

Paternity Leave*

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Abstract

This paper documents that a parental leave reform directed towards fathers causally impacts children's performance at the final exam at the end of compulsory schooling. School performance improves in families in which the father has higher education than the mother - implying that the effect of paternity leave depends on the care it displaces. Investigating data on parents' labor market outcomes yields no evidence of a Beckerian shift in gender specialization in the household.

Keywords: parental leave, child development, labor supply

JEL Classification: J13, J22, J24, I21

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1 Introduction

Paternity leave is often discussed as a policy measure to encourage greater gender equality, both in the family and the labor market. Politicians and policymakers in Northern Europe are strong believers that paternity leave strengthens women's position in the labor market, reduces the gender wage gap and promotes bonding between children and fathers.¹

Wishing to alter traditional patterns of household specialization, politicians provide incentives to increase men's involvement in the home. Even a few weeks of paternity leave, the argument goes, may result in substantial changes.² Thus Finland, Iceland, Norway and Sweden have all reserved a share of the parental leave for fathers. Similar proposals are also popular - though highly debated - in other European countries.³

In this paper we investigate how paternity leave impacts a broad range of outcomes using Norwegian register data. To handle the selection problem we use a parental leave reform, implemented on April 1, 1993, to evaluate the causal effects of paternity leave on children and parents. The main feature of the reform was the introduction of a four-week paternal quota. This reform caused a drastic change in fathers' leave-taking behavior.

Using a difference in difference approach, we find that children's school performance improves in families in which the father has higher education than the mother. This points to the crucial importance of the counterfactual situation: As paternal care becomes relatively more important, the effect will depend on the quality of both the paternal care and the maternal care it displaces.

To learn about the mediating factors behind this finding, such as changes in gender roles within the household, we estimate how the reform impacts several parental out-

¹These views are articulated in a series of white papers, cp. 'Ligestilling for Likelønn' (Stortingsmelding nr. 6 (2010-2011)) in Norway and 'Reformerad Föräldraförsäkring - Kärlek, Omvårdnad, Trygghet' (SOU 2005:73) in Sweden.

²"To strengthen 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 Norwegian Government's Long term program for 1990-1993 (Stortingsmelding nr. 4), our translation.)

³Germany, for example, introduced in 2007 a two months paternity quota intended to provide incentives for parents to share home and market work equally ('Entwurf eines Gesetz zur Einführung des Elterngeldes' (Deutscher Bundestag Drucksache 16/1989, 20.06.2006).

comes. We find no statistically significant effect on fathers' earnings and working hours. However, we do find strong and statistically significant negative effects on women's labor market outcomes of their spouse taking paternity leave. The latter result must be interpreted with some caution, as the paternity quota reform also increased maternal leave taking in some families. However, our main analysis and robustness checks give no support to policy makers' contention that the paternal quota reform would counter the working hours and earnings differential between men and women. Finally, paternity leave has no robust effect on a set of family outcomes such as fertility and divorce rates, nor on the length of paternity leave taken with later children.

While several papers have investigated how maternity leave (or general parental leave) impacts parent (e.g. Lalive and Zweimüller (2009)) and child outcomes (cf. Dustman and Schönberg (2012), and the references therein), there are only a handful studies aimed at identifying causal effects of paternity leave. These studies use Scandinavian paternal quota reforms and focus on parental outcomes. Using paternal quota reforms in Sweden, Ekberg et al. (2005) find no evidence that paternity leave affects the extent to which fathers care for children when they are sick, whereas Johansson (2010) finds no causal effect on mothers' and fathers' earnings. The precision of the estimates in both studies is very low. Rege and Solli (2010) find a negative impact of paternity leave on fathers' earnings in Norway using a differences-in-differences strategy on a sample of fathers with children in different age groups before and after the reform. Kotsadam and Finseraas (2011b,a), using Norwegian survey data, find long-lasting effects of paternity leave on the division of household work within the family. We are not aware of any previous studies on how children's cognitive outcomes are affected by paternity leave.

The rest of the paper is organized as follows: Section 2 discusses the institutional setting. Section 3 presents our empirical strategy. In Section 4 we describe our data and the various outcome measures that we use. In Section 5 we present the results. Section 6 concludes.

2 Paternity leave in Norway

In Norway, wage compensated parental leave has been extended repeatedly since the 1970s, from the 18 weeks of leave with full wage compensation first granted in 1977 to 47 weeks in 2011.⁴ Out of the total number of weeks, there has always been a share that the parents are free to share between them.

The parental leave scheme offers 100% wage compensation for both men and women (or they can choose 80% compensation and a longer leave period), but exceptions exist that are particularly relevant to men's right to full compensation. First of all, only parents who have worked 50% or more during at least six of the last ten months before the child's birth are eligible for wage compensated leave, and if the mother of a child does not fulfill this requirement, the right is also lost for the child's father.⁵ If the mother fulfills the requirement but works part time (between 50 and 100%), the father's compensation rate is reduced accordingly.

Second, income compensation also reaches an upper bound of six times the *basic amount* (*G*) of the Norwegian social security system.⁶ And third, until 2008, when self-employed individuals were granted rights to full compensation, the compensation rate for the self-employed was at 65% of their income.

The fact that men tend to take very little of the leave period that can be freely shared between the parents triggered the labor party government to introduce a paternal quota in their suggestion for the national budget of 1993. The reform was passed in parliament in December 1992. Following implementation on April 1, 1993, four of the now 42 weeks of paid leave were to be reserved for the child's father. Barring "special circumstances", families would lose the right to these four weeks unless taken by the father. For the father to be eligible for the paternal quota the mother had to resume work.⁷

⁴See Appendix Table 10 for a full description.

⁵Sick leave from employment, unemployment with right to benefits, and paid parental leave all count as work.

⁶*G* ("Folketrygdens grunnbeløp") 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).

⁷This requirement was relaxed in July 1994 (Brandth and Øverli (1998)).

Figure 1: Share of fathers taking leave, 1992-2006.

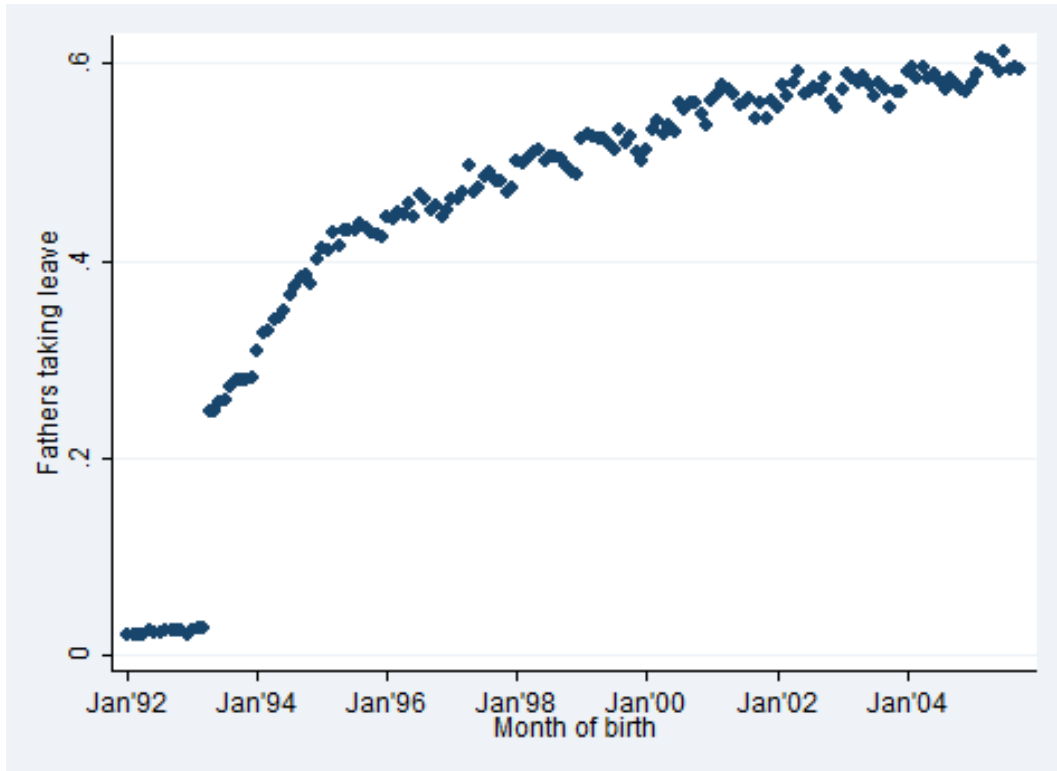


Figure 1, shows the fraction of fathers taking paternity leave, by the birth month of their child. There is a marked increase in the share of leave taking due to the reform, from 2.6% of fathers of children born in March 1993, to 24.6% of fathers of children born in April 1993. The share has continued to rise, and reached 60% in 2006. After the 1993 reform, every subsequent extension of parental leave in Norway has been fully absorbed in the paternal quota.

