A flying start?
Long term consequences of time investments in infants in their first year of life*

by

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Abstract

We study the impact of increasing the time the mother potentially spends with her child in the first year of life. In particular, we examine a reform that increased paid and unpaid maternity leave entitlements in Norway. The response to this reform was such that maternal leave increased by 4 months (from an average of 8 months) and family income was largely unaffected. We find that this increase in time with the child led to a 2.7% decline in high school dropout rates, going up to 5.2% for those whose mothers have less than 10 years of education. There is no impact of the reform on college attendance, suggesting that much of the impact is at the low end of the education distribution.

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

Council of Economic Advisers, 1999

1. Introduction

It is possible to trace out socio-economic gradients in education and health to the early years of an individual's life, even to in-utero experiences (see the evidence reviewed in Conti, Heckman and Zanolini, 2009, and Currie, 2009). Returns to early childhood interventions have been shown to be very high, and their success in changing the lives of the very poor reminds us of the value of a strong family (Carneiro and Heckman, 2003).

In this paper we estimate the return to maternal time with the child during her first year of life. Time with the child at this stage of one's life has several potential benefits for the child of which a few examples are: better attachment between mother and child, less stress for mother and child, fewer accidents and other health insults to the child, or prolonged breastfeeding. Such an investment imposes costs on the mother, such as the opportunity wage, potential loss of skill due to absence from work, and perceptions of lower labor market attachment.

The central question, for both parents and governments concerned with the coordination of work and family life, is whether this is a worthwhile investment. This is a notoriously difficult empirical question, as emphasized (for example) in Bernal and Keane (2010) and Dustmann and Schonberg (2009), since mothers who spend more time with their children may have many unobservable attributes that affect child development.

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.

Our paper addresses these two problems. We explore the impact of an exogenous reform in maternity leave benefits (in Norway) on time off work, which is orthogonal to individual attributes of mothers.

There exist already three other empirical studies of the effect of maternity leave reforms in Northern Europe on long term outcomes of children, using registry data with very large sample sizes. Dustmann and Schonberg (2009), studied Germany, Wurtz (2008) studied Denmark, and Liu and Nordstrøm Skans (2008), studied Sweden.[1] Our paper challenges the main argument of these papers: that there are basically no effects of maternity leave expansions on long run outcomes of children. None of these papers considers maternal eligibility status. When we ignore eligibility in our empirical work we cannot reject that our estimates of the effects of the reform on children are zero, since there is a large fraction of ineligible women who are not affected by the reform. This is potentially a problem in these three papers.

Moreover, the experiments in the papers above are useful for understanding the effects of the specific reforms they study but not for estimating the return to maternal time with child since they confound changes in income and changes in time with child.[2] In this important point, our analysis is unique, since we are able to isolate an increase in time at home from a decrease in household income.

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

[2] Even in the case of a paid maternity leave reform, a simple model of labor supply and time with children would predict a change in the amount of unpaid leave taken by the mother.

The reform we analyze increased mandatory paid maternity leave entitlements from 0 to 4 months and mandatory unpaid maternity leave entitlements from 0 to 12 months. Although there were no mandatory maternity leave entitlements before the reform, most women took some unpaid leave (8 months on average). While the increase in mandatory paid maternity leave on its own would probably lead to a decrease in the amount of unpaid leave taken by mothers, the increase in mandatory unpaid leave entitlements occurring at the same time can lead to an increase in the use of unpaid leave.

We estimate that the two responses compensate each other and, on net, there is no change in unpaid leave. Therefore, there is no change in household income during the first year of the child as a result of the reform, and since the uptake of paid leave is likely to be close to 100%, there is an increase of four months in time off work in the first year of life of the child. Furthermore, there are no other impacts in the medium or long run on labor market outcomes of mothers having children just after the law change.[3] This is why we can isolate an increase in time with children from a decrease in household income.

The reform applied to all eligible mothers having children after July 1st, 1977. We estimate the impact of this reform on children using regression discontinuity, comparing outcomes of children of eligible mothers born just after and just before the reform. We perform standard checks of the sensitivity of our results to month of birth effects, and potential manipulation of the date of birth.

Eligibility for maternity leave is a function of prior work and income history. Because of relatively low labor force participation of women it is important to condition

[3] This is also true in Dustmann and Schonberg (2009) and Ludsteck and Schonberg (2008). This could be a short run phenomenon, in the transition to a new steady state, while employers do not fully adjust to the fact that newly hired women will benefit from more generous maternity leave entitlements. After full adjustment we would expect changes in labor market outcomes of women (e.g., Gruber, 1994).

on eligibility when analyzing the effects of this type of reforms. It is also important to characterize these women. This issue has been largely ignored in the literature (even though work history affects the level of maternity leave entitlements in most countries), and it is empirically important in our analysis.

We are able to follow children as late as 2005, when they are 28 years of age. They are still too young for a reliable study of wages, but we can examine instead several other variables: completed education, IQ (males only), height (males only), and teenage pregnancy (females only). The reform induced (on average) an increase in the time mothers spent away from work from 8 to 12 months in the first year of life of their child. We find that this increase in time with the child led to a 2.7% decline in high school dropout rates. This figure goes up to 5.2% for those whose mothers have less than 10 years of education. There is no impact of the reform on college attendance, suggesting that much of the impact is at the low end of the education distribution. This is consistent with the finding that impacts are especially strong for children from less educated mothers, who are more likely to be at the margin between dropping out or not. This set of results is true for both boys and girls in our dataset.

We also find positive impacts of the reform on IQ (0.24 points, on a scale of 1 to 9) and height of males (0.67cm), which suggests that two channels through which maternal time in the first year operates are an increase in cognition and better health. We do not see any impact of the reform on teenage pregnancy rates for girls. While it is true that the effects of the reform on IQ are also higher for those whose mothers have little education, this is not true of height.

The paper proceeds as follows. Section 2 gives background information on maternity leave legislation in Norway while Section 3 presents the empirical strategy. Section 4 presents data and Sections 5 and 6 discuss the results. Section 7 concludes.

2. Maternity Leave Reform and Institutional Background

2.1 Maternity Leave Reform

In 1956 the maternity leave benefits became common to women in Norway through the introduction of compulsory sickness insurance for all employees.[4] The length of the maternity leave was 12 weeks, but the compensation varied substantially (Rønsen, 2002).[5] From July 1st 1977 paid maternity leave was extended to 18 weeks and unpaid maternity leave up to one year, as illustrated in Figure 1.[6]

By the July 1st 1977 reform parents were given universal right to 18 weeks of paid leave with guaranteed job protection (before and) after the birth of a child.[7] Maternity leave payments equaled 18 weeks of work in pre-birth employment (i.e., 100% replacement rate). Women had to take six weeks of this leave while the rest could be

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[4] The history of maternity leave in Norway dates back longer than 1956, to the introduction of compulsory sickness benefits in 1909. At this time it became compulsory to give working women maternity leave benefits up to 6 weeks after birth. Because mostly non-married women worked, this was extended in 1915 to include a one-time-benefit to married women of 40 NOK as long as the husband had sickness insurance.

[5] You had to work at least 6 of 10 months before you gave birth. The coverage was maximum 120 NOK per month plus 0.1% of your salary.

[6] These changes were introduced together with a new law for work relations in Norway introducing improved protection of workers rights (”Arbeidsmiljøloven”) accepted June 3rd 1977 by the Parliament and introduced July 1st 1977 (see Prepositions, Ot.prp. nr. 71 and Innst.o. nr. 90). There were additional reforms after 1977. From 1987 and onwards the paid maternity leave was extended almost yearly until 1993. From 1993 and up till now Norway has had the same paid maternity leave of 42 weeks with 100 % cover or 52 weeks with 80 % cover. We have in this paper decided to focus on the 77 law since it is a change in what we think is a critical period for the child for instance since breast feeding is still an issue, and since its easier to assess the first change in the law since the latter reforms were anticipated to a larger degree. Also we have a much richer set of available outcomes for children born in 1977.

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

shared between the parents. In practice, additional leave was almost exclusively taken by the mother (Rønsen, 2002). In addition, parents also gained entitlement to one year of unpaid job protection (on top of the 18 paid and job protected weeks of maternity leave).

Not all mothers were eligible to receive the new benefits. Their eligibility status depended on their work and income history. Only women working six of the past ten months prior to giving birth, and having more than 1 G[8] of yearly income, were eligible for leave and coverage. Limitations in our data (we do not observe time in employment, and we only have yearly income which includes wage income and benefits) force us to rely on an imperfect measure of eligibility, which we describe below. This is however likely to approximate eligibility fairly well. We estimate less than two thirds of all mothers giving birth in Norway in 1977 were eligible for maternity leave benefits.[9]

An important assumption for identification is that the eligibility status has to be independent of the maternity law. The maternity reform was introduced during a big offensive from the sitting (very radical) parliament at the end of its period. It is hard to believe it were expected since it came along with a lot of other changes (unrelated to the maternity leave reform) and at the end of the period. We also checked national newspapers around 1976 and 1977 to see whether they wrote about the reform. We do not find any evidence that newspapers wrote about the reform earlier than June 1977.[10] 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 announcement of the law in

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[8] 1 G=10000 NOK (1NOK=6.5USD).

[9] From our dataset we define eligible mothers as mothers having at least 1 G of salary in the year before giving birth. We use one year instead of 10 months because we only have yearly income data, however if this slightly overstate numbers of eligible mothers the definition is at least the same across months and years. The results are robust to alternative definitions of eligibility.

[10] Verdens Gang 30.06.1977, Bergens Tiende 27.06.1977, 30.06.1977, Aftenposten 30.06.1977

[11] Propositions and regulations from the Government: Ot.prp nr. 61 and Innst.o. nr 61

1977 was already pregnant when the law was introduced.[12] Based on the rule of working six of ten months prior to birth, women could not easily change their status in the short run. The assumption of eligibility status being independent of the maternity leave for mothers giving birth in 1977 is likely to hold.

The 1970s in Norway was the decade of oil discovery, labor participation of women increased dramatically and several welfare reforms were implemented. We have studied all possible laws and reforms that may have had an impact on women around pregnancy and birth time. The only law around the change in maternity leave is the abortion law implemented January 1st 1976. This change in the law made it easier for women to have abortion within 12 weeks of pregnancy. The first monthly birth cohort to possibly be affected by this reform is then July 1976. This possibly gives rise to a discontinuity in observed child outcomes between June and July 1976 and hence we do not use 1976 as a comparison to 1977.

2.2 Institutional Background

At the time of the maternity leave reform in 1977, labor force participation for women was 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 mothers). In Norway, the labor force participation rate around 1977 was about 50 percent for married women, which are the most relevant group in our case, and around 70 percent for non-married. Labor force participation was about the same in

[12] Possible effects on fertility will therefore not show up in the data before earliest in the beginning of 1978. It is possible that mothers delivering close to July 1st 1977 are able to delay their delivery. In fact, Gans and Leigh (2009) estimate that Australian mothers delayed child birth in response to a reform changing incentives to fertility. Nevertheless, for the reform we study there are no significant differences between the number of births occurring just before and just after the reform. This is shown in figure A1 in the appendix.

