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

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We study a change in maternity leave entitlements in Norway. Mothers giving birth before July 1, 1977, were eligible for 12 weeks of unpaid leave, while those giving birth after that date were entitled to 4 months of paid leave and 12 months of unpaid leave. The increased time spent with the child led to a 2 percentage point decline in high school dropout rates and a 5 percent increase in wages at age 30. These effects were larger for the children of mothers who, in the absence of the reform, would have taken very low levels of unpaid leave.

I. Introduction

There are huge disparities in maternity leave entitlements across different countries. At one extreme, countries in northern Europe, such as

Sweden, Norway, and Germany, mandate very generous paid leave and long periods of job protection after childbirth. At the other extreme, there are a handful of countries, such as the United States, that have no mandatory paid leave and offer little, if any, job protection after the birth of a child (International Labour Organization 1998).

These disparities were much smaller 30–40 years ago. In several countries, new mothers had benefits similar to the ones currently in place in the United States, where the federal mandatory leave, which is adopted in almost all states, is only 12 weeks of 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 of unpaid leave but 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.

The example of Norway is not unique. Following the strong growth in female labor force participation, maternity leave benefits have become more generous across the world. In the United States, however, they have remained fairly low, despite substantial debate on this topic. A central question is whether the absence of stronger maternal employment protection and leave entitlements in the United States is detrimental to child development or whether the high levels of benefits in northern Europe are mostly important for maternal health (and parental welfare more generally), with little consequence for children's lives. In other words, what is the impact (on child outcomes) of parental time with the child in the first months of life? This question is the focus of our paper.

Empirically, this is a notoriously difficult issue to analyze, as emphasized, for example, by Bernal (2008) and Dustmann and Schönberg (2012), because mothers who spend more time with their children after birth may have unobservable attributes that affect child development, or they may use child care arrangements that are special in unobservable dimensions. Furthermore, because additional time with children is generally associated with less time at work and thus lower household income, it is difficult to isolate the effects of the two variables.

In our paper, we address these empirical challenges by studying the impact of a reform of maternity leave benefits in Norway on the long-term outcomes of children, namely, their education and earnings at ages 25–33. The reform we analyze increased mandatory paid maternity leave from 0 to 4 months and mandatory unpaid maternity leave from 3 to 12 months.[1]

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[1] This is equivalent to moving from the current level of maternity leave entitlements in the United States to those in Holland and several other countries in southern and central Europe.

This new set of benefits applied to all eligible mothers who had children after July 1, 1977.[2] We estimated their long-term impact on children using regression discontinuity, by comparing the outcomes of children of eligible mothers born immediately after and immediately before this particular date. We were able to test for potential manipulation of the date of birth.

We followed children until 2010, when they were 33 years of age. We measured several medium- and long-term outcomes, such as high school completion, college attendance, and wages up to age 33.

We begin with a simple look at the data. Using data only on individuals (and their mothers) born in June and July of 1977 (immediately before and immediately 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 the extensive 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 this from the estimate of the effect of being born in July (vs. being born in June) obtained from the 1977 data, as in a difference-in-differences estimator.[3]

Table 1 presents estimates of the impact of the program, using the single- (col. 1) and double- (col. 2) difference estimators for a subset of the dependent variables we consider in the paper. The following child outcomes are shown at the top: indicators of whether a person was a high school dropout, whether the person ever attended college, and the person's log earnings at age 30. The results suggest that the reform

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

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

$$Y_i = \alpha + \beta \times D_i^{\text{July}} + u_i,$$

where Y_i is the outcome of interest, D_i^{July} is a dummy variable indicating whether an individual was born in July, and β measures the impact of the reform on the outcome of interest among children of eligible mothers. For the difference-in-differences estimator, using data from children born in the months of June and July of 1975, 1978, 1979, and 1977, we can run

$$Y_i = \alpha + \gamma \times D_i^{1977} + \gamma \times D_i^{1978} + \gamma \times D_i^{1979} + \varphi \times D_i^{\text{July}} + \beta \times D_i^{\text{July}} D_i^{1977} + u_i,$$

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

TABLE 1
DIFFERENCES IN AVERAGE OUTCOMES OF CHILDREN BORN IN JUNE AND JULY 1977

	BY BIRTH MONTH	
VARIABLE	Single-Difference (1)	Difference-in-Differences (2)
Children:		
High school dropout	−.020*	−.032**
	(.011)	(.013)
College attendance	.017	.036**
	(.014)	(.016)
Log earnings at age 30	.045**	.072***
	(.022)	(.026)
Mothers:		
Prereform characteristics:		
Years of education	−.023	−.009
	(.063)	(.071)
Log income 2 years prior to the birth of the child	−.014	.003
	(.031)	(.029)
Outcomes:		
Average log income +/−1 year around year of birth	.037	.008
	(.027)	(.031)
Employed 5 years after the birth of the child	−.002	−.007
	(.012)	(.014)
Log income 5 years after the birth of the child	−.018	−.080
	(.138)	(.157)

NOTE.—Column 1 shows the coefficients of a regression of each of the variables on an indicator for being born in July 1977. The sample included only individuals born in June and July of 1977. For col. 2, we added to the sample those born in June and July of 1975, 1978, and 1979, and we regressed each of the variables on a year indicator, a month of birth indicator, and the interaction of the two. We report the coefficient on the latter.

* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

reduced high school dropout rates by about 2–3 percentage points, increased college attendance by 3.5 percentage points (only in the double-difference specification), and increased earnings at age 30 by 5–7 percentage points.

We then examined two prebirth maternal variables, which should not have been affected by the reform: the number of years of education of the mother and her log annual income in 1975. In both these dimensions, the sets of mothers who gave birth in June and in July of 1977 were similar.[4]

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[4] We also checked whether there were differences in the birth weights of children born in June and July 1977, because we would not expect maternity leave benefits to have an impact on birth weight, 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 arose because of the reform and not unobserved differences in child health that could be manifested in differences in birth weight.

Finally, we found no impact of the reform on maternal income 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 (time off work) was taken by working mothers by analyzing how much their income fell after giving birth.

We also examined maternal labor supply and income 5 years after the birth of the child and found no significant effect of the reform on these variables, using both single- and double-difference specifications.[5] Therefore, the most likely mechanism through which this reform operated was an increase in the time spent with the child, with no short- or long-term consequences for maternal employment or income.

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

The paper proceeds as follows. Section II provides a short review of the literature. Section III gives background information on maternity leave legislation in Norway, while Section IV presents the empirical strategy. Section V presents the data and Section VI shows the results. Section VII discusses the evidence on the possible mechanisms through which the reform affected child outcomes. Section VIII presents concluding remarks.

II. Short Review of Relevant Literature

There is a very extensive 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; Gregg et al. 2005; Tanaka 2005). The literature is fairly inconclusive and is 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 United States, maternal employment in the first year of life does not have particularly detrimental consequences for children because its neg-

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

ative 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 United States, Rossin (2011) studies the effect of the 1993 reform on children's birth dates and infant health. She finds evidence of some positive effects of the reform on children's health outcomes. There is also a set of recent papers studying Canadian reforms, focusing on short-term outcomes for children, by Baker and Milligan (2008a, 2008b). These papers find no significant effects on children's outcomes.

In addition, there are also empirical analyses of the effect of maternity leave reforms on children's long-term outcomes using registry data with very large sample sizes for Germany (Dustmann and Schönberg 2012), Denmark (Rasmussen 2010), and Sweden (Liu and Skans 2010). As in our study, these three papers explore exogenous variation in maternity leave resulting from legislative reforms and are able to look at the long-term outcomes of children. Our data challenge the main conclusion of these papers, which is that maternity leave expansions have little or no effect on the long-term outcomes of children.

Two central aspects of our study distinguish it from those above and may explain our different results. First, we consider a change in maternity leave entitlements that occurred at a time when they were at a very low level, similar to that in the United States today. The three papers mentioned 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 involved an expansion from 2 to 6 months of paid maternity leave entitlements. Nevertheless, this is much less generous than the reform we consider herein because the payments women were entitled to in the expansion period (from the third to the sixth month after childbirth) corresponded, on average, to only 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 the time spent with the child. By contrast, in our case, we conjecture that the take-up of the 1977 reform in Norway was 100 percent for the eligible women (i.e., the full 4 months). In sum, even though the 1979 German reform looks similar to the 1977 Norwegian reform, in practice it was much less generous, and it probably led to a smaller impact on maternal time spent at home.

The reform studied by Liu and Skans (2010) was quite different from the reform analyzed in this paper. They assess an extension of maternity leave in Sweden from 12 to 15 months. In addition, the main alternative to maternity leave in Sweden at the time of the reform was subsidized day

care. Our study analyzes a reform affecting younger children, in a setting where the main source of alternative care was informal and possibly low-quality private-sector care.

The Danish study analyzed an extra 6 weeks of leave in addition to a paid maternity leave entitlement of 3½ months (Rasmussen 2010). Hence, when analyzing extensions of already-generous maternity benefits, these studies found little or no impact.

The second important feature that distinguishes our work is that we were able to examine the education and labor market outcomes of children as late as age 33. Other papers have examined earlier educational or labor market outcomes. One problem with examining early labor market outcomes is that individuals' careers may stabilize only much later.[6] In addition, our data enabled us to link mothers with their children, allowing us to perform a rich analysis of the impacts across various subgroups of mothers. Our data also allowed us to construct good measures of eligibility for the reform, which is important because generally only a fraction of mothers, that is, those who are working a certain minimum amount of time, were eligible for these benefits.[7]

III. Maternity Leave Reform and Institutional Background

A. Maternity Leave Reform

In 1956, maternity leave benefits first 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 to mothers in (nearly all states in) the United States in 2011, provided that they work in firms with 50 or more employees.

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[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 they do not contain direct measures of maternal employment. This information is not essential for estimating the effects of the reform, but it is useful for understanding the mechanisms through which they are operating. We do, however, observe total income in each year. The reform had no impact on maternal income in 1977 and 1978. This means that the reform did not change the amount of unpaid leave being taken by mothers who gave birth after the reform. We do not consider the reason for this to be that the reform had no effect at all on leave-taking behavior because this 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 of maternity leave, for which we can observe employment data, we see close to full uptake of the new benefits. Therefore, we argue that the reform led to an actual increase of 4 months in the paid leave taken by new mothers, without changing unpaid leave uptake or maternal income. In addition, all the reforms of either paid or unpaid leave programs examined in the literature described above had important impacts on the uptake of leave.

On July 1, 1977, Norway introduced 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 prebirth income from wages (i.e., 100 percent income replacement for 18 weeks). Of these 18 weeks, 6 had to be taken by the mother alone, while the rest could be shared between both parents. In practice, all leave was almost exclusively taken by the mother (Rønsen and Sundström 2002). In addition, parents also became entitled to 1 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 as eligibility depended on their work and income history. Only women who had worked at least 6 of the 10 months immediately prior to giving birth and were earning more than 10,000 Norwegian kroner (NOK) annually were eligible for leave and the payment.[10]

Because of limitations in our data (we could not measure maternal employment directly, and we had only yearly income data for wages and benefits), we had to rely on an imperfect measure of eligibility. We defined eligible mothers as those who had a salary of at least NOK 10,000 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 who gave birth in Norway in 1977 were eligible for maternity leave benefits. We tried alternative definitions of eligibility, but these produced no significant changes in our empirical results.