3 Identification: Reform as exogenous variation

Estimating the causal effects of paternity leave on parent, family and child outcomes is complicated by a selection problem. In families in which fathers take parental leave, both parents tend to be older, more educated and have higher income than in families in which fathers do not take parental leave. These families are likely to differ also with respect to

unobservable characteristics. We handle the selection problem by using the introduction of the paternal quota at April 1, 1993 as a source of exogenous variation in fathers' leave taking behavior.

This reform provides quasi-experimental variation in the uptake of paternity leave as long as there are no systematic differences between the families of children born before and after the reform date that also matter for the outcomes that we study. There are three potential pitfalls, to be discussed below.

3.1 Quasi-experiment not fully clean

The 1993 reform combined two reforms: A four-week paternal quota reform and a three-week extension of the leave period that could be shared between parents.⁸ In other words, there was a seven-week extension to a household's total parental leave, out of which four weeks were reserved for fathers (and three weeks were typically added to mothers' leave period).

Figure 2 gives the distribution of paternity leave spells for fathers of children born in a 26-week period surrounding April 1, 1993, in families who are eligible for parental leave.⁹ The 40% percent of fathers eligible for the paternal quota that do take leave take on average approximately 25 days of leave (five weeks), with almost three quarters taking exactly the four weeks of the quota. Only 6% of all eligible fathers take more than the quota, meaning that the 3 weeks of general parental leave extension were open to mothers.¹⁰

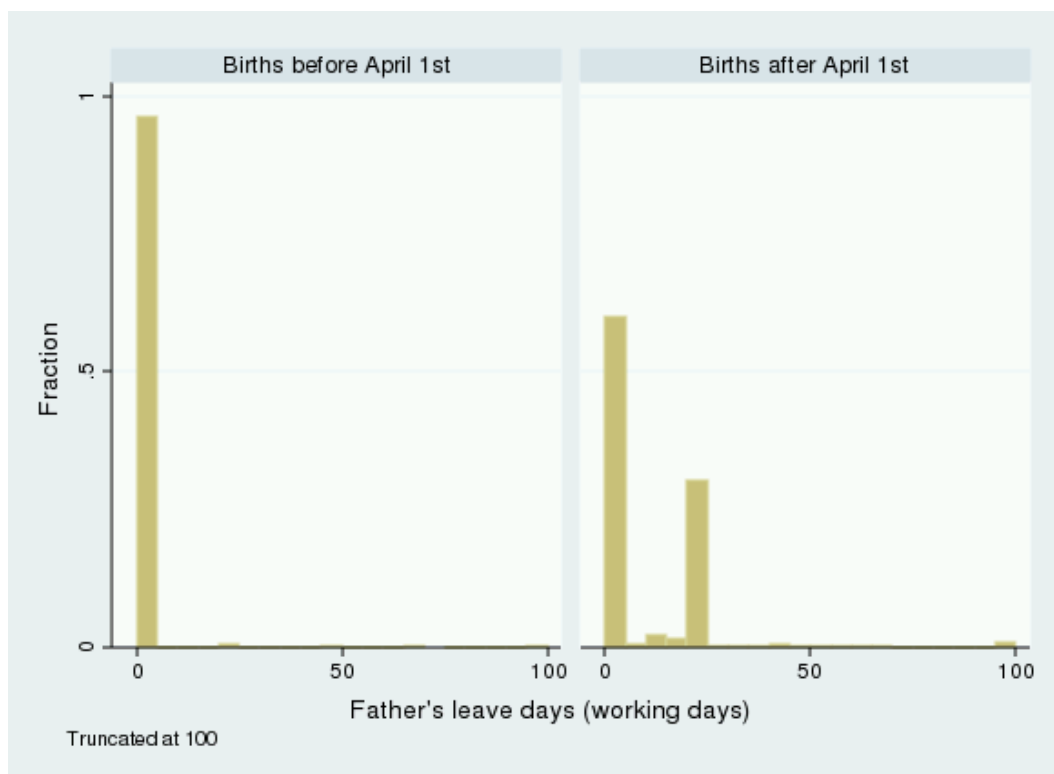
Because both parents' leave-taking behavior changes discontinuously at April 1 1993, we make additional use of a parental leave reform implemented exactly one year earlier.

⁸There was also a further change, which we believe has less significance. Before the reform, mothers were obliged to start their leave period at the latest two weeks before their due date. After the reform this was increased to three weeks.

⁹As mentioned in Section 2, not all families were eligible for wage compensated parental leave. Therefore, all of our analyses are restricted to eligible families. How eligibility status is defined and determined will be discussed in Section 3.5.

¹⁰Most fathers only have one spell of paternity leave - if any (the contingent probability is 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.

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

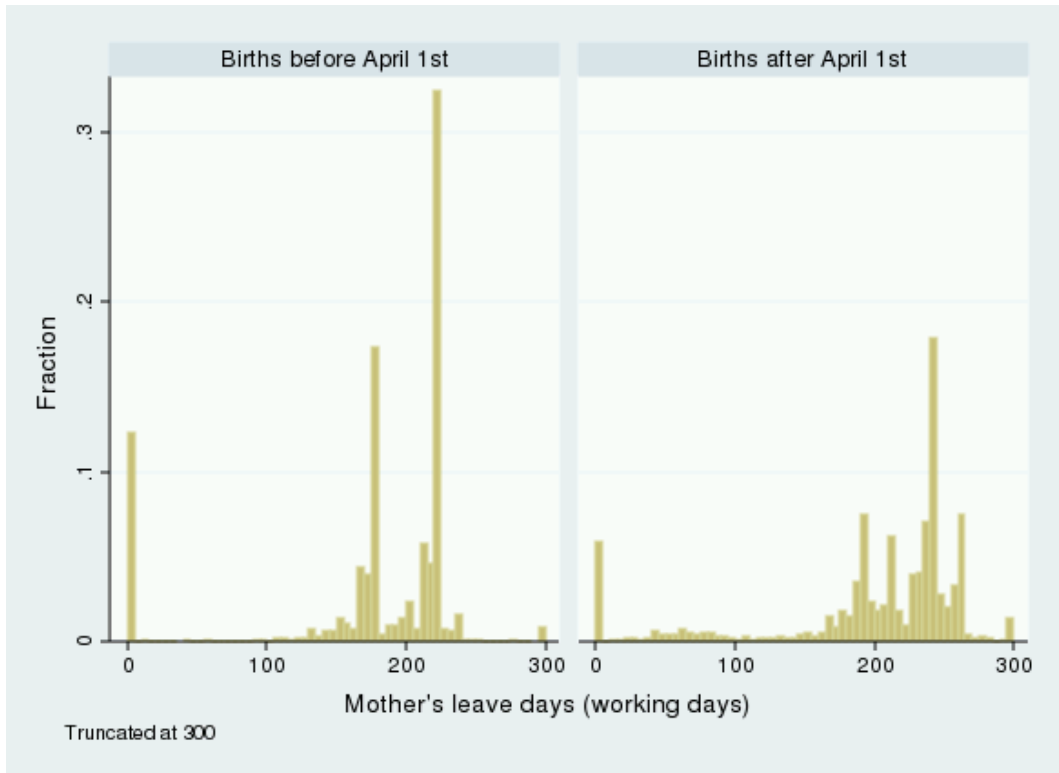


Note: The sample is fathers of children born in a 26-week period surrounding April 1, 1993, who were eligible for parental leave. Histogram bin width is five working days. For ease of exposure the number of leave days have been truncated at 100.

This parental leave reform included a three-week expansion of general parental leave. Based on the assumption that three weeks increase in general parental leave had similar effects in 1992 and 1993, a difference-in-differences approach, comparing the difference between the pre and post-reform cohorts in 1993 to that between corresponding cohorts in 1992, is suitable.

The paternity quota seems to have been enforced less strictly than the legislators intended. As mentioned above, families would normally lose the four weeks of parental leave unless they were taken by the father. However, exceptions could be granted by local caseworkers after a “special consideration of each individual case” (Ot. prp. nr. 13 (1992-93) p. 10). Figure 3 documents that a fairly large share of mothers got the paternal quota in addition to the rest of their leave. Before the reform there are three

Figure 3: Fraction of eligible mothers by number of leave days taken (working days).



Note: The sample is mothers with children born in a 26-week period surrounding April 1, 1993, who were eligible for parental leave. Histogram bin width is five working days. For ease of exposition the number of leave days have been truncated at 300.

spikes in maternal leave, one spike at zero, one spike at 35 weeks (reflecting maximum leave available at 100% income compensation) and one spike at 44 weeks (maximum at 80% income compensation). The additional bunching at 33, 34 and 42, 43 weeks reflects maximum leave taken for a child born before the due date.¹¹

After the reform the spikes identified in the pre-reform data shift to the right by three and four weeks, for 100% and 80% compensation respectively. In addition there are two spikes at four weeks more than the expected maximum, indicating that the paternal quota was transferred from the father to the mother in a substantial fraction of eligible families. The share of mothers taking up to four weeks more than the expected maximum

¹¹The total amount of leave days varies with the compensation rate the parents choose (80 or 100%), the number of children born (multiple births give longer leave), and the extent to which the child is born before the due date (two (three) weeks of parental leave is potentially lost if the child is born early before (after) the reform).

days of leave is about 25%. In the remaining 35% of the families neither the mother nor the father took the paternal quota, indicating that it was foregone.¹²

The fact that the paternal quota was transferred from the father to the mother in approximately 25% of the households strains our identification strategy. As the transfers are unique to the 1993 reform, the problem is not relieved by the use of 1992 as comparison year. Especially for the analysis of the effects on women's labor market outcomes, the transferred leave from fathers to mothers in 25% of the cases poses a fundamental problem.

We address this problem by carrying out two types of robustness checks. First, we use 1991 as our comparison year instead of 1992. As there was no parental leave reform in April 1991, this means that none of the increase in maternal leave is differenced out when using 1991 for comparison. Hence, if our findings reflect a failure to control for increased maternal leave, they ought to become stronger when 1991 is the comparison year. Our results are robust to this specification. In general, the estimated impact of paternity leave is smaller when we use 1991 as our comparison year, indicating that the effects we report in our main analyses are not likely to be driven by increases in maternity leave.¹³ One might still object that, while the 1992 increase in maternity leave is random, the 1993 increase in maternity leave is partly random (the extra three weeks of general parental leave) and partly selected (the four weeks of paternal quota that are transferred to mothers in 25% of the families). If we assume that the impact of the random and of the non-random increase in maternity leave go in the same direction, however, we still expect the use of 1991 as comparison year to reinforce effects that are due to maternity leave.

Our second type of robustness check is to limit the sample to only those households

¹²The reform also gave rise to one further change in the distribution of mothers' leave. More mothers seem to take at least some leave, which is reflected in mothers taking very little leave (typically 50 to 100 days, i.e. much less than the days available). The fathers in these families have a somewhat higher propensity to take leave (about 50%), and the mothers tend to have somewhat higher pre-birth income. This may indicate that the reform induced both parents in some relatively high-income families to take leave. Changes in reporting practices that coincide with the introduction of the paternal quota may also explain this finding. We have, however, not been able to find any documentation indicating that this is the case.

¹³An alternative is to carry out a placebo estimation, comparing the effects of the 1992 reform to corresponding differences in means in 1991. This yields the same conclusion.

with a low predicted probability of transferring the paternal quota from the father to the mother. We run a linear probability estimation using only the predetermined covariates listed in Table 4 on the post-reform sample, and then drop those observations (pre- and post-reform) that have a predicted probability of transferring at .25. By limiting the sample in this way, we retain about half our original sample, with a realized transfer rate at about 15%. Though not a perfect sample, we believe that any bias due to transfers ought to at least be smaller in this sample. Our results are robust also to limiting our sample in this way.¹⁴

3.2 Differences related to birth season

The children in our post-reform cohort will 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 (2012)).

The age difference may also matter for the parents' 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, than mothers of children born after the reform, one might spuriously attribute to the reform what is in reality a mere child age effect. The difference-in-differences approach also addresses this problem. Using *week fixed effects*, we eliminate differences between families according to when their child is born.

¹⁴Tables with the results of both types of robustness checks are available from the authors upon request.

3.3 Strategic timing of births

The 1993 reform was a large reform in the history of the Norwegian parental leave scheme. 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 1 rather than just before.

We see little reason to suspect that parents could time conception in anticipation of the reform. The national budget in which the paternal quota was introduced became publicly available on October 7, 1992. At this time mothers who gave birth close to April 1, 1993 were already 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.¹⁵ Searches in newspaper archives also suggest that the date of implementation was not publicly available before the national budget was presented.¹⁶

Even if conception was not timed strategically, expectant parents with due dates close to April 1 could possibly postpone induced births or planned cesarean sections. Grytten et al. (2012) document that Norwegian mothers are able to influence their mode of delivery so it is plausible that parents may also be able to influence the timing of a non-spontaneous birth.¹⁷ Following Gans and Leigh (2009) we investigate this empirically.

We regress the daily number of births on a dummy variable for the reform (dummy equals one for dates after April 1, 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

¹⁵Of the previous 7 parental leave reforms in Norway, implementation dates varied between April 1 (in 1989 and 1992), May 1 (in 1987 and 1990) and July 1 (in 1977, 1988 and 1991).

¹⁶The 1992 reform, implemented April 1 1992, 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.

¹⁷The vast majority of births in Norway are spontaneous vaginal deliveries. In 1993 the fraction of children born by cesarean section was 12.4 percent, and of these deliveries, 59.4 percent were emergency operations. On average, 12 percent of vaginal deliveries in 1993 were induced, while 88 percent were spontaneous (Folkehelseinstituttet, <http://mfr-nesstar.uib.no/mfr/>).

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

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

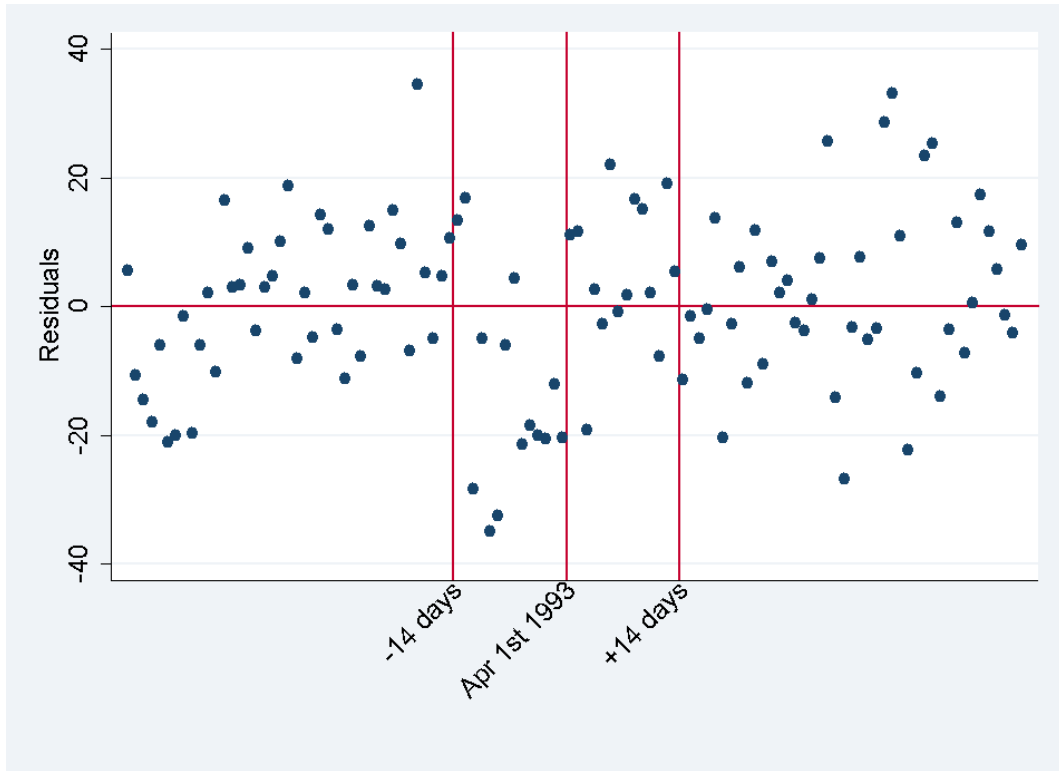
for 10 days during Easter.¹⁸ Our sample is daily births during the relevant time window (surrounding April 1) for the period 1975-2005, excluding 1989 and 1992 when parental leave reforms were implemented on April 1.

As is reported in Table 1, we 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. The estimate implies that a total number of 137 births, or about 5.7% of the births predicted to have occurred in the last two weeks of March, were moved from somewhere in the latter half of March to somewhere in the first half of April 1993.¹⁹ That the 1993 parental leave reform seems to have induced some parents to strategically time births is evident in the

¹⁸In Norway, the Thursday and Friday before and Monday after Easter day are public holidays.

¹⁹Following 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.

Figure 4: Daily births residuals



Note: Daily births residuals for the eight weeks centered around April 1, 1993 from a specification including 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 1 for the period 1975-2005 (excluding 1989 and 1992).

daily births residuals reported in Figure 4. This result has also been documented by Brenn and Ytterstad (1997).

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

3.4 Empirical specification

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

$$Y_i = \alpha Reform_i + \beta X_i + \delta_W Week_i + \delta_Y 1993_i + \epsilon_i, \quad (1)$$

in which i is the child/household/parent indicator. Y denotes the parent, family or child outcome of interest to be discussed in Section 4, and X is a vector of pre-birth controls. $Week$ is a vector of dummies indicating during which week of the year the child was born. By including this vector we eliminate inherent differences between families with children born at different times of the year. 1993 is a dummy indicating whether the child was born in 1993 or in 1992. ϵ is the error term. α is the parameter of interest. This will be an intention to treat (ITT) estimate, i.e., the estimated effect of being exposed to the reform, irrespective of whether the father takes leave. A similar empirical set-up has been used by, for instance, Lalive and Zweimüller (2009) and Dustman and Schönberg (2012) to estimate how parental leave reforms directed towards mothers affect parent and child outcomes.

We have seen that the paternal quota increased both paternal and maternal leave taking. In families in which mothers took the paternal quota this could be expected to reinforce rather than change traditional roles in the household. However, while increasing paternal leave from zero to four weeks may produce a qualitative change, the marginal effect of maternity leave, when it already is over 30 weeks, is likely to be much smaller. The ITT estimates are therefore arguably tapping the causal effects of paternity leave.

For child outcomes, the effect of paternity leave may depend on parents' *relative* "skill levels". In particular, this seems reasonable if paternity leave sets off a dynamic in which the father is more involved with his child and the mother becomes relatively less important, i.e. that maternal care to some extent is replaced by paternal care.²⁰ More

²⁰This is in line with Becker (1985, 1991) and the stated intentions of Norwegian policy makers.

specifically, we may expect to find a positive effect on cognitive skills when care from a highly educated father displaces that of a less educated mother.²¹ For child outcomes we therefore report heterogeneous effects of paternity leave according to whether the father has higher education than the mother, the parents have equal levels of education, or the mother has higher education than the father.

3.5 Eligibility and sample criteria

As mentioned in Section 2, eligibility for paid parental leave is contingent on having worked 50% or more during at least six of the last ten months before the child's birth. Fathers' right to paid leave depends in addition on the child's mother having worked the required amount.

To avoid low precision in our estimates, it would be preferable to analyze a sample consisting of families who were actually affected by the reform. As we do not perfectly observe eligibility status, we use parents' income history to determine our sample. The trade off is between using a strict income requirement and excluding families from our sample who were, in fact, eligible, and using a less strict income requirement and including families who were not in fact eligible. The first type of error may affect the generalizability of our results, whereas the latter may give imprecise estimates.

We chose to use the middle ground requirement that for a family to be included in our sample both parents must have an income above twice the 'basic amount' (2G) of the Norwegian social security system (see footnote 6) during the calendar year two years prior to the child's birth.

54% of all families fulfill this criterion. Looking at the leave taking behavior of mothers (our most certain indication of eligibility), the criterion strikes a balance between excluding and including: In 88% of the families included by this criterion, the mother

²¹If relative education is indeed what matters, we may also expect to find some sign of a more positive effect for highly-educated fathers, irrespective of the mother's education. However, this approach is likely to severely understate the potential effect of parental leave, because of the high correlation in parents' education.

is registered with paid maternity leave, whereas the number is only 41% in the families excluded. There is little to gain in terms of improving the former score by moving the income requirement up to three times the basic amount, as the share of mothers taking leave increases only marginally. There is, however, something to be lost, as 90% of the families thus excluded are registered with paid maternity leave.

Income the year before the child's birth is likely to give a precise indication of eligibility. However, as information on both the 1992 and 1993 reforms became public in October of the previous year, there is some scope for parents to select into eligibility. We do find some weak indication of this: Being treated by the reform is associated with a 1.0 percentage point higher probability of having income above 2G in 1992 (t-value = 1.80), also controlling for the other covariates used in the analysis. To ensure that none of our results are driven by selection into eligibility, we therefore use income data two years before the child's birth to determine eligibility. This reduces the association between treatment and eligibility status to .7 percentage points (t-value = 1.41), but otherwise has little impact on our results.²²

3.6 Time window

We face a trade-off between low bias and high precision when choosing the time window for estimating (1). In a narrow time window there is less chance that our main estimates are contaminated by omitted variables. A broader window would provide more precision by increasing the number of observations.

To balance these concerns we use a ± 13 week window as our baseline in all specifications. We also report results on a ± 7 week window. In line with the discussion of parents' strategic timing of births, we exclude observations from the two weeks before and the two weeks after April 1, but we also report results for our baseline window in which these weeks are included.

²²49% percent of the families have income above 2G both years, 38% below both years, for 5% income decreases from above 2G two years before the birth to below one year before and for 9% income increases from below 2G to above.

4 Data

4.1 Child outcomes

Given their young age, there is limited register information on these children. We do however have data on school performance (in 2009) from administrative registers.²³ 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 on 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.²⁴

The school performance sample is not identical to the samples for parental and family outcomes. This is because in the school sample, the unit of observation is each student completing compulsory schooling, while in the other samples, the unit is the family. We have some school results for about 95% of the total relevant cohorts of 16 years olds. For some of these children (about 5%) we do not observe an exam score. 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.

4.2 Labor market outcomes

Statistics Norway provides data on yearly "personal income" going back to 1967 for the entire Norwegian population. The personal income consists of wages, pensions and

²³In June, 2012, the first children exposed to the 1993-reform will complete upper secondary schooling. In future work we intend to analyze results from upper secondary education.

²⁴Exam grades have a standard deviation close to one, such that this standardization has limited impact, and the coefficients are also close to the estimated effects in units of grade points.

entrepreneurial income. In our analyses, 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.²⁵ 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 is registered with at least 20 hours (which we classify as part-time) or at least 30 hours (which we classify as full-time) of employment per week. The dummy variables are set to zero otherwise. Due to imperfect observability, the analysis does not take working hours for the self-employed into account.²⁶

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)). We construct the aggregated outcomes by normalizing all outcome variables to have a zero mean and a standard deviation of one, and then averaging over these outcomes.

Table 2 shows the variation in the labor market data based on averages from when the child is 2-5 years old, 6-9 years old and 10-14 years old. For families with children born in 1993 (1992) ‘earnings 2-5’ refers to average yearly earnings in the period 1995 through 1998 (1994 through 1997). We do not report results for earnings based on data from when the child is one year old, since most parents will be taking part of their leave during this year.

²⁵This data set is used in several previous studies of the Norwegian labor market. Bratsberg and Raaum (2010), for example, use this data set to analyze how immigrant employment affect wages in the construction sector.

²⁶Working hours will therefore on average be somewhat underestimated. This is not a problem for our analysis as long as self-employment status does not depend on being treated by the reform. We have studied the impact of the reform on two different measures of self-employment, and we find a positive but generally not statistically significant effect on both mothers’ and fathers’ probability of being classified as self-employed. Our results on working hours are robust to excluding the self-employed thus defined from the analysis.

Table 2: Summary statistics, labor market outcomes.

	Mean	SD
<i>Fathers</i>		
- earnings, 2-5	295.3	(126.6)
- earnings, 6-9	344.5	(166.2)
- earnings, 10-14	390.4	(198.2)
- full time, 2-5	0.78	(0.35)
- full time, 6-9	0.78	(0.35)
- full time, 10-14	0.76	(0.37)
- part time, 2-5	0.79	(0.33)
- part time, 6-9	0.80	(0.34)
- part time, 10-14	0.78	(0.36)
<i>Mothers</i>		
- earnings, 2-5	159.2	(80.8)
- earnings, 6-9	185.2	(101.5)
- earnings, 10-14	237.2	(116.4)
- full time, 2-5	0.42	(0.41)
- full time, 6-9	0.43	(0.42)
- full time, 10-14	0.51	(0.43)
- part time, 2-5	0.57	(0.40)
- part time, 6-9	0.60	(0.40)
- part time, 10-14	0.67	(0.39)
N	26173	

Note: Sample is children born during the 26 weeks surrounding April 1, either in 1992 or 1993, excluding two weeks before and after April 1, divided into those born during the 13 weeks preceding the reform and those born during the first 13 weeks after the reform. 20 hours of work or more per week is classified as part-time, 30 hours or more is classified as full-time. Earnings are given in constant 1998 NOK and are measured in 1000s.

In our sample, 76-78% of fathers work full time, and an additional 1-2% work part time. The numbers are stable across child ages. Mothers work less: When the child is 2-5 years old, 42% work full time, while an additional 15% work part time. The numbers increase as the child grows older.

4.3 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

Table 3: Summary statistics, family outcomes.

	Mean	SD
Mother's parity	2.52	(0.84)
Father's parity	2.63	(0.94)
Prob(Break up by child aged 14)	0.32	(0.47)
Prob(Next child together)	0.87	(0.33)
Child spacing (years)	3.53	(1.81)
Father's leave next child (days)	25.0	(26.9)
N	26173	

Note: Sample is children born during the 26 weeks surrounding April 1, either in 1992 or 1993, excluding two weeks before and after April 1, divided into those born during the 13 weeks preceding the reform and those born during the first 13 weeks after the reform.

breaking up (defined as either parent being registered as either divorced or a single parent at some point during the period from the child is 1 to 14 years old), the probability that the father has his next child with the same woman (conditional on having another child), child spacing and the number of days parent take leave if they have another child. Table 3 gives descriptive statistics.

4.4 Control variables

Table 4 give descriptive statistics for pre-birth characteristics for eligible parents whose children were born within a thirteen-week window prior or subsequent to April 1, 1993, excluding two weeks before and after April 1.²⁷ Columns (1) and (2) present averages and standard deviations for the pre- and post-reform groups. Column (3) reports the estimated difference, and a test of equality for the two groups. Finally, column (4) presents a difference-in-difference estimate for each of the variables, comparing the differences between the pre- and post-reform groups with the corresponding differences for children born in 1992.²⁸

The first thing to notice is how the 1993 reform changed the fathers' leave-taking

²⁷The characteristics are the same as those we control for in the regression. However, in Table 4 the categories for three, four and five or more older children have been combined for ease of exposition.

²⁸The descriptive statistics presented here are based on the school performance sample. Descriptive statistics based on the sample of births are very similar.

Table 4: Descriptive statistics

	(1)		(2)		(3)		(4)	
	Pre-reform Mean	SD	Post-reform Mean	SD	Difference Estimate	SE	Diff-in-diff Estimate	SE
<i>Fathers</i>								
- % take leave	3.86	(19.3)	39.9	(49.0)	36.1***	(0.64)	35.5***	(0.70)
- no. leave days	2.02	(12.7)	10.2	(19.5)	8.14***	(0.28)	7.52***	(0.34)
- % age < 25	2.96	(16.9)	3.39	(18.1)	0.43	(0.30)	0.57	(0.44)
- % age 25-29	26.9	(44.4)	27.6	(44.7)	0.60	(0.75)	0.85	(1.07)
- % age 30-34	36.6	(48.2)	38.6	(48.7)	1.97**	(0.82)	1.01	(1.16)
- % age > 34	33.5	(47.2)	30.5	(46.0)	-3.00***	(0.79)	-2.42**	(1.11)
- % lower sec. or less	36.9	(48.3)	36.0	(48.0)	-0.91	(0.82)	-0.46	(1.15)
- % upper secondary	35.1	(47.7)	34.9	(47.7)	-0.21	(0.81)	-1.07	(1.13)
- % higher ed. ≤ 4 yrs	19.3	(39.5)	20.5	(40.4)	1.23*	(0.68)	1.81*	(0.95)
- % higher ed. > 4 yrs	8.68	(28.2)	8.57	(28.0)	-0.12	(0.48)	-0.29	(0.68)
- annual income	253.8	(118.9)	250.9	(107.1)	-2.92	(1.92)	-5.30**	(2.60)
- % has no children	37.9	(48.5)	36.2	(48.1)	-1.66**	(0.82)	1.32	(1.16)
- % has one child	39.6	(48.9)	40.7	(49.1)	1.09	(0.83)	-0.59	(1.17)
- % has two children	16.6	(37.2)	17.6	(38.1)	1.05*	(0.64)	0.13	(0.89)
- % has ≥ three children	5.96	(23.7)	5.48	(22.8)	-0.48	(0.39)	-0.86	(0.55)
<i>Mothers</i>								
- % take leave	87.7	(32.8)	94.1	(23.5)	6.43***	(0.48)	7.63***	(0.75)
- no. leave days	174.2	(73.4)	199.3	(74.4)	25.0***	(1.25)	10.9***	(1.73)
- % leave days ≥ 100	86.8	(33.9)	87.3	(33.3)	0.53	(0.57)	1.85**	(0.83)
- % age < 25	10.0	(30.0)	10.4	(30.6)	0.41	(0.51)	0.43	(0.75)
- % age 25-29	38.8	(48.7)	40.6	(49.1)	1.76**	(0.83)	2.23*	(1.17)
- % age 30-34	34.2	(47.4)	33.9	(47.3)	-0.29	(0.80)	-1.02	(1.13)
- % age > 34	17.0	(37.6)	15.2	(35.9)	-1.87***	(0.62)	-1.64*	(0.86)
- % lower sec. or less	38.4	(48.6)	36.6	(48.2)	-1.77**	(0.82)	-0.91	(1.16)
- % upper secondary	30.2	(45.9)	31.7	(46.5)	1.57**	(0.78)	1.55	(1.10)
- % higher ed. ≤ 4 yrs	27.5	(44.7)	27.8	(44.8)	0.34	(0.76)	-0.047	(1.06)
- % higher ed. > 4 yrs	3.98	(19.5)	3.85	(19.2)	-0.13	(0.33)	-0.60	(0.46)
- annual income	175.5	(62.5)	174.4	(59.8)	-1.14	(1.04)	-2.61*	(1.45)
- % has no children	40.0	(49.0)	38.4	(48.6)	-1.60*	(0.83)	1.59	(1.17)
- % has one child	40.7	(49.1)	41.2	(49.2)	0.51	(0.83)	-1.03	(1.17)
- % has two children	15.2	(35.9)	16.7	(37.3)	1.51**	(0.62)	-0.13	(0.86)
- % has ≥ three children	4.13	(19.9)	3.71	(18.9)	-0.42	(0.33)	-0.42	(0.45)
N	6761		7168		13929		28173	

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. The sample used for column (1), (2) and (3) is children born during the 26 weeks surrounding April 1, 1993, excluding two weeks before and after April 1, divided into those born during the 13 weeks preceding the reform and those born during the first 13 weeks after the reform. In column (4) corresponding data for 1992 is used. For the difference and diff-in-diff estimates: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

behavior in our sample of eligible parents, as reported in Column (3). Fathers' propensity to take parental leave increased by 36 percentage points, and their average number of leave days taken increased by 8 (meaning an average increase of 22 work days for those fathers actually taking leave - slightly more than the 4 weeks of paternal quota). When we compare to the 1992 cohort in Column (4), the diff-in-diff estimates are very similar. There was thus very little change in fathers' leave-taking behavior around April 1, 1992.

Mothers' leave-taking behavior also changed. Average leave days taken increased by 25, i.e. ten days more than the general increase in the parental leave (the increase not reserved for the paternal quota), and also approximately ten days more than the 1992 increase. As discussed in Section 3.1, this probably reflects families in which the paternal quota is transferred to the mothers. The share of mothers taking leave also increases with the introduction of the reform. However, as there is little change in the share of mothers taking more than 100 days of leave, this seems to be driven by the fact that there are some mothers with very little leave after reform, as discussed in relation to Figure 3.

We include control variables for parents' age at birth of their child, their level of education and annual income the year before the child's birth, and the child's birth order. Education is measured on October 1 of the year before the child's 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. Birth order is controlled for by dummies for the number of children each parent already has, with six categories ranging from zero to five or more.

Other than parental leave, there are few statistically significant differences in the pre-birth characteristics between the pre- and post-reform 1993 cohorts. Furthermore, these are largely matched in the 1992 data, such that there is only two variables which has a difference-in-difference significant at the 5% level, and four more at the 10% level. With 26 variables tested, this is somewhat more than we would expect if there were no systematic differences.

5 Results

5.1 Characterizing compliers

Before moving to the main results it is useful to characterize families whose behavior was affected by the paternal quota reform - “compliers” in the terminology of Imbens and Angrist (1994). Table 5 presents the likelihood that a complier has a particular characteristic relative to the population of eligible families. This is obtained by running an auxiliary regression of the form (cf. Angrist and Pischke (2009), Angrist and Fernandez-Val (2010)):

$$PaternityLeave_i = \rho Reform_i + \gamma_W Week_i + \gamma_Y 1993_i + \nu_i. \quad (2)$$

First, we run this auxiliary regression for the entire population of eligible families. Unsurprisingly, given the descriptive statistics, we find that *Reform* is a strong predictor of paternity leave: The regression adjusted compliance rate is 0.36 for our baseline time window. Then, for each of several subsamples, each defined by a variable in Table 5²⁹, we run the auxiliary regression and take the ratio to the overall compliance ratio. Table 5 provides descriptive statistics on both “male reform compliers” and “female reform compliers” (i.e. families in which the mother took the paternity quota).³⁰

Both parents in the “male reform compliers” group have somewhat higher education, income and age than the average in our sample. Fathers’ in this subpopulation are much less likely to be self-employed relative to eligible families in general. In terms of relative education within the family, there are small differences, and in families in which the fathers have the highest relative education level the uptake rate is a bit lower than in the average eligible family.

The “female reform compliers” mirror the picture found for “male reform compliers”.

²⁹I.e. father has lower secondary or less education, mother has lower secondary or less, and so on.

³⁰Female reform compliers are defined as mothers who, after April 1, 1993, are registered with maternity leave that is one to twenty days longer than the maximum length available (when not counting the paternity quota) and for whom the father of the child did not take paternity leave.

Table 5: Complier characteristics ratios

	Male compliers		Female compliers	
Regression adjusted compliance rate	0.362		0.215	
Parents' relative education:				
- father highest educ ($F > M$)	0.923		0.956	
- equal educ ($F = M$)	1.052		1.047	
- mother highest educ ($F < M$)	1.041		0.99	
Families who had boys	1.014		1	
	Fathers	Mothers	Fathers	Mothers
Lower sec. or less	0.901	0.796	1.134	1.16
Upper secondary	0.999	1.044	1.065	1.069
Higher ed., lower level	1.162	1.173	0.809	0.798
Higher ed., higher level	0.995	1.154	0.677	0.548
Age 20-24	0.724	0.775	1.439	1.478
Age 25-29	1.04	1.022	1.108	1.054
Age 30-34	1.018	1.008	0.971	0.868
Age 35-44	0.973	1.087	0.883	0.821
Income quartile 1	0.861	0.66	1.327	1.205
Income quartile 2	1.098	0.947	0.94	1.062
Income quartile 3	1.112	1.158	0.884	0.943
Income quartile 4	0.929	1.242	0.845	0.787
Self-employed	0.534	1.008	1.592	0.563

Note: The first row gives the overall regression adjusted compliance rate. The ratios reported in the other rows can be interpreted as the likelihood that the compliant subpopulation has a certain feature relative to the likelihood of that same feature among all eligible families. The sample is parents of children born in the 26-week period surrounding April 1 in 1992 and in 1993 (minus the four weeks immediately around April 1), who were eligible for parental leave.

In these families both parents tend to be younger and less educated than the average eligible family. The father is much more likely to be self-employed than in the average eligible family. The regression adjusted transfer rate is 0.22.

5.2 Main Results

We now present our estimates of the causal effects of 1993 parental leave reform. For brevity, we present only the ITT in question (α in Equation 1). Tables including the coefficients on covariates are available upon request.

For every outcome, we run four different specifications. In Tables 6 through 8, column (1) shows the results from regressions estimating equation (1) for a ± 13 week window (excluding the ± 2 weeks that are affected by birth timing) without controls. In column (2) we have added the full set of family background variables available. This is our preferred specification. Column (3) shows results when the time window is reduced to ± 7 weeks. Lastly, in column (4) we have included the ± 2 weeks affected by birth timing in the ± 13 week window.

When results are discussed without explicit reference to one particular specification, the specification in column (2) is the one in question.

5.3 Children's school performance

In the upper panel of Table 6 regression results for exam scores at the end of 10th grade are presented. In our preferred specification (column (2)) we find a positive effect on average school performance of about 4 percent of a standard deviation, significant at the 10% level. The result is similar in the specification without controls (column (1)). When we reduce the time window the effect is imprecise and very close to zero (specification (3)). When the birth timing weeks are included (column (4)), the estimated effect is in about 3 percent of a standard deviation, but not significant.

In the bottom panel of Table 6 we report heterogeneous effects of paternity leave according to whether the father has higher education than the mother, the parents have equal levels of education, or the mother has higher education than the father. In our sample, 35.9% of students belong to the first group ($F > M$), 27.9% to the second ($F = M$), and 36.3% of students belong to the third group ($F < M$). Across these three groups the reform affected paternity leave uptake similarly (as documented in the section 5.1).

In families in which the fathers have the highest education level we find that paternity leave increases school performance by 7% of a standard deviation (statistically significant at the 5% level). If this effect comes solely from the families in which the father actually does take leave, this amounts to an average effect of about $1/5$ of a standard deviation

Table 6: Paternity leave and school performance at age 16

	(1)	(2)	(3)	(4)
	\pm Weeks 3-13	\pm Weeks 3-13	\pm Weeks 3-7	\pm Weeks 1-13
<i>Average effect</i>				
All students	0.041*	0.042*	0.003	0.030
	(0.024)	(0.022)	(0.031)	(0.020)
<i>Heterogeneous effects by parents' relative education</i>				
- father highest educ ($F > M$)	0.096**	0.072**	0.024	0.062*
	(0.039)	(0.036)	(0.052)	(0.033)
- equal educ ($F = M$)	0.046	0.052	-0.000	0.047
	(0.046)	(0.041)	(0.060)	(0.038)
- mother highest educ ($F < M$)	-0.018	0.006	-0.013	-0.014
	(0.039)	(0.036)	(0.052)	(0.033)
p-value ($F > M$) = ($F = M$)	0.410	0.707	0.759	0.773
p-value ($F > M$) = ($F < M$)	0.039	0.190	0.616	0.102
p-value ($F = M$) = ($F < M$)	0.286	0.401	0.873	0.222

Note: The top panel provides regression results for exam scores at the end of 10th grade. Each column provides ITT estimates from a regression based on equation 1. The bottom panel shows interaction effects with parental educational groups. A year dummy and calendar week fixed effects are included in all specifications. Included in specifications (2)-(4) are a set of socio-economic background characteristics explained in Table 4. Robust standard errors are in parentheses. The number of observations for the baseline window of ± 3 -13 weeks in which control variables are included (column 2) is 26941. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

in these families. This is a substantial effect, slightly smaller than the gender difference at the final exam.³¹ The effect is fairly stable across samples with respect to whether controls or birth timing weeks are included. The estimated effect for the most narrow time window is smaller, and also not statistically significant. However, the estimates for this window are not very precise, such that equality of the effect across columns (1) to (4) can not be rejected.

The estimated effect in families in which mother's education is the longest is consistently negative or very close to zero, but never statistically significant. The last three rows of table 6 provide p-values from tests of equality of the estimated coefficients. In the specification without controls, the hypothesis that the effect is the same for families in which the father has higher education than the mother as for families in which the mother has higher education than the father, is rejected at the 5% level. For the other

³¹The gender difference is about 1/4 of a standard deviation, girls tend to perform better. To put the results in further context, we note that the (unconditional) difference between students with fathers with and without completed upper secondary education is about 1/3 of a standard deviation.

specifications we are not able to reject at conventional levels that the estimates are the same.

Heterogeneous results by maternal education are found also by Liu and Skans (2010), who study the duration of general parental leave (typically taken by mothers) and children's school performance. Using Swedish data, Liu and Skans (2010) find positive effects of parental leave, but only for children of mothers with tertiary education. We differ in studying the parents *relative* education. This reflects a difference in the likely counterfactual. When the total level of parental leave is extended as in Liu and Skans (2010), maternal care typically replaces formal or informal child care. In our case, paternal care is likely to replace maternal care. In this case it is parents relative education, rather than the absolute level of parental education, that matters.

Time allocation data from the United States show that fathers of sons spend more time with their children than fathers of daughters (see survey by Lundberg (2005)). Fathers of sons are also found to be more involved in school work than fathers of daughters (Morgan et al. (1988)).³² Motivated by these findings we investigate whether the consequences of paternity leave differ according to the child's gender. We find a stronger effect of the paternity leave reform on girls' school outcomes, but the gender difference is not statistically significant.³³

5.4 Labor market outcomes

Table 7 shows the results from regressions based on equation (1), in which the dependent variables are the normalized averages of earnings and labor supply for fathers for the years when the child is 2-5 years old, 6-9 years old, and 10-14 years old, respectively. For fathers' earnings when the child is 2-5 years old we find negative effects of the same

³²In advanced countries first-born girls (versus first-born boys) are also found to reduce the stability of a marriage (Bedard and Deschênes (2005)), to increase fertility (Dahl and Moretti (2008)), and to positively affect mother's labor supply (Ichino et al. (2011)). In Scandinavia, however, fertility decisions indicate a preference for daughters rather than sons (Andersson et al. (2006)).

³³Results available upon request.

Table 7: Paternity leave and fathers' labor market outcomes

	(1)	(2)	(3)	(4)
	\pm Weeks 3-13	\pm Weeks 3-13	\pm Weeks 3-7	\pm Weeks 1-13
Earnings, 2-5	-0.028 (0.023)	-0.016 (0.017)	-0.018 (0.024)	-0.023 (0.015)
Earnings, 6-9	-0.004 (0.023)	0.002 (0.018)	0.006 (0.027)	-0.003 (0.017)
Earnings, 10-14	-0.003 (0.023)	-0.002 (0.019)	0.014 (0.027)	-0.012 (0.017)
Full time, 2-5	-0.004 (0.021)	-0.007 (0.020)	0.004 (0.029)	-0.001 (0.019)
Full time, 6-9	-0.014 (0.021)	-0.015 (0.021)	0.002 (0.030)	-0.014 (0.019)
Full time, 10-14	0.009 (0.021)	0.002 (0.021)	0.030 (0.030)	0.005 (0.019)
Part time, 2-5	-0.005 (0.021)	-0.009 (0.020)	0.005 (0.029)	-0.005 (0.019)
Part time, 6-9	-0.013 (0.021)	-0.016 (0.021)	-0.000 (0.030)	-0.013 (0.019)
Part time, 10-14	0.009 (0.021)	0.001 (0.021)	0.024 (0.030)	0.004 (0.019)

Note: Each cell provides ITT estimates from a regression based on equation 1. A year dummy and calendar week fixed effects are included in all specifications. Included in specifications (2)-(4) are a set of socio-economic background characteristics explained in Table 4. Robust standard errors are 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 ± 3 -13 weeks in which control variables are included (column 2) the number of observations is 25649, 25585 and 25502 for the respective earnings outcomes, and 25674 for the working hours outcomes.

magnitude as those reported by Rege and Solli (2010).³⁴ Scaled by uptake, the point estimate of -0.016 indicates that the paternity leave reform reduces father's earnings with 0.044 of a standard deviation. This effect is, however, not statistically significant. For earnings when the child is 6-9 and 10-14 years old the point estimates are close to zero and statistically insignificant. Also for working hours the effects are relatively small and statistically significant.

Table 8 shows the corresponding results on mothers' labor market outcomes. Contrary to what would be expected in a simple Beckerian framework, mothers' earnings are

³⁴The identification strategy of Rege and Solli (2010) 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 1993 paternity leave reform.

Table 8: Paternity leave and mothers' labor market outcomes

	(1)	(2)	(3)	(4)
	\pm Weeks 3-13	\pm Weeks 3-13	\pm Weeks 3-7	\pm Weeks 1-13
Earnings, 2-5	-0.080*** (0.023)	-0.053*** (0.018)	-0.051* (0.027)	-0.049*** (0.017)
Earnings, 6-9	-0.078*** (0.023)	-0.054*** (0.020)	-0.062** (0.029)	-0.053*** (0.018)
Earnings, 10-14	-0.060** (0.023)	-0.037* (0.020)	-0.033 (0.029)	-0.032* (0.018)
Full time, 2-5	-0.023 (0.021)	-0.012 (0.019)	-0.001 (0.028)	-0.017 (0.018)
Full time, 6-9	-0.032 (0.021)	-0.022 (0.020)	-0.001 (0.029)	-0.017 (0.018)
Full time, 10-14	-0.015 (0.021)	-0.008 (0.021)	0.004 (0.030)	-0.005 (0.019)
Part time, 2-5	-0.050** (0.020)	-0.040** (0.019)	-0.056** (0.028)	-0.038** (0.018)
Part time, 6-9	-0.060*** (0.020)	-0.053*** (0.020)	-0.068** (0.029)	-0.042** (0.018)
Part time, 10-14	-0.023 (0.021)	-0.019 (0.020)	-0.006 (0.029)	-0.010 (0.019)

Note: Each cell provides ITT estimates from a regression based on equation 1. A year dummy and calendar week fixed effects are included in all specifications. Included in specifications (2)-(4) are a set of socio-economic background characteristics explained in Table 4. Robust standard errors are 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 ± 3 -13 weeks in which control variables are included (column 2) the number of observations is 26150, 26112 and 26082 for the respective earnings outcomes, and 26159 for the working hours outcomes.

negatively affected by paternity leave, both in the short run (child age 2-5), the medium run (child age 6-9) and in the longer run (child age 10-14), though the latter estimates are somewhat smaller and statistically significant only at the ten percent level. In the baseline window, the effect is 0.053 of a standard deviation reduction in earnings during the first years, statistically significant at the one percent level. Scaled by uptake, this implies that the paternity leave reform reduces maternal earnings with 0.147 of a standard deviation. Point estimates and precision are fairly stable across time windows.

The estimated average reduction in earnings seems to be driven by a negative effect of paternity leave on mothers' working hours, more specifically the probability that mothers work part time or more. The estimated effects on work hours are comparable in size to

the effects on earnings.

Our results on working hours, thus, provide no support for the hypothesis that paternity leave will cause households to specialize less in line with traditional gender roles: It seems, rather, that traditional household specialization is intensified. Furthermore, the conjecture that paternity leave causes general earning differentials between men and women to decrease is not substantiated empirically.

The adverse effects of paternity leave on women's labor market outcomes may be due to complementarities in mothers' and fathers' time. If fathers choose to spend more time at home and less in the market due to a family policy that strengthens the ties between fathers and children, so do mothers.

Especially for the results on mothers' labor market outcomes, there is reason to worry that the causal mechanism goes not from paternity leave as identified with the paternal quota reform but rather through the increase in maternity leave that we document in Section 3.1. In order to assess this possibility, we investigate how results are affected by using 1991 as our comparison year instead of 1992. The main difference between these two years is that there was no parental leave reform during our time window in 1991. This means that using 1991 as a comparison year we do not difference out any effects of an increase in general/maternal leave. Hence, if maternity leave is what drives our results, they should become stronger when we use 1991. This is not the case.³⁵ It therefore seems unlikely that the changes in maternity leave are driving the results on mothers' labor market outcomes.

We have investigated whether the effects on parents' labor market outcomes differ according the child's gender. Like in the school performance analysis, the effects are stronger in families who had daughters. The results on mothers' labor market outcomes reported above, are driven by the sample of families who had girls, but the gender difference is not statistically significant.³⁶

³⁵Results available upon request.

³⁶Results available upon request.

5.5 Family outcomes

Our analysis is completed by looking at how paternity leave affects a set of outcomes more directly connected to family life. In light of the somewhat surprising results related to mothers' labor market outcomes, family outcomes could be informative. For instance, if parents' time investments in family life are complementary, this could show up in fertility and divorce rates.

Table 9 provides our results on family outcomes. Generally, the estimated effects are not statistically significant. The estimated effect on the probability that the parents have separated by the time the child is 14 years old is positive, at about 2 percentage points across specifications, and statistically significant at the 10% level except for in the window spanning 3-7 weeks before and after the reform.

The lack of effects on family outcomes affects the scope for potential mechanisms through which paternity leave influences other outcomes. For example, the negative effects on mothers' earnings and employment could have followed from an increase in subsequent fertility. Our results indicate that this is not the case.

6 Conclusion

This paper documents how a paternity leave reform in Norway impacts children's test scores at age 16 (the end of compulsory schooling). Our empirical strategy exploits the fact that incentives to take paternity leave changed discontinuously on April 1, 1993. Time windows closely bracketing this date therefore isolate plausible exogenous variation, allowing us to interpret our findings causally.

We find that children's school performance improves in families in which the father has higher education than the mother, indicating that paternity leave causes a shift from maternal to paternal care at home. The perceived mechanism is not merely a direct link from four weeks spent at home by fathers to a change in children's school performance 15 years later. Rather, consistent with Becker (1985, 1991) and the stated intentions

Table 9: Paternity leave and family outcomes

	(1)	(2)	(3)	(4)
	\pm Weeks 3-13	\pm Weeks 3-13	\pm Weeks 3-7	\pm Weeks 1-13
Mother's parity	0.003 (0.021)	0.006 (0.017)	0.017 (0.024)	-0.005 (0.015)
Father's parity	-0.006 (0.023)	0.004 (0.018)	0.025 (0.025)	-0.005 (0.016)
Prob(Break up by child 14)	0.023* (0.012)	0.021* (0.012)	0.019 (0.017)	0.020* (0.011)
Prob(Next child together)	-0.016 (0.011)	-0.016 (0.011)	-0.016 (0.015)	-0.023** (0.010)
Child spacing	24.333 (23.038)	23.321 (22.318)	11.120 (31.962)	26.088 (20.448)
Father's leave next child	-0.388 (1.178)	-0.297 (1.182)	0.927 (1.663)	-0.075 (1.068)

Note: Each cell provides ITT estimates from a regression based on equation 1. A year dummy and calendar week fixed effects are included in all specifications. Included in specifications (2)-(4) are a set of socio-economic background characteristics explained in Table 4. Robust standard errors are 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 ± 3 -13 weeks in which control variables are included (column 2) the number of observations is 25665, 25665, 23449, 15015, 13146 and 8237 for the respective outcomes.

of Norwegian policy makers, the relatively short period of paternity leave may affect the evolution of household roles, with a small change in initial comparative advantages yielding a larger impact in the longer run.

To learn about mediating factors we analyze the reform's impact on parents' subsequent labor market and fertility outcomes. In spite of the intention of policy makers to bring fathers home to participate in family life on more equal terms, we find no effect of the paternal quota reform on fathers' work hours and yearly earnings. Also, we find no indication of an increase in mothers' labor market attachment, aimed at by the same policy makers, due to the reform. Furthermore, we do not find any significant contribution by paternity leave to family stability - nor to fertility. Our results therefore give no support for the conjecture that family-oriented policies, even if directed towards fathers, are well suited for reducing earnings differentials between men and women, nor for the supposition that these policies contribute to increased fertility or lower divorce rates.

Our approach, which combines a discontinuity design with a difference-in-difference

approach, pins down the partial effect of paternity leave on families directly affected by the paternal quota reform, allowing for a causal interpretation of our estimates. In addition, there may be important general equilibrium effects of fathers taking parental leave to an increasing extent - such as, for instance, that employers start behaving differently towards all men and women irrespective of their actual leave-taking behavior. Such effects will not be captured in the framework of this paper. It would therefore be useful to complement our study with analyses utilizing alternative identification strategies.

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7 Appendix

Table 10: Parental leave reforms in Norway

Reform (date)	Parental leave (weeks)	Compensation rate	Maternal quota (weeks)	Paternal quota (weeks)
1.7.1977	18	100%	6	0
1.5.1987	20	100%	6	0
1.7.1988	22	100%	6	0
1.4.1989	24(30)	100%(80%)	6	0
1.5.1990	28(35)	100%(80%)	6	0
1.7.1991	32(40)	100%(80%)	8 (2 before birth)	0
1.4.1992	35(44.4)	100%(80%)	8 (2 before birth)	0
1.4.1993	42(52)	100%(80%)	9 (3 before birth)	4
1.7.2005	43(53)	100%(80%)	9 (3 before birth)	5
1.7.2006	44(54)	100%(80%)	9 (3 before birth)	6
1.7.2009	46(56)	100%(80%)	9 (3 before birth)	10
1.7.2011	47(57)	100%(80%)	9 (3 before birth)	12

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