Norway and in the US during the 1970s, but much higher in the former than in the latter by 1990. By 2008 (not in the figure) the labor force participation rate in the US is around 65 percent, and is thus comparable to the participation rate around the reform in Norway for all mothers (OECD, 2008).

It is expected that the effects of a change in the maternity leave law will depend on what child care arrangements were available for working mothers at the time. During this period there were basically two types of alternatives in Norway: day care centers and informal care provided by grandparents or neighbours. Day care centers where either owned by the municipalities or by private organizations like churches and cooperatives. Both of these offered heavily subsidized day care. The subsidies came from the municipalities. There were requirements concerning the education of staff and strict requirements when it came to playgrounds, playground facilities, and total area. When day care centers were not available it was very common to use grandparents or to use neighbours that could looked after their own children but also after friends' and neighbours' children. These were not formal day care centers and they did not receive any subsidy from the municipality. There were no requirements when it came to facilities.

In Figure 3 we depict the development of day-care coverage in Norway by age groups, and by urban-rural areas. In the mid 1970s very few children aged 0 to 2 were in a day care, and there is very little difference between urban and rural areas (1% vs 0,5 % percent). Although the total child care coverage from day care centers for all children aged 0-6 year olds was about 15 percent in 1977, the coverage for the first two years was very low, only 1-2 percent. This means that the alternative to mother's time at home in the early years of the child was mainly informal care by nannies, grandparents or neighbours.

3. Empirical Strategy

3.1 *Identification*

Children born just before and just after the reform should be equal except for the fact that mothers of those in the latter group benefit from a change in maternity leave entitlements taking place on July 1st 1977. Therefore, it is natural to use regression discontinuity (RD) to estimate the effect of this reform on long run child outcomes. It is possible that a simple comparison of outcomes for children born in different months is contaminated by month of birth effects due, say, to the fact that the age at which children start school depends on their month of birth and is potentially related to adult education and earnings (see Black, Devereux and Salvanes, 2008, for evidence for Norway). Therefore we combine RD with difference-in-differences (DD) by constructing three types of control groups (of children and mothers not affected by the reform; see Dustmann and Schonberg, 2009): one consists of children born in 1975 of eligible mothers; another consists of children born in 1979 of eligible mothers; and another consists of children born in 1977 of ineligible mothers. We use the first one in our main specification.

For those women giving birth in 1977, eligibility to the new maternity leave entitlements (E_i) is a deterministic function of month of birth (X_i) in the following way:

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

where c is the cutoff point of July 1st 1977. Therefore, all mothers giving birth to a child after c potentially receive the treatment defined by new maternity leave entitlements, while those giving birth after c are assigned to the control group. We use only eligible mothers based in our main analysis as defined in Section 3.[13]

It is possible to estimate the impact of being eligible for maternity leave benefits on various child and maternal outcomes (y_i) comparing children born just before and just after c. The parameter of interest is the following:

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

where α_{RD} is the average effect of the reform on the outcome of interest (Y) for those born at time c, $y_i(1)$ is the outcome for child i in the presence of the reform, and $y_i(0)$ is the outcome for child i in the absence of the reform. Assuming that $\mathrm{E}[y_i(1) \mid X_i = c]$ and $\mathrm{E}[y_i(0) \mid X_i = c]$ are continuous in x (continuity of these functions at the point where x=c is all that is needed):

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

Outcomes of interest for the mother include months of unpaid leave, and employment and earnings two and five years after giving birth. Outcomes of interest for the child include dropping out of high school, college attendance (both measured by age 29), having a child before age 19 (only for women), and IQ (only for men).

Therefore, we estimate $\alpha_{RD} = \lim_{x \downarrow c} E[y_i \mid X_i = x] - \lim_{x \uparrow c} E[y_i \mid X_i = x]$ by taking the difference of in the boundary points of two regression functions of y on x: one for eligibles and one for ineligibles. We estimate these regression functions local linear

[13] See Apendix A, Table 1 for a comparison of results using the total sample versus only the eligible sample.

regression (LLR) as in Fan (1992), Hahn, Todd and Van der Klauuw (2001) and Porter (2003). Hahn, Todd and Van der Klauuw (2001) show that LLR outperforms general kernel regression methods in terms of bias. Defining h as the bandwidth, the following estimator is applied:

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

The parameter of interested is estimated as

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

When estimating (4) from equation (3) we use the triangle kernel which is shown to be boundary optimal (Cheng, Fan and Marron, 1997). We obtain standard errors using the formulas in Porter (2003).[14] The choice of bandwidth is important, because it affects the smoothing of our data. In the main text we present results using a bandwidth of 3, and in the appendix we present further results using a bandwidth of 5.[15]

Although smoothing helps us remove potential monthly trends, there could still be an independent effect of month of birth on outcomes irrespective of the reform. These could be due to different mechanisms. For example, there is a literature showing that children born in the winter, or basically the first quarter of the year, have worse outcomes along many dimensions such as schooling, earnings and health (Angrist and Krueger, 1991; Plug, 2001; Costa and Lahey, 2005; Buckles, 2008). Several factors may drive this difference; exposure to illness and low temperatures during winter months, compulsory

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

[15] Using cross validation as in Imbens and Lemieux (2007), we get an optimal bandwidth of 3. However Ludwig and Miller (2007) points to different problems using CV and the literature seems to agree that visual inspection is still the best method to choose the bandwidth.

schooling laws especially in countries where the school leaving rules are based on age as in the US and not grades completed, and difference in fertility across months for socioeconomic groups.

Our preferred approach of dealing with the separate effect of months on outcomes for children is the DD method comparing outcomes of eligible mothers in the year of the reform with outcomes of eligible mothers in 1975.[16] We will first estimate equation (3) for those born in 1975 and those born in 1977. Then we calculate:

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

Since there is no reform in 1975 $\hat{\alpha}_{RD,1975}$ should only capture month of birth effects. On the other end, $\hat{\alpha}_{RD,1977}$ confounds effects of the reform with potential month of birth effects. Assuming the two effects do not interact, and that month of birth effects are the same (around c) for those born in 1975 and 1977, we can estimate the effect of the reform as $\hat{\alpha}_{RD,REFORM} = \hat{\alpha}_{RD,1977} - \hat{\alpha}_{RD,1975}$. There is no reason to presume that there would be important interactions between month of birth effects around c and the reform.[17]

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

4. Data description

[16] As we argued earlier we cannot use 1976 because of a reform in the abortion system. For symmetry we also try 1979 as our second control group and obtain very similar results.
[17] We also see this by visual inspection in our graphs comparing outcomes of eligible mothers in 1977 with those of eligible mothers in 1975 and eligible mothers in 1979. The pre-reform trends are very similar.

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 2006 and is a collection of different administrative registers providing information about educational attainment, labor market status, earnings, and a set of demographic variables (age, gender) as well as information on families.[18] To ensure that all individuals studied went through the Norwegian educational system, we include only individuals born in Norway. We are able to link individuals to their parents, and we are able to get labor market information for both. In addition, we have access to a rich set of variables.

We will mainly focus on three variables concerning mother's labor supply and maternity leave decision, and four outcome variables for children. The yearly income history for mothers in the 1970s provide us with a tool for predicting unpaid months of leave, and for studying the effects of taking leave on income two and five years after giving birth. The latter two are useful to examine possible channels by which the additional maternity leave may be affecting child outcomes. The outcome variables for children we consider are dropout rates from high school, university attendance, teenage pregnancy, IQ scores and height (in cm) for young men around the age of 18-19. The latter two variables are only available for young men in their late teens since they come from the military service files.

In terms of educational attainment, we measure education at the oldest age possible for each individual i.e. in 2006.[19] Dropout rates are defined in this paper as all children not obtaining a three year high school diploma. The IQ data are taken from the

[18] See Møen, Salvanes and Sørensen (2004) for a description of these data.
[19] 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.

Norwegian military records for the relevant cohorts when they were tested at around age 18. In Norway, military service is compulsory for every able young man. Before entering the service, their medical and psychological suitability is assessed; this occurs for the great majority between their eighteenth and twentieth birthday. IQ at these ages is particularly interesting as it is about the time of entry to the labor market or to higher education.

The IQ measure is a composite score from three speeded IQ tests - arithmetic, word similarities, and figures (see Sundet et al. (2004, 2005) and Thrane (1977) for details). The arithmetic test is quite similar to the arithmetic test in the Wechsler Adult Intelligence Scale (WAIS) (Sundet et al. 2005; Cronbach 1964), the word test is similar to the vocabulary test in WAIS, and the figures test is similar to the Raven Progressive Matrix test (Cronbach 1964). 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.[20] We have IQ scores for about 84% of the relevant population of men in Norway.[21] Height (in cm) is obtained from the same military records as ability.

Earnings are measured as total pension-qualifying earnings reported in the tax registry and are available from 1967 to 2005. These are not top-coded and include labor

[20] The correlation between this IQ measure and the WAIS IQ has been found to be .73 (Sundet et al., 1988).
[21] Eide et al (2005) examine patterns of missing IQ data for the men in the 1967-1987 cohorts. Of those, 1.2 percent died before 1 year and 0.9 percent died between 1 year of age and registering with the military at about age 18. About 1 percent of the sample of eligible men had emigrated before age 18, and 1.4 percent of the men were exempted because they were permanently disabled. An additional 6.2 percent are missing for a variety of reasons including foreign citizenship and missing observations. There are also some missing IQ scores for individuals who showed up to the military.

earnings, taxable sick benefits, unemployment benefits, parental leave payments, and pensions.

