

# The Effects of Paternity Leave on Parents and Children\*

Sara Cools<sup>†</sup>      Jon H. Fiva<sup>‡</sup>      Lars J. Kirkebøen<sup>§</sup>

December 15, 2010

## Abstract

Paternity leave is often discussed as a family policy measure to encourage more gender equality both at home and in the labor market, but empirical evidence on its effects is scarce. In this paper we use the precise timing of the introduction of the paternal quota in Norway to evaluate the effects of paternity leave on parents and children. We find that paternity leave reduces fathers' earnings, but not that it benefits the labor market outcomes of mothers. On the contrary we document an adverse impact of paternity leave on maternal earnings and employment. For children, we find no effects of paternity leave on cognitive skills, measured as school performance at age 16.

*Keywords: parental leave, labor supply, child development*

*JEL Classification: J13, J22, J24, I21*

---

\*We are grateful to Tarjei Havnes, Kalle Moene, Marte Rønning, Ingeborg Solli and seminar participants at Statistics Norway and the University of Oslo for helpful comments. This paper is part of the research activities at the center of Equality, Social Organization, and Performance (ESOP) at the Department of Economics at the University of Oslo. ESOP is supported by the Research Council of Norway.

<sup>†</sup>University of Oslo. E-mail: sara.cools@econ.uio.no

<sup>‡</sup>University of Oslo. E-mail: j.h.fiva@econ.uio.no

<sup>§</sup>Statistics Norway. E-mail: kir@ssb.no

# 1 Introduction

Because the division of labor within a household is established when children are young, parental leave policies could have substantial influence. Paternity leave (parental leave taken by fathers) is therefore proposed as a family policy measure to encourage more gender equality both at home and in the labor market. Such a policy may have important long-term consequences for both parents and children.

There are several mechanisms through which families are affected by parental leave policies. If the leave period itself is penalized in the labor market, paternity leave will directly affect the relative wages of men and women (Datta Gupta and Smith (2002)). Paternity leave will also affect relative skill acquisition at home at an early stage, further contributing to parents' degree of specialization in home and market work. Because comparative advantages within a household evolve dynamically, the earnings differential between men and women may also be changed in the longer run (Becker (1985)). For children, the effect of paternity leave depends on the type of care that is displaced (maternal or non-parental care). The direct shift during the child's first year may also have long-term consequences for the amount time spent together by fathers and children. This may have important consequences for children's long run outcomes.<sup>1</sup>

This paper investigates the consequences of reserving part of the parental leave period for fathers. Such a *paternal quota* is common in the Nordic countries, but similar arrangements are also found in other European countries.<sup>2</sup> Our analysis is based on data from Norway, where a four-week paternal quota, explicitly intended to increase father involvement in child rearing, was introduced in 1993.<sup>3</sup>

We document how paternity leave impacts a broad range of outcomes: Labor market outcomes of parents, cognitive outcomes of children (measured as school performance at age 16),

---

<sup>1</sup>Several studies document that early childhood influences affect long term outcomes. Almond and Currie (2010) offer a review of this rapidly growing literature.

<sup>2</sup>In Germany, for example, a policy introduced in 2007 extends the parental leave period by two months if both mother and father participate (Kluve and Tamm (2009)).

<sup>3</sup>“To strength the father's role in his child's life, it is important for him to participate in childcare during the child's first year. A portion of the parental leave period should therefore be reserved for the father”.(The Government's Long term program for 1990-93 (St.meld. nr. 4), our translation.)

and family outcomes such as fertility and divorce. By using register data, which comprises the entire Norwegian population, we are able to exploit the precise timing of the introduction of the paternal quota to identify causal effects. Because incentives to take paternity leave changed discontinuously on April 1, 1993, we implement an instrumental variable (IV) approach, isolating variation in paternity leave due to variation in date of birth. As the reform that we utilize was announced when mothers who gave birth around April 1, 1993 were already pregnant, time windows closely bracketing the reform isolate plausible exogenous variation. Although parents could not time conception strategically, we cannot rule out the possibility that parents postponed induced births or planned cesarean sections in order to benefit from the parental leave reform. In fact, we document strategic birth timing in the data. We address this issue by excluding births occurring during the last two weeks of March and the first two weeks of April. To eliminate possible inherent differences between families with children born at different times of the year we utilize data for the previous cohort.

While several papers have investigated the effects of maternity leave (or general parental leave) on various outcomes, there are few studies that have considered the particular effects of paternity leave.<sup>4</sup> Using paternal quota reforms in Sweden Ekberg et al. (2005) and Johansson (2010) find no evidence that paternity leave affects the extent to which fathers care for children when they are sick (Ekberg et al. (2005)), or affects mothers' and fathers' earnings (Johansson (2010)). However, the precision in the estimates of both of these studies is very low. Rege and Solli (2010) study the effect of paternal leave on fathers' earnings using Norwegian register data. Their identification strategy is different from ours and from that of the two Swedish studies. The identification strategy is based on the assumption that time trends in the earnings of fathers of children of different ages through the 1990s would be equal in the absence of the

---

<sup>4</sup>Examples of studies considering how maternal leave impacts parent and/or child outcomes are Lalive and Zweimüller (2009), Dustman and Schönberg (2009), Liu and Skans (2010), Baker and Milligan (2010) and Carneiro et al. (2010). Lalive and Zweimüller (2009) document that a one-year increase in moderately paid maternal leave increased fertility and reduced return to work in Austria. Based on Norwegian data, Carneiro et al. (2010) document that increased maternity leave has favorable long run effects on children (e.g. high school completion). Liu and Skans (2010) find no effects of maternity leave on several parental outcomes, based on Swedish data. They do however find a positive effect on school performance for children in those families, affected by the reform, in which mothers have higher education. Dustman and Schönberg (2009) and Baker and Milligan (2010) do not find any effects of maternity leave on child outcomes, based on data from Germany and Canada, respectively.

reform. They find a negative effect of paternity leave on fathers' earnings. Our paper is the first to study how children's outcomes are affected by paternity leave.

Our results show that fathers' future earnings are reduced by paternity leave, consistent with the findings of Rege and Solli (2010). However, our results also show that mothers' earnings are adversely affected by spousal parental leave, an effect which seems to be driven by a reduction in mothers' labor supply. Our findings are therefore not consistent with the hypothesis that paternity leave shifts the gender balance at home enough to increase mothers' time and/or effort spent in market work.

For children, we find no robust average effect of paternity leave on school performance at age 16. We also investigate whether there are any heterogeneous effects that depend on the education level of the parents. There are some positive effects of paternity leave for families where fathers have at least as high education as the mother, but the effect is not robust to varying the time window around the reform date.

Lastly, we do not find any significant impact of paternity leave on any of our measures of family outcomes.

The rest of the paper is organized as follows. In section 2 we introduce the institutional setting and explain how the reform affected parents' uptake rates. In section 3 we discuss the selection problem and present our empirical strategy. In section 4 we present our data and discuss the various outcome measures that we use. In Section 5 we present the results and section 7 provides a concluding discussion.

## **2 Paternity Leave in Norway**

In Norway, wage compensated parental leave has been extended continually since the 1970s, from 18 weeks leave with full wage compensation in 1977 to 46 weeks in 2009.<sup>5</sup> Although the parental leave scheme offers full (or 80%) wage compensation, eligibility for wage compensated leave is contingent on the *mother* having worked 50% or more during at least six out

---

<sup>5</sup>See Appendix Table 15 for a full description

of the last ten months before the child's birth.<sup>6</sup> This applies to both parents: Norwegian fathers have no independent right to paid parental leave. If the mother works part time (between 50 and 100%), the father's compensation rate is reduced accordingly. In addition, the father must himself have worked at least six out of the last ten months in order to be eligible for wage compensated leave.<sup>7</sup>

Historically, men have taken very little of the leave period that can be freely shared between the parents.<sup>8</sup> This fact was debated in Norway during the 1980's, and various measures that would induce men to take part of the leave were called for. In autumn 1992, the labor party government included in their suggestion for the national budget of 1993 a seven-week extension of the (100%) compensated parental leave period, of which four weeks would be reserved for fathers.

The reform was passed in parliament in December 1992. Following implementation on April 1, 1993, four of the 42 weeks of paid leave were reserved for the child's father. Except under special circumstances, families would lose the right to these four weeks unless taken by the father. In addition, the mother had to resume work for the father to be eligible for the paternal quota.<sup>9</sup> All subsequent extensions of the parental leave period have been put into the paternal quota.