Figure 1 shows the proportion of mothers who were eligible for maternity leave entitlements in 1975–79 according to the birth month of the child. Between 1975 and 1979, the proportion of eligible mothers was always between 60 percent and 70 percent, and in 1977 it was about

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[8] These changes were introduced together with a new law increasing workers' rights (Arbeidsmiljøloven) accepted on June 3, 1977, by the Parliament and introduced on July 1, 1977 (see Prepositions, Ot.prp. nr. 71 and Innst.o. nr. 90). There were additional reforms after 1977. From 1987 onward, the paid maternity leave was extended almost yearly until 1993. From 1993 to the present, Norway has had the same paid maternity leave of 42 weeks with 100 percent coverage or 52 weeks with 80 percent coverage. In this paper we have decided to focus on the 1977 law for three reasons. First, the change affects what we believe is a critical period for the child, for instance, because breast-feeding is still an issue. Second, it is easier to assess the first change in the law because the latter reforms were anticipated to a larger degree. Finally, given that data are available only until 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 to future work.

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

[10] The amount of NOK 10,000 (US$1,725) refers to the lowest level of income providing pension points in the Norwegian social security system in 1977.
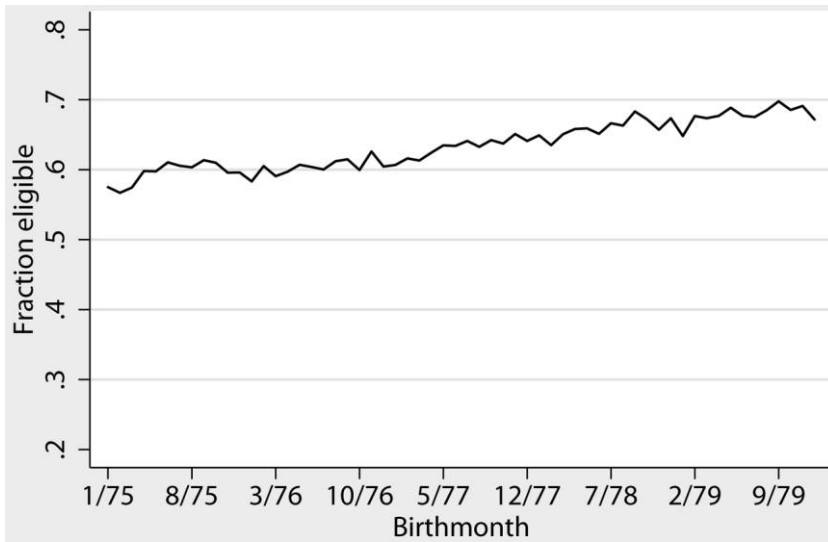
Fig. 1.—Proportion of mothers eligible for maternity leave from 1975 to 1979. The solid line shows the fraction of eligible mothers among the total population of mothers who gave birth in each month from January 1975 to December 1979.

65 percent. As we can focus only on eligible mothers in our analysis, this means that our estimates ignore 35 percent of mothers who gave birth and their children in that year.

In order for us to be able to identify the effects of the reform on children's outcomes, it is crucial that mothers were not able to change their eligibility status immediately after the reform was announced; otherwise, the set of eligible mothers who gave birth immediately before and immediately after the reform would not be comparable. The maternity leave reform was introduced during a burst of legislative activity from the sitting (very radical) Parliament at the end of its term. It is unlikely that the legislation was widely expected because it was introduced along with a number of other changes and at the end of the legislative period. The government report became official on April 15, 1977, and was approved on June 13, 1977.[11] This means that all women who gave birth immediately after the introduction of the law in 1977 were already pregnant when the law was announced,[12] and because of the rule regarding working 6 out of the 10 months prior to giving birth, it was diffi-

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[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 who gave birth close to July 1, 1977, were able to delay their delivery (although we 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 childbirth (by as much as a week) in response to a

cult for women to change their eligibility status in the short run. We also checked national newspapers around 1976 and 1977 for news about the reform but found no evidence that newspapers reported on the reform before June 1977.[13] Therefore, it is plausible that eligibility status was exogenous for mothers who gave birth in 1977.

The 1970s in Norway were the decade of oil discovery, with increasing labor force participation by women and the implementation of several welfare reforms. We have studied all laws and reforms 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 1, 1976. This law made it easier for women to have an abortion within 12 weeks of conception. The first cohort of children affected by this reform was born around July 1976. This possibly gives rise to a discontinuity in observed child outcomes between those born in June and July 1976, and hence, we do not use 1976 as a comparison with 1977.

B. Institutional Background

At the time of the maternity leave reform in 1977, the labor force participation of women was relatively high in Norway. Figure 2 shows the labor force participation in Norway compared with the United States from 1970 to 1990 (distinguishing Norwegian women who were mothers from those who were not). In Norway, the labor force participation rate around 1977 was about 50 percent for married women, who were the most relevant group for our study, and around 70 percent for unmarried women. The labor force participation of women was about the same in Norway and the United States during the 1970s but much higher in the former than in the latter by 1990.

It is also relevant to examine the provision of public child care. In the mid-1970s, very few children aged 0–2 years were in day care in Norway. Although day care centers provided coverage for 15 percent of children aged 3–6 years in 1977, the coverage for the first 2 years was very low, at only 1–2 percent. This means that the main alternative to maternal care in the early years of the child's life was informal care by nannies, grandparents, or neighbors.

IV. Empirical Strategy

Let $y_i(1)$ be the outcome for child i in the presence of the reform, and let $y_i(0)$ be the outcome for child i in the absence of the reform. Our

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reform that changed fertility incentives (mostly by changing the schedule of inductions and caesarian sections). As we use daily birth data, we can check whether this is also true in our data by studying whether there is any bunching of births immediately after the reform.

[13] *Verdens Gang*, June 30, 1977; *Bergens Tidende*, June 27, 1977, June 30, 1977; *Aftenposten*, June 30, 1977.
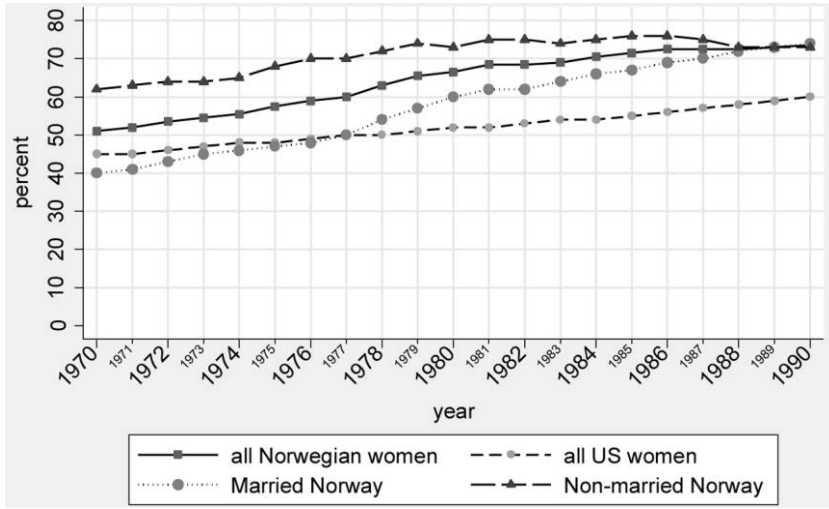
FIG. 2.—Female employment in Norway and the United States from 1970 to 1990. The four lines show the fraction of females working in the labor market. The first two lines show this for all Norwegian and US women. The other two lines represent married and unmarried Norwegian women. Source: Statistics Norway, Bureau of Labor Statistics (projected from OECD, *Population Bulletin* 63 [2008]).

main goal is to estimate the average impact of the reform on the long-term outcomes of the child: $\alpha = E(y_i(1) - y_i(0))$.[14]

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

For those women who gave birth in 1977, eligibility for the new maternity leave entitlements (E_i) was a deterministic function of the date on which they gave birth (X_i):

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

where c is the cutoff point of July 1, 1977. Therefore, all mothers who gave birth after c potentially could have received the treatment defined by new maternity leave entitlements, while those who gave birth before c were assigned to the control group. We used only eligible mothers (as defined in Sec. II) in our main analysis.

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[14] This answers the following question: What are the consequences of a maternity leave reform for the long-term outcomes of children whose mothers were exposed to it (i.e., what is usually called "intent to treat")? This is different from the question, What is the impact of taking maternity leave on 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.

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

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

As with any RD estimator, we are able to identify only a local effect, that is, for those born around the time of the reform. However, this is one case in which it is reasonable to conjecture that the effects of the reform did not vary substantially with date of birth, in which case α_{RD} should be a consistent estimator of α.

Assuming that $E[y_i(1)|X_i = c]$ and $E[y_i(0)|X_i = c]$ are continuous in x (continuity at $x = c$ is all that is needed), we can estimate them as follows:

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

The outcomes of interest for the child include dropping out of high school and college attendance (both measured at age 30), earnings at age 30, years of education at age 30, the probability of having had a child before age 19 for women, IQ for men, and earnings between the ages of 25 and 33. The outcomes of interest for the mother include the number of months of unpaid leave and her employment and earnings 5 years after giving birth. These are interesting because we can examine whether the reform induced changes in the home environment that could account for the effect of the reform on child outcomes.

We estimate

$$\alpha_{\mathrm{RD}} = \lim_{x \downarrow c} E[y_i|X_i = x] - \lim_{x \uparrow c} E[y_i|X_i = x]$$

by taking the difference between the boundary points of two regression functions of y on x: one for eligible women ($x \le c$) and one for ineligible women ($x > c$). We estimate these regression functions using a local linear regression (Fan 1992), as in Hahn, Todd, and Van der Klaauw (2001) and Porter (2003). Defining h as the bandwidth, we estimate (β, γ, τ) as follows:

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

The term α_{RD} is estimated as

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

We use the triangle kernel, which has been shown to be boundary optimal (Cheng, Fan, and Marron 1997). We obtain standard errors as rec-

ommended in Lee and Lemieux (2010) using heteroskedasticity-robust standard errors (White 1980). The choice of bandwidth is important, as usual. We present our main results using a bandwidth of 90 days, but 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 close to the date of the reform, which could potentially affect our results. We confirm that the number of births did not change in the days and weeks immediately preceding and following the date of the reform. We also confirm that the characteristics of mothers who gave birth immediately before and immediately after the reform were virtually identical.

Finally, we examine what happens to our estimates when we drop from our sample children who were born close to the date of the reform (within 1, 2, 4, or 6 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 move 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. [2011] for evidence for Norway). In this case, α_{RD} estimates $\alpha + \lambda_{\mathrm{Birth}}$, 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 the RD with the difference-in-differences (DD) specification by constructing two types of control groups: one consisting of children born in 1975, 1978, and 1979 to eligible mothers (our main specification) and another consisting of children born in 1977 to ineligible mothers.

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

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

As there was no reform for the control groups, $\hat{\alpha}_{\mathrm{con}}$ (the RD estimate for those in a control group) should capture only date-of-birth effects. On the other hand, $\hat{\alpha}_{\mathrm{RD,1977}}$ confounds the 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 the control years, for those born to ineligible mothers in 1977, and for those born to eligible mothers in 1977, we can estimate the effect of the reform as $\hat{\alpha}_{\mathrm{RD\text{-}DD}} = \hat{\alpha}_{\mathrm{RD,1977}} - \hat{\alpha}_{\mathrm{RD,con}}$.

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 dif-

ferent amounts of time with their 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 employment decisions in the medium run, but the full adjustment of these behaviors 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 because this would require waiting for the full adjustment of the fertility and employment habits of women (and possibly their spouses). In fact, the mothers of children born in June and July of 1977 are likely to have engaged in similar adjustments to fertility and employment in the medium run, especially if they were considering having more children (note that we will show that there were no differences in completed fertility and employment between mothers with children born in June and 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?

V. Data Description

Our data source is the Norwegian Registry data maintained by Statistics Norway. It is a linked administrative data set that covers the population of Norwegians up to 2010 and is a collection of different administrative registers providing information about each Norwegian's date of birth, educational attainment, labor market status, earnings, and a set of demographic variables (age and gender) as well as information on families. To ensure that all individuals in the sample went through the Norwegian educational system, we included only individuals born in Norway. We were able to link individuals to their parents, and it was possible to gather labor 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 were defined as all children who did not obtain a 3-year high school diploma, and college attendance was determined from the annual education files identifying whether a person ever started college. Earnings were measured as total gross pension-qualifying earnings reported in the tax registry and were available from 1967 to 2010. These were not top coded, and they included labor earnings, taxable sickness benefits, unemployment benefits, and parental leave payments.

We also collected data on maternal income, measured 2 and 5 years after the birth of the child. This is useful for examining potential chan-

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

nels through which maternity leave affected child outcomes, namely, by promoting the attachment of women to the labor market.

In order to construct a measure of unpaid leave, we started by calculating a measure of the mothers' prebirth monthly income by dividing their 1976 earnings by 12. We then calculated their total earnings in 1977–80 and divided them by our estimate of the 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 of potential postbirth earnings (the earnings that the mother would have received had she not gone on unpaid leave), adjusted for inflation.[16] We limited ourselves to a window of 36 months because the further we move away from prebirth earnings, the more likely it is that earnings may differ because of a change of job, taking on part-time work, the presence of new children, and other factors unrelated to the 1977 reform.[17] We assumed that the paid leave had a take-up rate of 100 percent for those who gave birth after July 1977. Section VII.A gives more information on the plausibility of this assumption.

The IQ data were 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 this age is particularly interesting, as this is about the time of entry into higher education (or into the labor market for those who decide not to go to university). The IQ measure used was 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; Cronbach and Lee 1964; Sundet et al. 2005), and the word test is similar to the vocabulary test in WAIS. The composite IQ test score was an unweighted mean of the three subtests. The IQ score was 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, with a mean of five and a standard deviation of two.

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[16] It is useful to illustrate this with a specific example. If the child was born in June 1977, we subtract 6 months of 1976 monthly earnings from the mother's 1977 earnings and compare the remaining earnings in 1977 and 1978 with her earnings in 1976. If the mother earns half of her 1976 earnings in the 18 months after the birth (corresponding to 6 months of full-time work), she has taken 12 months of unpaid leave. If she earns her 1976 earnings in the 18 months following birth, she has taken 6 months of leave. If the mother was able to take 4 months of paid leave (by giving birth after the reform), then we take that into account by subtracting 4 months of wages from the postbirth 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 who gave birth before and after the reform, so they will potentially affect only the estimate of the level of unpaid leave and not the difference (effect of the reform).

Teenage pregnancy was constructed as an indicator variable taking a value equal to one if the girl had given birth to a child before turning 20 and zero otherwise.

The distance to grandparents variable was created using post code information for the parents of each child in the study and post code information for both sets of grandparents in 1980. Living in the same post code area implies living within a maximum of a few blocks of each other, which means it was possible to have daily contact. We had post code information for about 80 percent of the sample. We created a distance dummy variable equal to one if the family lived in the same post code area as at least one set of grandparents and zero otherwise.

The rural-urban variable was constructed using information from Statistics Norway on the degree of centralization of municipalities in Norway. Urban municipalities included all municipalities with a large town center or close to a large town center, while rural municipalities had small or almost nonexisting city centers.

The working part-time variable was constructed using information from the 1980 census on whether mothers worked full-time, part-time, or not at all. We defined working part-time in 1980 as working between 10 and 1,300 hours per year versus the alternative of not working or working more than 1,300 hours per year.

The completed fertility of mothers was constructed using the population files in 2010 with information on each woman's total number of children. As we measured the total number of children 33 years after the reform, this should capture the completed fertility for all mothers who gave birth in 1977.

VI. Results

A. Descriptive Statistics

We focus only on mothers who were eligible for the reform, and therefore, it is important to show how they compared with those who were not eligible. We saw from figure 1 that the proportion of mothers who were eligible for maternity leave entitlements was about 65 percent in the year of the reform. This means that 35 percent of mothers who gave birth in that year and their children are not accounted for in our estimates of the impact of the reform on child outcomes because the mother was not eligible for maternity leave. Interestingly, current labor force participation rates in OECD countries are generally not much higher than 65 percent, except in the Scandinavian countries, where they are often above 80 percent.

Table 2 displays the main characteristics of eligible mothers and their children (born in 1977) compared with those of ineligible mothers and

TABLE 2
DESCRIPTIVE STATISTICS FOR ELIGIBLE AND INELIGIBLE MOTHERS IN 1977

	ELIGIBILITY STATUS	
VARIABLE	Eligible in 1977 (1)	Ineligible in 1977 (2)
Children:		
High school dropout	.186	.276
	(.388)	(.447)
College attendance	.46	.35
	(.50)	(.48)
Log earnings at age 30	12.6	12.5
	(.74)	(.76)
Mothers:		
Years of education	10.63	9.61
	(2.18)	(1.72)
Age at childbirth (in years)	26.1	26.5
	(.028)	(.041)
Income in 1975[a] (in NOK)	94,088	10,563
	(68,621)	(26,417)
Employed 2 years after childbirth	.725	.362
	(.447)	(.481)
Employed 5 years after childbirth	.758	.534
	(.428)	(.499)
Income in 1982[a] (in NOK)	71,216	29,434
	(73,324)	(48,202)

NOTE.—Entries are the means of the variables presented for the group of eligible (col. 1) and ineligible (col. 2) mothers in 1997. Standard deviations are presented in parentheses.
[a] Consumer price index adjusted to 1998 NOK.

their children. It is clear that eligible mothers had more education than ineligible mothers. They were also more likely to be employed after birth than ineligible mothers, and, as a consequence, their income was higher during that period. Their income 2 years before giving birth was nine times higher than that of ineligible mothers, presumably because many in the latter group did not work. Children of eligible mothers had lower high school dropout rates and higher college attendance rates, but similar earnings at age 30, compared with the children of ineligible mothers. In summary, eligible and ineligible mothers and their children were 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 was quite high, even for those mothers who gave birth before the reform was implemented. For our preferred measure, average unpaid leave was 8 months for those who gave birth before July 1977 according to our estimates, and it barely changed for those who gave birth after this date. The 25th percentile was about 2 months, and the 75th percentile was about 11 months. Any expansion in the time mothers spent with their newborns resulting from the reform was in addition to this preexisting level of leave.

Before proceeding to the results, we would like to check whether the treatment and control groups were balanced in terms of their (prereform) characteristics. An imbalance could indicate a threat to the validity of our method, suggesting the possibility that a nonrandom set of mothers manipulated the date of birth of their children (see Gans and Leigh 2009). The various panels of figure 3 show how the observable prereform characteristics of mothers varied with the day on which they gave birth, allowing us to check whether they were identical for mothers who gave birth immediately before and immediately after the reform. Maternal years of edu-
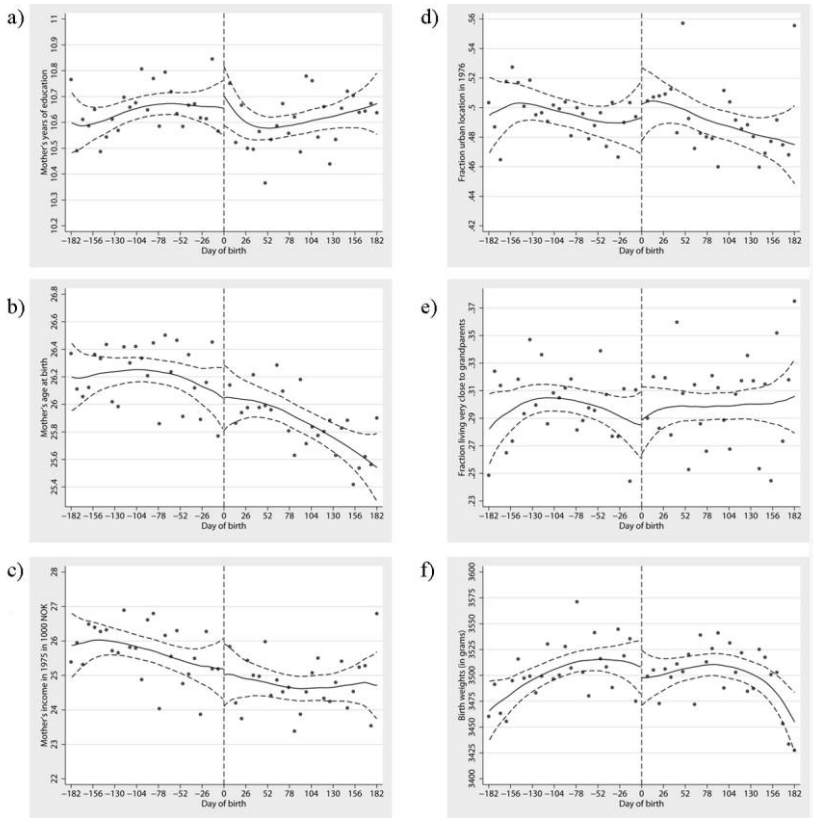

Fig. 3.—Mothers' preform characteristics in the vicinity of the reform date. *a*, Mothers' years of education. *b*, Mothers' age at time of childbirth. *c*, Mothers' income in 1975. *d*, Parents' urban location in 1976. *e*, Distance to grandparents in 1980. *f*, Birth weight of child, in grams. Each data point corresponds to the average value of each outcome, organized according to date of birth (in 1-week bins). Dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). The solid line represents fitted values for a local linear regression with a bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on either side of the discontinuity). The dashed lines mark the 95 percent confidence interval.

cation, age at birth, and income in 1975 were stable across birth months, and we see no discontinuity after July 1, 1977. In addition, there was no discontinuity regarding the urban location of the parents in 1976, the distance to grandparents in 1980 (although this variable is available only for 1980), or the birth weight of the child.

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 immediately before and immediately after the reform was implemented. In sum, selective manipulation of the 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.