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

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

We would like to have direct information on months of leave, but this is only available in Norway from 1992 and onwards. Even then we only have information on paid leave. Therefore, in order to compute total leave taken by each mother we proceed in the following way. First, we assume that the take-up of paid leave was 100% when it was first introduced in 1977.[22] In order to construct unpaid leave we start by constructing pre-

[22] Firstly, Rønsen et al (1996) show that for the 1968-1988 mothers in Norway almost no one returned to work before four months after birth. Secondly, from a survey conducted in 1977 on fertility behavior of women in Norway (Statistics Norway), 60 % answered that they thought mothers should stay home the first two years after giving birth to a child. In addition the coverage was 100 % which gives strong incentives for full take up. Third, since we observe days of paid leave after 1992 we are able to check to what extent eligible mothers take up this benefit, and how does the take up react to subsequent reforms. We find that take up of leave is very high and reacts strongly to reforms. Before the April 1992 reform mothers are able to take 224 days at full coverage or 280 days at 80% coverage. For mothers delivering children in March of 1992 average take up of paid leave was 250 days. After April 1992 there is an increase in maternity leave

birth monthly income using 1976 earnings. Then we use income in 1977-1980 to construct how many months did a mother stay home with the child the first 36 months after birth, conditioning on birth month, and assuming her potential post-birth earnings (the earnings she would get had she not gone on unpaid leave) equal her 1976 earnings.[23] We limit ourselves to a window of 36 months because. the further away we move from pre-birth earnings, the more likely earnings may differ because of change of job, part time work, presence of new children, and other related factors.[24]

5. Results

5.1 Descriptive statistics

Since the reform only affects mothers who are eligible it is important to characterize who they are (although our definition of eligibility is imperfect, as measured above). Figure 4 shows the proportion of mothers giving birth at each data who are eligible for maternity leave entitlements. It is possible to construct eligibility for mothers giving birth before the reform took place, even though maternity leave entitlements did not exist yet at that time. Between 1975 and 1979 the proportion of eligible mothers was always between 60% and 70%, and in 1977 it was about 65%. This means that 35% of

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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 increase paid leave to 266 days of full coverage or 336 days of 80% coverage. By April of 1993 average leave taken was almost 310 days. Therefore, it is probably reasonable to assume that the take up of paid leave is close to 100%.

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

[24] One problem with our approach can be that mothers return to part time work and hence some of our estimated leave is not absence from work however lower earnings due to part time work. This is not a problem as long as the reform in it selves do not effect this transition, as it will only effect levels and not the change. As we see no effects on earnings five years after this is not likely to be of large concerns.

mothers and children giving birth in that year are not accounted for in our estimates of the impact of the reform on child outcomes.

Table 1 displays the main characteristics of eligible mothers and their children (born in 1977) as compared to those of ineligible mothers and their children. It is clear that eligible mothers are much more educated than ineligible mothers. They are also much 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 more than nine times larger than that of ineligible mothers, presumably because the later do not work. Children of eligible mothers have much lower high school dropout rates, much lower teenage pregnancy rates, and much higher IQ than those of non-eligible mothers. Eligible and non-eligible mothers constitute two very different groups of mothers. This means that we cannot safely extrapolate our findings, which are valid for the former group, to the latter group of mothers and their children.

Table 2 characterizes two groups of eligible mothers giving birth in 1977 and their children: those giving birth up to the end of June, and those giving birth after July (before and after the reform was implemented). The two groups are fairly similar but there are some small differences. In the case of child outcomes these may be the result of the reform, but also of month of birth effects. Some of these differences can be shown to be statistically different from zero presumably because of our large sample sizes. Table 2 shows raw differences between two broad groups. These raw differences between the two groups are informative but they do not tell us directly about the impact of the program on child outcomes, or about the characteristics of mothers having children just before and just after the reform. This requires an examination these variables for mothers and children born just before and after the reform, which we describe in detail below.

The levels of unpaid maternity leave taken are quite high, even for those mothers having children before the reform is implemented. As mentioned in the previous section, our measure of unpaid maternity leave is imperfect (although it is very carefully constructed) since we do not observe this variable directly. Alternative ways of constructing this variable lead to different figures, but average unpaid leave for those giving birth in 1977 is rarely below 6 months or above 10 months.

For our preferred measure average leave is 8 months for those delivering before July 1977, and it barely changes for those delivering after this date. This means that if there is an expansion in the time mothers spend with their newborn child as a result of the reform,

In order to understand how this reform worked we need to have a good grasp on which groups of women changed behavior due to the reform and what alternative was available for taking care of the child in the first months. For instance, questions such as whether it was particularly women in urban areas, low educated mothers or mothers with low family income is relevant in terms of understanding the effect of the reform both on women’s behavior and on children’s outcome. First we present take-up rates two years before and two years after the reform defined as eligible mothers as a share of all women giving birth before and after the reform, split by education level and living in urban/rural areas. In Figure 5 we see that eligibility status is increasing over time which is natural given that the end of the 1970s experienced increased labour participation of women.

However we do not see any tendency towards a sharper increase post-reform, the trend is constant over the five years 1975-1979. We also see that there are more eligible mothers in the higher education group and among mothers living in urban areas, however, the trends in these groups are similar. We now turn to a more detailed analysis of mothers' characteristics by month in the year of the reform in order to secure a balanced sample for the analysis.

In Table 1 we present the descriptive statistics of the extension of the maternity leave both on mothers' and children's outcomes. We see the sample is balanced, mother's education are similar in the pre and post reform groups, age at birth is slightly lower pre-reform, however this is due to a trend across month of birth and there is no discontinuity between June and July in this variable. This is also the case for pre-reform income.[25] For mothers we see that unpaid leave is half a month lower after the reform, however given that mother's take four months more of paid leave this is not close to crowd out the total effect of around 3,5 months more time with young children. We also see a negative effect on employment after giving birth. For children we see a fall in dropout rates from high school and an increase in years of education, however, the unconditional means of IQ-scores and teenage pregnancy are very similar in the first and second half of 1977.

We next turn to the analysis of the potential causal effects of the results tendencies we saw in the descriptive statistics. We present the results by using different techniques in particular since there might be separate effects of month of birth driving some of these descriptive results. We start with the results for mothers.

[25] We have run graphs on a larger set of pre-income variables and fathers characteristics and find no discontinuities in any variables. This makes us sure that mothers giving birth in different months in 1977 are on average very similar in all aspects that may affect outcomes.

5.2 Mothers' outcomes

We present the different results for mothers' adjustment in the labor market in Table 3.[26] The first column present the nonparametric regression discontinuity (RD) results while column two presents the nonparametric differences-in-differences (DD) results using 1975 as control groups. We see that there is no change in predicted months of unpaid leave. The average amount of unpaid leave is 7.8 months.[27] As paid leave is part of the earnings measure this is evidence that the reform did not crowd out unpaid leave and is also in line with the part of the reform increasing job protection to 12 months.[28] The paid leave was taken in addition to unpaid leave and expected crowd out of unpaid leave when introducing paid leave is offset by longer job protection.[29] Turning to the effect on employment, we do not find any long term effects on mother's employment two and five years after the reform, nor on earnings[30] five years after.[31]

We present the results of Table 3 graphically, the RD results, in Figure 6a and the DD results, in Figure 6b. We clearly see the persistent of the effect on months of maternity leave and no discontinuity in the long term labour market outcomes.

The first stage predictions are clear; the maternity leave had a positive short term impact on mother's leave introduced paid leave in addition to (not crowding out) unpaid

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[26] See Appendix A, Table 1 for a comparison of results using the total sample. We notice that the results compares well with the sample of eligible mothers but weaker.

[27] Rønsen et al. (1996) show a mean return to work after birth of 16 months for the 1968-1988 mothers, however this estimate also include mothers not being eligible for leave and hence having a much weaker attachment to the labour market. However, we still use this as an indication that our estimate is at least a lower bound of unpaid leave in this period.

[28] Ludsteck et al. (2009) show that reforms in unpaid leave in Germany delayed mother's return to work substantially.

[29] We also find no effect on income in the year of birth, which support no change in unpaid leave. We do not report results on this measure as it does not give us any more valuable information.

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

[31] From Table A2 in the appendix we see that changing the bandwidth to 5 months around the discontinuity does not affect the main results, although there is a small, negative and marginally significant effect on employment two years after birth.

leave. The increased job-protection up to a year does not appear to have any significant positive effect on average employment although the sign is negative both in the short and long run.

5.3 Children's outcomes

We present the different results for children's outcomes in Table 4.[32] The first column present the RD results while column 2 presents the DD results using 1975 as control groups. From the first column we see a fall of around 2 percentage points in children's dropout rates, however this variable is only significant at ten percent. When taking into account potential month effects in the DD estimations in column 2 we see more robust results with the effect increasing to around 3 percentage points. The results are a bit stronger than earlier reflecting the negative birth month effect which is opposite of the positive reform effects. For years of education[33] we see the same tendency, an increase of about .10-.15 of a year. Interestingly, we also see a positive effect on IQ. We only have IQ scores for men, but due to the large sample sizes we can still estimate the effect on the reform on IQ for only the male part of the sample. The IQ measure is measured in a scale from one to nine, so a coefficient of .11 indicates a score of 0.1 point higher for children born after the extension. This is about a difference of one tenth of a stanine and 5 percent of a standard deviation. This effect is around .24 in column 2 and 3 which is 25 percent of stanine and 12 percent of a standard deviation. This is more than

[32] See Appendix A, Table 1 for a comparison of results using the total sample. We notice that the results compares well with the sample of eligible mothers however weaker results.

[33] Unfortunately the sample is still too young to estimate sensible effects on earnings, as we only have data on earnings until age 28.

1% in difference in earnings as an adult. We do not see any effect on teenage pregnancy in any of the specifications. [34]

In Figure 7a we present the results graphically for the first column in Table 4. We clearly see effects across outcomes for children and that the differences in results hold up across months before and after. However, we also see that there are monthly trends across the different outcomes. As pointed out earlier there are different reasons why children born earlier in the year might outperform children born later in the year. In Figure 7b we present results graphically for the second column in Table 4. We see that the effects are mostly persistent across months.

5.4 Placebo results

Table A4 in the appendix present placebo RD-results for eligible mothers in 1975 and non-eligible mothers in 1977. We do not find any significant results for mother's outcomes in these years. The results for children are in general not statistically significant however in one cases it turns slightly significant. This is due to the negative effect of birth month on children's outcomes as discussed earlier. Children born earlier in the year have better outcomes than children born later in the year. These trends drive some of the negative results in outcomes for the control groups.

6. Heterogeneous results and mechanisms

[34] In Table A3 in the appendix we report results with a bandwidth of 5 corresponding to more smoothing of the data. We see the same tendencies in coefficients however the results are weaker, especially for the RD results. This can be a feature of the possible effect of birth month on outcomes hence we will focus on differences-in-differences for the rest of the paper.

3.2 Heterogeneity in results and mechanisms of the reform for mothers and children

At the core of understanding the effect on mothers' outcomes and in particular the effect of extended maternity leave is: 1) what was the alternative pre-reform for the kids that stayed home with mother after the extensions of the maternity leave and 2) what are the possible mechanisms that could potentially have a positive effect on children given that the extensions from 12 to 18 (52) weeks is in what we expect to be a critical period of development for the child. In addition to try to understand which parts of the distribution of mothers changed behavior by the reform, by reporting take-up rates, we present results for heterogeneous effects of the reform on children's outcomes depending on mother's education and whether the child was born in urban or rural areas as well as whether the child was born close to grandparents and whether the availability of day cares was high or low in the municipality.

There are several possible mechanisms for why an increased maternity leave from 12 weeks unpaid leave to 18 weeks with full coverage and one year of unpaid leave may lead to an effect on mothers' labor market behavior and on the later performance of children. Some of these mechanisms we can test directly or indirectly. For mothers: Staying out of the labor market for a longer period may give a negative long-term earnings effect. On the other hand, longer stay out of the labor force and job protection for a longer period may also provide a possibility for a stronger attachment to the labor force for women. We are able to test both the effect on attachment to the labor force in the short and long run (after two and five years), and effect on earnings after five years. For children: the maternity leave compensation may also lead to a possible income effect of the reform which may be a positive effect on children, the extra time spent with infants may give more time to breast-feeding and less stress and more and stronger bounds to the child.