As is evident from Figure 1, the 1993 reform dramatically increased the number of fathers taking paternity leave. In our sample of families eligible for parental leave, the fraction taking paternity leave immediately increased from 4% in March 1993 to 39% in April 1993. The fraction has increased steadily over time, to above 80% of eligible fathers in 2005.

Table 2 gives the distribution of paternity leave spells, conditional on the father taking

---

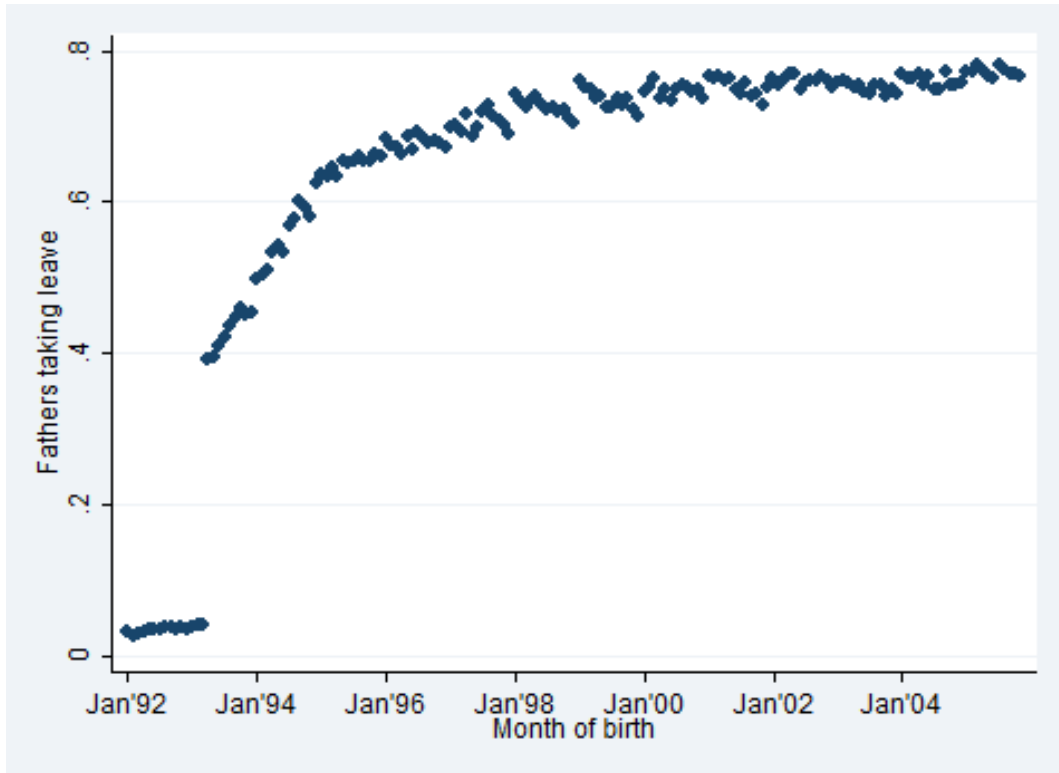
<sup>6</sup>Sick leave from employment, unemployment with right to benefits, and paid parental leave all count as work (<http://www.regjeringen.no/nb/dep/bld/dok/nouer/1996/nou-1996-13.html?id=116070> )

<sup>7</sup>Income compensation also reaches an upper bound of six times the *basic amount* (G) ("Folketrygdens grunnbeløp") of the Norwegian social security system. This amount is adjusted yearly (or more often) in accordance with changes in the general income level. From January 1 2010, G is NOK 72 881 (approximately USD 12 500). Until 2008, when self-employed were granted rights to full compensation, the compensation rate for self-employed was at 65% of their income.

<sup>8</sup>The parental leave period can be shared between the mother and father, except for the first six weeks after birth, which have been reserved for the mother. In 1991, women were obliged to start their leave period two weeks before expected delivery. From 1993 this is extended to three weeks.

<sup>9</sup>This requirement was relaxed in July 1994 (Brandth and Øverli (1998))

Figure 1: Share of fathers taking leave in families eligible for parental leave, 1992-2005.



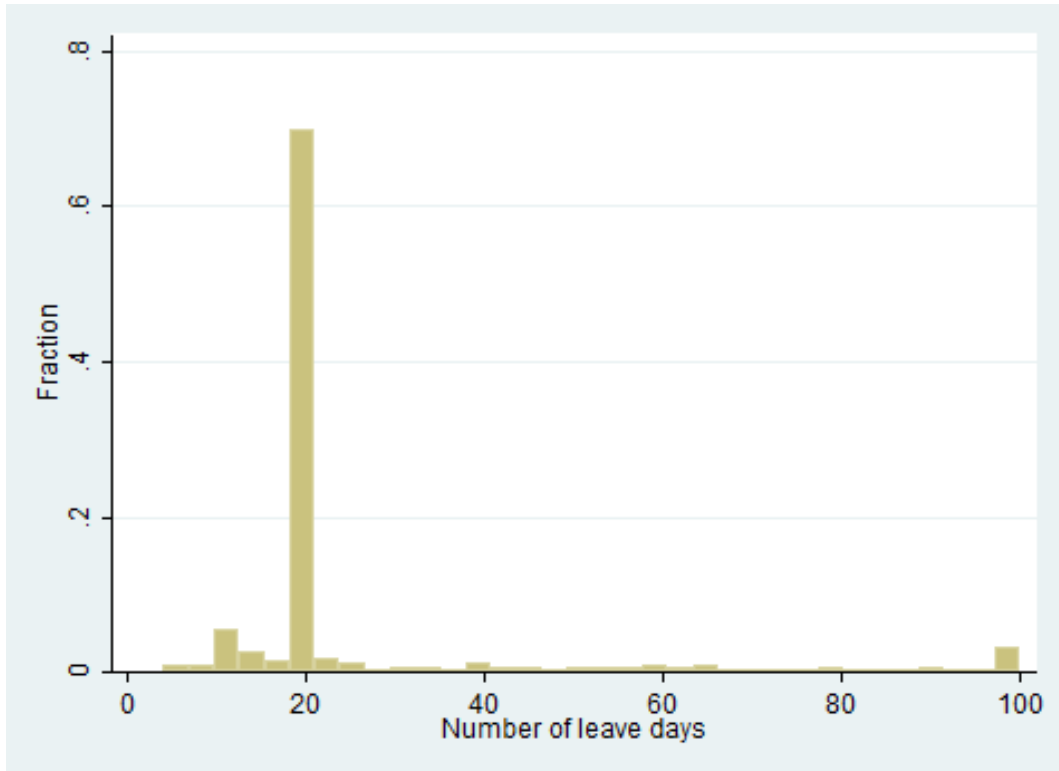
leave. The sample is fathers of children born from April 1st to May 12th 1993. On average, these fathers take 28 days of paternity leave (5.6 weeks). 70% of all fathers take exactly the 4 weeks of the paternal quota.<sup>10</sup> Most fathers only have one spell of paternity leave - if any (90%). On average, their leave period starts when the child is nine months old. Less than 5% take leave after the child has turned one year.

### 3 Identification

Estimating the causal effects of paternity leave on parent, family and child outcomes is complicated by a selection problem. In families where fathers take parental leave, both parents tend to be older, more educated and have higher income than in families where fathers do not take parental leave. As mentioned in Section 1, we handle the selection problem by exploiting the

<sup>10</sup>Only 10% of leave taking fathers took more leave than the paternal quota until 1999. The fraction rose to 18% in 2004.

Figure 2: Fraction of leave taking fathers by number of leave days taken (working days).



Note: The sample is fathers of children born from April 1st to May 12th 1993 who took leave before the child was 3 years old or had a younger sibling. Number of leave days have been truncated at 100, for ease of exposure.

introduction of the paternal quota at April 1, 1993.

### 3.1 Reform as exogenous variation

Our empirical strategy is based on the idea that when looking at births closely surrounding April 1, 1993, the paternal quota reform provides quasi-experimental variation in the uptake of paternity leave, since only families with children born after April 1, 1993 are eligible for the paternal quota.

As mentioned in Section 2, not all of these families were actually covered by the reform: Parents must be eligible for wage compensated parental leave for the reform to represent an actual change in incentives. Therefore, in all of our analyses, our samples are restricted to eligible families. How eligibility status is defined and determined will be discussed in Section 3.4.

To eliminate inherent differences between families with children born at different times of the year<sup>11</sup>, we rely on a difference-in-differences approach, comparing the difference between the 1993 pre-reform and post-reform cohorts in 1993 to that between corresponding cohorts from 1992.