B. 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

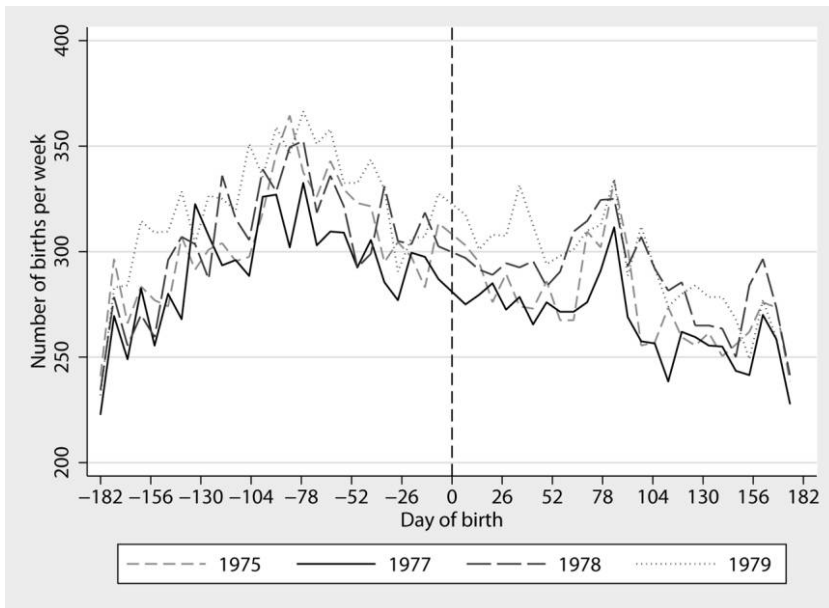FIG. 4.—The number of children born to eligible mothers in 1975, 1977, 1978, and 1979, by week of birth. The dashed vertical line denotes the reform cutoff of July 1, 1977 (normalized to zero). The window includes all children born in 1975, 1977, 1978, and 1979 to eligible mothers (182 days on either side of the discontinuity). The different lines plot the average number of births in 1-week intervals for each year separately.

TABLE 3
IMPACT OF THE REFORM ON CHILDREN'S OUTCOMES

				VARIABLE			
ESTIMATE	Dropout Rate (1)	College Attendance (2)	Ln(Earnings) Age 30 (3)	Completed Years of Schooling (4)	Teenage Pregnancy (Females) (5)	IQ (Males) (6)	Birth Weight (7)
RD	−.022*	.027	.062***	.152	.004	.200**	−10.54
	(.012)	(.019)	(.028)	(.093)	(.013)	(.092)	(22.0)
	[.18]	[.47]	[12.6]	[13.0]	[.054]	[5.4]	[3,518]
Observations	15,025	15,025	14,348	15,025	7,194	6,838	14,979
RD-DD	−.019**	.020*	.050***	.116***	−.001	.084	.429
	(.007)	(.011)	(.016)	(.053)	(.007)	(.054)	(12.8)
	[.19]	[.44]	[12.5]	[12.8]	[.051]	[5.4]	[3,505]
Observations	63,571	63,571	60,732	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD estimates used only eligible births in 1977, whereas the RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).

* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

cohorts born in 1975, 1978, and 1979 as a control group. In column 1, we see a negative effect of the reform of about −2 percentage points on children's dropout rates, which is statistically significant at the 10 percent 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—namely, an increase of around 2 percentage points—but this is statistically significant only 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 percent as estimated by RD, which decreases to 5 percent in the DD specification.[18]

In table 4 we use different control groups. The first line uses as a control group children born to ineligible mothers. The second line presents a triple-difference estimator whereby we take differences across eligibility statuses (eligible vs. ineligible) and across years (1977 vs. 1975, 1978, 1979). Overall, we find the same results, although they are slightly less precise, when we use only ineligible mothers who gave birth in 1977 as the control group.

Table 5 reports 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 the coefficient for log earnings at age 30 is statistically different from zero. This is expected because 35 percent of all mothers were not affected by the reform, so when the whole sample is used, the estimated impact of the reform will be diluted. The reason we perform this check is that 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 had no impact on our results).

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, however, is not as clear.[19]

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[18] Interestingly, in table 3, there is also a positive effect on IQ, although it is statistically significant only in the RD specification. IQ scores are available only for men, but because of the large sample sizes, we can still get precise estimates of the effect of the reform on IQ. The RD estimates show an effect of 0.2, or 9 percent of a standard deviation. When estimates of the effect of IQ on wages from the wage regressions for slightly older cohorts of individuals are used, this translates into more than a 1 percent difference in earnings as an adult. We do not see any effect of the reform on teenage pregnancy or on birth weight in any of the specifications. We would expect the effect on birth weight to be zero if our empirical strategy is valid as birth weight is predetermined by changes in the mother's time at home.

[19] There are also less clear patterns for years of schooling and IQ and no discontinuity in teenage pregnancy.

TABLE 4

Children's Outcomes: Alternative Nonparametric DD Specifications

	Variable		
Estimate	Dropout Rate (1)	College Attendance (2)	Ln(Earnings) Age 30 (3)
RD-DD ineligibles	−.016	.009	.054**
	(.012)	(.016)	(.026)
	[.21]	[.43]	[12.5]
Observations	23,658	23,658	22,523
RD-DD years and ineligibles	−.016**	.006	.043**
	(.008)	(.012)	(.018)
	[.22]	[.40]	[12.5]
Observations	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD estimates used only eligible births in 1977, whereas the RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).

* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

TABLE 5

Impact of the Reform on Children's Outcomes for Both Eligible and Ineligible Mothers

	Variable		
Estimate: Children	Dropout Rate (1)	College Attendance (2)	Ln(Earnings) Age 30 (3)
RD-DD years (1975, 1978, and 1979)	−.011	.011	.026**
	(.007)	(.009)	(.013)
	[.22]	[.40]	[12.5]
Observations	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).

* Significant at 10 percent.
** Significant at 5 percent.
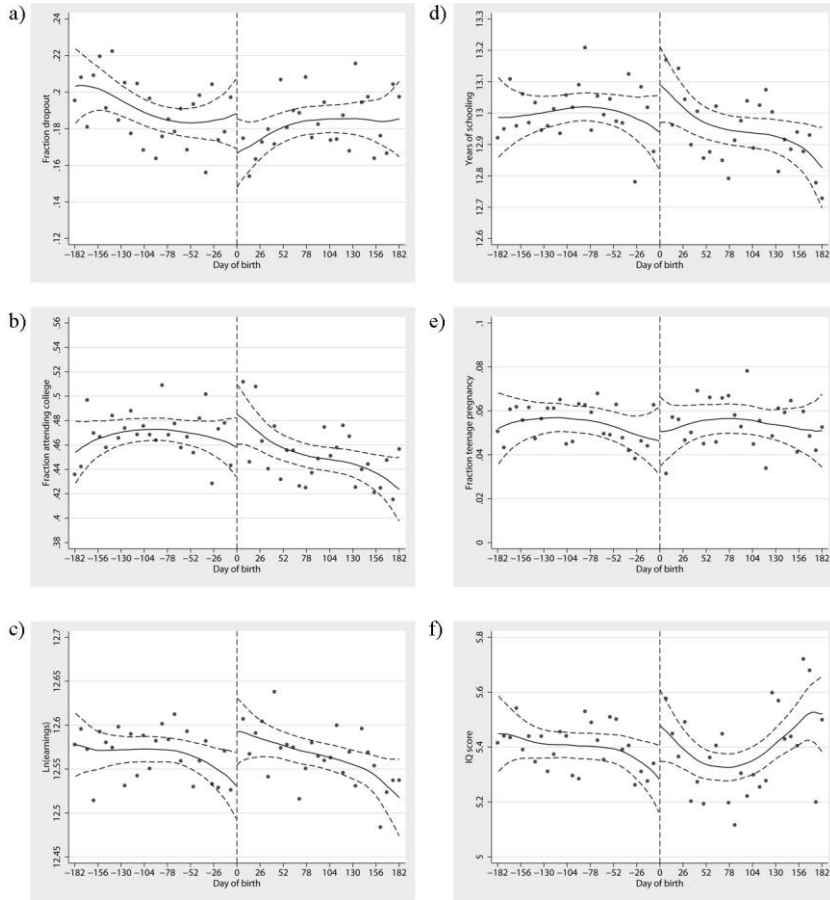*** Significant at 1 percent.

FIG. 5.—Impact of the reform on children's outcomes. *a*, High school dropout rates. *b*, College attendance. *c*, Ln(earnings) at age 30. *d*, Years of schooling. *e*, Teenage pregnancy. *f*, IQ score. Each data point corresponds to the average value of each outcome organized according to date of birth (in 1-week bins). The dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). The solid line represents fitted values for a local linear regression with a bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on either side of the discontinuity). The dashed lines mark the 95 percent confidence interval.

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 labor market. Figure 6 shows the estimates for the
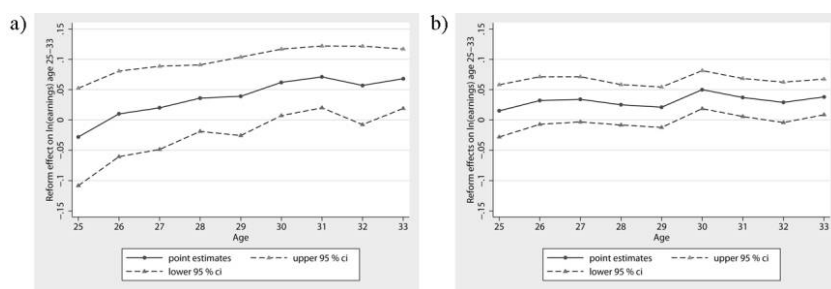
FIG. 6.—Effects of the reform on children's log earnings at ages 25–33. *a*, Effects on children's log earnings at ages 25–33: RD estimates. *b*, Effects on children's log earnings at ages 25–33: RD-DD estimates. The lines labeled "point estimates" are the reform effects calculated using the RD (panel *a*) and RD-DD (panel *b*) models on log earnings. The dashed lines mark the 95 percent confidence intervals. The *x*-axes show the ages of the children, ranging from 25 to 33 years.

whole earnings profile from ages 25 to 33. Notice how the impact of the reform on earnings becomes significant only after age 30 and then remains stable up to age 33. It is also noteworthy that we found a positive and statistically significant impact of the reform on the present value of the child's earnings (between ages 25 and 33).

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

The point estimates were not very sensitive to the choice of bandwidth, but as expected, the RD results were 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.

We next present the sensitivity of the results in table 3 to observations in the vicinity of the discontinuity. This is important because of the potential of strategic behavior, 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 1, 2, 3, 4, 5, and 6 weeks of the date of the reform (before and after) and reestimating the model using the remaining sample (Barreca et al. 2012). The results of this exercise are shown in figures 8*A* (dropout rates), 8*B* (college attendance), and 8*C* (log earnings at age 30) for both the RD and the RD-DD specifications used in tables 3
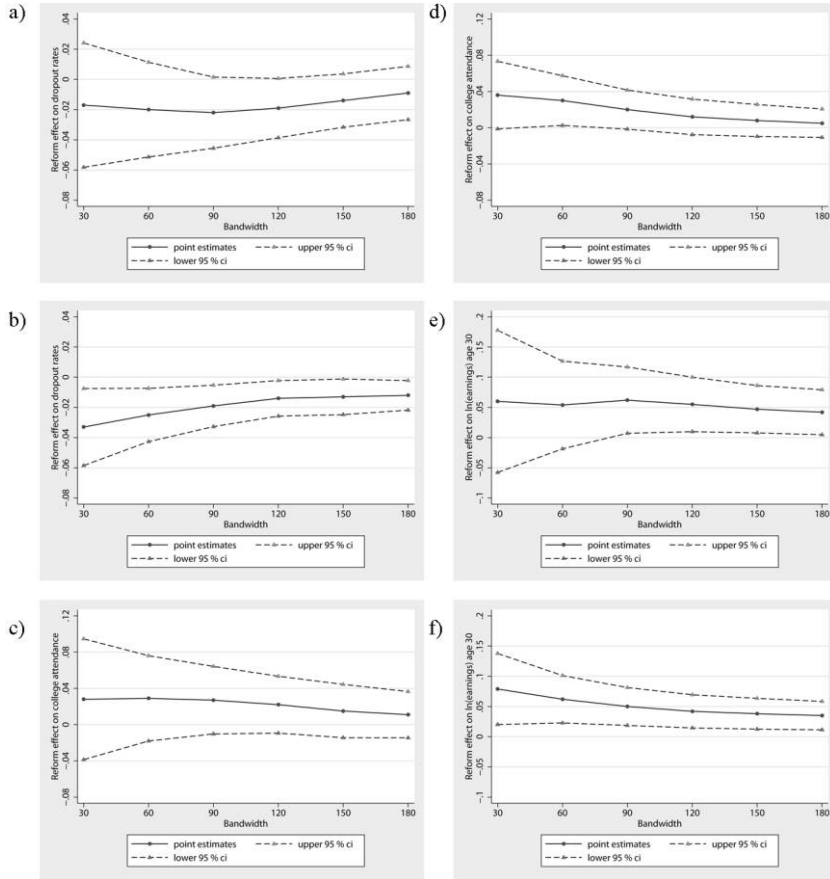
Fig. 7.—The effect of bandwidth on the estimated impacts on dropout rates, college attendance, and log earnings age 30. *a*, Dropout rates: RD. *b*, Dropout rates: RD-DD. *c*, College attendance: RD. *d*, College attendance: RD-DD. *e*, Ln(earnings) age 30: RD. *f*, Ln(earnings) age 30: RD-DD. The lines labeled "point estimates" show the reform effects on dropout rates (panels *a* and *b*), college attendance (panels *c* and *d*), and log earnings at age 30 (panels *e* and *f*). The dashed lines mark the 95 percent confidence intervals. On the *x*-axis are different bandwidths, ranging from 30 to 180 days.

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 comput-ing each estimate. On the vertical axis, we show the size of each estimate. The graphs on the left of each figure show the RD estimates, and those on the right show the RD-DD estimates.

The RD results were robust to small changes in the sample. However, they were sensitive to very large changes in the sample. This is expected
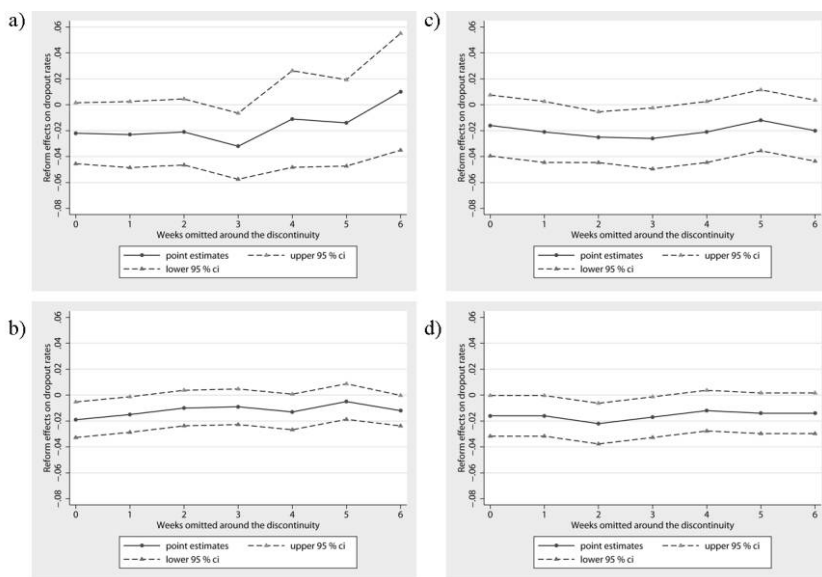
F<small>IG</small>. 8*A*.—Robustness to changes in window length: dropout rates. *a*, Dropout rates: RD. *b*, Dropout rates: RD-DD using nonreform years as controls. *c*, Dropout rates: RD-DD using ineligible mothers as controls. *d*, Dropout rates: RD-DD using ineligible mothers and nonreform years as controls. The lines labeled "point estimates" are the reform effects on dropout rates calculated using the RD (panel *a*) and RD-DD (panel *b*) models, RD-DD ineligible (panel *c*) model, and RD-DD years and ineligibles (panel *d*) model. The dashed lines mark the 95 percent confidence intervals. On the *x*-axes are the number of weeks omitted around the discontinuity. The length of the window is kept constant. The baseline we use in all regressions is 0 weeks omitted around the discontinuity.

because the RD design makes sense only when we use observations in the close vicinity of the date of the reform. Once we delete observations from the sample births close to that date, we can no longer apply this method.

However, the DD estimates would still be valid because they compared children born to eligible mothers immediately before and immediately after the reform date in 1977, relative to a control group, which could be the group of children born to eligible mothers in adjacent nonreform years, or the group of children born to ineligible mothers (or a combination of both). Figures 8*A* and 8*C* show that our estimates of the impact of the reform on high school dropout rates and log earnings at age 30 were 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 4–6 weeks of the date of the reform). Figure 8*B* shows that the estimated impacts of the reform on college attendance were statistically indistinguishable
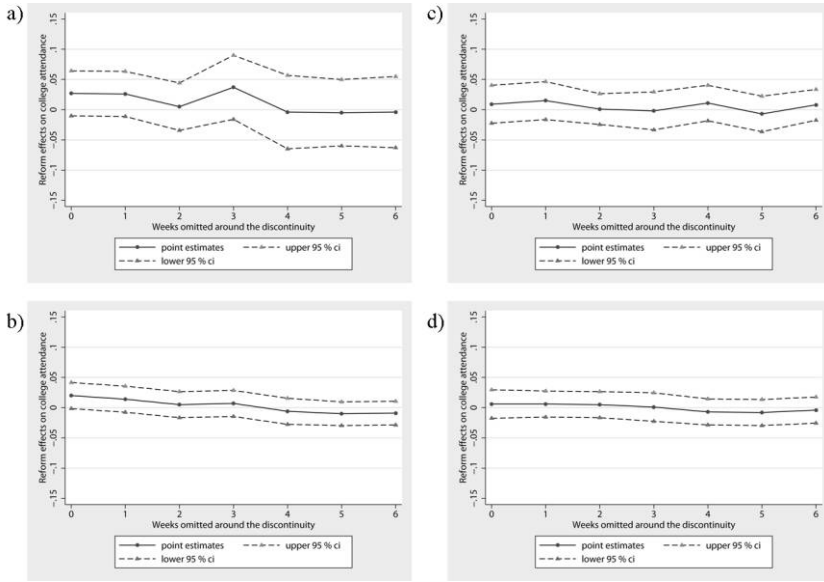
Fig. 8B.—Robustness to changes in window length: ln(earnings) age 30. *a*, Ln(earnings) at age 30: RD. *b*, Ln(earnings) at age 30: RD-DD using nonreform years as controls. *c*, Ln(earnings) at age 30: RD-DD using ineligible mothers as controls. *d*, Ln(earnings) at age 30: RD-DD using ineligible mothers and nonreform years as controls. The lines labeled "point estimates" are the reform effects on dropout rates calculated using the RD (panel *a*) and RD-DD (panel *b*) models, RD-DD ineligible (panel *c*) model, and RD-DD years and ineligibles (panel *d*) model. The dashed lines mark the 95 percent confidence intervals. On the *x*-axes are the number of weeks omitted around the discontinuity. The length of the window is kept constant. The baseline we use in all regressions is 0 weeks omitted around the discontinuity.

from zero once we moved away from the immediate vicinity of the reform date.

Table 6 estimates the impacts of being born immediately after July 1 using births in all the control years (1975, 1978, 1979) and births from ineligible mothers. If our strategy is valid, these coefficients should be zero. There is only one coefficient in this table that 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 for 1977.

VII. Interpretation of Empirical Results and Suggested Mechanisms

In the previous section, we established that the maternity leave reform had a substantial impact on the schooling and earnings of children. In this section, we attempt to understand the mechanisms through which this happened, using the limited information from the administrative
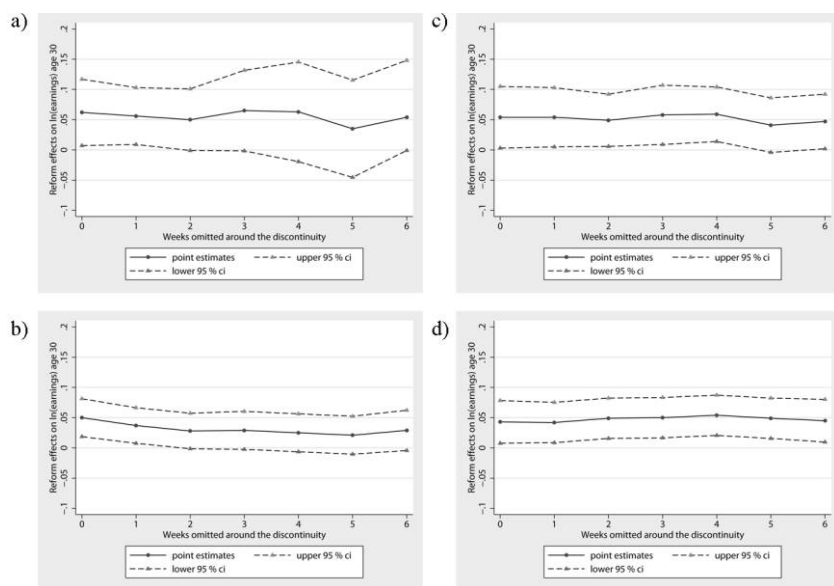
FIG. 8C.—Robustness to changes in window length: college attendance. *a*, College attendance: RD. *b*, College attendance: RD-DD using nonreform years as controls. *c*, College attendance: RD-DD using ineligible mothers as controls. *d*, College attendance: RD-DD using ineligible mothers and nonreform years as controls. The lines labeled "point estimates" are the reform effects on dropout rates calculated using the RD (panel *a*) and RD-DD (panel *b*) models, RD-DD ineligible (panel *c*) model, and RD-DD years and ineligibles (panel *d*) model. The dashed lines mark the 95 percent confidence intervals. On the *x*-axes are the number of weeks omitted around the discontinuity. The length of the window is kept constant. The baseline we use in all regressions is 0 weeks omitted around the discontinuity.

records we used. The results we present in this section are not individually decisive, but together they tell a consistent story.

A. Time with the Child

The main problem of our data set is that it does not have a direct measure of maternal employment or leave-taking behavior.[20] So how can we be confident that the reform significantly affected the leave-taking behavior of mothers?

First, Rønsen and Sundström (1996) show that for the mothers who gave birth to children in Norway in the period 1968–88, almost no one returned to work within 4 months of giving birth. Second, in a survey conducted in 1977 on the fertility behavior of women in Norway (by Statistics Norway), 60 percent of respondents thought that mothers

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[20] This is a typical problem when studying reforms that happened a long time ago. The main advantage of going back in time is the ability to measure the long-term outcomes of children.

TABLE 6
IMPACT OF THE REFORM ON CONTROL GROUPS

	VARIABLE		
ESTIMATE	Dropout Rate (1)	College Attendance (2)	Ln(Earnings) Age 30 (3)
RD 1975	.012	−.019	−.018
	(.012)	(.017)	(.021)
	[.20]	[.47]	[12.4]
Observations	15,818	15,818	15,140
RD 1978	.018	−.019	−.004
	(.013)	(.016)	(.021)
	[.17]	[.43]	[12.5]
Observations	16,053	16,053	15,325
RD 1979	.016	−.026	−.055*
	(.012)	(.016)	(.033)
	[.20]	[.42]	[12.5]
Observations	16,675	16,675	15,919
Ineligible in 1977	−.022	.020	.024
	(.023)	(.025)	(.040)
	[.27]	[.35]	[12.5]
Observations	8,633	8,633	8,175

NOTE.—Each cell presents the estimated discontinuity in the outcomes as a result of the placebo maternity leave reforms on July 1, 1975 (first row), 1978 (second row), 1979 (third row), and 1977 (fourth row). We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD estimates for the first three rows used only eligible births in 1975, 1978, and 1979, respectively (182 days on either side of the discontinuity). The RD estimates for the last row used only ineligible births in 1977 (182 days on either side of the discontinuity).

* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

should stay home for the first 2 years after giving birth. In addition, the reform provided mothers with their full prebirth salary for 4 months, which represents a strong incentive for full take-up.

Third, because we could directly measure the days of paid leave from 1992 onward, we were able to check to what extent eligible mothers took up (subsequent) maternity leave benefits by studying the 1992 and 1993 reforms (see fig. 9). Before the April 1992 reform, mothers were able to take 224 days of leave at full coverage or 280 days at 80 percent coverage. For mothers who gave birth to children in March 1992, the average take-up of paid leave was 250 days (which is close to the average of 224 and 280). After 1992, there was an increase in maternity leave entitlements to 245 days of full coverage or 310 days of 80 percent coverage. We observe that the average paid leave taken was 275 days for mothers who gave birth in April 1992. This figure is slightly higher, at 280, in March
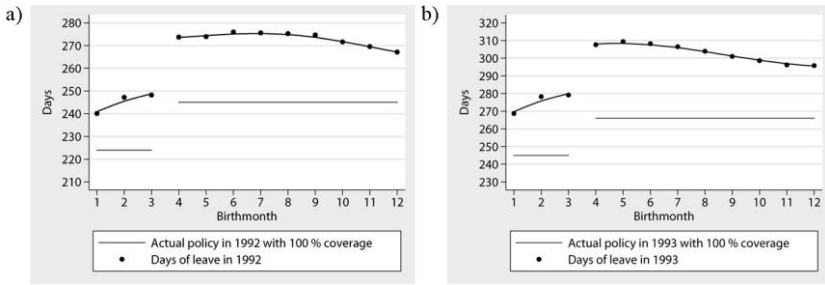
F<small>IG</small>. 9.—Impacts of reforms in 1992 and 1993 on mothers' days of paid leave. *a*, Days of leave in 1992. *b*, Days of leave in 1993. Each observation is the average outcome in 1-month bins based on the birth month of the child. The reforms were on April 1, 1992, and April 1, 1993. The solid lines are fitted triangular local linear regressions with a bandwidth of 3 months. The window includes all children born in 1992 (panel *a*) and 1993 (panel *b*) to eligible mothers. The gray lines are the actual leave entitlements specified in the policies in 1992 and 1993.

1993, immediately before the 1993 reform, which increased the paid leave to 266 days of full coverage or 336 days of 80 percent coverage. By April 1993, the average leave taken was almost 310 days. Given the high levels of leave take-up and strong reactions to these reforms in the 1990s, it is reasonable to assume that the take-up of paid leave was close to 100 percent in the sample we are using.[21]

Therefore, we are confident that after the 1977 reform, all mothers were taking 4 months of paid leave. A natural follow-up question is, Was there a change in unpaid leave take-up as a result of the reform? The best way to answer this question in our data set is by studying what happened to maternal income, which included maternity benefits, before and after the reform (because we do not directly observe the days of leave taken).[22] An increase in maternal income in the period immediately after the birth may indicate a reduction in the 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 who gave birth to children immediately after and immediately before the reform, and we found no impact of the reform on these variables. This is shown in table 7, which indicates that there was no change in the unpaid leave taken by mothers. This result holds independently

[21] We should also point out that the analyses of other reforms in other countries for which there are data available on maternal employment all indicate a substantial increase in the amount of leave taken after each reform.

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

TABLE 7
IMPACT OF THE REFORM ON MOTHERS' INCOMES AROUND THE TIME OF CHILDBIRTH

	VARIABLE						
	Log Income						
ESTIMATE	In Year of Childbirth (1)	1 Year before and 1 Year after Childbirth (2)	2 Years before and 2 Years after Childbirth (3)	Predicted Months of Unpaid Leave (4)	Employed 2 Years after Birth (5)	Employed 5 Years after Birth (6)	Ln(Income) 5 Years after Birth (7)
RD	.070	.027	.019	−.275	−.012	.005	.107
	(.108)	(.034)	(.033)	(.230)	(.015)	(.016)	(.182)
	[8.4]	[9.8]	[9.8]	[7.7]	[.74]	[.76]	[8.4]
Observations	15,025	15,025	15,025	15,025	15,025	15,025	15,025
RD-DD years (1975, 1978, and 1979)	−.032	−.009	−.007	.002	−.016*	−.009	−.100
	(.070)	(.021)	(.021)	(.160)	(.010)	(.009)	(.103)
	[9.0]	[10.2]	[10.3]	[7.6]	[.72]	[.76]	[8.3]
Observations	63,571	63,571	63,571	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD estimates used only eligible births in 1977, whereas the RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).

* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

of the measure of earnings we use: income in 1977, average income between 1976 and 1978, or average income between 1975 and 1979. This is true not only of the mean but also of the whole distribution of income.

In addition, as discussed above, with these data, it is possible to predict how much unpaid leave was taken by each mother by comparing her usual earnings in a year before childbirth to her earnings in a year (and subsequent years) after 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 because we emphasized above that there was no change in the average annual income of mothers who gave birth immediately before and immediately after the date of the reform, independent of the measure of earnings we use.

In summary, this means that, whatever the measure of unpaid leave we use, we can find no change in the amount of unpaid leave taken by mothers who gave birth immediately before versus immediately after the reform; otherwise, there would have been an increase in their income. Therefore, even if our measure of unpaid leave is imperfect, we can be confident that there was no large change in unpaid leave levels as a result of the reform. Even with no average response in unpaid leave, it is interesting to see whether there were any effects on the distribution of unpaid leave. In figure 10, we see no such response. We cannot rule out the possibility that not all mothers took the full 4 months of paid leave, although the earlier evidence provided shows that this was likely to be the case (Statistics Norway, fertility survey of 1977).

Notice that, even if the reform led to no change in family resources during the initial period of the child's life, it induced a slight change in the timing of these resources. Paid leave allows mothers to receive benefits immediately after their child is born, whereas unpaid leave does not. However, it is not likely that this change in the timing of benefits dramatically affects child outcomes unless we consider the 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 prereform income. Poorer mothers were more likely to be credit constrained, so our idea was to use prereform income as an indicator of the severity of such constraints.

B.   Maternal Labor Market Outcomes

It is possible that the reform increased the labor market attachment of mothers. The reason is 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 postreform period had better outcomes not only because they spent more time with their mothers during their first year of their life, but also because their mothers became more attached to the labor mar-
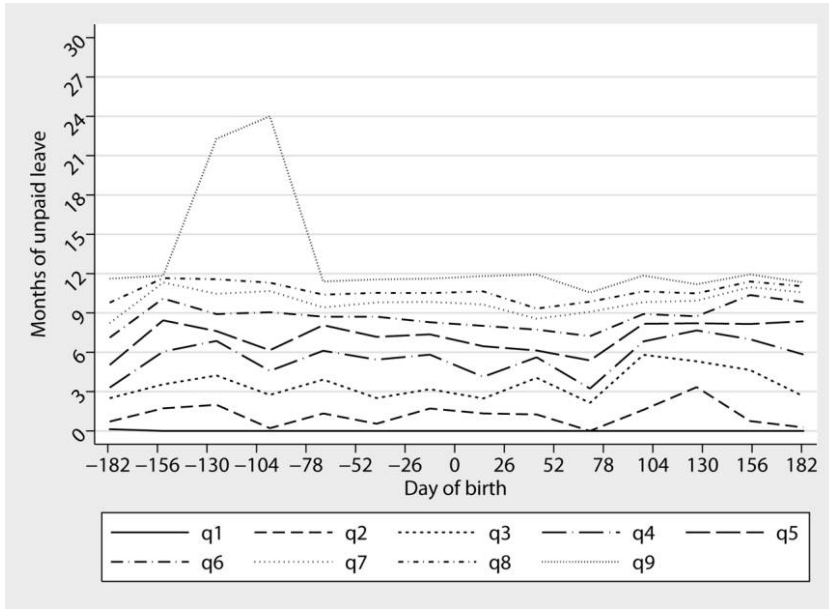
Fig. 10.—Impacts of the reform on quantiles of predicted months of unpaid leave. The reform cutoff of July 1, 1977, is normalized to zero. The window includes all children born in 1977 to eligible mothers (182 days on either side of the discontinuity). The different lines plot the average number of months of unpaid leave in 1-week intervals for each quantile of predicted unpaid leave taken by the mother (1–9) separately.

ket in the medium and long run, thereby becoming 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 mothers' employment 2 and 5 years after the reform or on their earnings 5 years after.[23] This supports the idea that our estimates of the impact of the reform on children's outcomes can be directly related to mothers' time investments in children during the first year of life.

In figure 11, we present the results of table 7 graphically. The figure confirms the results of the table. There is no discontinuity in long-term labor market outcomes.

C. Maternal Education

In this section we examine whether the impacts of the maternity leave extension varied with the level of education of the mother.[24] We split

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[23] We have also examined mothers' earnings between 1 and 10 years after birth, and this gives similar results, showing no long-term effect of the reform on income.

[24] In table 10 below, we present the results according to distance to grandparents and centralization (urban vs. rural).

Fig. 11.—Impacts of the reform on mothers' outcomes. *a*, Predicted months of unpaid leave. *b*, Log income in the years before and after childbirth. *c*, Employed 2 years after childbirth. *d*, Employed 5 years after childbirth. *e*, Log income 5 years after childbirth. Each observation is the average outcome in 1-week bins based on the birth date of the child. The dashed vertical lines denote the reform cutoff of July 1, 1977 (normalized to zero). The solid lines are fitted triangular local linear regressions with a bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on either side of the discontinuity). The dashed lines mark the 95 percent confidence intervals.

the sample into two groups: mothers with less than 10 years of education and mothers with 10 years or more of education. The results are shown in table 8.

We see that the fall in the dropout rate resulting from the reform was 3.6 percent for the children of mothers with less than 10 years of education, whereas it was only 1.8 percent for the children of mothers with 10 years of education or more. When we look at college attendance, we also see a stronger impact for children born to mothers with lower education. Interestingly, the pattern is the opposite for earnings at age 30.

TABLE 8
CHILDREN'S AND MOTHERS' OUTCOMES, BY MOTHERS' EDUCATION:
NONPARAMETRIC RD-DD REGRESSIONS

	MOTHERS' EDUCATION SUBGROUP	
VARIABLE	Less than 10 Years (1)	10 Years or More (2)
Children:		
Dropout rate	−.036**	−.018*
	(.015)	(.010)
	[.28]	[.14]
Observations	21,219	41,430
College attendance	.030*	.020
	(.018)	(.013)
	[.30]	[.52]
Observations	21,219	41,430
Ln(earnings) at age 30	.042	.057***
	(.027)	(.020)
	[12.4]	[12.5]
Observations	20,269	39,602
Mothers:		
Predicted months of unpaid leave	−.167	−.004
	(.314)	(.184)
	[12.4]	[6.8]
Observations	21,219	41,430
Employed 2 years after childbirth	−.015	−.013
	(.017)	(.012)
	[.64]	[.77]
Observations	21,219	41,430
Employed 5 years after childbirth	.003	−.015
	(.017)	(.010)
	[.70]	[.79]
Observations	21,219	41,430
Ln(income) 5 years after childbirth	.066	−.184
	(.188)	(.116)
	[7.5]	[8.8]
Observations	21,219	41,430

NOTE.—Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity). Column 1 shows the results when mothers had less than 10 years of education. Column 2 shows the results when mothers had 10 years or more of education. We have missing information about the mother's education for 922 observations.

 * Significant at 10 percent.
 ** Significant at 5 percent.
 *** Significant at 1 percent.

However, we cannot statistically reject the null hypothesis that the impacts of the maternity leave reform do not vary with the education of the mother.

D.   Results by Quartiles of Mothers' 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 had 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 have taken 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 2 years after the birth of the child, which declined slightly at both extremes of the unpaid leave distribution. This indicates a substantial increase in mothers' time spent at home across the distribution of eligible mothers (because paid leave increased for all of them).

For children, we see that the effect on dropout rates was very large at the first and second quartiles of unpaid leave (5 percent and 2 percent, respectively), while we see no effect at the third and fourth quartiles. This is also confirmed by the earnings results, which showed that the reform led to 7 percent higher earnings at the first and second quartiles but had no effect at the top two quartiles.

Mothers in the first two quartiles had levels of unpaid leave well below the average (0.4 and 5.1 months, respectively). We see the largest effects on dropout rates and earnings for these mothers (the outcomes that are the most robust among our results), which suggests that additional time with the child was the most important factor during the earliest months of the child's life. It is possible that these differences were not entirely a result of increases in health (say, because of breast-feeding; see the evidence discussed in the Appendix, which uses time-series data to suggest that there was no detectable impact of the reform on breast-feeding). There may have been an impact on mother-child attachment and a reduction in stress in the home, leading to changes in personality traits that made these children less likely to drop out of high school.

E.   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 was higher for children born in rural areas and for those who grew up liv-

TABLE 9
CHILDREN'S AND MOTHERS' OUTCOMES, BY QUARTILES OF MOTHERS' MONTHS
OF UNPAID LEAVE: NONPARAMETRIC RD-DD REGRESSIONS

| | QUARTILE OF MOTHERS' MONTHS OF UNPAID LEAVE | | | |
| | Lowest | | | Highest |
VARIABLE	(1)	(2)	(3)	(4)
Average level of unpaid leave (SD)	.40	4.64	8.55	16.68
	(.63)	(1.39)	(.80)	(9.8)
Children:				
Dropout rate	−.053***	−.028*	.010	−.006
	(.015)	(.016)	(.016)	(.017)
	[.17]	[.17]	[.19]	[.24]
Observations	15,893	15,893	15,892	15,893
College attendance	.032	.007	.008	.033
	(.025)	(.021)	(.021)	(.022)
	[.50]	[.48]	[.43]	[.37]
Observations	15,893	15,893	15,892	15,893
Ln(earnings) at age 30	.074**	.077**	.035	.014
	(.032)	(.034)	(.031)	(.031)
	[12.5]	[12.5]	[12.5]	[12.5]
Observations	15,183	15,197	15,234	15,118
Mothers:				
Employed 2 years after childbirth	−.012*	−.000	−.007	−.035*
	(.007)	(.011)	(.021)	(.020)
	[.97]	[.92]	[.62]	[.39]
Observations	15,893	15,893	15,892	15,893
Employed 5 years after childbirth	.011	−.021	−.022	−.000
	(.013)	(.015)	(.020)	(.020)
	[.91]	[.86]	[.68]	[.58]
Observations	15,893	15,893	15,892	15,893
Ln(income) 5 years after childbirth	.203	−.308*	−.274	.010
	(.154)	(.172)	(.214)	(.219)
	[10.5]	[9.6]	[7.3]	[6.0]
Observations	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbes in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity). Column 1 shows the results for the lowest quartile of mothers' months of unpaid leave; col. 2, the second quartile; col. 3, the third quartile; and col. 4, the highest quartile. The first row shows the average levels of unpaid leave in each quartile.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

ing close to their grandparents. By contrast, the impact on earnings was larger for those living in urban areas and for those who grew up far from their grandparents. In theory, these effects could have worked in either direction. One would expect mothers living in rural areas to be poorer, but it is also likely that far fewer of them were eligible for the reform.

TABLE 10
CHILDREN'S AND MOTHERS' OUTCOMES, BY URBANIZATION AND DISTANCE
TO GRANDPARENTS: NONPARAMETRIC RD-DD REGRESSIONS

VARIABLE	LOCALIZATION SUBGROUP		DISTANCE TO GRANDPARENTS SUBGROUP	
	Urban (1)	Rural (2)	Close (3)	Far (4)
Children:				
Dropout rate	−.016	−.021*	−.037**	.001
	(.011)	(.011)	(.017)	(.010)
	[.19]	[.19]	[.19]	[.19]
Observations	31,569	32,002	15,945	36,912
College attendance	.016	.023	.021	.017
	(.015)	(.016)	(.023)	(.015)
	[.45]	[.44]	[.42]	[.44]
Observations	31,569	32,002	15,945	36,912
Ln(earnings) at age 30	.065***	.035	.047	.053**
	(.023)	(.024)	(.029)	(.021)
	[12.5]	[12.5]	[12.5]	[12.5]
Observations	30,034	30,698	15,375	35,189
Mothers:				
Predicted months of unpaid leave	.097	−.076	.359	−.013
	(.217)	(.225)	(.328)	(.200)
	[7.4]	[7.9]	[8.1]	[7.6]
Observations	31,569	32,002	15,945	36,912
Employed 2 years after childbirth	−.018	−.015	−.005	−.021*
	(.014)	(.013)	(.020)	(.012)
	[.73]	[.71]	[.70]	[.72]
Observations	31,569	32,002	15,945	36,912
Employed 5 years after childbirth	−.016	−.001	−.007	−.009
	(.013)	(.013)	(.018)	(.012)
	[.76]	[.76]	[.74]	[.75]
Observations	31,569	32,002	15,945	36,912
Ln(income) 5 years after childbirth	−.160	−.050	−.064	−.115
	(.143)	(.148)	(.203)	(.133)
	[8.4]	[8.3]	[8.0]	[8.2]
Observations	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity). Columns 1 and 2 show the results when parents were from an urban or rural area, respectively. Columns 3 and 4 show the results for children who lived close to or far from their grandparents, respectively. We have missing information about the distance to grandparents for 10,714 observations.

* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

Thus, there is probably a differential selection into eligibility among mothers in rural and urban areas. Similarly, living close to grandparents could imply that low-cost child care was more easily available, but the quality of care provided by the grandparents could have been higher or lower than the alternative care children received when they lived far from their grandparents.

The reform had different effects according to prereform family income and the state of the local labor market at the time of childbirth. Table 11 shows the results according to quartiles of family income. In contrast to maternal education, these are relatively short-term measures of the household environment. Most of the impacts of the reform on child outcomes were still stronger for children born to mothers whose prebirth income was below the median. Interestingly, the reform had a relatively strong and negative impact on the labor market outcomes of mothers in the upper part of the prebirth income distribution, although this did not correspond to child outcomes.

We mentioned above that the reform could also have had an effect by shifting the availability of income toward those months immediately after childbirth, even if there was no change in total income. If some households were severely credit constrained, this may have made a difference to the child. Although it is unlikely that severe credit constraints of this sort were important in Norway in this period, our results are consistent with this interpretation if we presume that those with low levels of prereform income were the most likely to be credit constrained. There are, of course, other explanations that do not require credit constraints specifically but are associated only with poverty.

In addition, we found that the completed fertility and marital stability of mothers had no mediating effect on the reform's impact on children (see table 12).

We also analyzed the impact of the reform on older siblings (see table 13). The fact that mothers spent additional time in the home could have benefited siblings. However, this was not found to be the case, which suggests that what determined the impact of the reform was 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 was determined mainly by females, while the impact of the reform on earnings at age 30 was determined by males (see table 14).

Table 15 compares the impacts of the reform for firstborn versus later-born children. It shows that for education and earnings, the impact was stronger for later-born children. This is consistent with the idea that time with the mother is important because later-born children would probably face more competition for maternal time than firstborn children

TABLE 11

Children's and Mothers' Outcomes, by Quartiles of Log Family Income
2 Years Prior to Birth: Nonparametric RD-DD Regressions

| | Quartiles of Ln(Family Income) 2 Years Prior to Birth | | | |
| | Lowest | | | Highest |
Variable	(1)	(2)	(3)	(4)
Children:				
Dropout rate	−.030*	−.020	−.015	−.007
	(.016)	(.016)	(.015)	(.014)
	[.24]	[.21]	[.18]	[.14]
Observations	15,893	15,893	15,892	15,893
College attendance	.033	.038*	−.021	.028
	(.024)	(.022)	(.021)	(.022)
	[.41]	[.42]	[.45]	[.50]
Observations	15,893	15,893	15,892	15,893
Ln(earnings) at age 30	.041	.081**	.025	.042
	(.036)	(.033)	(.033)	(.035)
	[12.4]	[12.5]	[12.5]	[12.5]
Observations	15,090	15,227	15,248	15,167
Mothers:				
Predicted months of unpaid leave	.054	−.095	.001	.176
	(.391)	(.334)	(.327)	(.316)
	[8.4]	[8.1]	[7.4]	[6.6]
Observations	15,893	15,893	15,892	15,893
Employed 2 years after childbirth	.015	−.017	−.052***	−.011
	(.019)	(.019)	(.018)	(.018)
	[.69]	[.70]	[.73]	[.77]
Observations	15,893	15,893	15,892	15,893
Employed 5 years after childbirth	.006	.019	−.030	−.037**
	(.017)	(.017)	(.018)	(.018)
	[.73]	[.75]	[.77]	[.79]
Observations	15,893	15,893	15,892	15,893
Ln(income) 5 years after childbirth	.007	.251	−.346*	−.416**
	(.189)	(.182)	(.207)	(.211)
	[7.8]	[8.1]	[8.5]	[8.9]
Observations	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity). Column 1 shows the results for the lowest quartile of family income; col. 2, the second quartile; col. 3, the third quartile; and col. 4, the highest quartile.
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

TABLE 12
MOTHERS' PART-TIME WORK IN 1980, COMPLETED FERTILITY
(Number of Children in 2007), AND MARITAL STABILITY IN 2007:
NONPARAMETRIC RD AND RD-DD REGRESSIONS

| | VARIABLE | | |
| | Working Part-Time in 1980 | Completed Fertility in 2007 | Married in 2007 |
ESTIMATE	(1)	(2)	(3)
RD	−.013	−.027	−.008
	(.017)	(.032)	(.017)
	[.43]	[2.53]	[.73]
Observations	15,036	15,025	15,025
RD-DD years	−.003	−.011	−.000
(1975, 1978, and 1979)			
	(.011)	(.021)	(.009)
	[.42]	[2.54]	[.73]
Observations	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD estimates used only eligible births in 1977, whereas the RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).

 * Significant at 10 percent.
 ** Significant at 5 percent.
 *** Significant at 1 percent.

would, and they would therefore benefit more from a relaxation of the time constraint.

VIII.   Concluding Remarks

We investigated the long-term consequences of mothers' time investment in their children during their first year of life. We explored empirically the variation in time spent with the child induced by the maternity leave reform in Norway in 1977, which offered up to 4 months of paid leave and an additional full year of unpaid leave. The reform resulted in a substantial increase in the time mothers spent at home (away from work) after birth, presumably caring for their newborn children, instead of relying on informal care alternatives. We found that the reform had strong effects on children's subsequent high school dropout rates and earnings at age 30, especially for those whose mothers had less than 10 years of education.

    In order to understand these results, it is important to specify the possible child care arrangements available in the 1970s in Norway as

TABLE 13
OUTCOMES FOR OLDER SIBLINGS: NONPARAMETRIC RD AND RD-DD REGRESSIONS

	VARIABLE		
ESTIMATE	Dropout Rates of Older Siblings (1)	College Attendance of Older Siblings (2)	Ln(Earnings) in 2007 of Older Siblings (3)
RD	−.036	.031	−.164
	(.024)	(.029)	(.146)
	[.19]	[.50]	[12.3]
Observations	6,264	6,264	6,264
RD-DD years (1975, 1978, and 1979)	−.010	−.008	−.102
	(.014)	(.017)	(.083)
	[.20]	[.48]	[12.3]
Observations	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 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD estimates used only eligible births in 1977, whereas the RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).
  * Significant at 10 percent.
 ** Significant at 5 percent.
*** Significant at 1 percent.

alternatives to maternal care. There was almost no high-quality child care for under 2-year-olds, so the alternative was care by grandparents or other types of informal care. Neither of these was necessarily a good substitute for a mother's time in this period of a child's life.

The estimated positive effects of early time with the child on his or her medium- to long-term outcomes resemble the relatively large effects found recently as a result of other early investments in children, such as the Perry Program and Project STAR (Heckman et al. 2010; Chetty et al. 2011).

Our results suggest that policies facilitating increases in parents' time with children during the first year of life may have a positive impact on children's abilities later in life, especially if there are no existing maternity leave benefits within the jurisdiction in question. This has been an important argument behind expansions in maternity leave programs across many countries. However, this study is the first to show that the argument may be empirically justified in terms of children's long-term outcomes.

The level of maternity leave benefits in the United States today is remarkably similar to that in Norway before the 1977 reform. Parental leave is currently under debate in the United States (*USA Today*, July 26, 2005; *New York Times*, April 16, 2008), and an introduction of 4 months

TABLE 14
Children's and Mothers' Outcomes, by Gender:
Nonparametric RD-DD Regressions

| | Gender Subgroup | |
| | Females (1) | Males (2) |
Variable		
Children:		
Dropout rate	−.023**	−.015
	(.011)	(.012)
	[.17]	[.21]
Observations	30,737	32,834
College attendance	.030*	.011
	(.016)	(.014)
	[.53]	[.37]
Observations	30,737	32,834
Ln(earnings) at age 30	.019	.074***
	(.025)	(.020)
	[12.3]	[12.6]
Observations	29,234	31,498
Mothers:		
Predicted months of unpaid leave	−.047	.048
	(.255)	(.221)
	[7.7]	[7.6]
Observations	30,737	32,834
Employed 2 years after childbirth	−.008	−.024*
	(.015)	(.014)
	[.72]	[.73]
Observations	30,737	32,834
Employed 5 years after childbirth	−.007	−.010
	(.013)	(.013)
	[.76]	[.76]
Observations	30,737	32,834
Ln(income) 5 years after childbirth	−.107	−.092
	(.151)	(.144)
	[8.3]	[8.3]
Observations	30,737	32,834

Note.—Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).
* Significant at 10 percent.
** Significant at 5 percent.
*** Significant at 1 percent.

of paid leave and better job protection is within the set of feasible policies (Federal Employees Paid Parental Leave Act of 2007). Recall, however, the evidence from other countries, namely, Germany, Sweden, and Denmark, suggesting that expansions of maternity leave benefits on top of already-generous systems may be far less effective.

TABLE 15
CHILDREN'S AND MOTHERS' OUTCOMES, BY BIRTH ORDER:
NONPARAMETRIC RD-DD REGRESSIONS

| | BIRTH ORDER SUBGROUPS | |
| | Firstborn (1) | Later-Born (2) |
VARIABLE		
Children:		
Dropout rate	−.006	−.034**
	(.010)	(.012)
	[.19]	[.20]
Observations	33,653	29,918
College attendance	.013	.029
	(.014)	(.018)
	[.45]	[.44]
Observations	33,653	29,918
Ln(earnings) at age 30	.034	.071***
	(.022)	(.023)
	[12.5]	[12.5]
Observations	32,103	28,629
Mothers:		
Predicted months of unpaid leave	.034	−.015
	(.206)	(.277)
	[7.7]	[7.6]
Observations	33,653	29,918
Employed 2 years after childbirth	−.028**	−.001
	(.013)	(.013)
	[.70]	[.75]
Observations	33,653	29,918
Employed 5 years after childbirth	−.019	.003
	(.012)	(.013)
	[.72]	[.80]
Observations	33,653	29,918
Ln(income) 5 years after childbirth	−.217	.037
	(.135)	(.146)
	[7.8]	[8.9]
Observations	33,653	29,918

NOTE.—Each cell presents the estimated discontinuity in the outcomes as a result of the maternity leave reform on July 1, 1977. We used local linear regressions including triangular weights, a bandwidth of 91 days, and separate trends on each side of the discontinuity. Numbers in parentheses are the standard errors clustered at the date of birth. Numbers in brackets are the means of the different outcomes for the prereform sample. We include the number of observations for each outcome. The RD-DD estimates used eligible births in 1975, 1977, 1978, and 1979 (182 days on either side of the discontinuity).
  * Significant at 10 percent.
  ** Significant at 5 percent.
  *** Significant at 1 percent.

Using a rich set of family background variables to address the heterogeneity of effects gave us the advantage of making the study less dependent on the institutional settings in Norway. For example, showing that the effects were larger for children from less educated households may be important for policy discussions related to lowering inequality

in general. Many countries, such as the United States or the United Kingdom, have substantial degrees of inequality in education and income levels, which have 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.

Appendix

Breast-Feeding

Mainly using a survey from one maternity hospital in Norway over time, Liestøl, Rosenberg, and Walløe (1988) describe patterns of breast-feeding over about 150 years. They show that breast-feeding in Norway started to decline around 1920 and reached its lowest point around 1967, when only 30 percent of mothers were breast-feeding their infants for 3 months and as few as 5 percent were breast-feeding them for 9 months. By the late 1970s, the level of breast-feeding in Norway had returned to the level in 1940. Around the period of the maternity
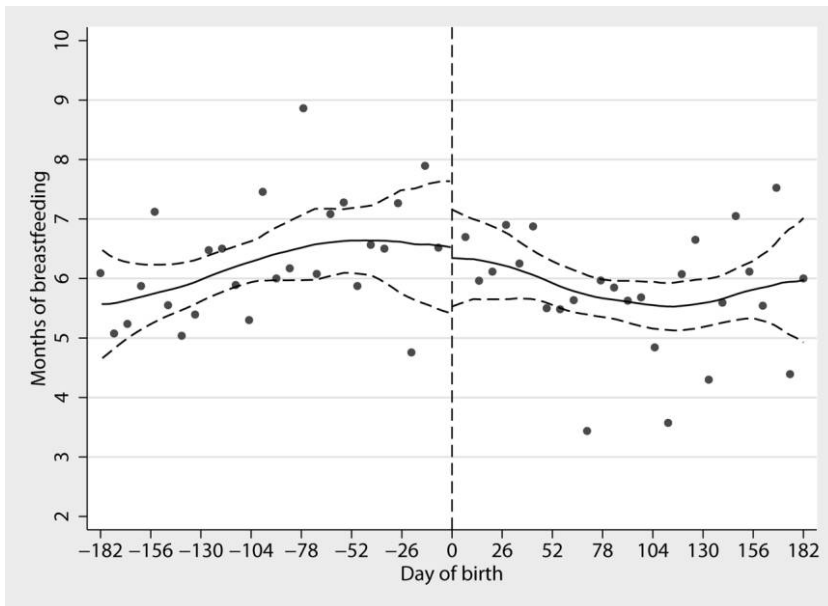

Fig. A1.—Breast-feeding in Norway: eligible mothers in 1977. Each observation is the average outcome in a 1-week bin based on the birth date of the child. The dashed vertical line denotes the reform cutoff of July 1, 1977 (normalized to zero). The solid line is a fitted triangular local linear regression with a bandwidth of 91 days. The window includes all children born in 1977 to eligible mothers (182 days on either side of the discontinuity). The dashed lines mark the 95 percent confidence interval.

leave reform that we consider, about 75 percent of mothers were breast-feeding for 3 months, 50 percent for 6 months, and 25 percent for 9 months or more. Clearly, there was an increase in the rate of breast-feeding in this period in our data set.

We used survey data recording mothers' answers when asked about their breast-feeding practices for all of their children and calculated the average number of months of breast-feeding. The survey data were from a health data set covering all 40-year-olds in the early 1990s (the 40-Year-Old Survey). We were able to match about 5 percent of the children in our sample. However, as we have data on the entire population of children, we still have more than 100 observations for each month. This data set is nonetheless too small to establish a convincing regression design, but in figure A1 we show the average number of months of breast-feeding according to the date of birth for all eligible mothers in 1977 and 1975. First, this shows that breast-feeding increased from 1975 to 1977, which is consistent with the data from Bernal and Keane (2010). However, there was no increase in breast-feeding after the reform in 1977.[25] If anything, there was a small decline in the average number of months of breast-feeding in 1977. This indicates that breast-feeding is probably not the most important mechanism behind the positive effects of the reform on children's outcomes.

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[25] We also tried different measures as indicator variables for breast-feeding for at least 6, 8, and 9 months, and we obtained similar results. There is no clear pattern across the months of birth for eligible mothers in 1977 (or for our control groups of eligible mothers in 1975 and ineligible mothers in 1977).

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