More specifically for the test for mechanisms for children; the medical literature suggests a positive effect on cognitive development of breast feeding especially in a critical period for breast feeding as 12 to 18 (52) weeks represents. An indication that this indeed may be an important channel in Norway, breastfeeding has been shown to be very common in Norway from the mid to late 1970s. We present the data for breast feeding over time for Norway in Figure 4 based on data from several maternity hospitals in Norway over time (Liestøl, Rosenberg and Walløe, 1988). The results show that breastfeeding in Norway started to decline around 1920 and reached its lowest point around 1967 when only 30 percent of women breastfed for three months and as few as five percent for 9 months. In the late 1970s, the level of breastfeeding in Norway was back to the level of around 1940 after a decline from the 1920s onwards. Around the period of the maternity leave reform we are using, about 75 percent breastfed for three months, 50 percent for six months (as shown in the figure) and 25 percent of mothers where breastfeeding for nine months or more. Since breastfeeding in Norway is quite substantial in the mid 70s –and we actually see an increase around the time of the reform- we may expect that this is an important channel as the reform extent leave from 12 to 18 (52) weeks, a time where breastfeeding is expected to be important. We test the breast feeding channel by trying to isolate an effect that primarily is expected to be connected to the nurture channel from more breastfeeding. The height in the late teens is known from the literature to be strongly connected to the quality of nurture, and we test whether there is any effect of maternity leave on the height of young men in their late teens (18-19) (Haines and Kintner, 2008).

At the core of understanding the results we have earlier pointed to the alternatives for the child of an extended stay with her mother. In addition, the fact that the extension from 12 to 18 (52) weeks probably also constitutes a critical period in the child's life is important. We have a rich variety of family characteristics like parental education, income, place of residence in terms of urban/rural, closeness to grandparents and availability of child care at the municipality level. We will next report results on possible channels through which the maternity leave may have affected children's outcomes

6.1 Parental background: mothers' education, urban/rural, access to day care, closeness to grandparents

We report results by subgroups based on parental background in Table 5.[35] The first split we make is to check whether the maternity leave extension had a different effect on mothers with different educational background. We split the sample in two; mothers with less than 10 years of education versus mothers with 10 years or more of education. We see, from the first two columns of Table 5, that the effects on mothers are very similar for the two groups; there is a large, positive effect on months of paid leave and time mother stays home in the sense that there is no effect on unpaid leave and no significant effects on the long term labour market outcomes. For children we see that the fall in dropout rate is around 5 percentage points for children of mother's with less than 10 years of

[35] Also see Appendix, Figure A1 and A2, to see illustrations of effects on dropout rates across sub groups.

education while it is around 2 percentage points for children of higher educated mothers. The tendencies are the same for years of education and IQ.

In column 3 and 4 of Table 5 we present the results for whether parent live in urban versus rural areas.[36] We see that the effects are again similar for mothers. For children they are often larger when parents live in a rural area however this is not consistent for all outcomes, for example the educational attainment are larger for children with parents in urban areas. This suggest that effects on children might be concentrated in different parts of the educational distribution and show us the importance of taking into account different measures of children's long term outcomes.

Another aspect of measuring the heterogeneity of effects to the alternative to mother's time is the proximity to grandparents. The grandmothers to the children in our samples would normally not be in the labor force. It was only in the 1960s and 1970s that relatively young women to a large degree entered the labor market. This means that grandparents may be an available substitute to maternal time with children. This hinges on the assumption that grandparents live close to their grandchildren so that they can help out in daily life. We split the sample in two; close means in the same municipality as your grandparents, while far is further away. Again, as we see from column 5 and 6 of Table 5 the effects on mothers are not significantly different, except that mothers living close to grandparents increase unpaid leave by around one month, in addition to effect on paid leave. The effects on children are for the most part larger when the family live close to grandparents, however for years of education there is no difference in coefficients

[36] We verify the results by also running parametric specifications controlling for mother's characteristics.

between the two groups.[37] It implies that there does not seem to be a trade-off between mothers staying home with the child and possibly breast feeding, and grandparents' time with the child.

A reasonable explanation for this result could be that grandparents are a weak substitute to maternal time with the child in the first months of a child life. Before the reform the mother left the child with her parents while working, but after the reform she stays home at least for the first four months of the child's life. This support the idea that mother's time is important in the first months due for instance to breast-feeding and less stress for both mother and child.

As the last background aspect, we condition the results by availability of child care in the last two columns of Table 5. For dropout rates the results are stronger if mother's live in an area with low child care coverage. For other outcomes it does not seem to be a strong pattern, and there is hardly any significant difference between the groups. This is not so surprising since the coverage rates across municipalities in 1977 for children aged 0-2 was very low.

6.3. Birth order

How to fit this into the story?? Table 6, why are effects concentrated on higher order births?

6.4 Mechanism: Breastfeeding?

[37] We have also verified the results by using a polynomial function of birth month and controlled for mothers education, income and age in order to be sure that the results are not driven by differences in mother's characteristics across municipalities.

We present the results for whether we find an effect on maternity leave on the height of men at the age of 18-19, which is an outcome which may only come from better nurture and most likely increased breastfeeding. In Table 7 we present the results both from the RD design and the DD results using eligible mother from 1975 as comparison group. The results suggest that there is a positive effect of about .5-1 centimeter for men born of mothers post-reform. The increase per decade in height among men measured at 18 was about one centimeter for cohorts born from 1950 to 1990 in Norway, so the 0.5 centimeter is quite substantial. This clearly indicates that there is a positive effect of the reform through better nurture which is hard to attribute to other factors than breastfeeding. Also aggregate data on breastfeeding show and increase in breastfeeding by Norwegian mothers from about 1977 onwards (Liestøl *et al*, 1988; Eide *et al*, 2003) as we see clearly from Figure 4

A regression of log wages on high school graduation, college attendance, IQ, height, and other controls for experience and family background using males in 2005 shows that the return to high school graduation is 11.5%, the return to an extra point in IQ is 0.4%, and the return to an extra centimeter of height is 0.3%. Therefore, we estimate that an additional month of time with the child in her first year of life leads to about 0.15% increase in the wages of the average child (1.8% for 12 months). So if we think of wages alone, this is a small return, especially after considering that the lag between this time investment and adult wages of the child could be well above 20 years.

7. Concluding remarks

Can families, by not working or working less in the first years of a child's life, influence the ability of children? And are the potential positive effects large enough to trade-off a possible negative effect of the parents' career and earning prospect by taking time off work with their children? Exploiting a maternity leave reform in Norway, opening up for up to four months of paid leave and an additional one year of unpaid leave, show us that this is the case. Children have a higher probability of completing high school and have more years of education and boys have a higher IQ score at age 18. By using yearly income of mothers we have been able to split the sample into eligible mothers. This has not been done in the literature before and gives us more precise estimate of the effect of maternity leave on children's long term outcomes. We use the rich dataset on family background variables to exploit the effect on mother's months of unpaid leave, income effects and employment two and five years after giving birth. We find that the reform increases the time mother spend at home with infants- however little long term effects on employment and income. This support our main source of mechanism of why an increase in maternity leave should effect children's outcomes, mother's spend more time – quality time – with the child in its first year of life leading to more breastfeeding. Then we use the data on family background to understand more about heterogeneous effects of the reform. We find that the reform is more effectual for children of low educated mothers.

To understand mechanisms driving the results we find evidence supporting a nurture channel most likely via extended breastfeeding. This channel is supported by an effect on maternity leave on the height of young men which may only come through improved nurture. Also aggregate data on breastfeeding show an increase in breastfeeding by Norwegian mothers from 1977 onwards (Liestøl *et al*, 1988; Eide *et al*, 2003). The alternative for staying home with mothers at the time in Norway around the time of the reform is crucial to understand the results. There was almost no available high quality child care for under-two year olds available so the alternative was grandparents or other informal care which is not necessarily a good substitute to mother's time at this period of a child's life.

For policy implications we conclude that constructing policies to increase parents' time with children after birth may have an impact on children's abilities later in life. This effect has been an important part of the goals behind expansions in maternity leave across countries; however this study is the first to show that this may also be achieved. There are not only short terms effects of increasing maternity leave (Tanaka, 2005; Bernal and Keane, 2006), but also substantial benefits in the long run. As mentioned in the introduction the situation on maternity leave is remarkably similar in the US today as it was in Norway before the reform. Parental leave is currently under debate in the US[38] and an introduction of four months of paid leave and better job protection are typically within feasible policies.[39] Using the rich set on family background variables to address heterogeneity of effects also give us the advantage of making the study less dependent on institutional settings in Norway. For example by showing that the effects are bigger for

[38] USAtoday 26.07.2005, The New York Times 16.04.2008
[39] http://www.govtrack.us/congress/bill.xpd?bill=h110-3799

children from lower educated households this may be important for policy discussions related to lowering inequalities in general. Many countries, like the US, Britain, South America have a substantial inequality in education and income. While increasing maternity leave for women and men in these countries will not solve these problems we have shown that it might reduce the existing gap.

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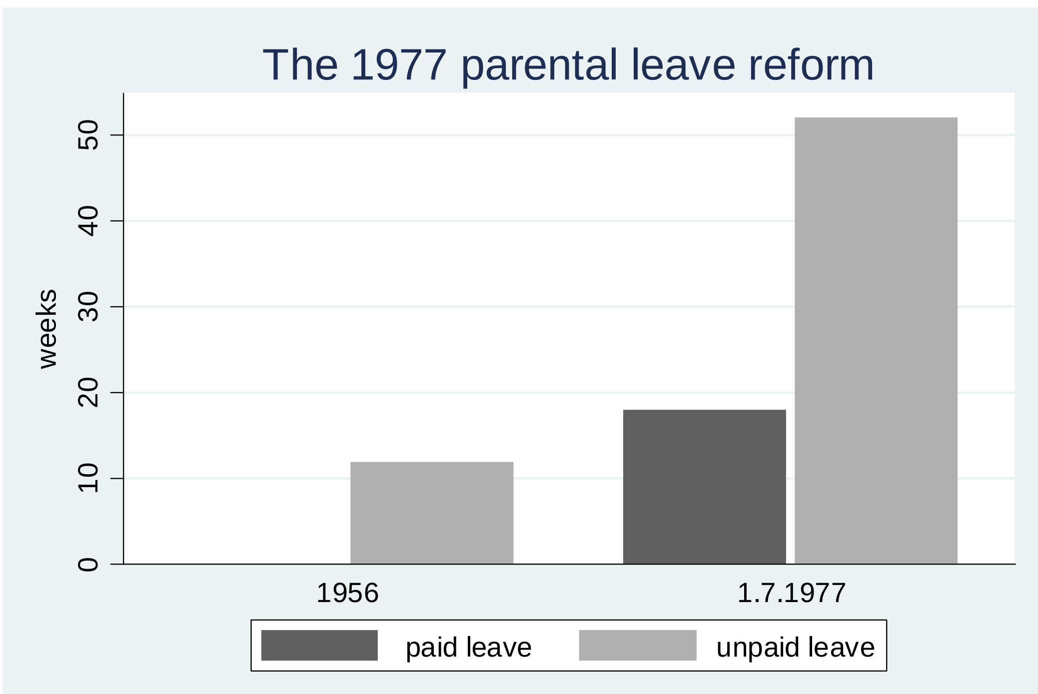
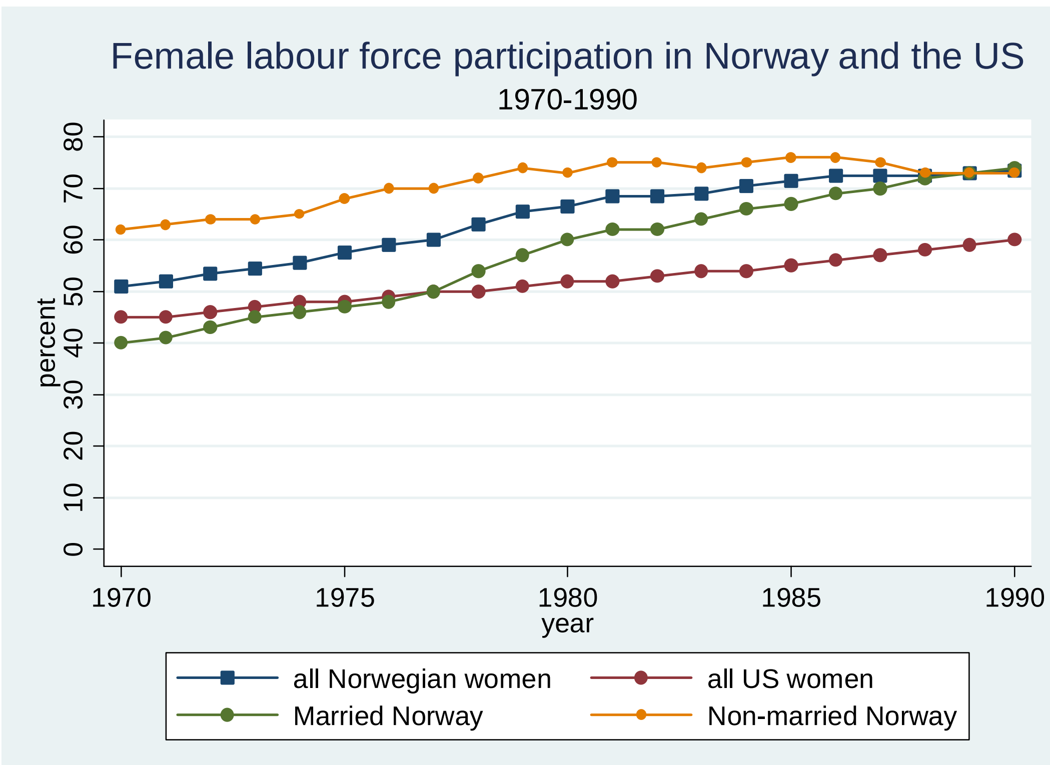
Figure 1: the 1977 reform



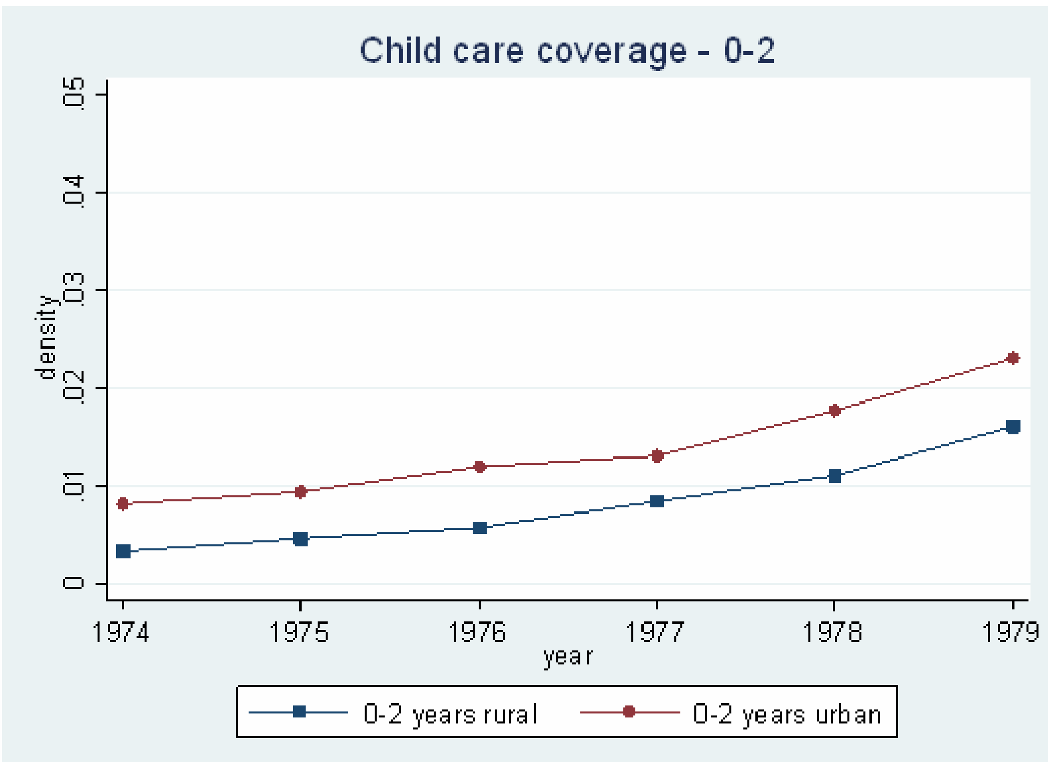
Source: regjeringen.no, lovdata.no

Figure 2
Female employment in Norway and the US 1970-1990



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

Figure 3
Day-care coverage in Norway split by age and urban-rural areas



Data souce: NSD municipality data

Figure 4
Proportion of mothers eligible for maternity leave

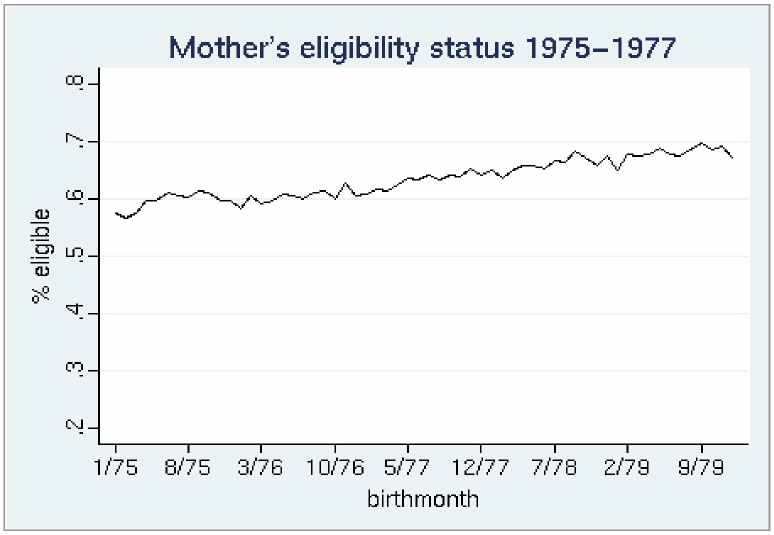
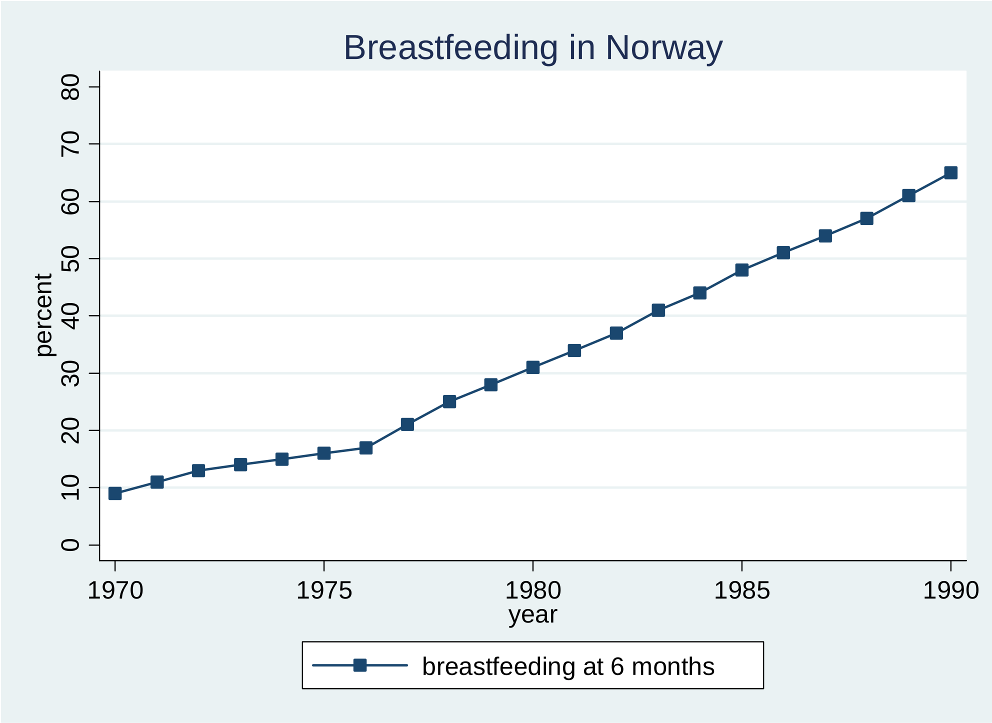
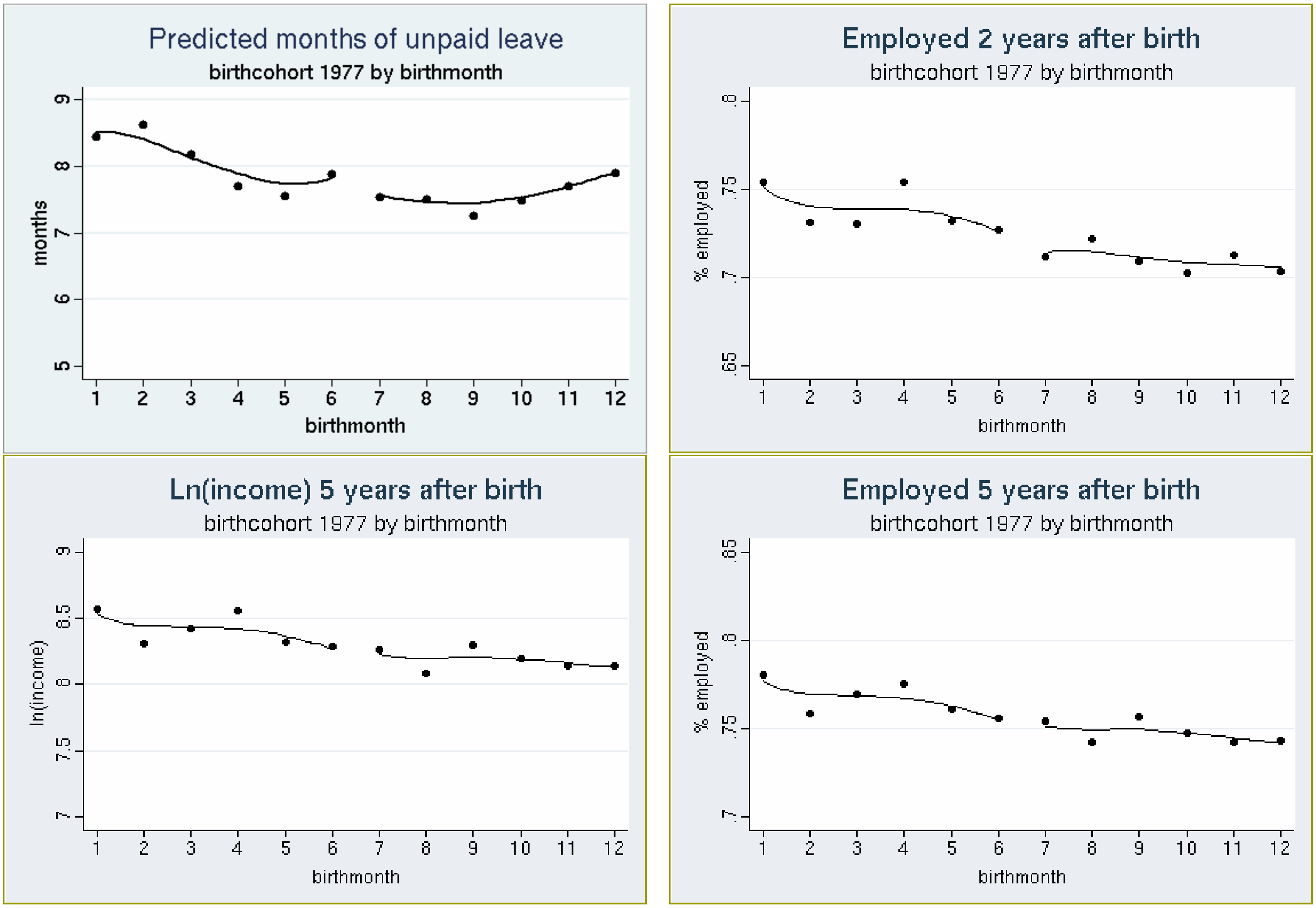


Figure 5
Breast Feeding in Norway



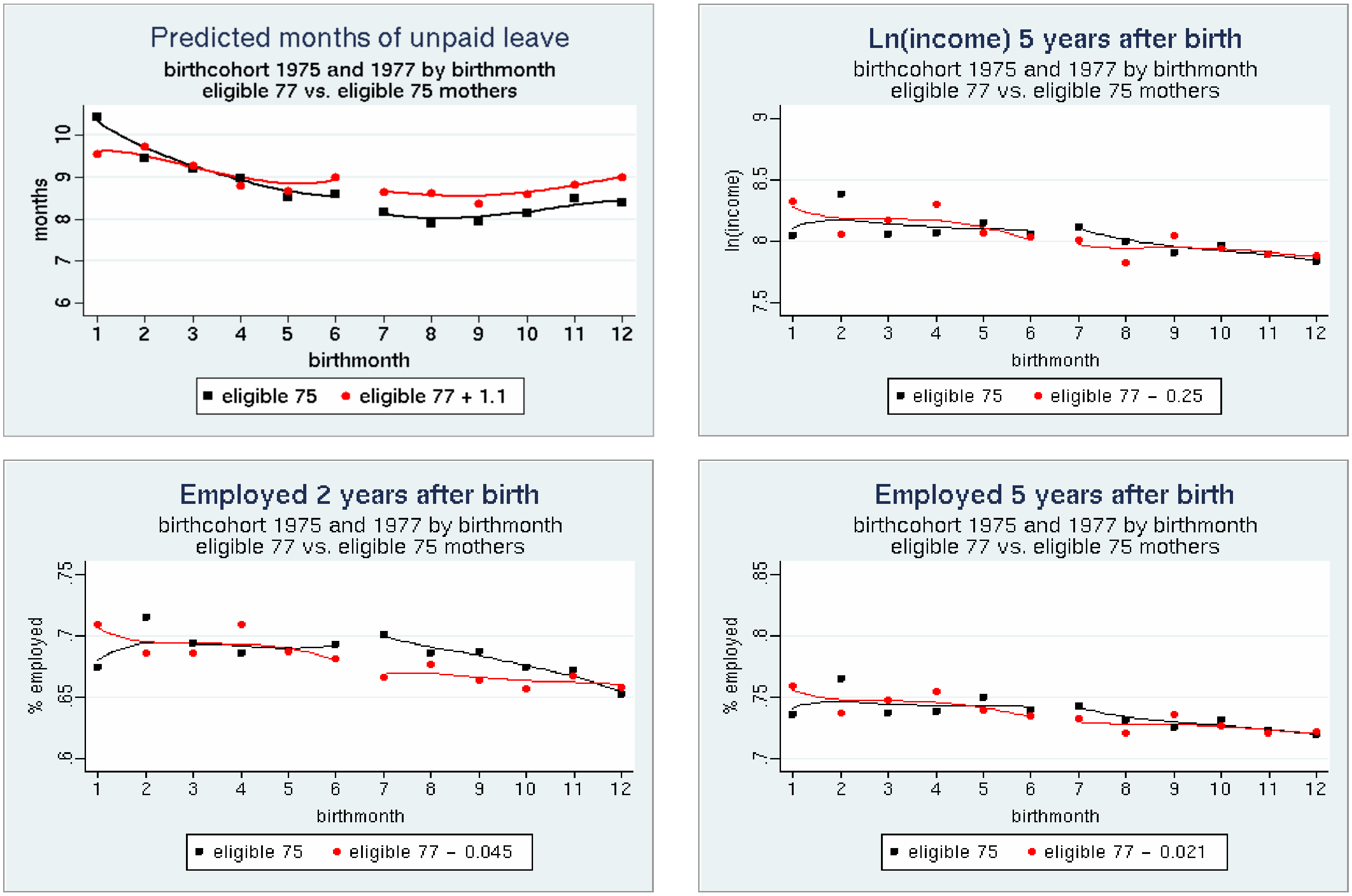
Source: Projected from Liestøl, Rosenberg and Walløe, 1988

Figure 6a
Mother's outcomes by birth month, eligible mothers 1977



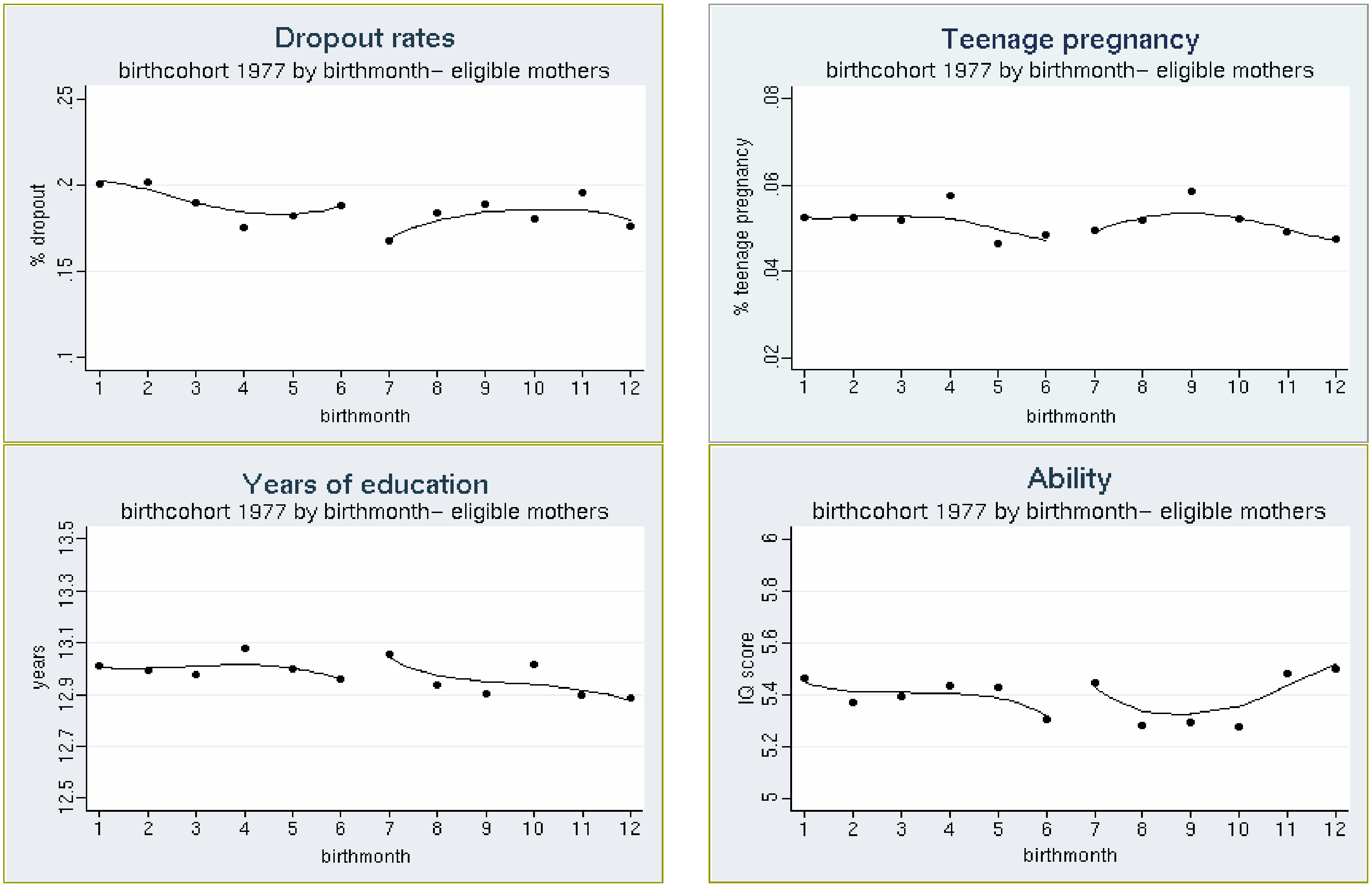
Note: Each graph shows the estimated mean for mother's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three

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



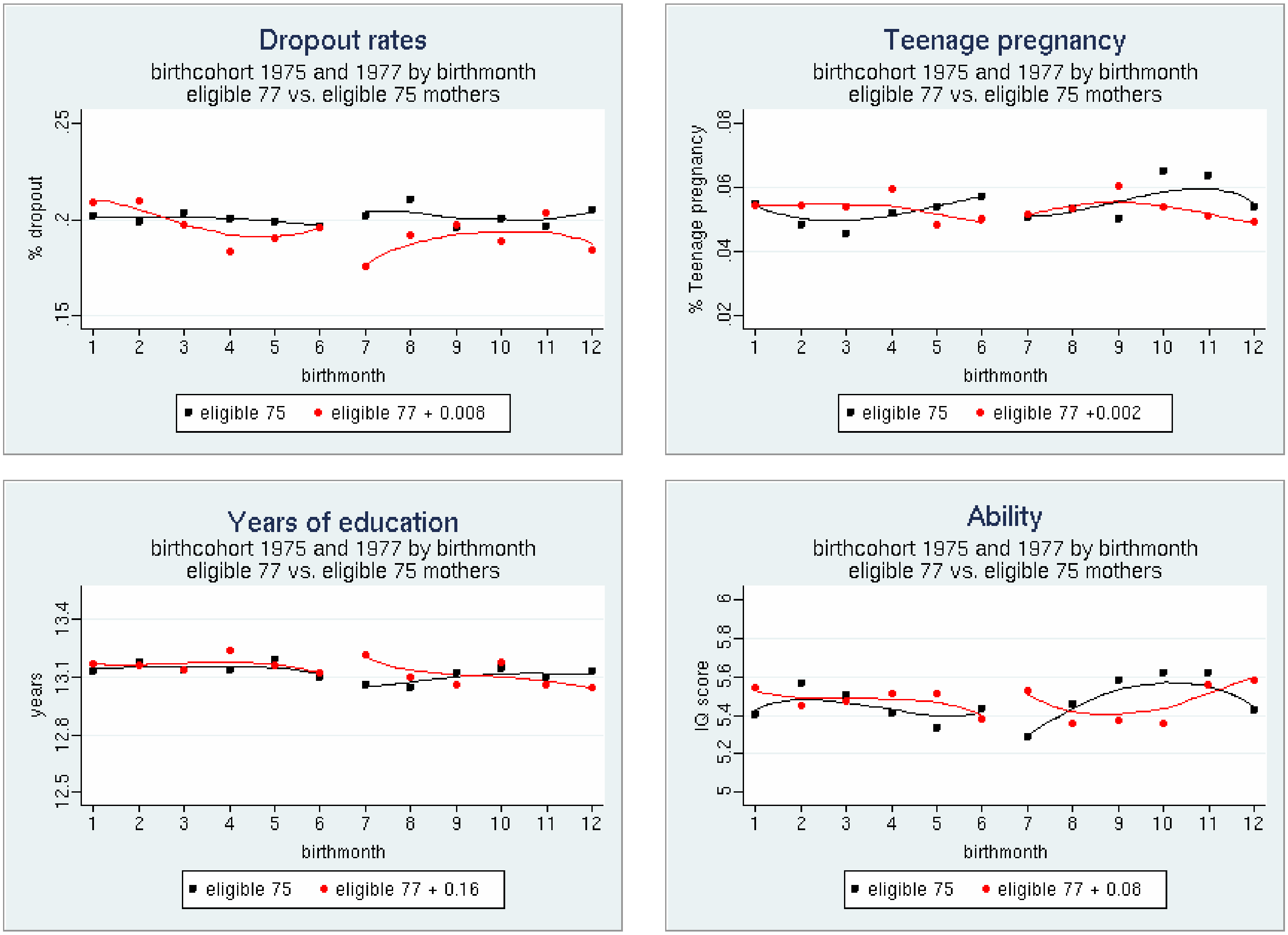
Note: Each graph shows the estimated mean for mother's outcomes by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three.

Figure 7a
Children's outcomes by birth month, eligible mothers 1977



Note: Each graph shows the estimated mean characteristic for children's outcomes, by birth month. The solid line is non-parametrically fitted using triangle kernel with a bandwidth of three.

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



Note: Each graph shows the estimated mean dropout rates, by birth month. The solid line is nonparametrically fitted using triangle kernel with a bandwidth of three.

Table 1: Characteristics of eligible and non-eligible mothers

Eligibility status	Eligible 1977	Non-eligible 197
Mothers		
Years of education	10.63 (2.18)	9.61 (1.72)
Age at birth (in months)	313 (56.5)	318 (64.6)
Income in 1975 in NOK	94088 (68621)	10563 (26418)
Employed 2 years after	.725 (.447)	.362 (.481)
Employed 5 years after	.758 (.428)	.534 (.499)
Income in 1982 in NOK	71216 (73324)	29434 (48202)
Children		
Dropout rates	.186 (.388)	.276 (.447)
Years of education	12.98 (2.43)	12.37 (2.37)
Teenage pregnancy	.054 (.227)	.087 (.283)
IQ young men	5.389 (1.72)	4.934 (1.75)

Table 1
Descriptive statistics, eligible mothers and children of eligible mothers in 1977

Birth month	January - June	July - December	Difference	Number of obs.
Mothers				
Years of education	10.65 (2.20)	10.61 (2.16)	.042 (.026)	28721
Age at birth (in months)	311.85 (56.46)	313.61 (56.61)	-1.759*** (.663)	29163
Income in 1975 in NOK	95871 (68419)	92160 (68789)	3711*** (804)	29163
Predicted months of unpaid leave	8.05 (8.38)	7.56 (7.17)	.487*** (.092)	29163
Employed 2 years after	.738 (.440)	.711 (.454)	.028*** (.005)	29163
Employed 5 years after	.767 (.423)	.748 (.434)	.019*** (.005)	29163
Income in 1982 in NOK	72561 (73574)	69761 (73027)	2800*** (859)	29163
Children				
Dropout rates	.189 (.392)	.180 (.386)	.009* (.005)	29163
Years of education	13.01 (2.44)	12.95 (2.42)	.055* (.029)	29163
Teenage pregnancy	.054 (.23)	.054 (.23)	.000 (.004)	14070
IQ young men	5.400 (1.70)	5.379 (1.75)	.021 (.030)	13150

Note: 1USD=5.23NOK (16.06.2008)

Table 2
Levels of unpaid leave for eligible mothers 1977 by the following subgroups: mother's education, urbanization, distance to grandparents and child care coverage

Birth month	January - June	July - December	Difference	Number of obs.
Unpaid leave				
Mother's education: Less than 10 years	9.87 (9.22)	9.15 (9.75)	.716*** (.173)	9763
Mother's education: 10 years or more	7.14 (7.77)	6.76 (6.71)	.385***	19396
Centralization: urban	7.69 (8.08)	7.42 (7.16)	.272** (.128)	14379
Centralization: rural	8.40 (8.65)	7.70 (7.17)	.702*** (.131)	14780
Distance to grandparents: close	8.96 (8.72)	8.00 (7.13)	.963*** (.188)	7263
Distance to grandparents: far	7.96 (8.21)	7.59 (7.19)	.367*** (.119)	17011
Child care coverage: low	8.44 (8.70)	7.75 (7.25)	.688*** (.139)	13285
Child care coverage: high	7.72 (8.09)	7.40 (7.09)	.323*** (.121)	15874

Table 3
Mother's labor supply

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

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

Table 4
Children's outcomes

Variables		Nonparametric Regression discontinuity	Nonparametric Differences-in-differences using 1975 as controls
	Bandwidth Mean	**3**	**3**
Dropout rate	.19	-.019* (.010)	-.027** (.014)
Years of schooling	12.98	.090 (.063)	.153* (.090)
Teenage pregnancy	.052	.002 (.008)	.008 (.012)
IQ (boys)	5.39	.110* (.067)	.240*** (.094)
N		29163	59564

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

Table 5

Differences-in-differences using eligible mothers in 1975 as control group; Results by mother's education, urbanization, distance to grandparents and child care coverage

Variables	Nonparametric differences-in-differences							
Bandwidth	**3**		**3**		**3**		**3**	
	Mother's education		**Centralization**		**Distance to grandparents**		**Child care coverage**	
subgroups	**Less than 10 years**	**10 years or more**	**Urban**	**Rural**	**Close**	**Not-close**	**low**	**high**
Children								
Dropout rate	-.052**	-.019	-.025	-.028	-.050*	-.003	-.043**	-.013
	(.026)	(.016)	(.020)	(.021)	(.029)	(.019)	(.021)	(.020)
Years of schooling	.320**	.099	.219*	.078	.095	.096	.133	.172
	(.138)	(.113)	(.127)	(.126)	(.179)	(.120)	(.128)	(.125)
Teenage pregnancy	.014	.002	.001	.015	.048*	-.011	.027	-.009
	(.024)	(.012)	(.016)	(.017)	(.025)	(.015)	(.018)	(.015)
IQ (boys)	.371**	.219*	.198	.269**	.597***	.034	.278**	.204
	(.150)	(.114)	(.131)	(.134)	(.194)	(.124)	(.137)	(.129)
Mothers								
Predicted months of unpaid leave	-.259	.157	-.036	.344	1.12*	.083	.560	-.255
	(.524)	(.337)	(.399)	(.425)	(.604)	(.387)	(.444)	(.383)
Employed 2 years after birth	-.008	-.018	-.012	-.025	-.048	-.014	-.049**	.009
	(.029)	(.020)	(.023)	(.024)	(.035)	(.022)	(.025)	(.022)
Employed 5 years after birth	.004	-.004	-.023	.015	.002	-.006	-.039*	.025
	(.028)	(.019)	(.22)	(.23)	(.034)	(.021)	(.023)	(.021)
Ln(Income) 5 years after birth	.098	-.093	-.246	.100	.037	-.136	-.487*	.289
	(.305)	(.216)	(.248)	(.254)	(.371)	(.239)	(.264)	(.240)
N	22067	37497	30314	29250	13824	33704	27362	32202

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

Table 6

Children's outcomes: Differences-in-differences using eligible mothers in 1975 as control group; Results by birth order

Variables	Nonparametric differences-in-differences	
Bandwidth	**3**	
	Birth order	
Sub groups	**First born**	**Not first born**
Children		
Dropout rate	.002 (.019)	-.064*** (.022)
Years of schooling	-.000 (.118)	.365*** (.137)
Teenage pregnancy	-.001 (.015)	.019 (.019)
IQ (boys)	.127 (.124)	.402*** (.142)
Mothers		
Predicted months of unpaid leave	.259 (.354)	-.102 (.489)
Employed 2 years after birth	-.051** (.022)	.026 (.024)
Employed 5 years after birth	-.029 (.022)	.026 (.021)
Ln(Income) 5 years after birth	-.366 (.246)	.298 (.244)
N	34248	25316

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

Table 7
Boy's height

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

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

Appendix
Figure A1
Children's dropout rates from high school by birth month, eligible mothers 1977 versus 1975
Subgroups: mothers education and centralization

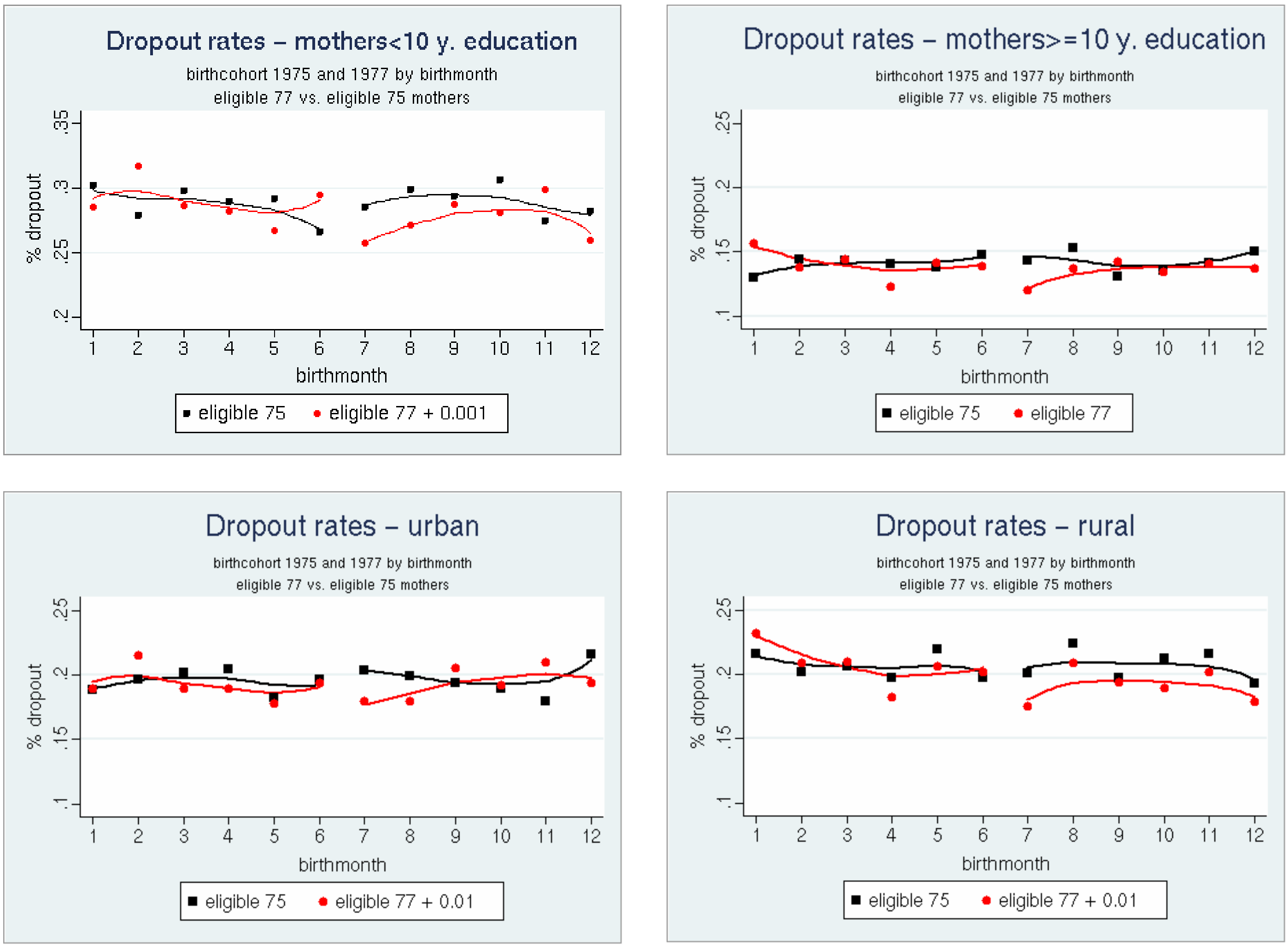


Figure A2
Children's dropout rates from high school by birth month, eligible mothers 1977 versus 1975
Subgroups: distance to grandparent and child care coverage

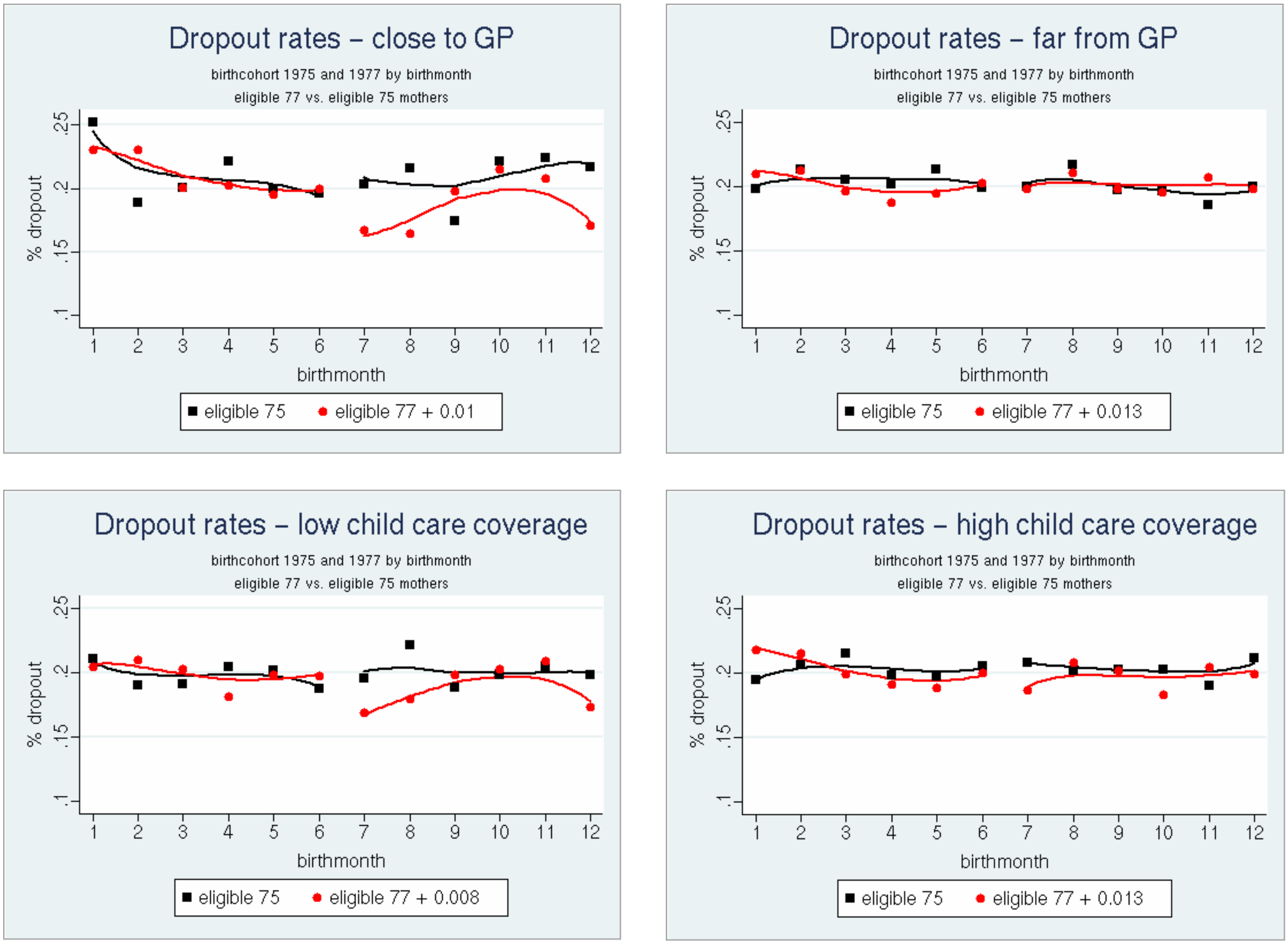


Table A1
Mother's labor supply and children's outcomes, total sample of all mothers and children in 1977 with control groups in 1975

Variables	Nonparametric regression discontinuity	
Bandwidth	**3**	**3**
Control group	**RD**	**1975**
Dropout rate	-.013 (.009)	-.012 (.012)
Years of schooling	.035 (.050)	.047 (.070)
Teenage pregnancy	.001 (.007)	-.001 (.010)
IQ (boys)	027 (.054)	.086 (.075)
Predicted months of unpaid leave	-.288* (.158)	-.004 (.227)
Employed 2 years after birth	-.006 (.010)	-.010 (.014)
Employed 5 years after birth	-.005 (.010)	-.009 (.014)
Ln(Income) 5 years after birth	-.057 (.108)	-.125 (.149)
N	46245	97312

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

Table A2
Mother's labor supply using bandwidth 5

Variables		Nonparametric Regression discontinuity	Nonparametric Differences-in-differences using 1975 as controls
	Bandwidth	**5**	**5**
	Mean		
Predicted months of unpaid leave	7.81	-.182	.262
		(.164)	(.242)
Employed 2 years after birth	.73	-.015	-.025*
		(.010)	(.014)
Employed 5 years after birth	.76	-.007	-.006
		(.009)	(.013)
Ln(Income) 5 years after birth	8.31	-.091	-.105
		(.104)	(.147)
N		29163	59564

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

Table A3
Children's outcomes using bandwidth 5

Variables		Nonparametric Regression discontinuity	Nonparametric Differences-in-differences using 1975 as controls
	Bandwidth Mean	**5**	**5**
Dropout rate	.19	-.012 (.008)	-.019* (.012)
Years of schooling	12.98	.037 (.053)	.111 (.076)
Teenage pregnancy	.052	.004 (.007)	.011 (.010)
IQ (boys)	5.39	.034 (.056)	.100 (.078)
N		29163	59564

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

Table A4
Placebo results: Mother's labor supply and children's outcomes
Eligible mothers 1975, eligible mothers 1979 and non-eligible mothers 1977

Variables	Nonparametric regression discontinuity	
Bandwidth	**3**	**3**
Control group	**Eligible 1975**	**Non-eligible 1977**
Dropout rate	.007 (.010)	.001 (.015)
Years of schooling	-.063 (.063)	-.068 (.081)
Teenage pregnancy	-.006 (.008)	-.004 (.014)
IQ (boys)	-.129* (.066)	-.126 (.090)
Predicted months of unpaid leave	-.318 (.214)	-
Employed 2 years after birth	.007 (.012)	-.002 (.016)
Employed 5 years after birth	.001 (.011)	-.010 (.017)
Ln(Income) 5 years after birth	.029 (.125)	-.121 (.180)
N	30401	17082

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