

Causal Effects of Paternity Leave on Children and Parents*

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Abstract

Reserving a share of the parental leave period for fathers is considered necessary for inducing fathers to take leave, and for men's increased participation in child-rearing. We investigate how a parental leave reform directed towards fathers impacted leave taking, and in turn children's and parents' long term outcomes. A paternal leave quota greatly increases the share of men taking paternity leave. We find evidence that children's school performance improves as a result, particularly in families where the father has higher education than the mother. We find no evidence that paternity leave counters the traditional allocation of parents' labor supply.

Keywords: parental leave, child development, labor supply

JEL Classification: J13, J22, J24, I21

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I 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 in the longer run.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 and learn about the causal effects of paternity leave on children and parents we use two parental leave reforms, implemented in 1992 and 1993. A main feature of the 1993 reform was the introduction of a four-week paternal quota. This reform caused a drastic change in fathers' leave-taking behavior. To isolate the effect of the paternal leave we contrast the 1993 reform with the 1992 reform, which did not contain a paternal quota, but which otherwise was similar.

Fathers that took parental leave after the implementation of the 1993 reform typically spent four weeks at home with the child. In itself, this may seem like too short a period to have much impact as the child grows older. Yet, spending four weeks as the main caregiver of an infant—and preparing for this period from the child is born—may set off

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).

a different dynamic both between parents as care-givers and between the father and his child. First, in the framework of Becker (1985), small changes in initial conditions may result in substantial changes in the household's time allocation in the long run. Second, as an early childhood intervention, self-productivity and strategic complementarities could multiply the investment made by the father in his child during these weeks (Cunha and Heckman, 2007). Third, the paternal quota, typically taken when the child was about ten months old, coincides with a critical stage in the child's attachment to primary caregivers (Bowlby, 1969). In sum, both economic and psychological theory give us reason to expect that these four weeks may cause a lasting change in the balance between maternal and paternal care.

Using a difference in difference approach, we find that children's school performance improves due to paternity leave. This effect seems to be concentrated in families in which the father has at least as high education as the mother, although the estimates are imprecise. This points to the crucial importance of the counterfactual situation: As paternal care becomes more important relative to maternal care, 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, we estimate how the reform impacts several parental outcomes. We find no statistically significant effect on fathers' earnings and working hours. We do, however, find some evidence of 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, we find no evidence that paternity leave affects fertility and marital stability.

While several papers have investigated how parental leave policies impact parents outcomes (e.g., Lalive and Zweimüller (2009)), the literature on the effect of parental leave policies on children's outcomes is still in its infancy (cf. Björklund and Salvanes

(2011)). Using expansions in Canadian maternity leave entitlements, Baker and Milligan (2010, 2011) find little evidence that the reforms affected short term child development. Dustmann and Schönberg (2012) evaluate the impact of a series of policy changes in Germany and find no support that expansions in leave coverage improved children’s long term outcomes. Rasmussen (2010) reaches a similar conclusion based on the evaluation of a Danish reform. Using Swedish data, Liu and Skans (2010) find positive effects of parental leave, but only for children of mothers with higher education. In contrast, Carneiro et al. (2011), using a Norwegian reform, find positive effects on children’s educational attainment, primarily of children to mothers with low education. Our paper adds to this literature by evaluating how parental leave reserved for the father impacts children.

A handful of studies have already used careful empirical designs to identify the causal effects of paternity leave. These studies use Scandinavian paternal quota reforms and focus primarily on parental outcomes.⁴ Using a paternal quota reform in Sweden, Ekberg et al. (2013) find no evidence that paternity leave affects the extent to which fathers care for children when they are sick. Kotsadam and Finseraas (2011, 2013), using Norwegian survey data, find long-lasting effects of paternity leave on the division of household work within the family. Rege and Solli (2013) 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.

The rest of the paper is organized as follows: Section II discusses the institutional setting. Section III presents our empirical strategy and our data. In Section IV we present the results. Section V concludes.

⁴Dahl et al. (2012) is an exception. Their focus is on peer effects of program participation. Using the 1993 Norwegian paternity leave reform they document substantial peer effects in both workplace and family networks.

II Parental 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 49 weeks in 2013.⁵ 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, 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.⁶ 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 is capped at six times the *basic amount* (G) of the Norwegian social security system.⁷ And third, until 2008, the compensation rate for the self-employed was at 65% of their income.

The 1992 and 1993 parental leave reforms

Our research design, which we will present in more detail in the next section, uses the 1992 and 1993 parental leave reforms. Both reforms were presented in October the year before, as part of the National budget, passed in parliament in December and were the implemented for births on April 1 and onwards. Furthermore, both reforms added three weeks of leave that parents can share freely.

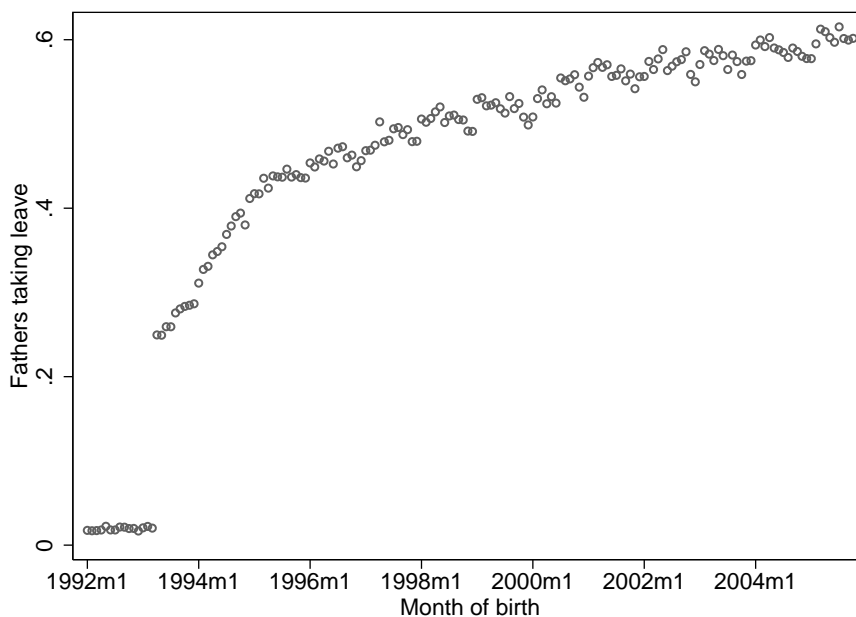
The fact that men tended to take very little leave triggered the labor party government

⁵See Appendix Table A1 for a full description.

⁶Sick leave from employment, unemployment with right to benefits, and paid parental leave all count as work.

⁷ *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).

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



Note: The graph show the share of fathers taking parental leave by the child's month of birth. Calculated from administrative leave data including all fathers.

to introduce a four-week paternal quota in the 1993 reform (thus, the reform increased the total amount of leave by seven weeks). Barring “special circumstances”, families would lose these four weeks unless taken by the father. For the father to be eligible for the paternal quota the mother had to resume work.⁸

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 accompanied with extension of the paternal quota. For families with children born after July 1, 2013 the paternal quota is 14 weeks.⁹

⁸This requirement was relaxed in July 1994 (Brandth and Øverli, 1998)

⁹There was also a further change in the 1993 reform, 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.

III Research design

Estimating the causal effects of paternity leave on parental, 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 this selection problem by contrasting the 1993 and 1992 parental leave reforms.

These reforms provide quasi-experimental variation in the uptake of parental leave. Parents of children born just before and just after the introduction of a reform are likely to be similar *ex ante*, but may—because of the reform—take different amounts of leave. In the following we will detail how we use this to estimate reform effects, in addition to what challenges there may be for identification and how we address these challenges.

Empirical specification

We estimate reform effects by comparing outcomes in families with children born in the three months before and after April 1 in 1992 and 1993, respectively. In addition to a reform effect, there may be seasonal differences between families.¹⁰ We address this by comparing the pre-/post-reform difference with the corresponding difference from 1991, when there was no reform on April 1, estimating the following difference-in-differences model by OLS:

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

Here, Y denotes an outcome pertaining to child/household/parent i . X is a vector

¹⁰Our main outcome will be children's school performance, which generally differ by birth season, see Strøm (2004) for Norwegian evidence. The age difference may also matter for the parents' outcomes, which generally are 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.

of pre-birth controls. *Week* is a vector of dummies indicating during which week of the year the child was born. *Year* is a dummy indicating whether the child was born in the reform or baseline year. 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.¹¹

Including week fixed effects allows us to eliminate persistent differences by season of birth, while the *Year* variable captures differences due to birth year. Thus, the *Reform* variable captures excess change at the introduction of the reform, relative to the baseline year. This is a valid estimation strategy as long as there are no systematic differences between the families of children born before and after the reform date—other than reform exposure—that also matter for the outcomes that we study. In the next sections, after presenting the data used in the estimation, we discuss several potential challenges for identification of the effect of paternity leave.

We proceed to estimate the effect of the 1993 reform (relative to the baseline difference in 1991) and the effect of the 1992 reform (also relative to 1991). This allows us to contrast the effects of the two reforms, in order to learn about what elements may have been effective. We also contrast the reforms more formally by estimating the effect of the 1993 reform relative to the 1992 reform. To the extent that the effect of these reforms differ only due to the paternal quota, the latter estimates the effect of the 1993 paternal quota, purged of the effect of the three-week extension in general leave.¹²

As emphasized in the introduction, we do not expect paternity leave to affect child outcomes so much directly as by causing a long-term change in parental behavior. For example, fathers may be more likely to help with homework as the child grows older if they have been actively participating in childcare during the child's first year. This may in turn affect children's school performance. For many activities, such as homework, the increased participation of the father may mean a corresponding decrease in participation

¹¹A similar empirical set-up has been used by, for instance, Lalive and Zweimüller (2009) and Dustmann and Schönberg (2012) to estimate how parental leave reforms directed towards mothers affect parent and child outcomes.

¹²This argument assumes linearity in the effect of maternal leave.

on behalf of the mother. If paternity leave sets off a dynamic in which the father is more involved with his child at the expense of the mother, as in the example, then the effect of paternity leave on child outcomes will depend on parents' *relative* "skill levels". More 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.¹³ 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.

Data

Data on parental leave is available from 1992 onwards. Thus, while we are unable to investigate leave taking in the baseline year 1991, we can observe changes around the 1992 and 1993 reforms. Our key variable is the number of weeks of wage-compensated leave for each parent.

To measure children's cognitive skills we rely on their final exam scores from lower secondary school.¹⁴ The exam subject is chosen randomly from the core subjects Norwegian, English and mathematics. For ease of interpretation grades are demeaned and measured as standard deviations by year.¹⁵

Our labor market outcomes are each parent's annual income (in constant 1998 NOK, truncated above the 99th percentile) and employment status. Employment is measured by two dummy variables, for whether the individual is registered with at least 20 hours (which we classify as part-time) and at least 30 hours (which we classify as full-time) of employment per week. To facilitate interpretation and increase the precision of our estimates we average labor market outcomes over the years when the child is 2 to 5 years

¹³If 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.

¹⁴There is no grade retention in Norway, such that a vast majority of children complete lower secondary school the year they turn 16. The 1993 cohort completed lower secondary in June, 2009.

¹⁵This is done with data for all students, not only those in our sample.

old.¹⁶ We do not use data from when the child is one year old, since most parents will be taking part of their leave at this age.

We also investigate the impact of paternity leave on the following family outcomes: Parents' total number of children 14 years after the reform, the distance in years to the next child (conditional on the parents having another child together), and parents' probability of being married to each other when the child is 14.

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.

All the data used for our main analysis come from Statistics Norway's administrative registers. For a more thorough description of the data, including descriptive statistics for the leave, outcome and control variables, we refer the reader to Online Appendix B. Online Appendix B also presents balancing tests, where we test jointly for differences in sample composition around the introduction of the 1992 and 1993 reforms, relative to the 1991 difference. We do not find evidence of differential change in sample composition, suggesting that comparing families having children before and after the reforms is a valid identification strategy.

Eligibility and sample criteria

As mentioned in Section II, 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 furthermore depends on the child's mother having worked the required amount.

To get precise estimates, we would like to analyze a sample consisting of families that were actually affected by the reform. As we do not perfectly observe eligibility status, we use parents' income history to determine our sample. We face a trade-off between using

¹⁶Such 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)).

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 eligible. The first type of error may affect the generalizability of our results, whereas the latter may give imprecise estimates.

We choose, as a 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 7) during the calendar year two years prior to the child’s birth. 53% of all families fulfill this criterion. Looking at the leave taking behavior of mothers in 1992 and 1993 (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 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 87% 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. 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.

We also face a trade-off between bias and precision when choosing the time window on which to estimate (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. We use a six month window around April 1, which we believe balance these concerns.¹⁷ Also, we disregard the two weeks

¹⁷Previous analyses have shown that a three month window around April 1 is not very informative due to lack of precision. For details, see Cools et al. (2011). On the other hand, including data from before January would combine students from two different cohorts, and including data from July and onwards would make part of the baseline sample exposed to the parental leave reform on July 1, 1991.

immediately before and after April 1 due to birth timing issues, to be discussed in more detail the next section.

Finally, in the analysis of school performance, we lack data on exam scores for about 4% of the students, and we exclude about 100 students that either do not complete upper secondary with their birth cohort or that have multiple written exam scores.

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. The 1992 reform also was announced a similar time before its implementation. Admittedly the reforms may not have been very surprising to followers of the policy debate in Norway at the time, but there is little reason to expect that future parents guessed the implementation dates.¹⁸ 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, expectant parents with due dates close to April 1 could possibly postpone induced births or planned cesarean sections.¹⁹ Brenn and Ytterstad (1997) present evidence that births were postponed because of the 1993

¹⁸Of 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).

¹⁹Grytten et al. (2013) document that Norwegian mothers are able to influence their mode of delivery. In 1993 the fraction of children born by cesarean section was 12.4%, and of these deliveries, 59.4% were emergency operations. On average, 12% of vaginal deliveries in 1993 were induced, while 88% were spontaneous (Folkehelseinstituttet, <http://mfr-nesstar.uib.no/mfr/>).

reform. Following Gans and Leigh (2009), we have investigated this further, regressing the daily number of births on a post-reform dummy variable. In this analysis, documented in Online Appendix C, we find strong evidence of strategic timing of births. However, as could be expected, this is restricted to births close to April 1, we do not find any effect more than two weeks away from this date. 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 first weeks of April.²⁰

Content of reforms

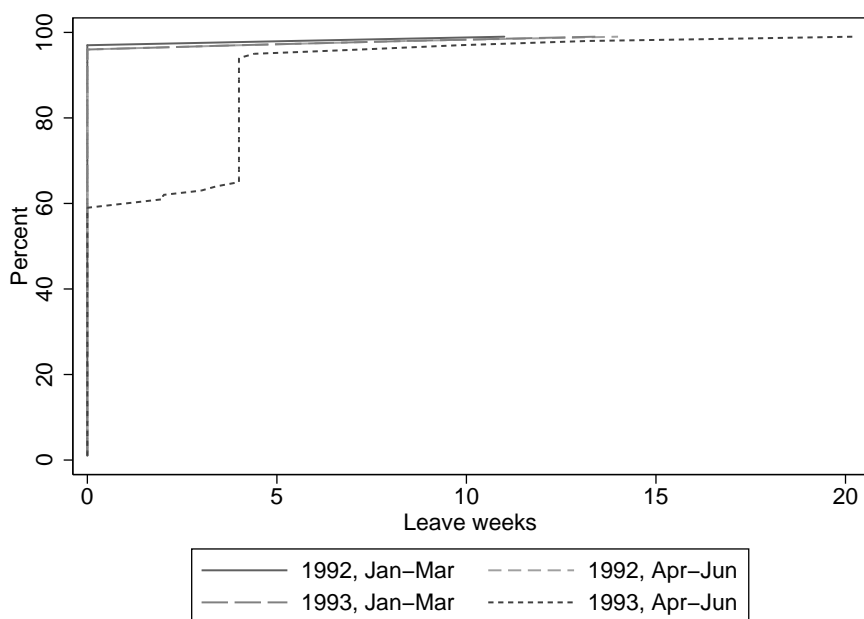
As we identify the effect of paternity leave by contrasting the 1992 and 1993 reforms, we first provide a more detailed description of how the reforms affected leave taking. Figure 2 shows the distributions of leave taken by fathers of children born in the three months before and after April 1, for the years 1992 and 1993 in our sample of eligible parents (and excluding births within two weeks of April 1). Before the reform a small share of fathers take leave. With the introduction of the paternal quota a part of the distribution function shifts out: Most fathers still do not take any leave, but about 40% do. A large majority of these take exactly four weeks. The share taking leave increases with 36 percentage points, from a level of 4% (cf. Online Appendix Table B1). The average leave length increases by just over eight days, up from two.²¹

Figure 3 shows the corresponding distributions of maternal leave. Unlike for the fathers, the level and also the variation in leave taking is substantial before April 1, 1993, and also before April 1, 1992. Still, we note that in all periods before April 1, 1993 there is strong clustering at certain amounts of leave, corresponding to 100% and 80% wage

²⁰An alternative to our diff-in-diff design could be a regression discontinuity, allowing for some kind of flexible trend. However, the fact that we find evidence of strategic timing of births suggests that identification just around April 1 may be exposed to bias.

²¹Most 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 ten months old. Less than 5% take leave after the child has turned one year. The cumulative distribution of the child's age at the beginning and end of their father's leave is shown in Appendix Figure B1.

Figure 2: Cumulative distribution of fathers' leave (weeks).



Note: The graphs show the cumulative distribution of fathers' leave, calculated as the leave at percentiles 1-99. Sample is restricted to fathers eligible for parental leave and excludes fathers of children born within two weeks of April 1. (See Section III for more details.)

compensation.²² Maternal leave clearly increases over time, with each distribution being below and to the right of the previous one.²³

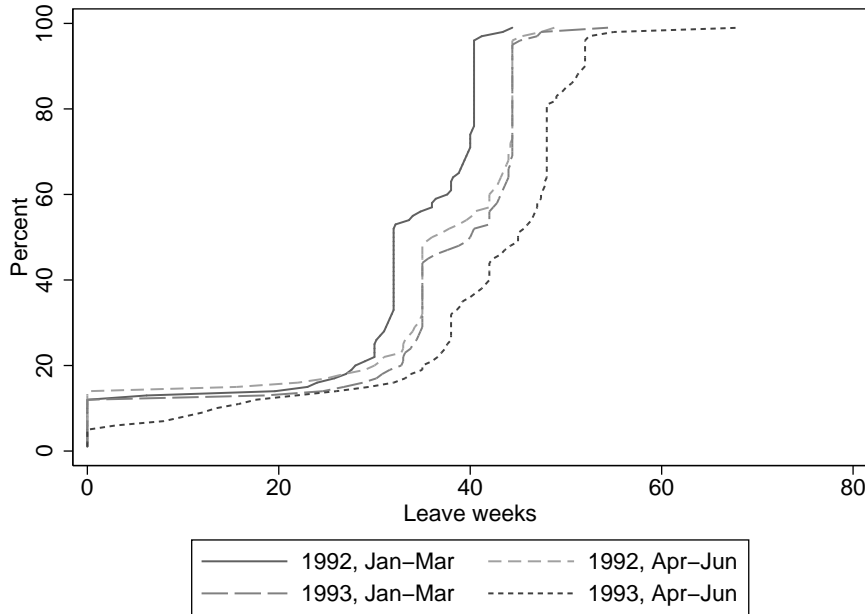
This is also the case with the 1993 reform, which had the same nominal extent (three weeks increase of shared leave, generally taken by the mother). Thus, we will contrast the effect of the 1993 reform (which include a paternal quota and a general expansion) with the effect of the 1992 reform (which include only the general expansion). To the extent that the general expansion has a similar effect both years, we can eliminate this effect by comparing the two reforms.

The marginal effect of more leave may differ with the initial level of leave, because of non-linear effects or critical periods. While the initial level of maternal leave differs

²²Multiple births give longer leave, while a part of the two weeks reserved before the birth (three weeks after the 1993 reform) is forgone when a child is born before the due date. Between them these features can explain most of the spread around the peaks in the distribution.

²³The distributions seem to shift down over time, as more parents choose 80% wage compensation. In Online Appendix Table B1 we see that there are similar changes in these fractions in 1992 and 1993.

Figure 3: Cumulative distribution of mothers' leave (weeks).



Note: The graphs shows the cumulative distribution of mothers' leave, calculated as the leave at percentiles 1-99. Sample is restricted to mothers eligible for parental leave and excludes fathers of children born within two weeks of April 1. (See Section III for more details.)

from 1992 to 1993, this difference is small both relative to the amount of leave and to the within-period spread in leave; the distributions overlap considerably. It is thus of little concern.

However, in the case of the 1993 reform there are also some unexpected changes. First, the share of mothers taking any leave increases by about 6.5 percentage points (cf. Online Appendix Table B1). Also, the maternal leave distribution is less clustered. A share of the mothers have more leave than what would correspond to a three-week outward shift of the pre-reform distribution. Both changes contribute to the 1993 change in maternal leave being greater than the 1992 change. While the 1992 change is just below the expected three weeks (see Online Appendix Table B1), the 1993 change is five weeks—i.e., an extra two weeks on average—but unequally distributed. This has consequences for our estimates, which will capture all elements of the 1993 reform, not only the effects of increased use of paternity leave. We can, however, learn about the empirical relevance of

the additional elements of the 1993 reform by conducting some sensitivity analyses.

We have not been able to find any reason why there is an increase in the share of mothers taking leave. However, this increase seem to be restricted to short leave spells, less than approximately 25 weeks. We find no increase in the share of mothers with at least this amount of leave (cf. Online Appendix Table B1). Thus, in a robustness analysis we will restrict the sample to families in which the mothers take at least 25 weeks of leave.

Families should normally lose the four weeks of parental leave if these were not 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 strongly suggests that a fairly large share of mothers got the paternal quota in addition to the rest of their leave. This shows up as clustering from the two values of nominal maximum leave durations to about four more weeks. The share of mothers taking up to four weeks more than the expected maximum amount of leave is about 25%.

To address this issue we compare the estimated effects for families that seem to transfer paternal leave and those who do not. This will give us an indication of where a given effect arises. Furthermore, from the 1992 reform, we get an estimate of the effect of having all mothers take more paid leave. While this may differ from the effect for the subsample that transfers the paternity quota, we can still get an idea about what distribution of effects of maternal leave would be necessary for the extra maternal leave to explain our findings for the 1993 reform.

IV Results

In this section we present our main results. For every outcome, we run six different specifications. In Tables 1-3, columns (1) and (2) compare the change in outcomes of children born around April 1, 1993 with that of children born around April 1, 1991. Because there was no reform on April 1, 1991, this just removes seasonal effects in order to estimate the total effect of the 1993 reform. Columns (3) and (4) do the same for the

1992 reform, comparing the changes for the 1992 and 1991 cohorts. Columns (5) and (6) compare the 1993 and 1992 cohorts. This gives an estimate of the excess effect of the 1993 reform over the 1992 reform, i.e. effects that we contribute to the paternal quota. Columns (1), (3) and (5) only control for calendar effects, while columns (2), (4) and (6) also control for the covariates discussed earlier. For brevity, we present only the ITT in question (α in Equation 1).

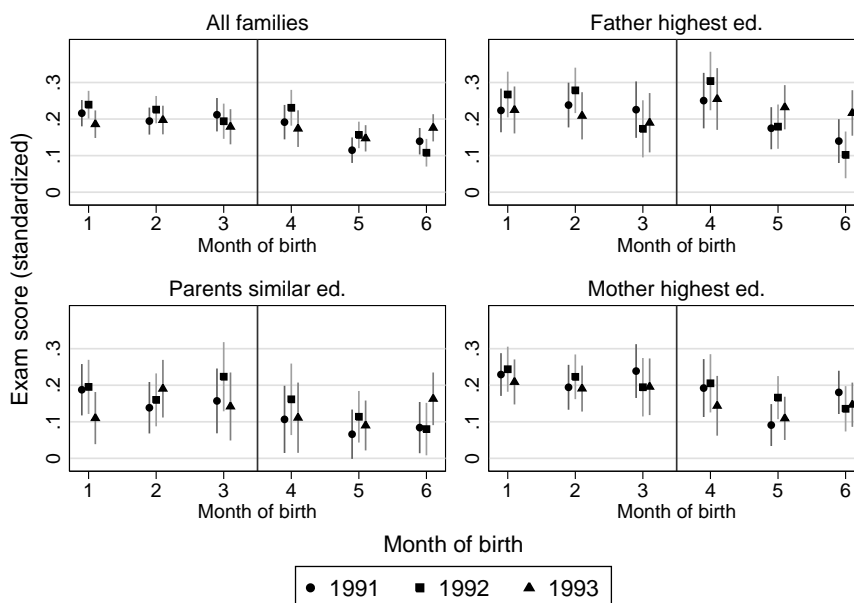
Children's school performance

We start with a graphical analysis in Figure 4. The figure plots the average exam score and the 95% confidence interval by birth month for children born January-June in the years 1991-1993, for all families and by parents' relative education. In our sample, 36.0% of students are from families where the father has higher education than the mother, 27.4% have parents with equal levels of education and 36.5% of students are from families where the mother has higher education than the father. Across these three groups, the reform affected paternity leave uptake similarly (documented in Online Appendix D).

Looking at all families, we see a falling trend over the year, as expected. This points to the importance of controlling for birth time-of-year effects in our diff-in-diff framework. There is no clear difference in pre-reform trends. While the graph shows monthly bins for expositional clarity, the regressions control for week fixed effects. A formal joint test of the year \times week effects for the pre-reform period fail to reject the hypothesis that there are no differences between years within weeks.²⁴ This indicates that the exam scores might indeed have evolved similarly in all years absent any reform, and lends support to our identification strategy. After April 1, the figure indicates that exam scores improve slightly in 1993 relative to the other years, in particular for the sample where the father's

²⁴For the entire pre-reform sample and each of the pre-reform subsamples by relative education we have regressed exam score on common week effects with birth year \times week interactions. The p -values for the joint test of all interactions being zero are 0.286 for the pooled sample, 0.181 when the father has the highest education, 0.710 for similar education and 0.841 when the mother has the highest education. Furthermore, restricting the regression to two years, as we do in the analysis, we are unable to reject all interactions being zero for any combination of relative education and years.

Figure 4: Exam results by birth month and year



Note: Average exam score by month and year of birth. The error bars represent 95% confidence intervals. Sample is restricted to children of parents eligible for parental leave and excludes children born within two weeks of April 1. Each month includes 2500-3000 observations, except for March and April which each include around 1500 observations. Stratifying by relative education, we have from 600-1100 observations in each bin, except for March and April, where the numbers range from 400-650. Exam score is measured as standardized deviation from yearly mean (for all children, not only sample families). The solid vertical line indicates April 1.

education is higher than the mother's. However, there is not sufficient precision to draw any conclusions from the figure alone.

In Panel A of Table 1 we present regression results based on Equation 1 for children's exam scores. In column (1) and (2) we see that the 1993 cohort's exam scores increase with about 4% of a standard deviation after April 1 relative to the 1991 cohort, suggesting that the 1993 reform may indeed have had a positive impact on children's school performance. However, the effect is not very precisely estimated, and significant only at the 10% level. Controlling for covariates marginally increases precision, and has little impact on the estimated effect.

The corresponding estimates of the effect of the 1992 reform, presented in columns (3) and (4), are similarly imprecise. However, the difference in point estimates is striking.

Table 1: Paternity leave and school performance at age 16

	(1)	(2)	(3)	(4)	(5)	(6)
	93vs91	93vs91	92vs91	92vs91	93vs92	93vs92
<i>Panel A: Average effect</i>						
All students	0.041*	0.038*	-0.002	-0.010	0.044*	0.045**
	(0.023)	(0.021)	(0.023)	(0.021)	(0.024)	(0.022)
Controls	No	Yes	No	Yes	No	Yes
R^2	0.001	0.158	0.002	0.180	0.002	0.170
N	27991	27991	28147	28147	26888	26888
<i>Panel B: Heterogeneous effects by parents' relative education</i>						
Father highest education	0.070*	0.048	-0.018	-0.022	0.090**	0.068*
	(0.039)	(0.035)	(0.039)	(0.035)	(0.040)	(0.036)
Parents similar education	0.056	0.046	0.004	-0.008	0.053	0.053
	(0.045)	(0.041)	(0.045)	(0.040)	(0.046)	(0.041)
Mother highest education	0.004	0.024	0.011	0.002	-0.008	0.017
	(0.038)	(0.036)	(0.038)	(0.035)	(0.039)	(0.036)
Controls	No	Yes	No	Yes	No	Yes
R^2	0.003	0.159	0.003	0.180	0.003	0.170
N	27991	27991	28147	28147	26888	26888

Note: Panel A provides regression results for exam scores at the end of 10th grade. Each column provides ITT estimates from a regression based on Equation 1. Columns (1) and (2) compare children born before/after April 1 in 1993 and 1991, columns (3) and (4) compare children born before/after April 1 in 1992 and 1991, and columns (5) and (6) compare children born before/after April 1 in 1993 and 1992. Panel B shows the same comparisons, with the ITT effects interacted with relative parental education groups. A year dummy and calendar week fixed effects are included in all specifications. Specifications (2), (4) and (6) control for a set of socio-economic background characteristics presented in Online Appendix Table B3. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$.

While we find a positive effect of the 1993 reform, albeit imprecise and only marginally significant, the estimated effect of the 1992 reform is very close to zero. This suggests that the expansion of general leave is not a main cause of the effect of the 1993 reform. Estimating the effect of the 1993 reform relative to the 1992 reform (columns (5) and (6)), the estimated effect increases marginally, and is significant at the 5% level when we control for covariates. From Table 1 we see that the explanative power of the reform is low. R^2 is about 0.001-0.003 without controls, and increases to about 0.16 with controls included. This is unsurprising, as there will typically be large amounts of idiosyncratic variation in individual-level regressions. As long as any omitted variable is uncorrelated

with reform exposure, this is not a threat to our identification. Furthermore, despite low explanative power, there may still be effects large enough to be relevant for policy.

If the average effect of 4.5% of a standard deviation comes solely from the families in which the father actually does take leave, this amounts to an average effect of about 1/8 of a standard deviation in these families. This is a substantial effect. Hægeland et al. (2012) document that it takes about one standard deviation increase in school resources to improve average exam performance with 1/5 of a standard deviation.²⁵ Liu and Skans (2010) study the effect of expanding maternal leave from 12 to 15 months on a similar outcome. They find an effect of 0.016 standard deviations for each extra month for highly educated mothers. Thus, their estimate is larger than our estimated (statistically insignificant) effect of an *expansion of maternal leave*, but smaller than our effect of the *introduction of paternal leave*.

Previous Norwegian studies have found that preschool and early family interventions may matter considerably for performance at the end of lower secondary school. Drange and Telle (2010), for example, find that the introduction of free core-hour child care (at age 5) improves girls' school performance later on with about 0.29 of a standard deviation. They find no effect for boys. Bettinger et al. (2013) find an ITT effect on GPA of 0.02 standard deviations of a cash-for-care subsidy. If this effect comes only from (the small and possibly non-representative share of) families where the mother was induced to stay home, this translates to an effect of 1.5 standard deviations.

In Panel B of Table 1 we report heterogeneous effects of paternity leave by parents' relative education. These results are less precise and less stable than the average effects. However, we see that there are no effects of the 1992 reform. On the other hand, there are some substantial effects of the 1993 reform, although these are only in some cases significant at conventional levels, and only so for families in which the father has higher education than the mother. The differences between the relative education groups are

²⁵To put our 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, and that the gender difference between girls and boys is about 1/4 of a standard deviation.

not significant at conventional levels. Still, the estimated reform effect in Panel A seems to come from families where the parents have similar education or, in particular, where the father has higher education than the mother. In these families we estimate that exam scores increase by 7% of a standard deviation (statistically significant at the 10% level).

As discussed in Section III, the 1993 reform contained several elements, making it unclear what causes the positive effect shown in Table 1. The complete lack of effect of the 1992 reform strongly suggests that the effect is not the result of the general expansion in paid leave. Furthermore, the lack of effect when extending leave for *all* mothers makes it hard to believe that *some* mothers taking leave or taking more leave would explain the positive effect of the 1993 reform. If so, this would require a substantial *negative* effect of leave for some other group of mothers. As a robustness check we estimate reform effects by whether the mother seems to be on the margin of taking some leave, and by whether the family seems to have transferred paternity leave to the mother. This analysis, reported in Online Appendix Table C2, does not suggest that increased parental leave taking by mothers is the cause of the effect we observe.

The precision of our estimates does not enable us to rule out neither very small nor very large effects. However, the 1993 reform does seem to have had a non-trivial impact on children's school performance at the end of lower secondary education. Furthermore, with no indication that more leave for mothers is the cause of this effect, we conclude that the paternal quota is likely to have been effective. Given the size of the estimated effect, we believe that it is unlikely to be caused solely by the four weeks (some) fathers spent at home in the first year of the child. Rather, it appears more plausible that it results from fathers participating more actively in child rearing when the child grows older, for example through increased involvement in school work. This interpretation is consistent with the survey evidence of Kotsadam and Finseraas (2011, 2013), showing long-lasting effects of the 1993 parental leave reform on the division of household work within the family.

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. Our analysis differs by studying 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 the short run. In our case, paternal care probably replaces both maternal care and informal or formal child care in the short run, but more importantly, it replaces maternal care as the child grows older. We therefore expect the parents' relative education, rather than absolute levels, to matter. We have estimated reform effects by each parent's absolute level of education without finding any clear pattern. This suggests that there is indeed a change in relative importance of the parents.

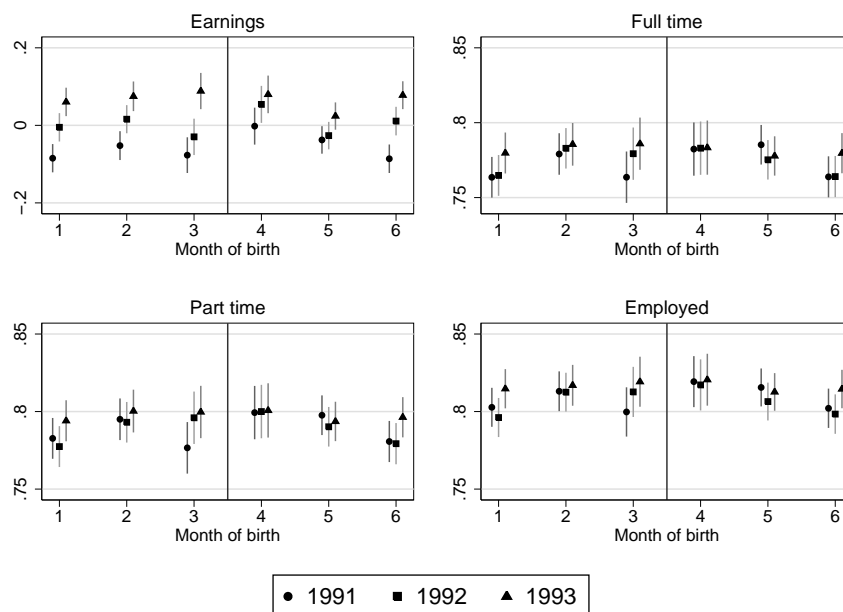
Labor market outcomes

Part of the motivation behind introducing the paternal quota is that increased participation in paternity leave changes the time allocation in the household between parents and between market and home production. Such a reallocation could in turn explain the positive effect of paternity leave on the school performance of children whose father has higher education than their mother.

More specifically, paternity leave is considered an important tool in narrowing the gender gap in income and employment. Figure 5 and 6 plots fathers' and mothers' labor market outcomes and 95% confidence intervals by birth month for children born January-June in the years 1991-1993. All the outcomes are averages over the years when the child is 2-5 years old.²⁶ There is one panel for each of the four labor market outcomes that we study. Visual inspection of these two figures reveals no clear indication that paternity leave causes households to specialize less in line with traditional gender roles.

²⁶The outcomes are further described in Online Appendix B, and descriptive statistics are given in Online Appendix Table B2.

Figure 5: Fathers' labor market outcomes by birth month and year



Note: Averages for fathers' labor market outcomes by month and year of birth. The error bars represent 95% confidence intervals. Sample is restricted to parents eligible for parental leave and excludes children born within two weeks of April 1. Each month includes 2500-3000 observations, except for March and April which each include around 1500 observations. The solid vertical line indicates April 1. Every outcome is averaged over the years when the child is 2 to 5 years old. Earnings are standardized.

Table 2 shows the regression results based on Equation 1 for fathers' labor market outcomes. As in Figure 5, the outcomes are averages over the years when the child is 2-5 years old. The point estimates are all negative, but generally statistically insignificant.²⁷

Table 3 shows the regression results based on Equation 1 for mothers' labor market outcomes over the years when the child is 2-5 years old. Using 1991 as the comparison year, in column (1) and (2) there is little evidence that the 1993 reform impacted mothers labor market outcomes. If anything, they move in the same direction as fathers' outcomes, but the estimates are not statistically significant. In contrast, the corresponding estimates of the effect of the 1992 reform (presented in columns (3) and (4)), which only extended general (and in practice maternal) leave, show a positive effect on earnings and the propensity to work at least part time. This suggests that the expansion of general leave

²⁷In magnitude, the effect on fathers' earnings is about the same as the effect reported by Rege and Solli (2013).

Table 2: Paternity leave and fathers' labor market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	93vs91	93vs91	92vs91	92vs91	93vs92	93vs92
Earnings	-0.040*	-0.014	-0.014	-0.001	-0.024	-0.012
	(0.023)	(0.017)	(0.023)	(0.017)	(0.023)	(0.017)
Full time	-0.011	-0.008	-0.010	-0.006	-0.001	-0.002
	(0.009)	(0.008)	(0.009)	(0.008)	(0.009)	(0.008)
Part time	-0.008	-0.005	-0.006	-0.002	-0.002	-0.003
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Employed	-0.007	-0.005	-0.006	-0.004	-0.001	-0.002
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Controls	No	Yes	No	Yes	No	Yes

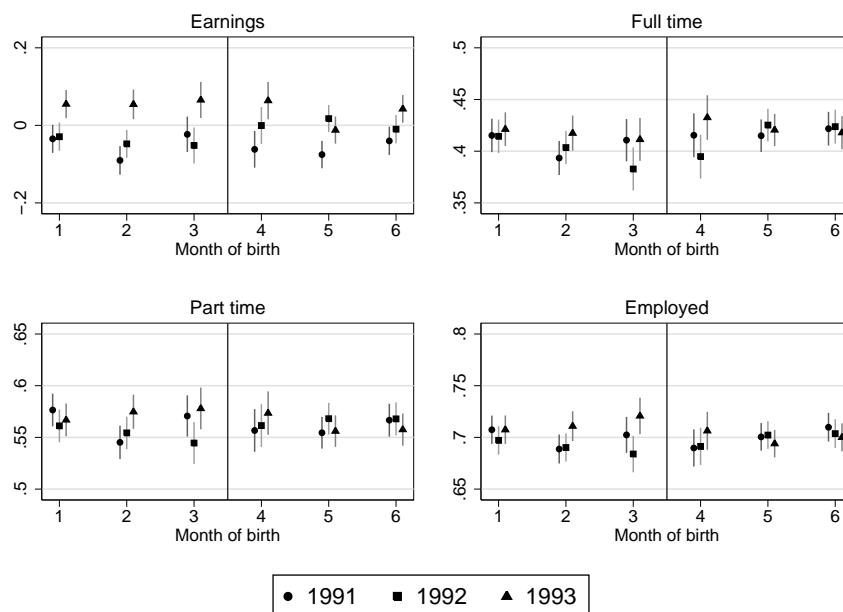
Note: Each cell provides ITT estimates from a regression based on Equation 1 for the different labor market outcomes specified in the row headings (all outcomes are averages over the years when the child is 2 to 5 years old). Columns (1) and (2) estimated 1 comparing fathers of children born before/after April 1 in 1993 and 1991, columns (3) and (4) compares fathers of children born before/after April 1 in 1992 and 1991, and columns (5) and (6) compare fathers of children born before/after April 1 in 1993 and 1992. The number of observations varies with outcomes and slightly with sample; for column (6) the number of observations is 25952 for earnings and 25991 for the working hours outcomes. A year dummy and calendar week fixed effects are included in all specifications. Specifications (2), (4) and (6) control for a set of socio-economic background characteristics presented in Online Appendix Table B3. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$.

Table 3: Paternity leave and mothers' labor market outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	93vs91	93vs91	92vs91	92vs91	93vs92	93vs92
Earnings	-0.025	0.001	0.052**	0.055**	-0.078**	-0.053**
	(0.023)	(0.019)	(0.023)	(0.018)	(0.023)	(0.019)
Full time	-0.007	-0.001	0.004	0.007	-0.011	-0.009
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Part time	-0.008	-0.003	0.016	0.020**	-0.024**	-0.023**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Employed	-0.015*	-0.013	0.007	0.009	-0.022**	-0.021**
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.008)
Controls	No	Yes	No	Yes	No	Yes

Note: Each cell provides ITT estimates from a regression based on Equation 1 for the different labor market outcomes specified in the row headings (all outcomes are averages over the years when the child is 2 to 5 years old). Columns (1) and (2) estimated 1 comparing mothers of children born before/after April 1 in 1993 and 1991, columns (3) and (4) compares mothers of children born before/after April 1 in 1992 and 1991, and columns (5) and (6) compare mothers of children born before/after April 1 in 1993 and 1992. The number of observations varies with outcomes and slightly with sample; for column (6) the number of observations is 25977 for earnings and 25991 for the working hours outcomes. A year dummy and calendar week fixed effects are included in all specifications. Specifications (2), (4) and (6) control for a set of socio-economic background characteristics presented in Online Appendix Table B3. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$.

Figure 6: Mothers' labor market outcomes by birth month and year



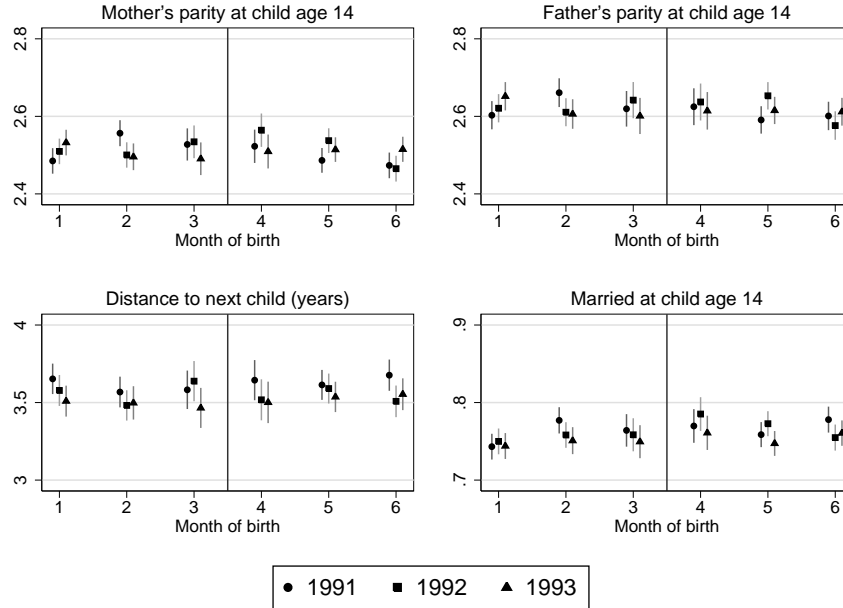
Note: Averages for mothers' labor market outcomes by month and year of birth. The error bars represent 95% confidence intervals. Sample is restricted to parents eligible for parental leave and excludes children born within two weeks of April 1. Each month includes 2500-3000 observations, except for March and April which each include around 1500 observations. The solid vertical line indicates April 1. Every outcome is averaged over the years when the child is 2 to 5 years old. Earnings are standardized.

may not be the main explanation for there being no net positive effect of the 1993 reform on mothers' labor market outcomes. Contrasting the two reforms in columns (5) and (6), our results indicate that mothers' earnings are actually negatively affected by paternity leave—quite contrary to what would be expected in a simple Beckerian framework.

The following lines in Table 3 indicate that the negative effect on mothers' earnings in columns (5) and (6) are driven by reduction in labor supply, and in fact in large part by net movement in and out of employment. Interestingly, Ekberg et al. (2013) also find a negative effect of paternity leave on female labor force participation in Sweden. The adverse effects of paternity leave on women's labor market outcomes may be due to complementarities in mothers' and fathers' time at home.

Still, 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

Figure 7: Family outcomes by birth month and year



Note: Averages for family outcomes by month and year of birth. The error bars represent 95% confidence intervals. Sample is restricted to parents eligible for parental leave and excludes children born within two weeks of April 1. Except for the child spacing outcome, each month includes 2500-3000 observations, except for March and April which each include around 1500 observations. For child spacing the number of observations is approximately half of this. The solid vertical line indicates April 1.

the paternal quota reform but rather through the increase in maternity leave that we document in Section III. In the robustness checks reported in the Online Appendix C, we exclude from our sample families where the paternal quota was transferred to the mother. Although this requirement is endogenous to the paternal quota reform, the estimation results in this sample give some indication of whether the transferrers are driving the results. There is still a negative effect—if any—on mothers' labor market outcomes in this sample (as can be seen in columns (4) and (5) in Online Appendix Table C4), but it is only statistically significant for earnings.

Family outcomes

We complete our analysis by looking at how paternity leave affects a set of outcomes directly connected to family life. The first three lines of Table 4 show how experiences

with paternity leave affected completed fertility, as measured by mothers' and fathers' total fertility by 2008, and the distance (measured in days) to the next child—if indeed parents have another child together. Figure 7 provides a graphical analysis.²⁸

We find no evidence in columns (5) and (6) of Table 4 of fertility being impacted by the paternity leave reform. In a setting similar to ours, Liu and Skans (2010) find that a maternal leave reform in Sweden slightly reduces child spacing, but they find no effect on completed fertility. Lalive and Zweimüller (2009) document considerable fertility effects due to a one-year parental leave extension in Austria. These effects appear to be driven by a particular feature of the reform related to the scope for automatic renewal of the parental leave period.

The last row of Table 4 shows the estimates of how the reforms affected the probability of the child's parents being married to each other in the year when the child is 14 years old. The point estimate is negative but very small and not statistically significant. This result is also in line with Liu and Skans (2010).

The lack of effects on family outcomes affects the scope for potential mechanisms through which paternity leave influences other outcomes. For example, the lack of a positive effects on mothers' earnings and employment could have followed from an increase in subsequent fertility. Our results give no indication that this is the case.

An important channel through which parental leave policies may affect fertility is the expectation about leave options when having another child. