform was implemented. In sum, selective manipulation of day or week of birth is not likely to be a serious concern in our data. This is quite reasonable given that in 1977 it was probably difficult to delay childbirth much beyond the due date.

6.2 Children's outcomes

In Table 3 we present our main estimates of the impact of the reform on a set of children's outcomes using date of birth data. The first row shows the RD results, while the second row presents the DD results using the cohorts born in 1975, 1978 and 1979 as a control group. In the first column, we see a negative effect of the reform of about -two percentage points in children's dropout rates, statistically significant at the 10% level.

When we take into account potential date of birth effects in the DD specifications, our estimate of the impact of the reform on children's dropout rates barely changes but the standard error declines substantially. We see the same pattern for college attendance: an increase of around two percentage points, but this one is only statistically significant in the DD specification (effects on completed years of schooling show a similar pattern). In

addition, we see a positive effect on earnings at age 30 of 6.2% estimated by RD, which decreases to 5% in the DD specification.[18] [19]

In Figure 5 we present graphically the RD results of Table 3. We clearly see reform induced discontinuities in dropout rates and earnings at age 30. The effect on college attendance is not as clear, however. [20] Therefore, the most robust impact of the reform seems to be at the low end of the education distribution, with treated children being less likely to drop out of high school. This also shows up as higher earnings by age 30 It is worthwhile pointing out that if we use earlier measures of earnings (say, at age 25) we cannot detect this effect. It is important to wait until individuals have completed their education and acquired some maturity in the labour market. Figure 6 shows the estimates for the whole earnings profile from ages 25 up to 33. Notice how the impact of the reform on earnings becomes significant only after age 30, and remains stable up to age 33. It is also noteworthy to point out that when examining the impact of the reform on the present value of the child's earnings (between ages 25 and 33) we find it to be positive and statistically significant.

[18] Interestingly, in Table 3, there is also a positive effect on IQ, although only statistical significant in the RD specification. 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 0.2, or 9% 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 nor on birthweight in any of the specifications. We would expect the effect on birthweight to be zero if our empirical strategy is valid as birthweight is pre-determined to changes in mother's time at home.

[19] See also Table 4 where we use different control groups. The first line uses as control group children born to ineligible mothers. The second line presents a triple difference estimator where we take differences across eligibility status (eligible vs. ineligible) and across years (1977 vs. 1975, 1978, 1979). Overall we find the same results although a bit less precise when we use only non-eligible mothers giving birth in 1977 as the control group. Table 5 reports, for comparison, results using the whole sample (eligible and ineligible mothers). Those results compare well with the results for the sample of eligible mothers, but the coefficients are smaller and only log earnings at age 30 are statistically different from zero. This is expected since 35% of all mothers are not affected by the reform, so by using the whole sample the estimated impact of the reform will be diluted. The reason we perform this check is because it gives us estimates for a sample that is independent of the procedure used to define eligibility (although we also checked that alternative definitions of eligibility status have no impact on our results).

[20] There are also less clear patterns for years of schooling and ability, and no break in teenage pregnancy.

Next we examine how sensitive our results are to the choice of bandwidth. Figure 7 shows estimates of the impact of the reform (and corresponding confidence intervals) for different values of the bandwidth (which vary along the horizontal axis).For the main outcomes we consider: dropout rates, college attendance and log earnings age 30. The graphs on the left correspond to RD estimates, and those on the right correspond to RD-DD estimates.

The point estimates are not very sensitive to the choice of bandwidth but, as expected, the RD results are less precise for the smaller bandwidth. This is less of an issue for the DD estimates which are not as dependent on the observations in the immediate vicinity of the date of the reform.

The next question we ask is how sensitive the results in Table 3 are to observations in the vicinity of the discontinuity. This is important because of the potential of strategic behaviour as indicated by Gans and Leigh (2009). In order to address this issue, we present different estimates of the impact of the reform obtained by successively removing from the sample children born within one, two, three, four, five and six weeks on either side of the date of the reform, and re-estimating the model on the remaining sample (Barreca, Guldi, Lindo, and Waddell, 2012). The results for this exercise are shown in Figures 8.1. (dropout rates), 8.2. (college attendance) and 8.3. (log earnings at age 30) for both the RD, and the RD-DD specifications used in Tables 3 and 4. On the horizontal axis of each graph we show how many weeks of births we are deleting on either side of the discontinuity when computing each estimate. On the vertical axis we show the size of each estimate. The graphs on the left of each figure show RD estimates and those on the right show RD-DD estimates.

The plain RD results are robust to small changes in the sample. However, they are sensitive to very large changes in the sample. This is expected since the RD design only makes sense when we use observations in the close vicinity of the date of the reform. By deleting from the sample birth dates close to that date, we can no longer apply this method.

Nevertheless, the DD estimates would still be valid since they compare children born of eligible mothers, just before and just after the reform date in 1977, relative to a control group, which can be the group of children born of eligible mothers in adjacent non-reform years, or the group of children born from ineligible mothers (or a combination of both). Figures 8.1. and 8.3. show that our estimates of the impact of the reform on high school dropout rates and log earnings at age 30 are remarkably robust to large changes in the sample (including the deletion from the sample of all births occurring in June and July, which would correspond to dropping births occurring within four to six weeks of the date of the reform). Figure 8.2. shows that the estimated impacts of the reform on college attendance are statistically indistinguishable from zero once we move away from births in the immediate vicinity of the reform date.

Table 6 estimates the "impacts" of being born just after July 1st using births in all the control years (1975, 1978, 1979), and births from ineligible mothers. If our strategy is valid, these numbers should be equal to zero. There is only one coefficient in this table which is statistically different from zero, the estimated impact on earnings using births in 1979. However, the sign is opposite to the sign of the effect we find in 1977.

7. 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 through 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.

7.1 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.[21] 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 four months after giving birth. Second, in a survey conducted in 1977 on fertility behavior of women in Norway (Statistics Norway), 60% of respondents answered that they thought mothers should stay home for the first two years after giving birth. In addition, the reform provides mothers with their full pre-birth salary for four months, 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 (subsequent) maternity leave benefits, by studying the 1992 and 1993 reforms (see Figure 9). Before the April 1992 reform, mothers are able to take 224 days leave 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 (which is right in the middle of 224 and 280). After1992 there is an increase in maternity leave

[21] This is a typical problem when studying reforms that happened a long time ago. The main advantage of going back in time is the measures of long term outcomes for children.

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 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%.[22]

Therefore, we are confident that after the 1977 reform all mothers were taking four months of paid leave. A natural follow-up question is: was there a change in unpaid leave as a result of the reform? The best way to answer this question in our dataset is by studying what happened to maternal income, which includes maternity benefits, before and after the reform (since we do not directly observe days of leave taken).[23] 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 substitution of the additional paid leave mothers became entitled to).

We examined maternal income in the years surrounding the reform for those delivering children just after and just before the reform, and we found no impact of the reform on these variables. This is shown in Table 7, and it indicates that there was no change in unpaid leave taken by mothers. This result holds independently of the measure of earnings we take: income in 1977, average income between 1976 and 1978, or average

[22] 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.
[23] Remember that all maternity benefits are part of our measure of income.

income between 1975 and 1979. 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 childbirth.

We find no effects of the reform on the amount of unpaid leave taken by mothers, as shown in Table 7. This is not surprising since we emphasized above 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 immediately 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. Even with no average response in unpaid leave, it is interesting to see if there are any effects across the distribution of unpaid leave. In Figure 10, we see no such responses. We cannot rule out that not all mothers took four 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).

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, below 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.

7.2 Maternal Labour Market Outcomes

It is possible that the reform increased the labor 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 more than a year after giving 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 during their first year of life, 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 7 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, nor on earnings[24] 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 11 we present the results of Table 7 graphically. The figures confirm the results of the table. There is no discontinuity in long term labour market outcomes.

7.3 Maternal Education

[24] 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.

In this section we examine whether impacts of the maternity leave extension varied with the education of the mother.[25] We split the sample into two groups: mothers with less than 10 years of education, and mothers with 10 years or more of education. Results are shown in Table 8.

We see that the fall in the dropout rate resulting from the reform is 3.6 percentage points for children of mothers with less than 10 years of education while it is only 1.8 percentage points for children of mothers with 10 years of education or more. When we look at college attendance, we also find stronger impacts of the reform for children born of mothers with lower education. Interestingly, the pattern is the opposite for earnings at age 30. However, we cannot statistically reject that the impacts of the maternity leave reform do not vary with the education of the mother.

7.4 Results by quartiles of mother's unpaid leave.

Table 9 shows how the impact of the reform on mothers' and children's outcomes varies according to the quartiles of unpaid leave taken by the mother. In principle, this variable should be affected by the reform and therefore we should not condition on it. In practice, we see that the reform has no effect on unpaid leave. 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 on mothers who would take different levels of unpaid leave in the absence of the reform, and on their children.

We see almost no effect on mothers' outcomes at any quartile. The only exception is maternal employment two years after the birth of the child which declines slightly at both extremes of the unpaid leave distribution. This indicates a substantial increase in

[25] In Table 10, we present results by distance to grandparents and centralization (urban vs rural).

mothers' time spent at home across the distribution of eligible mothers (since paid leave has increased for all of them).

For children we see that the effect on dropout rates is very large at the first and second quartiles of unpaid leave (five and two percentage points respectively), while we see no effect in the third and fourth quartiles. This is also confirmed by the earnings results which show that the reform lead to 7% higher earnings in the first and second quartiles, but had no effect in the top 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 the Appendix, which uses time series data to suggest that there was no detectable impact of the reform on breastfeeding). There could exist an impact on maternal-child attachment and less stress in the home, leading to changes in personality traits which make these children less likely to drop out of high school.

7.5 Are there substantial differences in the impact of the reform according to other criteria?

Table 10 shows that the impact of the reform on dropout rates is higher for children born in rural areas, and for those who grew up living close to their grandparents. In contrast, the impacts on earnings are larger for those living in urban areas, and for those growing up far from grandparents. In theory, it could have gone either way. One would think that

mothers living in rural areas are poorer, but also probably far fewer of them are eligible for the reform. So there is probably a differential selection into eligibility by mothers in rural and urban areas. Similarly, living close to grandparents could mean increased availability of low cost childcare. On the other end, the quality of care provided by grandparents could be higher or lower than the alternative care children get when they live far from grandparents.

We have checked and found some 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. Table 11 shows results by quartiles of family income. In contrast to maternal education, these are relatively short run measures of household environments. Most of the impacts of the reform on child outcomes are still stronger for children born of mothers whose pre-birth income is below the median. Interestingly, there are relatively strong and negative impacts of the reform on labor market outcomes of mothers in the upper part of the pre-birth income distribution, although they have no correspondence in terms of child outcomes.

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 total income. If some households are severely credit constrained this may make a difference to the child. Although it is unlikely that severe credit constraints of this sort are important in Norway in this period, our results are consistent with a credit constraints story if we believe that those with low levels of pre-reform income are the most likely to be credit constrained. But there are, of course, other explanations that do not require credit constraints, and are just associated with poverty.

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 (see Table 12).

We also analysed the impact of the reform on older siblings (see Table 13). The fact that mothers spend additional time in the home could benefit 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 found that the impact of the reform on education is mainly driven by females, while the impact of the reform on earnings at age 30 is driven by males (see Table 14).

Table 15 compares the impacts of the reform for first born vs. later born children. It shows that for education and earnings these impacts are stronger for later born children. This is consistent with the idea that time with the mother is important, since later born children would probably face more competition for maternal time than first-born children, and they would therefore benefit more from relaxing the time constraint.

8. Concluding remarks

We investigate the long term consequences of time investments in children during their first year of life. We explore empirically variation in time spent with the child induced by the maternity leave reform in Norway in 1977, offering up to four months of paid leave and an additional full year of unpaid leave. The reform resulted in a substantial increase in time mothers spent at home (off work) after birth, presumably caring for their newborn children, instead of relying on essentially informal care alternatives. We estimate strong

impacts of the reform on children's subsequent high school dropout rates and earnings at age 30, especially for those whose mothers have less than 10 years of education.

In order to understand these results it is important to specify what were the possible childcare arrangements that could work as an alternative to maternal care. There was almost no availability of high quality childcare for under-two-year-olds, so the alternative was essentially grandparents' care, or other types of informal care. Neither of these is necessarily a good substitute to mother's time at this period of a child's life.

Our estimated positive effects of early time with the child on medium to long term outcomes resemble 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).

Our results suggest 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, especially when maternity leave benefits are almost non-existent. This argument has been important for expansions in maternity leave across countries. However, this study is the first to show that this may actually be justified when we think of long term outcomes of children.

The level of maternity leave benefits is remarkably similar in the US today to what it was in Norway before the 1977 reform. Parental leave is currently under debate in the US[26] and an introduction of four months of paid leave and better job protection is typically within feasible policies.[27] Recall, however, the evidence from other countries,

[26] USA TODAY, July 26th 2005, The New York Times, April 16th 2008
[27] http://www.govtrack.us/congress/bill.xpd?bill=h110-3799

namely Germany, Sweden and Denmark, which suggests that expansions of maternity leave benefits on top of already generous systems may be far less effective.

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 larger for children from lower educated households may be important for policy discussions related to lowering inequalities in general. Many countries, such as the US or the UK, have a substantial degree of inequality in education and income, which has been linked to higher rates of intergenerational transmission of poverty. 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.

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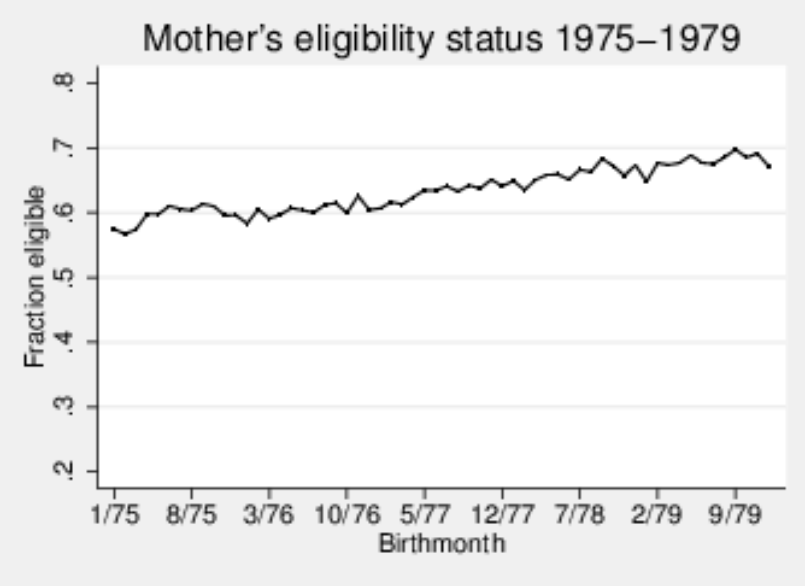
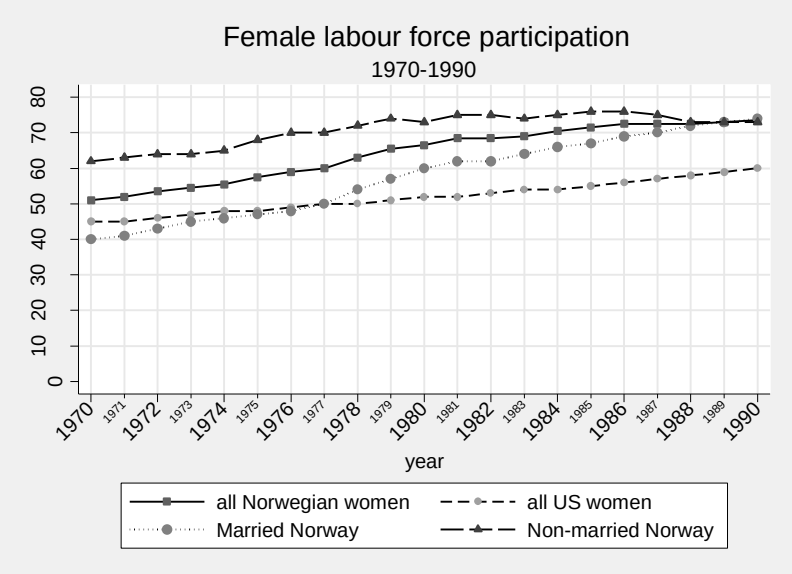
Figure 1. Proportion of mothers eligible for maternity leave in 1975-1979.



Note: The solid line shows the fraction of eligible mothers in the population of births for each birth month ranging from January 1975 to December 1979.

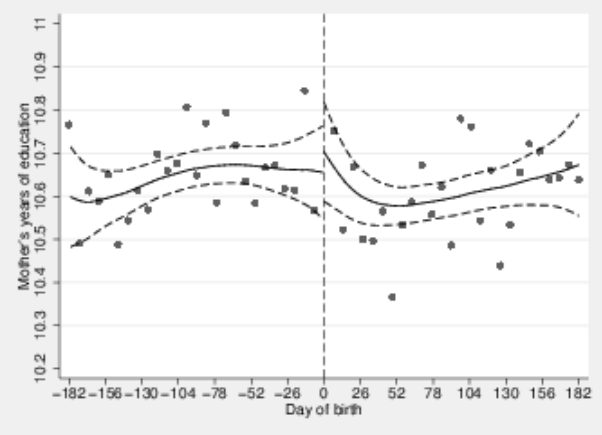
Figure 2. Female employment in Norway and the US from 1970 to 1990.



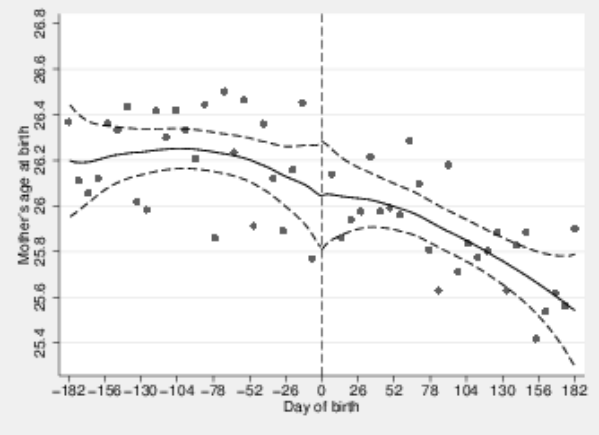
Note: The four lines show the fraction of females working in the labor market. The first two lines show this for all Norwegian and US women. The other two lines split the Norwegian women into married and not married.
Source: Statistics Norway, Bureau of Labor Statistics (projected from Population Bulletin, Vol. 63 (2008), OECD

Figure 3. Mothers' pre-reform characteristics in the neighborhood of the reform date.

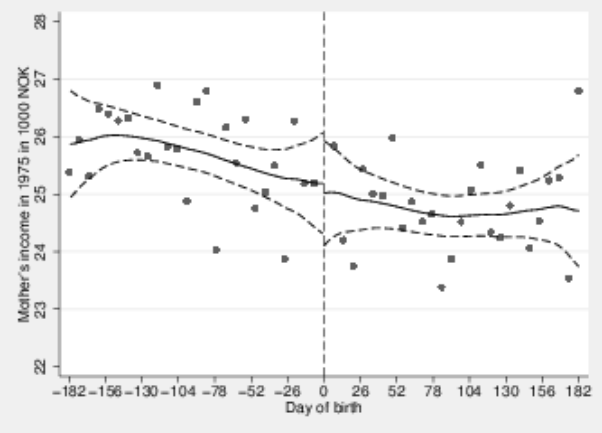
a) Mother's years of education



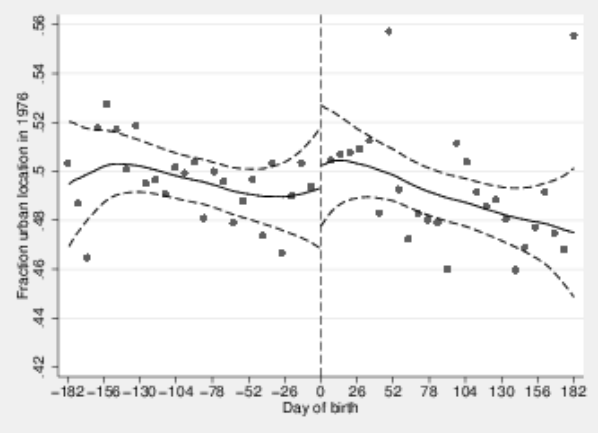
b) Mother's age at birth



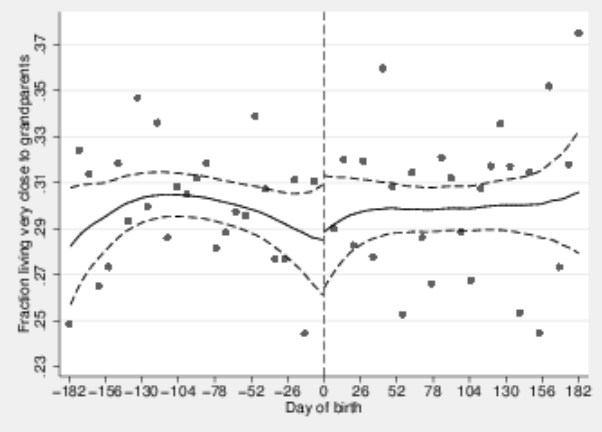
b) Mother's income in 1975



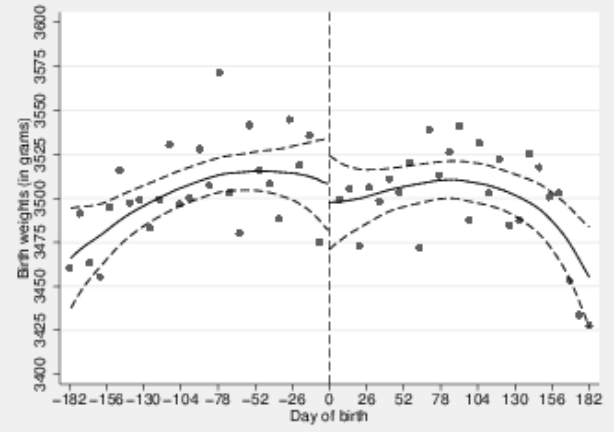
d) Parents urban location in



e) Distance to grandparents in 1980

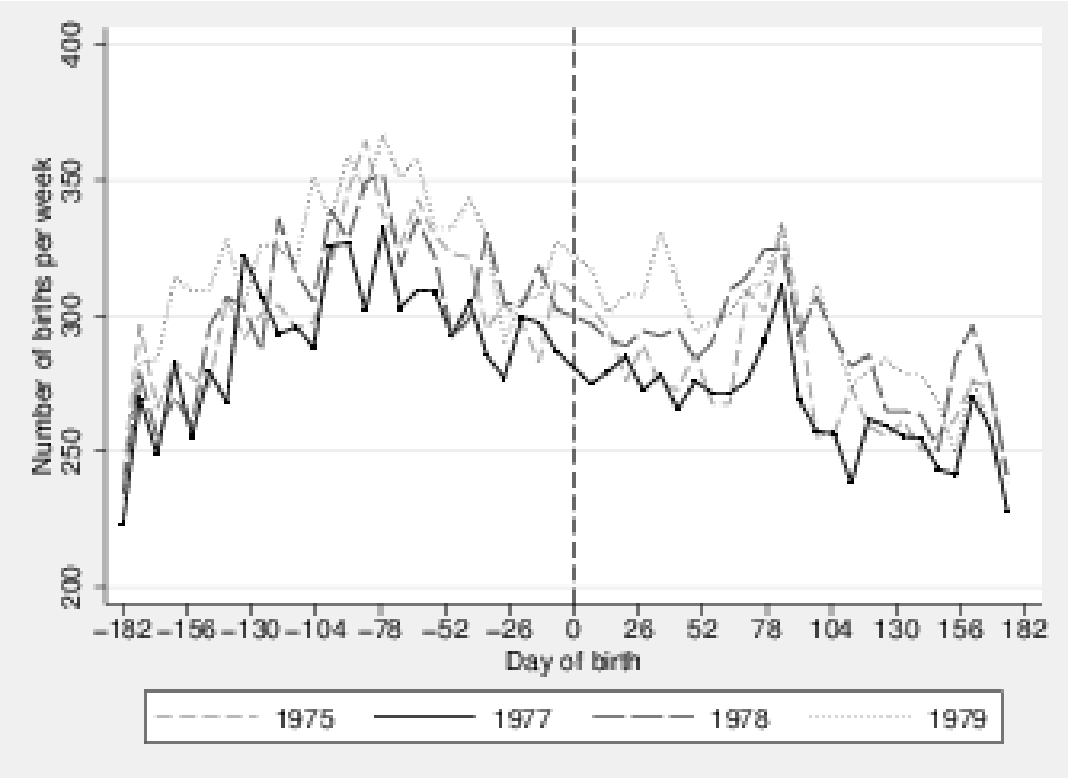


f) Birth weight child in grams



Note: Each dot in the graphs corresponds to the average value of each outcome, according to birth date, organized into one-week bins. Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to 0). The solid line represents fitted values for a local linear regression with bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on each side of the discontinuity). The dashed line is the 95 % confidence interval.

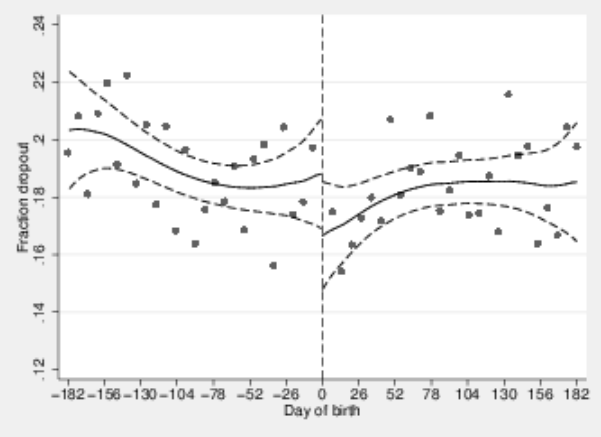
Figure 4. The number of children born to eligible mothers in 1975, 1977, 1978 and 1979, by week of birth.



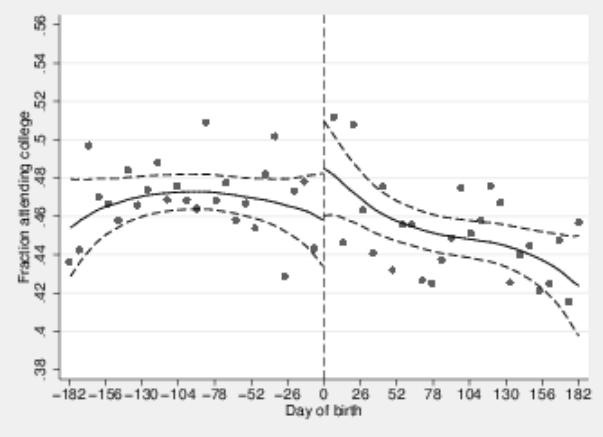
Note: Dashed vertical line denotes the reform cutoff of July 1, 1977 (normalized to 0). The window includes all children born in 1975, 1977, 1978 and 1979, respectively, born to eligible mothers (182 days on each side of the discontinuity). The different lines plot the average number of births in one-week intervals for each year separately.

Figure 5. Impact of the Reform on Children's Outcomes.

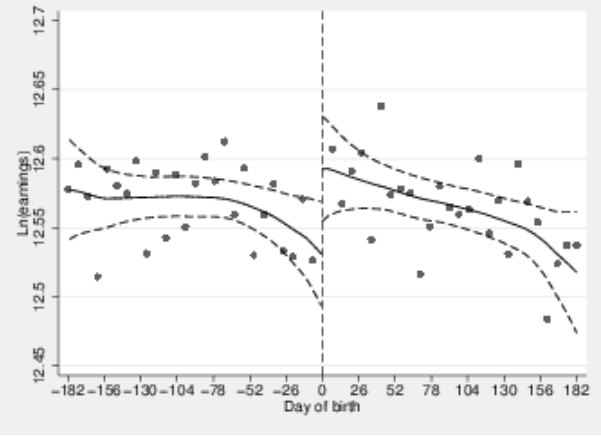
a) Dropout rates



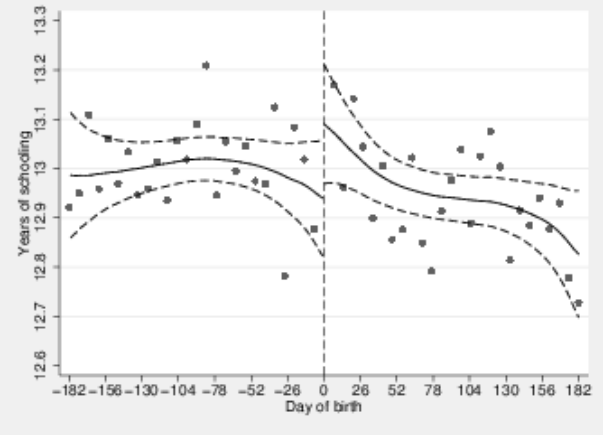
b) College attendance



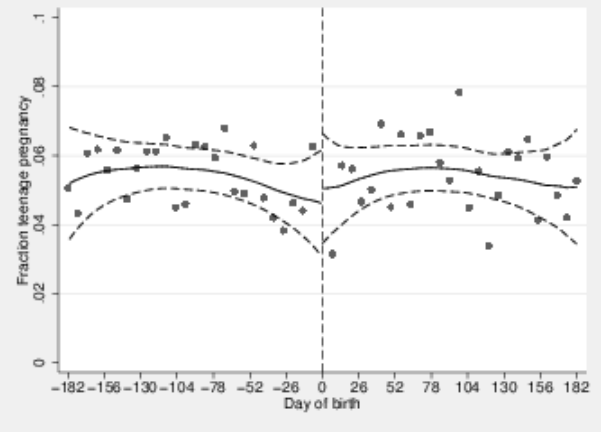
c)Ln(earnings) age 30



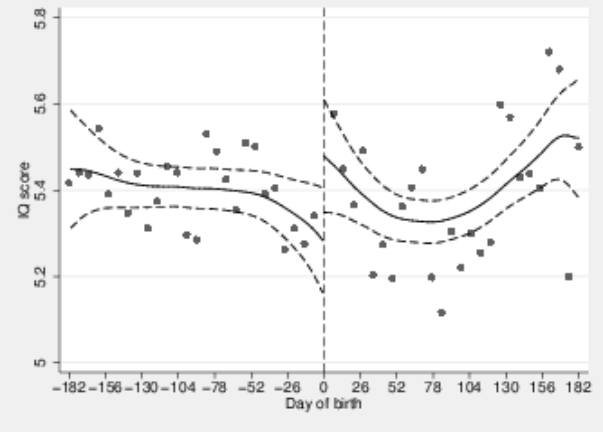
d) Years of schooling



e) Teenage pregnancy



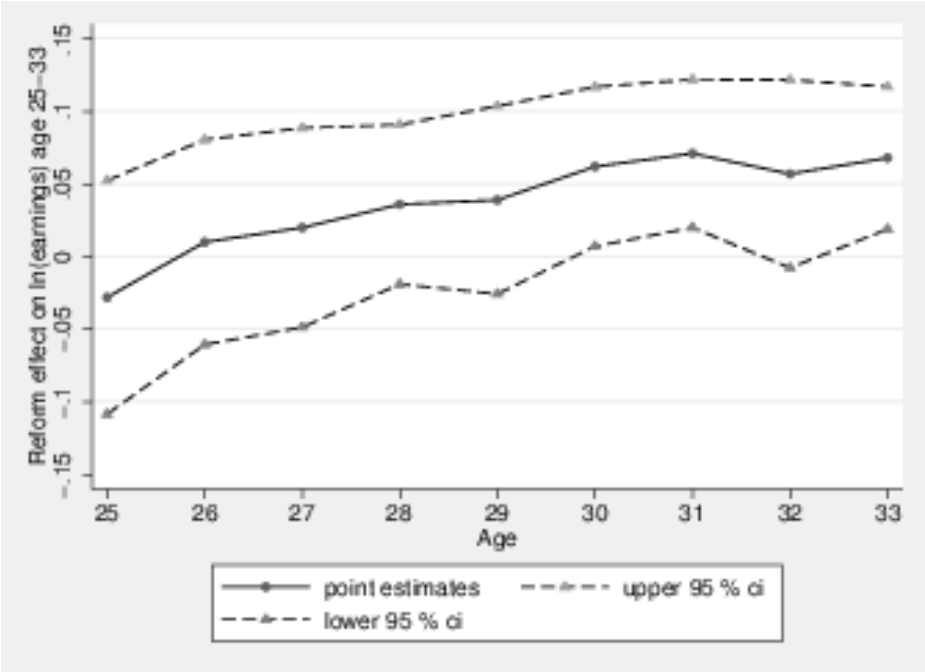
f) Ability



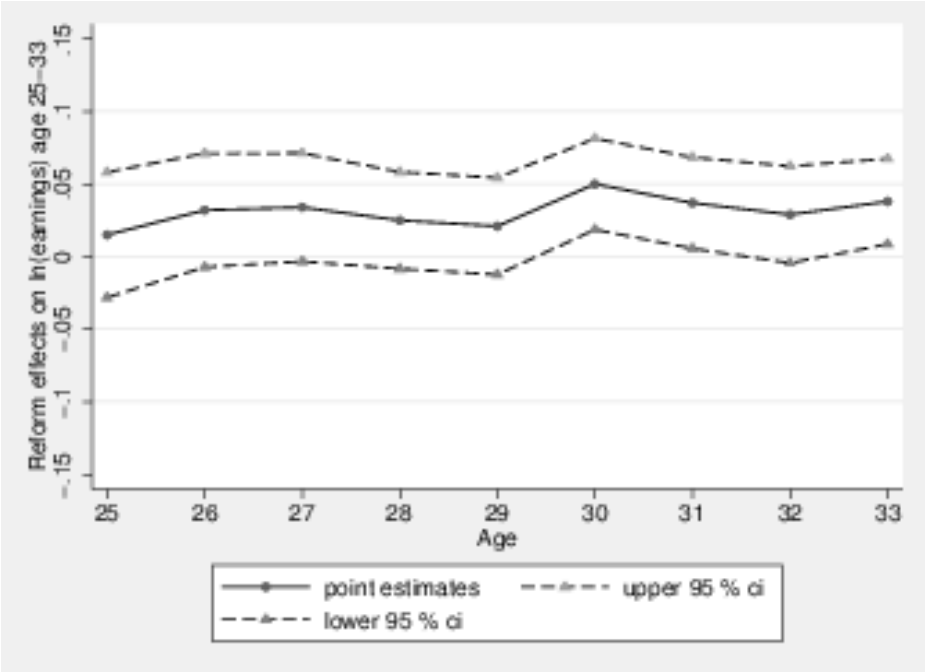
Note: Each dot in the graphs corresponds to the average value of each outcome, according to birth date, organized into one-week bins. Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to 0). The solid line represents fitted values for a local linear regression with bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on each side of the discontinuity). The dashed line is the 95 % confidence interval.

Figure 6. **Effects of the reform on children's log earnings at ages 25-33.**

a) Effects on children's log earnings at ages 25-33 – RD estimates



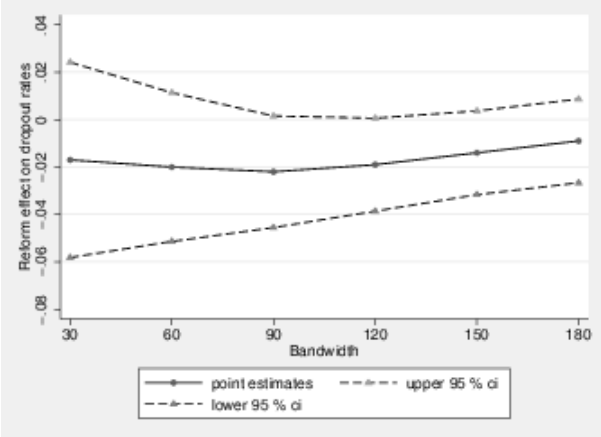
b) Effects on children's log earnings at ages 25-33 – RD-DD estimates



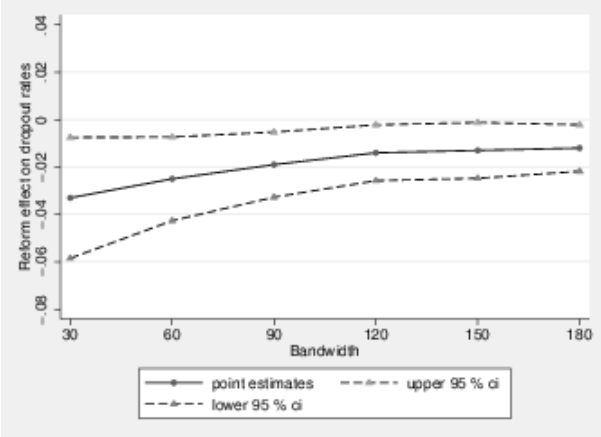
Note: The line labeled point estimates is the reform effect from the RD (panel a) and RD-DD (panel b) on log earnings. The dashed line is the 95 % confidence intervals. On the x-axis are ages of children, ranging from age 25 to 33.

Figure 7. **How the estimated impacts on dropout rates, college attendance and log earnings age 30 change with different bandwidths.**

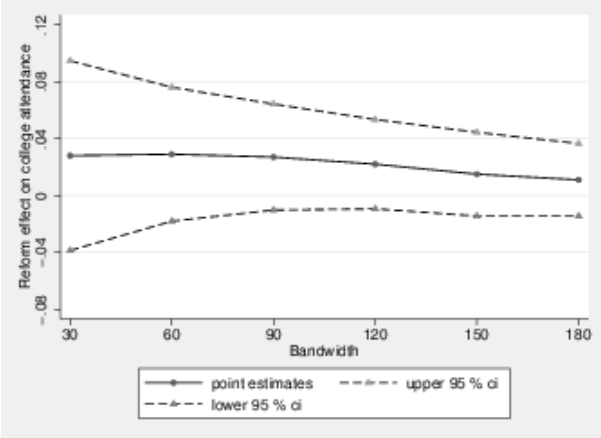
a) Dropout rates - RD



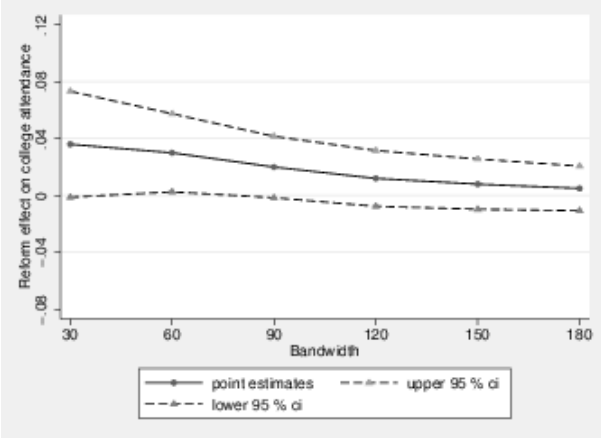
b) Dropout rates - RD-DD



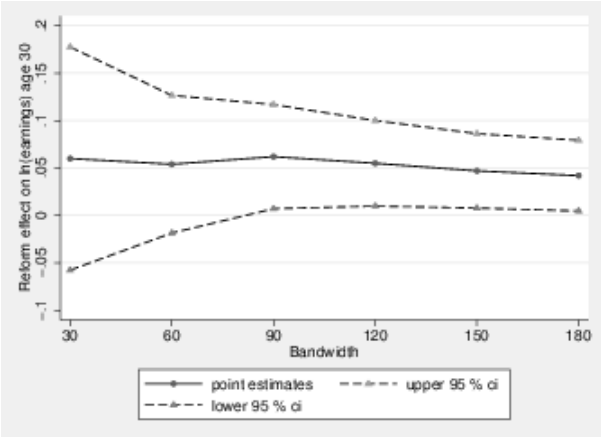
c) College Attendance - RD



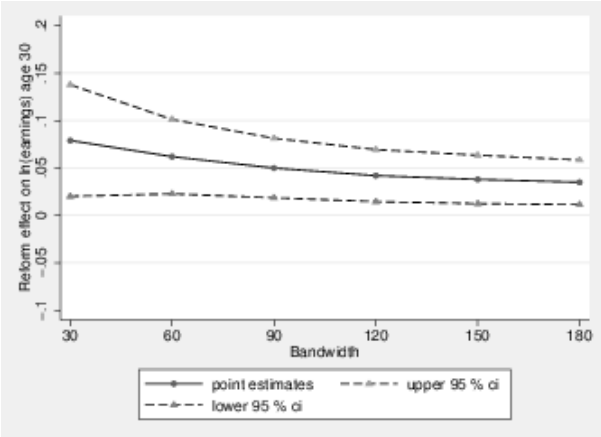
d) College Attendance - RD-DD



e) Ln(earnings) age 30 - RD



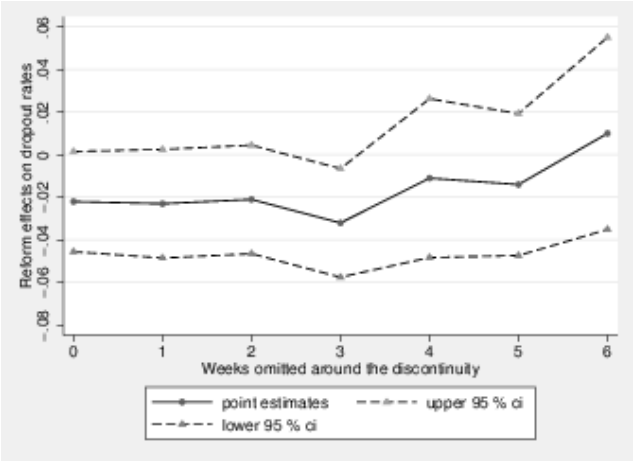
f) Ln(earnings) age 30 - RD-DD



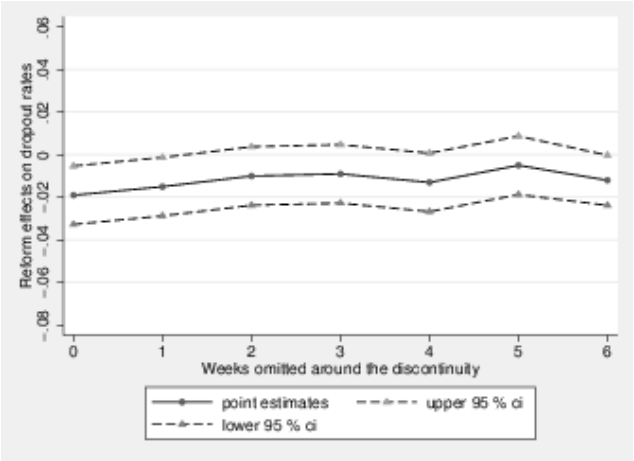
Note: The line labeled point estimates shows the reform effect on dropout rates (panels a and b), college attendance (panels c and d) and log earnings age 30 (panels e and f). The dashed line is the 95 % confidence intervals. On the x-axis are different bandwidths ranging from 30-180 days.

Figure 8.1.: **Robustness to Changes in Window – Dropout rates.**

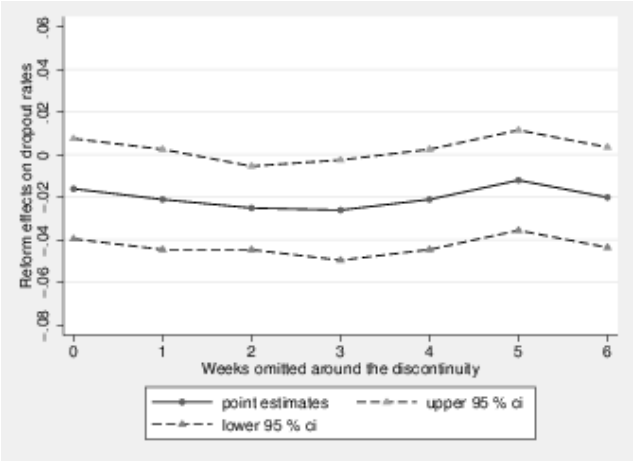
a) Dropout rates - RD



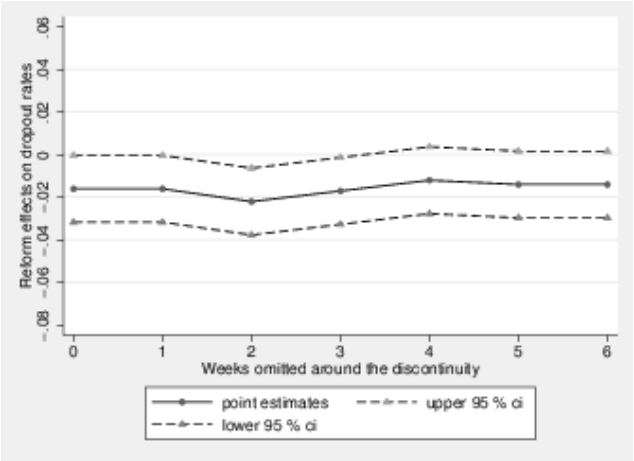
b) Dropout rates – RD-DD using non-reform years as controls



c) Dropout rates – RD-DD using non-eligible mothers as control



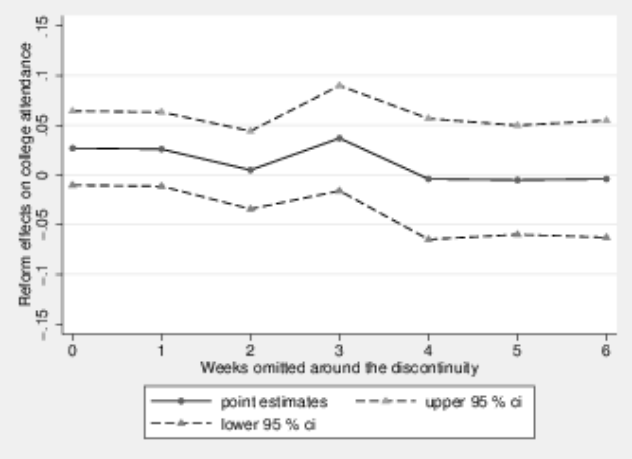
d) Dropout rates – RD-DD using non-eligible mothers and non-reform years as control



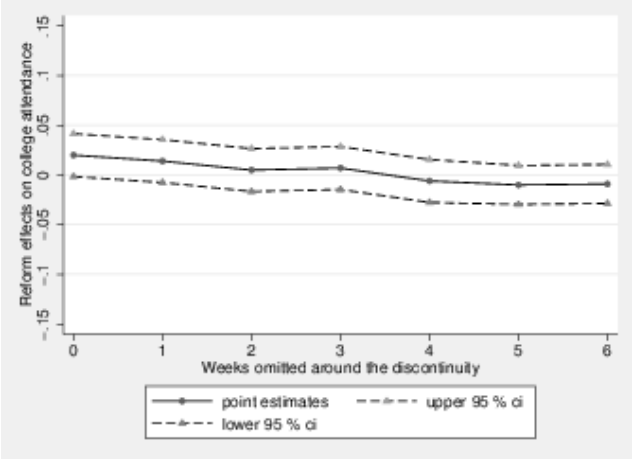
Note: The line called point estimates is the reform effects on dropout rates from the RD (panel a) RD-DD (panel b), RD-DD non-eligible (panel c) and RD-DD years &n oneligibles (panel d). The dashed line is the 95 % confidence intervals. On the x-axis are different weeks omitted around the discontinuity. We always keep the window constant. The baseline we use in all regressions is 0 weeks omitted around the discontinuity.

Figure 8.2.: **Robustness to Changes in Window – College attendance.**

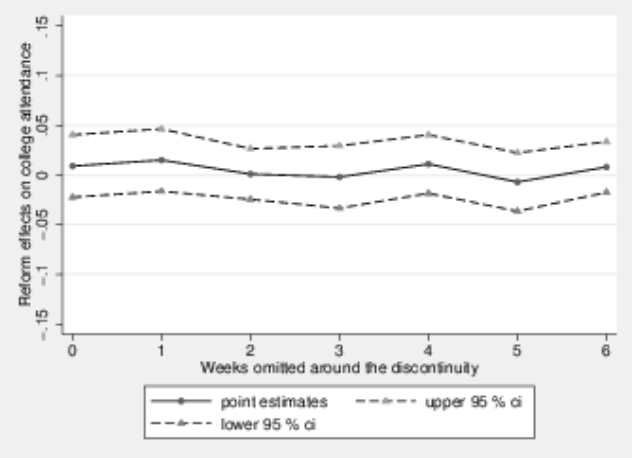
a) College attendance- RD



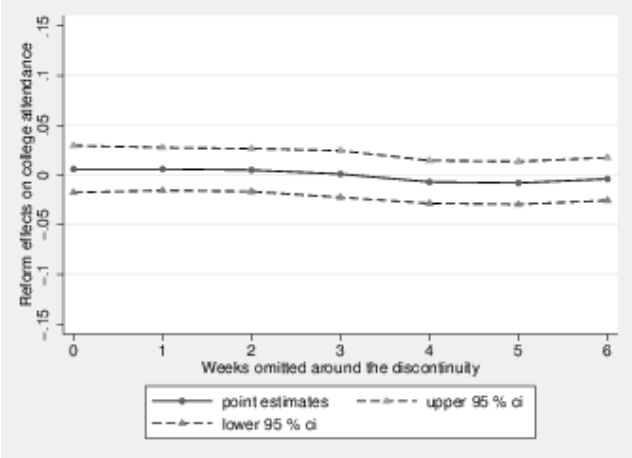
b) College attendance – RD-DD using non reform years as controls



c) College attendance – RD-DD using non eligible mothers as controls



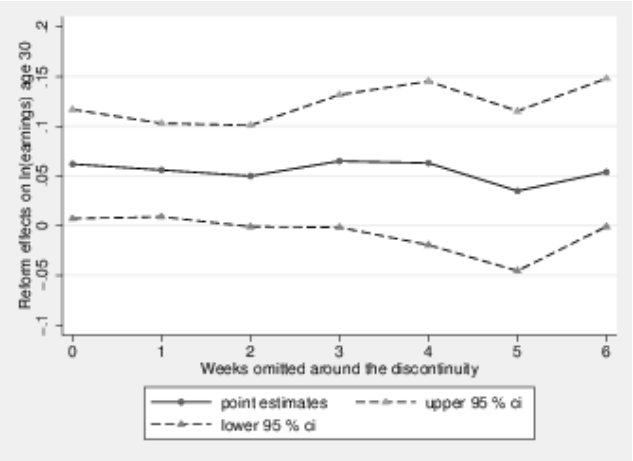
d) College attendance – RD-DD using non eligible mothers and non reform years as control



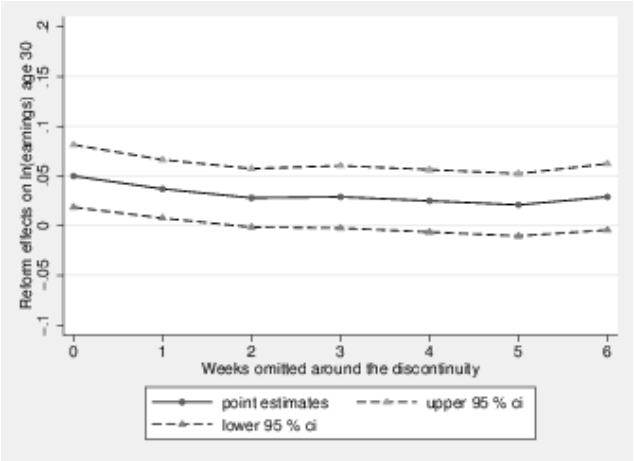
Note: The line called point estimates is the reform effects on college attendance from the RD (panel a) RD-DD (panel b), RD-DD non-eligible (panel c) and RD-DD years & noneligibles (panel d). The dashed line is the 95 % confidence intervals. On the x-axis are different weeks omitted around the discontinuity. We always keep the window constant. The baseline we use in all regressions is 0 weeks omitted around the discontinuity.

Figure 8.3.: **Robustness to Changes in Window – Ln(earnings) age 30**

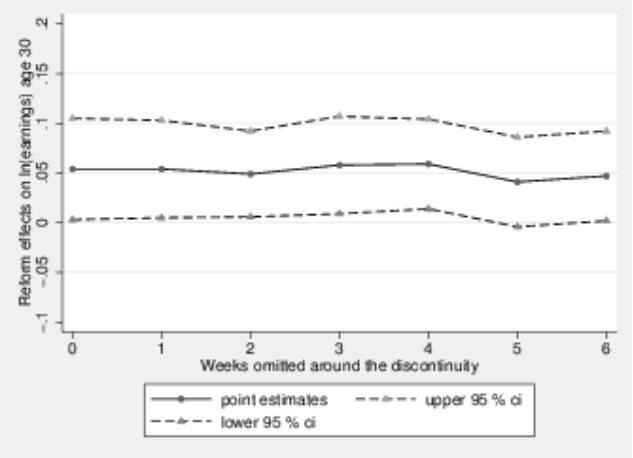
a) Ln(earnings) age 30 - RD



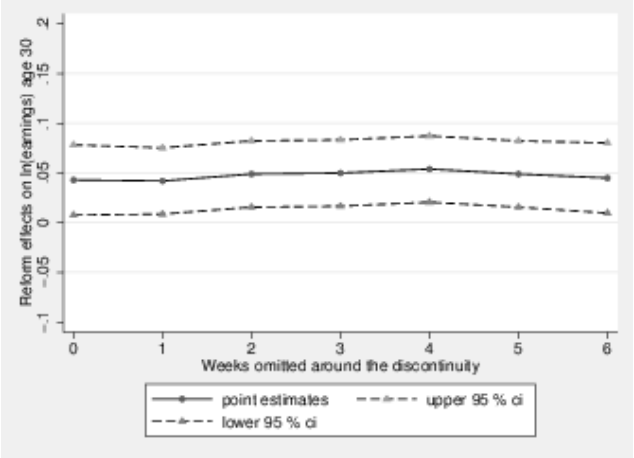
b) Ln(earnings) age 30 – RD-DD using non reform years as controls



c) Ln(earnings) age 30 – RD-DD using non eligible mothers as controls



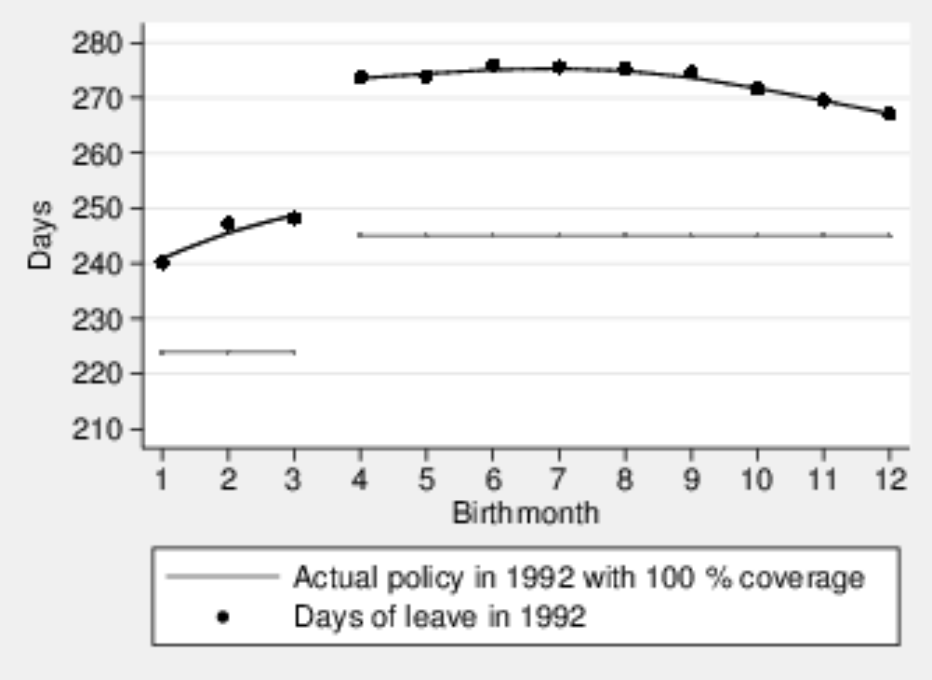
d) Ln(earnings) age 30 – RD-DD using non eligible mothers and non- reform years as controls



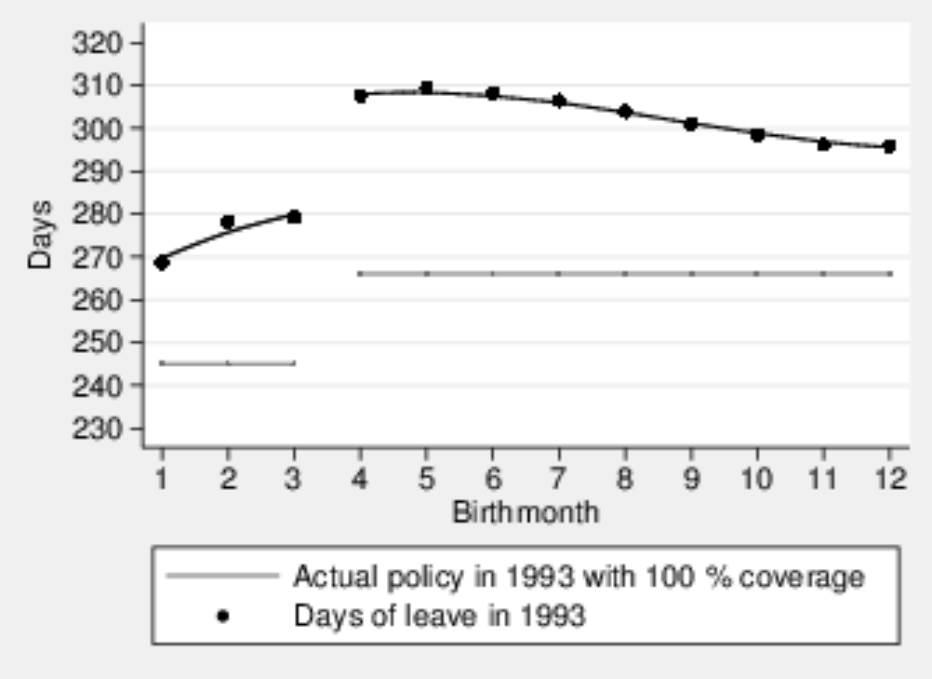
Note: The line called point estimates is the reform effects on ln(earnings) age 30 from the RD (panel a) RD-DD (panel b), RD-DD non-eligible (panel c) and RD-DD years & noneligibles (panel d). The dashed line is the 95 % confidence intervals. On the x-axis are different weeks omitted around the discontinuity. We always keep the window constant. The baseline we use in all regressions is 0 weeks omitted around the discontinuity.

Figure 9. Impacts of reforms in 1992 and 1993 on mothers' days of paid leave

a) Days of leave in 1992

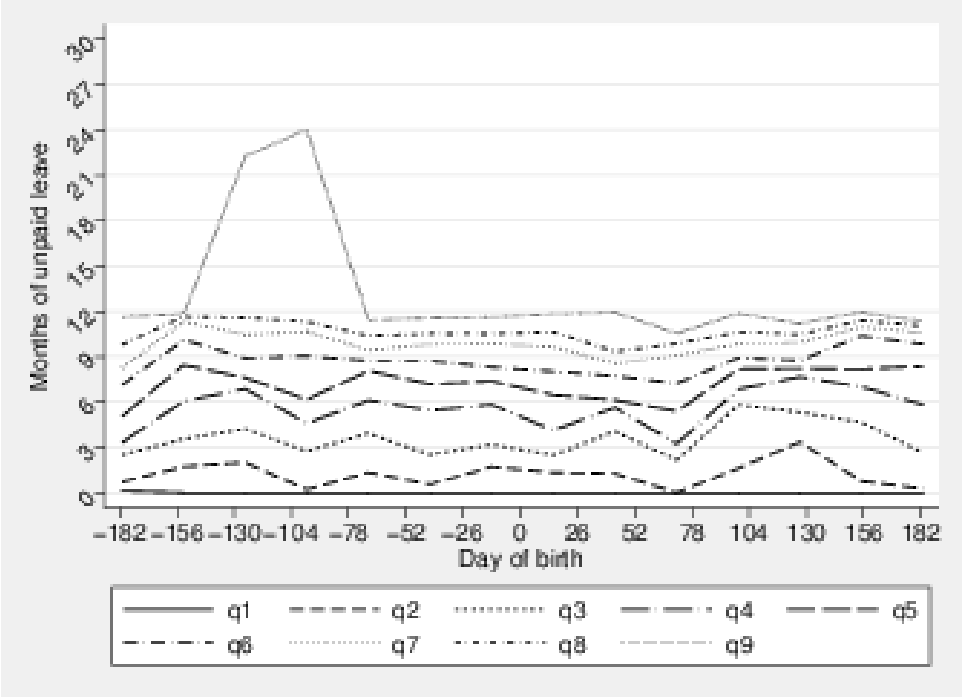


b) Days of leave in 1993



Note: Each observation is the average outcomes in one-month bins based on the birthmonth of the child. The reforms are on April 1, 1992 and April 1, 1993. The solid line is a fitted triangular local linear regression with bandwidth of 3 months. The window includes all children born in 1992 (panel a) and 1993 (panel b) to eligible mothers. The grey line is the actual policy in 1992 and 1993 respectively.

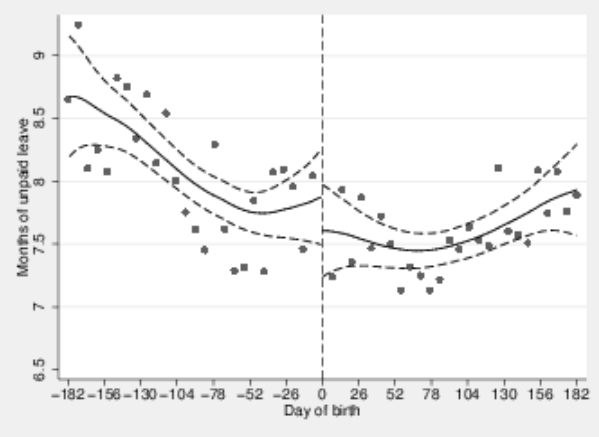
Figure 10. Impacts of the reform on quantiles of predicted months of unpaid leave



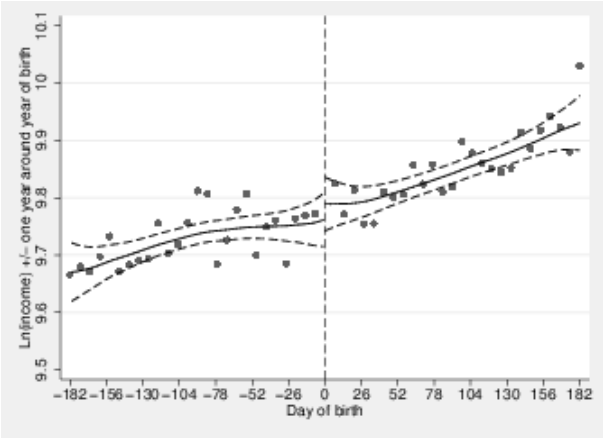
Note: The reform cutoff of July 1, 1977 is normalized to 0. The window includes all children born in 1977 to eligible mothers (182 days on each side of the discontinuity). The different lines plot the average number of months of unpaid leave in one-week intervals for each quantile of predicted unpaid leave taken by mother (1-9) separately.

Figure 11. Impacts of the Reform on Mother's outcomes.

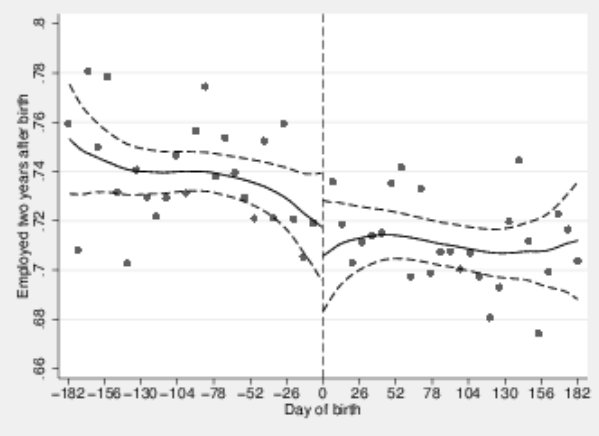
a) Predicted months of unpaid leave



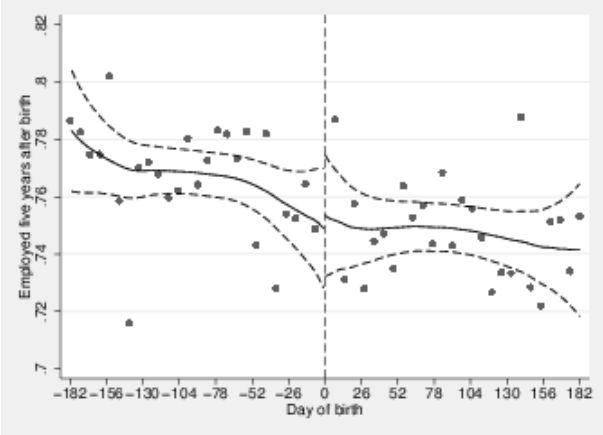
b) log income in the years before and after birth



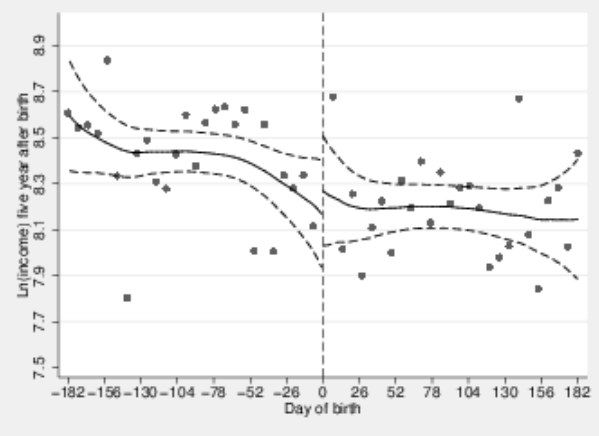
c) Employed two years after birth



d) Employed five years after birth



e) log income five years after birth



Note: Each observation is the average outcomes in one-week bins based on the birthdate of the child. Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to 0). The solid line is a fitted triangular local linear regression with bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on each side of the discontinuity). The dashed line is the 95 % confidence interval.

Table 1. Differences in average outcomes of children born in June and July 1977

Birth month	Single Difference	Differences-in-differences using 1975, 1978 and 1979 as controls
Children		
High School Dropout	-0.020*	-0.032**
	(0.011)	(0.013)
College Attendance	0.017	0.036**
	(0.014)	(0.016)
Log Earnings at Age 30	0.045**	0.072***
	(0.022)	(0.026)
Mothers		
Pre-Reform Characteristics		
Years of Education	-0.023	-0.009
	(0.063)	(0.071)
Log Income Two Years Prior to the Birth of the Child	-0.014	0.003
	(0.031)	(0.029)
Outcomes		
Average Log Income +/- one year around year of birth	0.037	0.008
	(0.027)	(0.031)
Employed 5 Years After the Birth of the Child	-0.002	-0.007
	(0.012)	(0.014)
Log Income 5 Years after the Birth of the Child	-0.018	-0.080
	(0.138)	(0.157)

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, 1978 and 1979 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. ***significant at 1 %, **significant at 5%, *significant at 10%

Table 2. Descriptive statistics for eligible and non-eligible mothers in 1977.

Eligibility status	Eligible 1977	Non-eligible 1977
Children		
High School Dropout	0.186	0.276
	(0.388)	(0.447)
College attendance	0.46	0.35
	(0.50)	(0.48)
Log Earnings at Age 30	12.6	12.5
	(0.74)	(0.76)
Mothers		
Years of Education	10.63	9.61
	(2.18)	(1.72)
Age at Birth	26.1	26.5
(in years)	(0.028)	(0.041)
Income in 1975*	94088	10563
(in NOK)	(68621)	(26417)
Employed 2 years After Birth	0.725	0.362
	(0.447)	(0.481)
Employed 5 years After Birth	0.758	0.534
	(0.428)	(0.499)
Income in 1982*	71216	29434
(in NOK)	(73324)	(48202)

*CPI adjusted to 1998 NOK

Note: The first column presents the main variables we focus on in the paper, the second and third columns gives the mean of the variables for the group of eligible and ineligible mothers in 1997. The standard deviation is presented in round brackets below each mean.

Table 3. Impact of the Reform on Children's Outcomes

Variables	Dropout rate	College attendance	Ln (earnings) age 30
Estimates			
RD	-0.022*	0.027	0.062**
	(0.012)	(0.019)	(0.028)
	[0.18]	[0.47]	[12.6]
	15,025	15,025	14,348
RD-DD years (1975, 1978 and 1979)	-0.019**	0.020*	0.050***
	(0.007)	(0.011)	(0.016)
	[0.19]	[0.44]	[12.5]
	63,571	63,571	60,732

Variables	Completed years of schooling	Teenage pregnancy (females)	IQ (males)	Birth weight
Estimates				
RD	0.152	0.004	0.200**	-10.54
	(0.093)	(0.013)	(0.092)	(22.0)
	[13.0]	[0.054]	[5.4]	[3518]
	15,025	7,194	6,838	14,979
RD-DD years (1975, 1978 and 1979)	0.116**	-0.001	0.084	0.429
	(0.053)	(0.007)	(0.054)	(12.8)
	[12.8]	[0.051]	[5.4]	[3505]
	63,571	30,737	29,075	63,388

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD estimates use only eligible births in 1977 while the RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 4. Children's outcomes. Alternative non-parametric DD specifications.

Variables	Dropout rate	College attendance	Ln(earnings) age 30
Estimates			
RD-DD non-eligibles	-0.016	0.009	0.054**
	(0.012)	(0.016)	(0.026)
	[0.21]	[0.43]	[12.5]
	23,658	23,658	22,523
RD-DDD	-0.016**	0.006	0.043**
Years and non-eligibles	(0.008)	(0.012)	(0.018)
	[0.22]	[0.40]	[12.5]
	98,455	98,455	93,731

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD estimates use only eligible births in 1977 while the RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 5. Impact of the Reform on children's outcomes for both eligible and ineligible mothers.

Variables	Dropout rate	College attendance	Ln (earnings) age 30
Estimates: children			
RD-DD years (1975, 1978 and 1979)	-0.011	0.011	0.026**
	(0.007)	(0.009)	(0.013)
	[0.22]	[0.40]	[12.5]
	98,455	98,455	93,731

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 6. *Impacts* of the reform on control groups

Variables	Dropout rate	College attendance	Ln (earnings) age 30
Estimates			
RD 1975	0.012	-0.019	-0.018
	(0.012)	(0.017)	(0.021)
	[0.20]	[0.47]	[12.4]
	15,818	15,818	15,140
RD-1978	0.018	-0.019	-0.004
	(0.013)	(0.016)	(0.021)
	[0.17]	[0.43]	[12.5]
	16,053	16,053	15,325
RD-1979	0.016	-0.026	-0.055*
	(0.012)	(0.016)	(0.033)
	[0.20]	[0.42]	[12.5]
	16,675	16,675	15,919
Non-Eligible in 1977	-0.022	0.020	0.024
	(0.023)	(0.025)	(0.040)
	[0.27]	[0.35]	[12.5]
	8,633	8,633	8,175

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the placebo maternity leave reform on July 1st 1975 (first row), 1978 (second row), 1979 (third row) and 1977 (fourth row). We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD estimates for the first three rows use only eligible births in 1975, 1978 and 1979 respectively (182 days on each side of the discontinuity). The RD estimates for the last row use only non-eligible births in 1977 (182 days on each side of the discontinuity). ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 7. Impacts of the reform on mother's income around time of birth

Variables	Log income Year of birth	Log income One year before and one year after birth	Log income Two years before and two years after birth
Estimates			
RD	0.070	0.027	0.019
	(0.108)	(0.034)	(0.033)
	[8.4]	[9.8]	[9.8]
	15,025	15,025	15,025
RD-DD years	-0.032	-0.009	-0.007
(1975, 1978 and 1979)	(0.070)	(0.021)	(0.021)
	[9.0]	[10.2]	[10.3]
	63,571	63,571	63,571

Variables	Predicted months of unpaid leave	Employed 2 years after birth	Employed 5 years after birth	Ln(Income) 5 years after birth
Estimates				
RD	-0.275	-0.012	0.005	0.107
	(0.230)	(0.015)	(0.016)	(0.182)
	[7.7]	[0.74]	[0.76]	[8.4]
	15,025	15,025	15,025	15,025
RD-DD years	0.002	-0.016*	-0.009	-0.100
(1975, 1978 and 1979)	(0.160)	(0.010)	(0.009)	(0.103)
	[7.6]	[0.72]	[0.76]	[8.3]
	63,571	63,571	63,571	63,571

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD estimates use only eligible births in 1977 while the RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 8. Children's and mothers' outcomes by mother's education. Non-parametric RD-DD regressions.

Variables		
	Mother's education	
subgroups	Less than 10 years	10 years or more
Children		
Dropout rate	-0.036**	-0.018*
	(0.015)	(0.010)
	[0.28]	[0.14]
	21,219	41,430
College attendance	0.030*	0.020
	(0.018)	(0.013)
	[0.30]	[0.52]
	21,219	41,430
Ln(earnings) at age 30	0.042	0.057***
	(0.027)	(0.020)
	[12.4]	[12.5]
	20,269	39,602
Mothers		
Predicted months of unpaid leave	-0.167	-0.004
	(0.314)	(0.184)
	[12.4]	[6.8]
	21,219	41,430
Employed 2 years after birth	-0.015	-0.013
	(0.017)	(0.012)
	[0.64]	[0.77]
	21,219	41,430
Employed 5 years after birth	0.003	-0.015
	(0.017)	(0.010)
	[0.70]	[0.79]
	21,219	41,430
Ln(Income) 5 years after birth	0.066	-0.184
	(0.188)	(0.116)
	[7.5]	[8.8]
	21,219	41,430

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). The second column shows results when mothers have less than 10 years of education, the third column shows results when mothers have 10 years or more of education. We have missing information on mother's education for 922 observations. ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 9. Children's and mothers' outcomes by quartiles of mother's months of unpaid leave. Non-parametric RD-DD regressions.

Variables				
	Quartiles of mothers months of unpaid leave			
Quartiles	**1 (lowest)**	**2**	**3**	**4 (highest)**
Average levels of unpaid leave (Std.Dev)	0.40 (0.63)	4.64 (1.39)	8.55 (0.80)	16.68 (9.8)
Children				
Dropout rate	-0.053*** (0.015) [0.17] 15,893	-0.028* (0.016) [0.17] 15,893	0.010 (0.016) [0.19] 15,892	-0.006 (0.017) [0.24] 15,893
College attendance	0.032 (0.025) [0.50] 15,893	0.007 (0.021) [0.48] 15,893	0.008 (0.021) [0.43] 15,892	0.033 (0.022) [0.37] 15,893
Ln(earnings) at age 30	0.074** (0.032) [12.5] 15,183	0.077** (0.034) [12.5] 15,197	0.035 (0.031) [12.5] 15,234	0.014 (0.031) [12.5] 15,118
Mothers				
Employed 2 years after birth	-0.012* (0.007) [0.97] 15,893	-0.000 (0.011) [0.92] 15,893	-0.007 (0.021) [0.62] 15,892	-0.035* (0.020) [0.39] 15,893
Employed 5 years after birth	0.011 (0.013) [0.91] 15,893	-0.021 (0.015) [0.86] 15,893	-0.022 (0.020) [0.68] 15,892	-0.000 (0.020) [0.58] 15,893
Ln(Income) 5 years after birth	0.203 (0.154) [10.5] 15,893	-0.308* (0.172) [9.6] 15,893	-0.274 (0.214) [7.3] 15,892	0.010 (0.219) [6.0] 15,893

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). The second column shows results for the lowest quartile of mother's months of unpaid leave, the third column for the second quartile, the fourth column for the third quartile and the last column for the highest quartile. The first row gives the average levels of unpaid leave in each quartile respectively.***significant at 1 %, **significant at 5%, *significant at 10%.

Table 10. Children's and mothers' outcomes by urbanization and distance to grandparents. Non-parametric RD-DD regressions.

Variables				
	Localization		**Distance to grandparents**	
subgroups	Urban	Rural	Close	Far
Children				
Dropout rate	-0.016	-0.021*	-0.037**	0.001
	(0.011)	(0.011)	(0.017)	(0.010)
	[0.19]	[0.19]	[0.19]	[0.19]
	31,569	32,002	15,945	36,912
College attendance	0.016	0.023	0.021	0.017
	(0.015)	(0.016)	(0.023)	(0.015)
	[0.45]	[0.44]	[0.42]	[0.44]
	31,569	32,002	15,945	36,912
Ln(earnings) at age 30	0.065***	0.035	0.047	0.053**
	(0.023)	(0.024)	(0.029)	(0.021)
	[12.5]	[12.5]	[12.5]	[12.5]
	30,034	30,698	15,375	35,189
Mothers				
Predicted months of unpaid leave	0.097	-0.076	0.359	-0.013
	(0.217)	(0.225)	(0.328)	(0.200)
	[7.4]	[7.9]	[8.1]	[7.6]
	31,569	32,002	15,945	36,912
Employed 2 years after birth	-0.018	-0.015	-0.005	-0.021*
	(0.014)	(0.013)	(0.020)	(0.012)
	[0.73]	[0.71]	[0.70]	[0.72]
	31,569	32,002	15,945	36,912
Employed 5 years after birth	-0.016	-0.001	-0.007	-0.009
	(0.013)	(0.013)	(0.018)	(0.012)
	[0.76]	[0.76]	[0.74]	[0.75]
	31,569	32,002	15,945	36,912
Ln(Income) 5 years after birth	-0.160	-0.050	-0.064	-0.115
	(0.143)	(0.148)	(0.203)	(0.133)
	[8.4]	[8.3]	[8.0]	[8.2]
	31,569	32,002	15,945	36,912

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). The second and third columns show results when parents are from an urban or rural area. The fourth and fifth columns show results when children live close or far from grandparents. We have missing information on distance to grandparents for 10714 observations. ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 11. Children's and mothers' outcomes by quartiles of family income two years prior to birth. Non-parametric RD-DD regressions.

Variables				
	Quartiles of ln(family income) two years prior to birth			
Quartiles	**1 (lowest)**	**2**	**3**	**4 (highest)**
Children				
Dropout rate	-0.030*	-0.020	-0.015	-0.007
	(0.016)	(0.016)	(0.015)	(0.014)
	[0.24]	[0.21]	[0.18]	[0.14]
	15,893	15,893	15,892	15,893
College attendance	0.033	0.038*	-0.021	0.028
	(0.024)	(0.022)	(0.021)	(0.022)
	[0.41]	[0.42]	[0.45]	[0.50]
	15,893	15,893	15,892	15,893
Ln(earnings) at age 30	0.041	0.081**	0.025	0.042
	(0.036)	(0.033)	(0.033)	(0.035)
	[12.4]	[12.5]	[12.5]	[12.5]
	15,090	15,227	15,248	15,167
Mothers				
Predicted months of unpaid leave	0.054	-0.095	0.001	0.176
	(0.391)	(0.334)	(0.327)	(0.316)
	[8.4]	[8.1]	[7.4]	[6.6]
	15,893	15,893	15,892	15,893
Employed 2 years after birth	0.015	-0.017	-0.052***	-0.011
	(0.019)	(0.019)	(0.018)	(0.018)
	[0.69]	[0.70]	[0.73]	[0.77]
	15,893	15,893	15,892	15,893
Employed 5 years after birth	0.006	0.019	-0.030	-0.037**
	(0.017)	(0.017)	(0.018)	(0.018)
	[0.73]	[0.75]	[0.77]	[0.79]
	15,893	15,893	15,892	15,893
Ln(Income) 5 years after birth	0.007	0.251	-0.346*	-0.416**
	(0.189)	(0.182)	(0.207)	(0.211)
	[7.8]	[8.1]	[8.5]	[8.9]
	15,893	15,893	15,892	15,893

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). The second column shows results for the lowest quartile of family income, the third column for the second quartile, the fourth column for the third quartile and the last column for the highest quartile. The first row gives the average levels of unpaid leave in each quartile respectively.***significant at 1 %, **significant at 5%, *significant at 10%.

Table 12. Mothers' part time work in 1980, completed fertility (number of children in 2007) and marital stability in 2007. Non-parametric RD and RD-DD regressions.

Variables	Working part time in 1980	Completed fertility in 2007	Parents are married in 2007
Estimates			
RD	-0.013	-0.027	-0.008
	(0.017)	(0.032)	(0.017)
	[0.43]	[2.53]	[0.73]
	15,036	15,025	15,025
RD-DD years	-0.003	-0.011	-0.000
(1975, 1978 and 1979)	(0.011)	(0.021)	(0.009)
	[0.42]	[2.54]	[0.73]
	63,571	63,571	63,571

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD estimates use only eligible births in 1977 while the RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 13. Outcomes for Older siblings. Non-parametric RD and RD-DD regressions.

Variables	Dropout rates older siblings	College attendance older siblings	Ln(earnings) in 2007 older siblings
Estimates			
RD	-0.036	0.031	-0.164
	(0.024)	(0.029)	(0.146)
	[0.19]	[0.50]	[12.3]
	6,264	6,264	6,264
RD-DD years	-0.010	-0.008	-0.102
(1975, 1978 and 1979)	(0.014)	(0.017)	(0.083)
	[0.20]	[0.48]	[12.3]
	27,234	27,234	27,234

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD estimates use only older siblings of eligible births in 1977 while the RD-DD estimates use older siblings of eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 14. Children's and mothers' outcomes by gender. Non-parametric RD-DD regressions.

Variables		
	Gender	
subgroups	Females	Males
Children		
Dropout rate	-0.023**	-0.015
	(0.011)	(0.012)
	[0.17]	[0.21]
	30,737	32,834
College attendance	0.030*	0.011
	(0.016)	(0.014)
	[0.53]	[0.37]
	30,737	32,834
Ln(earnings) at age 30	0.019	0.074***
	(0.025)	(0.020)
	[12.3]	[12.6]
	29,234	31,498
Mothers		
Predicted months of unpaid leave	-0.047	0.048
	(0.255)	(0.221)
	[7.7]	[7.6]
	30,737	32,834
Employed 2 years after birth	-0.008	-0.024*
	(0.015)	(0.014)
	[0.72]	[0.73]
	30,737	32,834
Employed 5 years after birth	-0.007	-0.010
	(0.013)	(0.013)
	[0.76]	[0.76]
	30,737	32,834
Ln(Income) 5 years after birth	-0.107	-0.092
	(0.151)	(0.144)
	[8.3]	[8.3]
	30,737	32,834

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. The RD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). The second column shows results for females, the third column shows results for males. ***significant at 1 %, **significant at 5%, *significant at 10%.

Table 15. Children's and mothers' outcomes by birth order. Non-parametric RD-DD regressions.

Variables		
	Birth order	
subgroups	First born	Later born
Children		
Dropout rate	-0.006	-0.034**
	(0.010)	(0.012)
	[0.19]	[0.20]
	33,653	29,918
College attendance	0.013	0.029
	(0.014)	(0.018)
	[0.45]	[0.44]
	33,653	29,918
Ln(earnings) at age 30	0.034	0.071***
	(0.022)	(0.023)
	[12.5]	[12.5]
	32,103	28,629
Mothers		
Predicted months of unpaid leave	0.034	-0.015
	(0.206)	(0.277)
	[7.7]	[7.6]
	33,653	29,918
Employed 2 years after birth	-0.028**	-0.001
	(0.013)	(0.013)
	[0.70]	[0.75]
	33,653	29,918
Employed 5 years after birth	-0.019	0.003
	(0.012)	(0.013)
	[0.72]	[0.80]
	33,653	29,918
Ln(Income) 5 years after birth	-0.217	0.037
	(0.135)	(0.146)
	[7.8]	[8.9]
	33,653	29,918

Note: Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1st 1977. We use local linear regressions including triangular weights, a bandwidth of 91 days and separate trends on each side of the discontinuity. The round brackets show the standard errors clustered at day of birth. The squared brackets show the mean of the different outcomes for the pre-reform sample. Last we include number of observations for each outcome. TheRD-DD estimates use eligible births in 1975, 1977, 1978 and 1979 (182 days on each side of the discontinuity). The second column shows results for the first born, the third column shows results for later born children. ***significant at 1 %, **significant at 5%, *significant at 10%.

Appendix

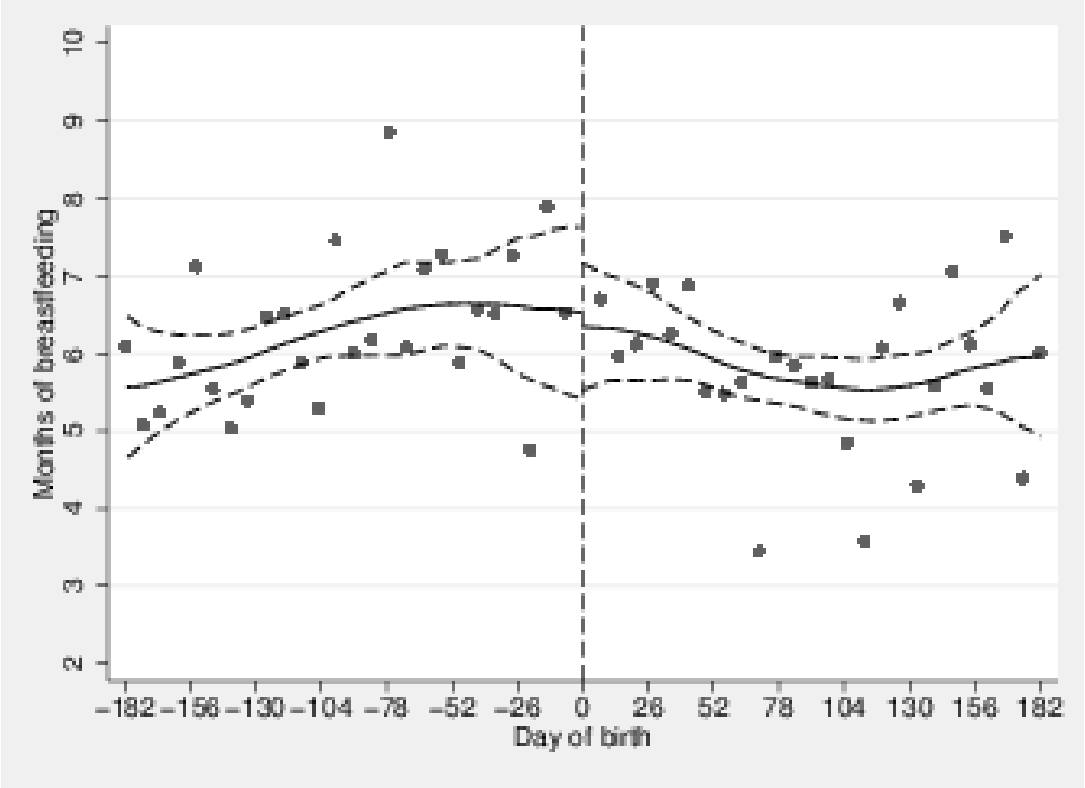
Breastfeeding

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% of women breastfed for three months and as few as 5% for nine 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% breastfed for three months, 50% for six months and 25% of mothers were breastfeeding for nine 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.[28] 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.

Figure A1
Breast Feeding in Norway – eligible mothers 1977



Note: Each observation is the average outcomes in one-week bins based on the birthdate of the child. Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to 0). The solid line is a fitted triangular local linear regression with bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on each side of the discontinuity). The dashed line is the 95 % confidence interval.

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