Families of children born during the same calendar month in the previous year constitute a natural comparison group. Using 1992 as our comparison year is particularly useful to us, as there was a reform extending general parental leave rights by three weeks on April 1st 1992. As mentioned in Section 2, the 1993 reform was not a clean paternal quota reform: For parents of children born after April 1st, the compensated parental leave period was extended by a total of seven weeks, *and*, from this date on, four (of the now 42) weeks of parental leave were reserved for fathers. Since our interest in the 1993 reform lies with the impact of only the paternal quota, we use the 1992 reform to remove the effect of the increase in general parental leave typically taken by the mother.<sup>12</sup>

We thus have four groups of parents: The 1992 pre- and post-reform cohorts, and the 1993 pre- and post-reform cohorts. Their respective parental leave rights are given in Table 1.

Table 1: Parental leave scheme in Norway 1992-1993

	Before April 1st	After April 1st	Difference
1992	32w, 0w paternal quota	35w, 0w paternal quota	3w gen. leave
1993	35w, 0w paternal quota	38w, 4w paternal quota	3w gen. leave + 4w paternal quota

Using difference-in-differences estimation on these two discontinuities, the identifying assumption is that - other than the four weeks of paternal quota - there are no differences between

<sup>11</sup>The children in our post-reform cohort will of course on average be somewhat younger than those in our pre-reform cohort. This may matter for the child outcomes we consider, as several studies have documented an association between season of birth and school performance (e.g. Strøm (2004) provides evidence for Norway). It is widely believed that this relationship is caused by differences in age at school entry, but it may also simply reflect that children born at different times of the year are conceived by women with different socioeconomic characteristics (Buckles and Hungerman (2008)). This age difference may also matter for some of the parental outcomes, as they are generally measured annually. Because mothers of children born before the reform, all else equal, will have a higher probability of a full year's income even several years later, one might spuriously attribute to the reform what is in reality a mere child age effect.

<sup>12</sup>The 1992 reform was also announced during the autumn prior to its implementation; accordingly, there is little reason to fear that parents anticipated it and planned conception in order to fit the reform.



the 1993 pre- and post-reform cohorts that do not also appear between the 1992 pre- and post-reform cohorts. Tables 2 and 3 give descriptive statistics for these groups of eligible parents whose children were born within a six-week window prior or subsequent to April 1st. The first thing to notice is that the 1992 reform barely affected fathers' leave taking behavior. The fraction of fathers taking leave was not significantly affected, and the number of leave days taken by fathers is imprecisely estimated to increase by half a day. All of the extra leave granted by the 1992 reform is taken by mothers. The only other statistically significant differences between the 1992 pre- and post-reform cohorts are of negligible size.

The 1993 reform, on the other hand, greatly affected leave taking behavior for our sample of eligible families. Fathers' propensity to take parental leave increased by 35 percentage points, and their average number of leave days taken increased by 8 (meaning an average increase of 23 work days for those fathers actually taking leave - slightly more than the 4 weeks of paternal quota).

As with the 1992 cohorts, the few statistically significant differences in the pre-birth characteristics between the pre- and post-reform 1993 cohorts, are of negligible size. On the whole, the estimated differences in observables support the idea of the reform providing exogenous variation along the relevant dimension (and only this one): Paternity leave.

### **3.2 Strategic timing of births**

The 1993 reform was an historically big reform in the parental leave scheme. With the exception of the 1977 reform, all previous parental leave reforms meant an extension of four weeks or less. Seven weeks leave with full wage compensation is a considerable benefit at a time when childcare slots were rationed and many parents went on unpaid leave to care for small children. The 1993 reform therefore provided parents with strong incentives to have children born after April 1st rather than just before.

We see little reason to suspect that parents could time conception in anticipation of the reform. The national budget where the paternity quota was introduced became publicly available at October 7, 1992. At this time mothers who gave birth close to April 1, 1993 were already

Table 2: Descriptive statistics for cohorts born before and after April 1, 1992

	(1)		(2)		(3)	
	Before April 1st		After April 1st		t-test of (1) vs (2)	
	Mean	SD	Mean	SD	Estimate	SE
<i>Fathers</i>						
- take leave	0.026	(0.16)	0.029	(0.17)	0.0030	(0.0036)
- no. leave days	1.26	(8.96)	1.72	(12.1)	0.45**	(0.23)
- age <25	0.048	(0.21)	0.050	(0.22)	0.0021	(0.0047)
- age 25-29	0.29	(0.45)	0.28	(0.45)	-0.0045	(0.0097)
- age 30-34	0.38	(0.49)	0.37	(0.48)	-0.011	(0.010)
- age >34	0.28	(0.45)	0.30	(0.46)	0.014	(0.0098)
- lower sec. or less	0.38	(0.49)	0.37	(0.48)	-0.012	(0.010)
- upper secondary	0.34	(0.47)	0.34	(0.47)	0.0012	(0.010)
- higher ed. lower level	0.20	(0.40)	0.20	(0.40)	0.0013	(0.0087)
- higher ed. higher level	0.085	(0.28)	0.094	(0.29)	0.0090	(0.0062)
- work experience	10.7	(4.86)	10.6	(4.88)	-0.10	(0.11)
- annual income	238316.2	(102670.0)	244439.5	(125408.1)	6123.3**	(2479.0)
<i>Mothers</i>						
- age <25	0.13	(0.34)	0.13	(0.34)	0.00096	(0.0073)
- age 25-29	0.41	(0.49)	0.41	(0.49)	-0.0039	(0.011)
- age 30-34	0.32	(0.47)	0.32	(0.47)	0.0072	(0.010)
- age >34	0.14	(0.35)	0.13	(0.34)	-0.0043	(0.0074)
- lower sec. or less	0.40	(0.49)	0.38	(0.49)	-0.018*	(0.011)
- upper secondary	0.29	(0.46)	0.29	(0.45)	-0.0051	(0.0098)
- higher ed. lower level	0.26	(0.44)	0.29	(0.45)	0.024**	(0.0097)
- higher ed. higher level	0.041	(0.20)	0.039	(0.19)	-0.0015	(0.0042)
- work experience	7.63	(3.96)	7.54	(3.89)	-0.091	(0.090)
- annual income	159258.2	(68689.5)	159295.8	(71164.5)	37.6	(1511.7)
N	4235		4341		8576	

Note: All observations except those regarding parental leave are taken from the year before the child's birth. Age categories are based on parents' age at birth of the first child. Sample is children born during the 12 weeks surrounding April 1st 1992, divided into those born during the six weeks preceding the reform and those born during the first six weeks after the reform.

Table 3: Descriptive statistics for cohorts born before and after April 1, 1993

	(1)		(2)		(3)	
	Before April 1st		After April 1st		t-test of (1) vs (2)	
	Mean	SD	Mean	SD	Estimate	SE
<i>Fathers</i>						
- take leave	0.034	(0.18)	0.39	(0.49)	0.35***	(0.0080)
- no. leave days	1.87	(12.2)	9.91	(18.8)	8.04***	(0.34)
- age <25	0.045	(0.21)	0.044	(0.21)	-0.00035	(0.0044)
- age 25-29	0.28	(0.45)	0.29	(0.45)	0.015	(0.0097)
- age 30-34	0.37	(0.48)	0.37	(0.48)	-0.0062	(0.010)
- age >34	0.31	(0.46)	0.30	(0.46)	-0.0083	(0.0099)
- lower sec. or less	0.37	(0.48)	0.37	(0.48)	0.0057	(0.010)
- upper secondary	0.35	(0.48)	0.34	(0.47)	-0.0068	(0.010)
- higher ed. lower level	0.20	(0.40)	0.21	(0.41)	0.011	(0.0086)
- higher ed. higher level	0.090	(0.29)	0.080	(0.27)	-0.0096	(0.0060)
- work experience	10.9	(4.89)	10.7	(4.96)	-0.17	(0.11)
- annual income	242404.1	(114129.6)	242550.2	(120200.4)	146.1	(2521.2)
<i>Mothers</i>						
- age <25	0.13	(0.33)	0.12	(0.33)	-0.0058	(0.0071)
- age 25-29	0.39	(0.49)	0.42	(0.49)	0.026**	(0.011)
- age 30-34	0.34	(0.47)	0.33	(0.47)	-0.0095	(0.010)
- age >34	0.15	(0.35)	0.13	(0.34)	-0.010	(0.0074)
- lower sec. or less	0.38	(0.49)	0.35	(0.48)	-0.028***	(0.010)
- upper secondary	0.30	(0.46)	0.32	(0.47)	0.019*	(0.0100)
- higher ed. lower level	0.28	(0.45)	0.29	(0.45)	0.0086	(0.0097)
- higher ed. higher level	0.038	(0.19)	0.039	(0.19)	0.00077	(0.0041)
- work experience	7.82	(4.04)	7.71	(3.91)	-0.11	(0.090)
- annual income	162480.1	(71256.9)	161706.4	(68373.9)	-773.7	(1499.4)
N	4177		4504		8681	

Note: All observations except those regarding parental leave are taken from the year before the child's birth. Age categories are based on parents' age at birth of the first child. Sample is children born during the 12 weeks surrounding April 1, 1993, divided into those born during the six weeks preceding the reform and those born during the first six weeks after the reform.

pregnant. Admittedly the reform itself was probably not very surprising to followers of the policy debate in Norway at the time, but there is little reason to expect that future parents knew the exact date of its implementation.<sup>13</sup> Searches in newspaper archives also suggest that the date of implementation was not publicly available before the national budget was presented.

Even if conception was not timed strategically, expecting parents with due dates close to April 1st could possibly postpone induced births or planned cesarean sections. Although the scope for strategic birth timing is limited since the vast majority of births in Norway are normal spontaneous births, we investigate this claim empirically.<sup>14</sup> Following Gans and Leigh (2009), we run a regression where we relate the daily number of births to the reform (a dummy taking the value 1 for days after April 1st in 1993). We control for day of year fixed effects and for day of week fixed effects interacted with year fixed effects. In addition we add dummies for 10 days during Easter.<sup>15</sup> Our sample is daily births during the relevant time window (surrounding April 1st) for the period 1975-2005, excluding 1989 and 1992 when parental leave reforms were implemented on April 1st.

The analysis shows that day of week effects are considerable. This indicates that there is scope for non-medical reasons to affect the time at which children are born. As is reported in Table 4, we do find statistically significant evidence of strategic timing of births. The reform seems to have increased the daily number of births by 19.5 on average for the first two weeks of April relative to the last two weeks of March; 11% of all births in this period. This estimate implies that a total number of 137 births were moved from somewhere in the latter half of March to somewhere in the first half of April 1993.<sup>16</sup> That the 1993 parental leave reform seems to have induced some parents to strategically time births is also documented by Brenn and Ytterstad (1997).

---

<sup>13</sup>Of the previous 7 parental leave reforms in Norway, implementation dates varied between April 1st (in 1989 and 1992), May 1st (in 1987 and 1990) and July 1st (in 1977, 1988 and 1991).

<sup>14</sup>In 1993 the fraction of children born by cesarean section was around 12.4 percent, and of these deliveries, 59.4 percent were emergency operations. On average 12 percent of births in 1993 were induced, while 88 percent was spontaneous (Folkehelseinstituttet, <http://mfr-nesstar.uib.no/mfr/>).

<sup>15</sup>In Norway, the Thursday and Friday before and Monday after Easter day are public holidays.

<sup>16</sup>Following Gans and Leigh (2009), the total number of births moved is calculated by dividing daily number of births by two (as one birth moved means one birth less in March and one more in April) and then multiplying by the number of days in the window. Similarly, the share of births moved is calculated by dividing the coefficient by two before converting log points to percentage points.

Table 4: Birth rate effects

	(1)	(2)	(3)	(4)
	± 1 week	± 2 weeks	± 3 weeks	± 4 weeks
<i>Panel A: Dependent variable is daily number of births</i>				
Reform	18.0**	19.5***	9.00**	6.41*
	(7.54)	(5.39)	(4.39)	(3.79)
_cons	192.7***	202.6***	179.4***	181.7***
	(11.8)	(9.71)	(6.67)	(5.94)
<i>Number of births moved</i>	63	136.5	94.5	89.7
Observations	406	812	1218	1624
R <sup>2</sup>	0.863	0.760	0.723	0.696
<i>Panel B: Dependent variable is ln(daily number of births)</i>				
Reform	0.099**	0.11***	0.051**	0.035
	(0.043)	(0.031)	(0.025)	(0.022)
_cons	5.27***	5.32***	5.18***	5.19***
	(0.067)	(0.055)	(0.038)	(0.034)
<i>Share of births moved</i>	5.1%	5.7%	2.6%	1.8%
Observations	406	812	1218	1624
R <sup>2</sup>	0.866	0.766	0.730	0.706

Note: Sample is daily births within the relevant window (always centered around April 1st), for the years 1975-2005. “Reform” is a dummy taking the value 1 for days in April 1993.

If strategic timing of births is related to (unobservable) characteristics that matter for the outcomes that we consider, this will bias our estimates of paternal leave. We address this potential problem by excluding births occurring during the two last weeks of March and the two first weeks of April.

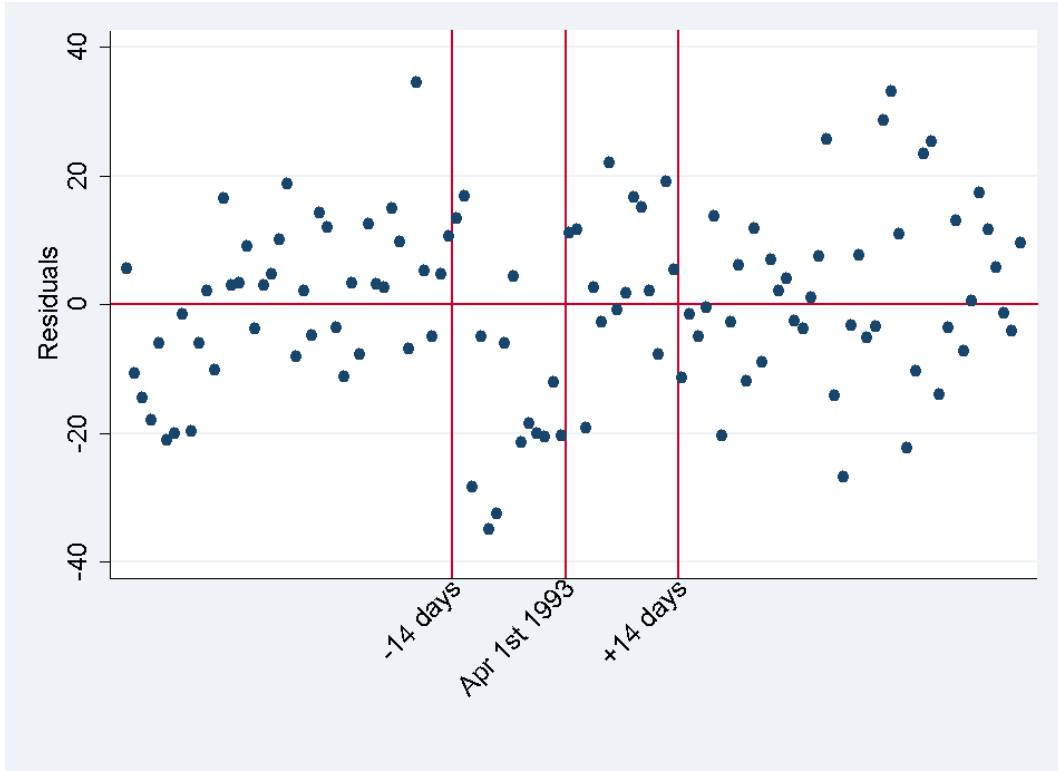
### 3.3 Empirical specification

We estimate the following relationship based on data from families with children born in 1992 and 1993:

$$Y_i = \alpha \text{PaternityLeave}_i + \bar{X}_i' \bar{\beta} + \delta_W \text{Week}_i + \delta_Y 1993_i + \varepsilon_i, \quad (1)$$

where  $i$  is the child/household/parent indicator.  $Y$  denotes the different outcomes to be discussed in Section 4, and  $\bar{X}$  is a vector of pre-birth controls.  $\text{Week}$  is a vector of dummies

Figure 3: Daily births residuals



Note: Daily births residuals for the eight weeks centered around April 1, 1993 from a regression on day of year fixed effects, day of week fixed effects interacted with year fixed effects, and dummies for 10 days during Easter. Sample is based on data from daily births during eight-week time windows around April 1st for the period 1975-2005 (excluding 1989 and 1992).

indicating during which week of the year the child was born. 1993 is a dummy indicating whether the child was born in 1993 or in 1992.  $\varepsilon$  is the error term.

$\alpha$  is the parameter of interest. For  $\alpha$  to be given a causal interpretation we instrument *PaternityLeave* with whether the child was born before or after the introduction of the paternal quota. Our first stage is given by:

$$PaternityLeave_i = \rho Reform_i + \bar{X}_i' \bar{\eta} + \gamma_W Week_i + \gamma_Y 1993_i + v_i, \quad (2)$$

where *Reform* is a dummy variable taking the value 1 if the child was born after April 1st 1993.  $\rho$  is the regression adjusted compliance rate and  $v$  is the error term.

Following Imbens and Angrist (1994),  $\alpha$  is the *local average treatment effect* (LATE). This

is the average treatment effect for *compliers*: Families whose treatment status (paternity leave) is affected by the paternal quota reform. An empirical description of this group is given in Section 5.6.

Equation 1 and 2 will be estimated with ordinary least squares.<sup>17</sup>

For the child outcomes we also investigate whether there are any heterogenous effects. We do this by interacting both *PaternityLeave* and *Reform* with indicator variables for three groups: Whether the parents, the year before having the child, had education at the same level<sup>18</sup>, or if the mother or the father had education at a higher level.

### 3.4 Eligibility and sample criteria

As mentioned in Section 2, both parents' right to paid parental leave is contingent on the child's mother having worked at least 50% during six out of the last ten months before the child's birth. Hence, families where mothers did not work the required amount were not covered by the paternal quota reform.

Since we do not perfectly observe eligibility status, we rely on parents' income history to capture this. In order to be considered eligible, both parents need to have an income above 2G (see footnote 7) the year before the child was born. 50% of all families fulfill this criterion. As our income criterion is rather strict, we may be excluding families that were actually eligible. But in this way we are fairly confident that our sample consists of families that were truly eligible for the paternal quota if they had children born after April 1, 1993. This is similar for the 1992 reform, as the rules for eligibility did not change with either reform.<sup>19</sup>

---

<sup>17</sup>Since *PaternityLeave* is binary it may seem appropriate to use a nonlinear probability model to estimate Equation 2. This is however not necessary and may even produce inconsistent estimates in the second stage (Angrist and Krueger (2001))

<sup>18</sup>The levels of education are the same as in table 3 and are measured in October

<sup>19</sup>Since information on both the 1992 and 1993 reforms became public in October of the previous year, and we use income data from that calendar year to capture eligibility status, parents may have some scope to select into eligibility. In the data this does not seem to be a problem, all of our results are basically unaltered if we lag the eligibility criteria one year.

### 3.5 Time window

We face a trade-off between low bias and high precision when choosing the time window on which to estimate (1). In a narrow time window there is less chance that our main estimates will be contaminated by omitted variables. A broader window will on the other hand yield more precision by increasing the number of observations.

To balance these concerns we take a  $\pm 6$  week window as our baseline in all specifications, but we also report results on a  $\pm 12$  week window. In line with the discussion of parents' strategic timing of births, we also present results where observations from the two weeks before and the two weeks after April 1st have been excluded. The samples we use for the different specifications are given in Table 5.

Table 5: Samples

<i>Sample</i>	<i>Children born at dates</i>		
Baseline	Feb 19 1992 - May 12 1992	and	Feb 18 1993 - May 12 1993
Double-window	Jan 8 1992 - Jun 23 1992	and	Jan 9 1993 - Jun 23 1993
Baseline - no BT	Feb 19 1992 - Mar 17 1992 Apr 15 1992 - May 12 1992	and	Feb 18 1993 - Mar 17 1993 Apr 15 1993 - May 12 1993
Double-window - no BT	Jan 8 1992 - Mar 17 1992 Apr 15 1992 - Jun 23 1992	and	Jan 9 1993 - Mar 17 1993 Apr 15 1993 - Jun 23 1993

## 4 Data

### Labor market outcomes

Statistics Norway provides data on yearly income going back to 1967 for the entire Norwegian population. Earnings are given in constant 1998 NOK, and are truncated above the 99th percentile. Data on employment status are obtained from Statistics Norway *Employment register* ("Arbeidstakerregisteret"), which contains data on all Norwegian employees. This time series starts in 1993. Work hours are only reported in three broad categories: 1-19 hours, 20-29 hours and 30 or more hours. To measure labor supply we construct dummy variables capturing



whether the individual work at least 20 hours (which we classify as part-time) or at least 30 hours (which we classify as full-time). If an individual is not registered with any employment and is not defined as self-employed<sup>20</sup>, his or her hours are set to zero.

To facilitate interpretation we rely on averages of labor market outcomes based on earnings and labor supply for multiple years. Such aggregation is also useful since it improves statistical power to detect effects that go in the same direction within a domain, without increasing the probability of a Type I error (Kling et al. (2007), Deming (2009), Almond and Currie (2010)). The averages are constructed by normalizing all outcome variables to have a zero mean and a standard deviation of one, and then averaging over these outcomes.

Table 6 shows the variation in the labor market data based on averages from when the child is 2-5 years old and 6-9 years old<sup>21</sup>. We do not report results for earnings based on data from when the child is one year old, since parental leave stretches into 1994 for families treated by the parental leave reform. In our sample, 78% of fathers work full time, and an additional 2% work part time. These numbers are constant across child ages. Mothers work much less: When the child is 2-5 years old, 41% work full time, while an additional 15% work part time. The numbers increase slightly at later child ages.

## **Child outcomes**

Given their young age, there is limited register information on these children. We do however have data on school performance from administrative registers. In Norway, primary and lower secondary school (in total 10 years of schooling) are mandatory. At the end of lower secondary school students are graded. These grades matter for admission to upper secondary schools. Most grades are set by the student's own teachers; however, every student is also required to take a written exam, which is anonymous and graded by teachers from another school. To get an unbiased measure of student ability, e.g. avoid problems with relative grading, we focus

---

<sup>20</sup>If we observe that the individual has income from work that exceeds 1G and the amount of transfers she receives, and in addition the individual's entrepreneurial income exceeds her income from employment, the individual is classified as self-employed.

<sup>21</sup>This is a natural division as compulsory schooling in Norway starts at age six.

Table 6: Summary statistics, labor market outcomes.

	Mean	SD
<i>Fathers</i>		
- earnings, child age 2-5	292734.1	(124461.3)
- earnings, child age 6-9	341150.9	(164127.8)
- full time, child age 2-5	0.78	(0.34)
- full time, child age 6-9	0.78	(0.35)
- part time (or more), child age 2-5	0.80	(0.33)
- part time (or more), child age 6-9	0.80	(0.34)
<i>Mothers</i>		
- earnings, child age 2-5	158502.6	(78433.7)
- earnings, child age 6-9	183358.2	(99573.8)
- full time, child age 2-5	0.41	(0.41)
- full time, child age 6-9	0.42	(0.42)
- part time (or more), child age 2-5	0.56	(0.40)
- part time (or more), child age 6-9	0.60	(0.40)
N	17257	

on these latter exam scores. The exam subject is chosen randomly from the core subjects Norwegian, English and mathematics. Grades take integer values from one to six. For ease of interpretation grades are standardized and measured in units of standard deviations.

The school performance sample is not identical to the samples for parental and family outcomes. The main reason is that for some children we do not observe an exam score.<sup>22</sup> The two samples are similar in terms of observables, both in terms of distribution and in terms of change around the introduction of the paternal quota.

## Family outcomes

Data on marriage, divorce and parity come from Statistics Norway's family and demography files. We investigate the impact of paternity leave on the following family outcomes: parents' total number of children 15 years after the reform (2008), their probability of divorce, the probability that the father has his next child with the same woman (conditional on having

<sup>22</sup>Almost all children who continue to live in Norway will end up in the lower secondary school data. However, a small minority leave school early or late, these are excluded from our analysis, and about 4 percent of the students have no written exam score. In addition, families having multiple births will be represented with more than one observation in the analysis of children's outcomes.

Table 7: Summary statistics, family outcomes.

	Mean	SD
Mother's parity	2.54	(0.85)
Father's parity	2.63	(0.96)
Prob(Divorced by child age 14)	0.21	(0.41)
Prob(Next child together)	0.87	(0.34)
Child spacing (years)	3.51	(1.80)
Mother's leave next child (days)	225.2	(42.8)
Father's leave next child (days)	24.8	(26.4)
N	17257	

another child), child spacing and the number of days parent take leave if they have another child. Table 7 provide descriptive statistics.

For the family outcomes we also construct an index. Following Deming (2009), we decide whether an outcome is to be considered positive or negative and change the sign of negative outcomes. Hence, completed fertility (measured as parity in 2008) for mothers and fathers and the probability of the couple having their next child together enter positively, whereas the probability that the couple is registered with divorce 14 years later and the distance in time to their next child enter negatively.<sup>23</sup>

## Control variables

We include control variables for parents' age at birth of their child, level of education, years of employment and annual income two years before birth. We also control for birth order. Education is measured (October, 1) the year before birth, and is divided into four mutually exclusive categories; lower secondary education or less, upper secondary education, higher education lower degree and higher education higher degree. Years of employment are measured as the number of years, up to and including the year before birth, where the parent had income above 1G (see footnote 7). Descriptive statistics on these pre-birth characteristics were given in Tables 2 and 3.

<sup>23</sup>Outcomes that, for obvious reasons, cannot be observed for a given family are excluded when generating the index.

Table 8: First stage results

	(1)	(2)	(3)	(4)	(5)
	No controls	Baseline	Double-window	Baseline, no BT	Double, no BT
Father takes leave	0.35*** (0.0088)	0.35*** (0.0088)	0.36*** (0.0063)	0.35*** (0.011)	0.36*** (0.0070)

Note: Each cell represents coefficients from an OLS regressions on equation 2. A year dummy and calendar week fixed effects are included in all specifications. Included in specification (2)-(5) are a set of socio-economic background characteristics explained in Table 3. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 5 Results

### 5.1 First stage results: How the reform affect leave taking behavior

We begin by presenting the results of the first stage regression, specified in equation (2). The excluded instrument (*Reform*) is a strong predictor of paternal leave. As reported in Table 8 we find a regression adjusted compliance rate of 0.35 in the baseline time window. This is as expected based on the descriptive statistics presented in Tables 2 and 3.<sup>24</sup> Table 8 clearly shows that we don't have any problems with weak instruments.

### 5.2 Paternity leave and fathers' labor market outcomes

Table 9 shows the second stage results from regressions on equation (1), where the dependent variables are the normalized averages of earnings and labor supply (elaborated on in section 4) for fathers. For brevity, we present only the LATE of interest,  $\alpha$ .

For every outcome that we consider we present results from regressions without any controls (specification (1)), we then add the full set of family background variables that we have available (specification (2)), before we increase the time window to  $\pm 12$  weeks (specification (3)), and finally we exclude  $\pm 2$  weeks from our baseline specification (specification (4)) and for the double-window (specification (5)).

<sup>24</sup>It seems that the reliability of the parental leave data has improved over time, and that there may have been some underreporting during 1992 and 1993. If this is the case the LATE to be presented below are inflated (assuming that the absolute level of underreporting is lower after reform than before). An alternative is to present simple intention-to-treat effects. However, these intention-to-treat effects can be backed out from the 2SLS estimates of the LATE by multiplying the point estimate by the relevant regression adjusted compliance rate from the first stage.

Table 9: The impact of paternity leave on fathers' labor market outcomes

	(1)	(2)	(3)	(4)	(5)
	No controls	Baseline	Double-window	Baseline, no BT	Double, no BT
Earnings, 2-5	-0.23*** (0.082)	-0.13** (0.061)	-0.11** (0.043)	-0.096 (0.074)	-0.089* (0.047)
Earnings, 6-9	-0.13 (0.082)	-0.045 (0.066)	-0.015 (0.046)	-0.037 (0.080)	-0.0050 (0.051)
Full time, 2-5	-0.079 (0.072)	-0.068 (0.070)	-0.043 (0.050)	-0.054 (0.086)	-0.033 (0.055)
Full time, 6-9	-0.10 (0.074)	-0.095 (0.072)	-0.052 (0.051)	-0.085 (0.089)	-0.040 (0.056)
Part time, 2-5	-0.080 (0.071)	-0.070 (0.070)	-0.053 (0.050)	-0.039 (0.086)	-0.037 (0.055)
Part time, 6-9	-0.11 (0.073)	-0.10 (0.072)	-0.057 (0.051)	-0.10 (0.089)	-0.049 (0.056)

Note: Each cell represents coefficients from an IV regressions on equation 1. A year dummy and calendar week fixed effects are included in all specifications. Included in specification (2)-(5) are a set of socio-economic background characteristics explained in Table 3. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The number of observations varies with outcomes and window, for the baseline window of  $\pm 6$  weeks the number of observations is 17216, 17166, 17245, 17232, 17245 and 17232 for the respective outcomes.

For our baseline  $\pm 6$  week window we find negative effects of paternity leave on earnings of fathers when the children are aged 2 to 5. The point estimate indicates a negative effect of 1/10 of a standard deviation decrease in earnings. The effects are statistically significant at the five percent level. Precision naturally increases by extending the time window and by adding covariates. For the double-window the standard errors are about 30 percent smaller than in our baseline window. For the no birth timing windows there is still a negative effect, but only statistically significant (at the ten percent level) for the double-window. For earnings when the children are aged 6 to 9 and for measures capturing labor supply the effects go in the same negative direction, but they are not statistically significant.

That paternity leave seems to reduce short-term earnings of fathers, but not their labor supply, could indicate that fathers wages decrease. An alternative explanation would be that fathers' overtime, which we don't observe, is reduced by paternity leave.

### 5.3 Paternity leave and mothers' labor market outcomes

Next we consider how the introduction of the paternal quota affected labor market outcomes of mothers. Several mechanisms might drive such a relationship. In particular, if fathers taking paternity leave are more likely to share in various forms of home production than other fathers, this might be reflected in mothers' earnings and labor supply.

As in the previous section, we present results for normalized averages of earnings and labor supply, see Table 10. In our baseline specification (2) we find negative effect of paternity leave on mother's earnings, both in the short run (child age 2-5) and long run (child age 6-9), of about 15 percent of a standard deviation. The effect is statistically significant at the five percent level. This effect is similar when we increase the time-window to  $\pm 12$  weeks (specification (3)) and when we omit weeks where birth timing may be an issue (specification (4) and (5)).

We find a negative effect of paternity leave on mothers' probability of working part time or more when the child is aged 2-5. This effect is statistically significant only at the ten percent level in and not in the double-window specifications. The effects are similar when the child is 6-9 years old, and here they are statistically significant in specification (3), (4) and (5). There is no effect of paternity leave on the probability of mothers working full time. The negative effect on mothers' earnings seems therefore to be driven, at least partly, by a reduction in labor supply for mothers that in the absence of paternity leave would have worked part time.

In summary, these results provide some evidence that paternity leave has made fathers less oriented towards market work (and hence more oriented towards home production). However there is no support for the hypothesis that mothers' labor market outcomes move in the opposite direction. The conjecture that paternity leave cause general earning differentials between men and women to decrease is therefore not substantiated empirically. Paternity leave do not seem to affect the extent to which parents specialize in market and non-market work. An alternative hypothesis consistent with our findings is that maternal and paternal investment in children is complementary.

Table 10: The impact of paternity leave on mothers' labor market outcomes

	(1)	(2)	(3)	(4)	(5)
	No controls	Baseline	Double-window	Baseline, no BT	Double, no BT
Earnings, 2-5	-0.17** (0.081)	-0.15** (0.066)	-0.17*** (0.046)	-0.17** (0.081)	-0.18*** (0.051)
Earnings, 6-9	-0.16** (0.082)	-0.15** (0.070)	-0.15*** (0.049)	-0.15* (0.086)	-0.15*** (0.054)
Full time, 2-5	-0.013 (0.073)	-0.014 (0.068)	-0.0015 (0.048)	-0.020 (0.084)	-0.0024 (0.053)
Full time, 6-9	0.025 (0.073)	0.020 (0.070)	-0.041 (0.050)	-0.0040 (0.087)	-0.063 (0.055)
Part time, 2-5	-0.13* (0.071)	-0.13* (0.068)	-0.069 (0.048)	-0.15* (0.084)	-0.067 (0.052)
Part time, 6-9	-0.093 (0.072)	-0.096 (0.069)	-0.10** (0.049)	-0.15* (0.086)	-0.12** (0.054)

Note: Each cell represents coefficients from an IV regressions on equation 1. A year dummy and calendar week fixed effects are included in all specifications. Included in specification (2)-(5) are a set of socio-economic background characteristics explained in Table 3. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The number of observations varies with outcomes and window, for the baseline window of  $\pm 6$  weeks the number of observations is 17223, 17209, 17239, 17220, 17239 and 17220 for the respective outcomes.

## 5.4 Paternity leave and children's school performance

As mentioned in Section 4, our measure of cognitive skills is based on the national written external examination undertaken by all Norwegian students at the end of 10th grade (the end of lower secondary school).

Table 11 presents regression results for exam scores. There are no clear and robust reform effects. In the wider windows there is a positive effect. Disregarding students born close to the reform date the estimated effect is ten percent of a standard deviation, and statistically significant at the ten percent level. Other than this, none of the estimates are significant. While there is an insignificant, negative effect in the baseline window, the effect is very close to zero when we exclude the students who are born close to the reform date.<sup>25</sup>

As argued in the introduction, and as explicitly stated in the political motivation for the reform, introducing a paternal quota may have lasting and dynamic consequences. Although we do not find that mothers' work more when fathers take paternity leave, it may still be that

<sup>25</sup>Several studies, surveyed in Almond and Currie (2010), indicate that it is easier to intervene with girls than with boys. We have tested this and find no significant heterogenous effect with respect to the child's gender.

Table 11: The impact of paternity leave on school performance at age 16

	(1)	(2)	(3)	(4)	(5)
	No controls	Baseline	Double-window	Baseline, no BT	Double, no BT
Father takes leave	-0.090 (0.086)	-0.022 (0.079)	0.068 (0.055)	0.0052 (0.097)	0.100* (0.060)
Observations	16474	16474	31723	11015	26264
$R^2$	.	0.169	0.174	0.168	0.174

Note: Each column provide results from an IV regressions on equation 1. A year dummy and calender week fixed effects are included in all specifications. Included in specification (2)-(5) are a set of socio-economic background characteristics explained in Table 3. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

paternity leave increases the *relative* importance of fathers, since fathers' involvement in child rearing may increase from a low level. The effect of paternity leave on school performance may therefore be dependent on fathers' and mothers' characteristics. In a related study to ours, Liu and Skans (2010) find that the duration of general parental leave, typically taken by mothers, has no effect of school performance for the average child. However, they do find positive effects for children of mothers with tertiary education, suggesting that the parental leave reform strengthened the relationship between maternal education and school performance.

In Table 12 we report heterogenous effects of paternity leave according to whether the father's level of education is higher, lower or equal to that of the mother.<sup>26</sup> 25.5% of students belong to the first group ( $F_i < M$ ) 27.6% of students belong to the second group ( $F_i = M$ ) and 46.9% of students belong to the third group ( $F_i > M$ ).

The last three rows of table 12 provide p-values from test of equality of the estimated coefficients. There are some differences between the three groups, but few of these are statistically significant. In column (5) we see that equality for the extreme groups is rejected at the 10 percent level. Although the estimates are generally not statistically significant and vary considerably between the different windows, we consistently get negative estimates for the students whose mothers have higher education than the fathers, and a positive estimates when mothers and fathers have similar education levels. The estimates for the group with more highly educated fathers vary most, but are positive in the wider windows.

<sup>26</sup>First-stage results are reported in Appendix table 16. An alternative way of addressing this question is to estimate group-specific equations, which provides similar estimates. We prefer to interact the group indicators, as this increases precision and facilitates comparison of the group-specific coefficients.



Table 12: The impact of paternity leave on school performance at age 16: Interaction effects with parental educational groups

	(1)	(2)	(3)	(4)	(5)
	No controls	Baseline	Double	Baseline, no BT	Double, no BT
Father takes leave					
- father highest ( $F > M$ )	-0.16 (0.17)	-0.063 (0.15)	0.16 (0.11)	-0.018 (0.20)	0.22* (0.12)
- equal education ( $F = M$ )	0.043 (0.13)	0.081 (0.12)	0.086 (0.084)	0.17 (0.15)	0.12 (0.091)
- mother highest ( $F < M$ )	-0.19 (0.16)	-0.14 (0.14)	-0.039 (0.10)	-0.21 (0.17)	-0.051 (0.11)
Observations	16474	16474	31723	11015	26264
$R^2$	0.002	0.169	0.174	0.165	0.174
p-value ( $F > M$ ) = ( $F = M$ )	0.34	0.46	0.60	0.44	0.51
p-value ( $F > M$ ) = ( $F < M$ )	0.89	0.73	0.18	0.46	0.089
p-value ( $F = M$ ) = ( $F < M$ )	0.25	0.25	0.34	0.090	0.22

Note: Each column provide results from an IV regressions on equation 1. A year dummy and calender week fixed effects are included in all specifications. Included in specification (2)-(5) are a set of socio-economic background characteristics explained in Table 3. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

## 5.5 Paternity leave and family outcomes

Table 13 provides results for family outcomes. The point estimates in the baseline specification tend to go in the opposite direction of what proponents of the parental quota would have hoped for, for example we find positive effects of paternity leave on the probability of having a divorce and child spacing. The child spacing effect is the only one which is statistically significant at conventional levels, it is however no robust to extending the time window.

The effect on the index of family outcomes, presented in the bottom row of Table 13, are consistently negative effects, but statistically significant only in specification (1) and (3).<sup>27</sup> We conclude that our analysis does not suggest that paternity leave improves (or worsens) family outcomes. Ofcourse, our estimate is only the partial effect of the reform. On potential anticipatory effects of the reform on fertility our estimates are silent.

<sup>27</sup>We don't include outcomes based on leave taken with the next child when constructing the index.

Table 13: The impact of paternity leave on family outcomes

	(1)	(2)	(3)	(4)	(5)
	No controls	Baseline	Double	Baseline, no BT	Double, no BT
Mother's parity	-0.055 (0.074)	-0.021 (0.058)	-0.051 (0.041)	0.051 (0.072)	-0.026 (0.045)
Father's parity	-0.063 (0.083)	-0.050 (0.062)	-0.064 (0.043)	-0.016 (0.075)	-0.052 (0.047)
Divorce by child age 14	0.048 (0.049)	0.038 (0.049)	0.038 (0.049)	-0.015 (0.067)	-0.012 (0.067)
Next child together	-0.065* (0.037)	-0.063* (0.036)	-0.041 (0.026)	-0.024 (0.044)	-0.018 (0.029)
Child spacing	158.7** (73.7)	172.5** (71.1)	65.7 (52.3)	160.3* (87.4)	34.9 (57.9)
Mother's leave next child	5.38 (4.61)	4.99 (4.59)	2.94 (3.33)	6.29 (5.66)	2.87 (3.68)
Father's leave next child	0.67 (2.97)	0.72 (2.94)	1.50 (2.18)	-0.60 (3.73)	1.06 (2.45)
Index of outcomes	-0.11* (0.061)	-0.083 (0.052)	-0.077** (0.037)	-0.022 (0.063)	-0.050 (0.041)

Note: Each cell represents coefficients from an IV regressions on equation 1. A year dummy and calendar week fixed effects are included in all specifications. Included in specification (2)-(5) are a set of socio-economic background characteristics explained in Table 3. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The number of observations varies with outcomes and window, for the baseline window of  $\pm 6$  weeks the number of observations is 17229, 17229, 8471, 9992, 8675, 8646, 5482 and 17229 for the respective outcomes.

## 5.6 Characterizing compliers

Since any instrumental variable strategy identifies effects only for the subpopulation affected by the instrument, external validity is always a concern (Moffitt (2005)). It is therefore useful to characterize families whose behavior was actually affected by the paternal quota reform - ‘compliers’ in the terminology of Imbens and Angrist (1994). If the compliant subpopulation is similar to the general population, the case for extrapolating estimated causal effects to the general population is stronger.

Table 14 presents the likelihood that a complier has a particular characteristic relative to the population of eligible families. This is obtained by taking the ratio of the first stage across a particular covariate group to the overall first-stage (Angrist and Pischke (2009), Angrist and Fernandez-Val (2010)).<sup>28</sup>

Fathers in the compliant subpopulation have somewhat higher education and age than what is the average for fathers in our sample of eligible families. When it comes to income and work experience, there are slightly fewer compliant fathers in both the lowest and highest quartiles. Overall, fathers’ characteristics in the compliant subpopulation are not very different from eligible families in general. For mothers’ characteristics, the differences are larger: Compliant mothers only have less than half the propensity to belong to the lowest income quartile. They are also more highly educated. The difference regarding mothers’ income between the compliant subpopulation and eligible families in general should be kept in mind when interpreting the effects reported above.

---

<sup>28</sup>We report results for all covariates we use in our regression analysis. While ‘annual income’ and ‘years of employment’ enters linearly in our regression analysis, we have split them in quartiles for the purpose of characterizing the complier group.

Table 14: Complier characteristics ratios

	Father's	Mother's
lower sec. or less	0.899	0.795
upper secondary	0.995	1.037
higher ed. lower level	1.202	1.193
higher ed. higher level	0.973	1.105
age 20-24	0.77	0.788
age 25-29	1.073	1.133
age 30-34	1.003	1.099
age 35-44	1.061	1.059
income quartile 1	0.947	0.449
income quartile 2	1.085	0.943
income quartile 3	1.086	1.317
income quartile 4	0.879	1.294
experience quartile 1	0.973	0.914
experience quartile 2	1.153	1.009
experience quartile 3	0.99	1.104
experience quartile 4	0.875	1.001

Note: Each cell represents the ratio between the first stage estimate for particular covariate group relative to the overall first stage. The ratios can be interpreted as the likelihood that the compliant subpopulation have a certain feature relative to the likelihood of that same feature among all eligible families. Which feature is given by the table's row names, and whether the feature applies to mothers or fathers is given by the column name.

## 6 Conclusion

The combination of high fertility and high labor force participation in the Nordic countries is often attributed to family friendly policies such as highly subsidized child care and generous parental leave schemes. In this paper we analyze a feature of parental leave policies common to the Nordic countries, namely that part of the leave is reserved for the father of the child.

We find no evidence that paternity leave leads to increased equality in the labor market. We document that earnings of both mothers' and fathers' are adversely affected by paternity leave. The negative effect for mothers seem at least partly to be driven by a reduction in labor supply. In families where fathers take paternity leave fewer mothers work more than 20 hours per week. For fathers, on the other hand, there is no evidence that the documented reduction in earnings stem from a reduction in labor supply. This may be due to the lack of reliable data on overtime worked.

We argue that these effects should be given a causal interpretation, as the variation in paternity leave that we rely on comes from the sudden introduction of the four week quota of paternity leave.

That mothers seem to respond to paternity leave by reducing labor supply is at odds with the standard economic framework emphasizing specialization in household vs. labor market work, where one would expect mothers to respond to fathers' increased involvement at home by increasing their effort and time put in to market work. Further investigation into the mechanisms behind the negative effects on mothers' earnings and labor supply is needed. Candidate explanations are, first, that mothers' and fathers' investment in children are complements. Though in need of further research, this hypothesis is not immediately supported by our results on children's school performance, on which we fail to find any effects of paternity leave.

A second explanation could be that mothers' and fathers' time at home are complements. Our analysis suggests that family oriented policies, even if directed towards fathers, may be ill-suited at reducing earnings differentials between men and women.

Still, a weakness of our study is that it only captures the *partial* effects of paternity leave on

those treated by the paternal quota reform. There may of course in addition be important general equilibrium effects of fathers taking parental leave to an increasing extent. If, for instance, paternity leave leads to less discrimination against women in the labor market, this would not be captured by our estimates. It would therefore be useful to complement our findings with studies based on alternative identification strategies.

## References

- Almond, D. and Currie, J. (2010). Human capital development before age five. In Ashenfelter, O. and Card, D. E., editors, *Handbook of Labor Economics*, volume 4. North Holland.
- Angrist, J. and Fernandez-Val, I. (2010). Extrapolate-ing: External validity and overidentification in the late framework. NBER working paper 16566.
- Angrist, J. D. and Krueger, A. B. (2001). Instrumental variables and the search for identification: From supply and demand to natural experiments. *Journal of Economic Perspectives*, 15(4):69–85.
- Angrist, J. D. and Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, NJ: Princeton.
- Baker, M. and Milligan, K. (2010). Evidence from maternity leave expansions of the impact of maternal care on early child development. *Journal of Human Resources*, 45:1–32.
- Becker, G. S. (1985). Human capital, effort, and the sexual division of labor. *Journal of Labor Economics*, 3(1):pp. S33–S58.
- Brandth, B. and Øverli, B. (1998). Omsorgspermisjon med ”kjærlig tvang”: en kartlegging av fedrekvoten. *Rapport. Trondheim: Allforsk*.
- Brenn, T. and Ytterstad, E. (1997). Daglige fødselstall for norge 1989-93. *Tidsskrift for den norske legeforening*, 117:1098–101.

- Buckles, K. and Hungerman, D. M. (2008). Season of birth and later outcomes: Old questions, new answers. NBER working paper 14573.
- Carneiro, P., Løken, K., and Salvanes, K. G. (2010). A flying start? long term consequences of time investments in infants in their first year of life. Memo University of Bergen.
- Datta Gupta, N. and Smith, N. (2002). Children and career interruptions: The family gap in denmark. *Economica*, 69(276):609–29.
- Deming, D. (2009). Early childhood intervention and life-cycle skill development: Evidence from head start. *American Economic Journal: Applied Economics*, 1(3):111–34.
- Dustman, C. and Schönberg, U. (2009). The effect of expansions in maternity leave coverage on children's long-term outcomes. Memo University College London.
- Ekberg, J., Eriksson, R., and Friebel, G. (2005). Parental leave - a policy evaluation of the swedish 'daddy month' reform. IZA Discussion Paper No. 1617.
- Gans, J. S. and Leigh, A. (2009). Born on the first of july: An (un)natural experiment in birth timing. *Journal of Public Economics*, 93(1-2):246 – 263.
- Imbens, G. W. and Angrist, J. D. (1994). Identification and estimation of local average treatment effects. *Econometrica*, 62(2):467–475.
- Johansson, E.-A. (2010). The effect of own and spousal parental leave on earnings. IFAU working paper 2010:4.
- Kling, J. R., Liebman, J. B., and Katz, L. F. (2007). Experimental analysis of neighborhood effects. *Econometrica*, 75(1):83–119.
- Kluve, J. and Tamm, M. (2009). Now daddy's changing diapers and mommy's making her career: evaluating a generous parental leave regulation using a natural experiment. Ruhr Economic Papers #145.

- Lalive, R. and Zweimüller, J. (2009). How does parental leave affect fertility and return to work? evidence from two natural experiments. *Quarterly Journal of Economics*, 124(3):1363–1402.
- Liu, Q. and Skans, O. N. (2010). The duration of paid parental leave and children's scholastic performance. *The B.E. Journal of Economic Analysis and Policy*, 10(1).
- Moffitt, R. (2005). Remarks on the analysis of causal relationships in population research. *Demography*, 42(1):pp. 91–108.
- Rege, M. and Solli, I. (2010). The impact of paternity leave on long-term father involvement. CESifo Working Paper No. 3130.
- Strøm, B. (2004). Student achievement and birthday effects. Memo Norwegian University of Science and Technology.



## 7 Appendix

Table 15: Parental leave reforms in Norway

Date	Total parental leave	Compensation rate	Paternal quota
1.7.1977	18 weeks	100%	-
1.5.1987	20 weeks	100%	-
1.7.1988	22 weeks	100%	-
1.4.1989	24(30) weeks	100(80)%	-
1.5.1990	28(35) weeks	100(80)%	-
1.7.1991	32(40) weeks	100(80)%	-
1.4.1992	35(44.4) weeks	100(80)%	-
1.4.1993	42(52) weeks	100(80)%	4 weeks
1.7.2005	43(53) weeks	100(80)%	5 weeks
1.7.2006	44(54) weeks	100(80)%	6 weeks
1.7.2009	46(56) weeks	100(80)%	10 weeks
1.7.2011	47(57) weeks	100(80)%	12 weeks

Source: <http://www.nav.no/rettskildene/Rundskriv/183541.cms>.

Table 16: First stage results by parental education.

	(1)	(2)	(3)	(4)	(5)
	No controls	Baseline	Double	Baseline, no BT	Double, no BT
- father highest ( $F > M$ )	0.34*** (0.0083)	0.34*** (0.0083)	0.35*** (0.0061)	0.32*** (0.010)	0.34*** (0.0067)
- equal educ ( $F = M$ )	0.34*** (0.0091)	0.34*** (0.0091)	0.35*** (0.0065)	0.34*** (0.011)	0.35*** (0.0071)
- mother highest ( $F < M$ )	0.38*** (0.0093)	0.38*** (0.0093)	0.39*** (0.0067)	0.40*** (0.011)	0.40*** (0.0074)

Note: Coefficients for the other educational groups are included in the regressions, but not reported. A year dummy and calendar week fixed effects are included in all specifications. Included in specification (2)-(5) are a set of socio-economic background characteristics explained in Table 3. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .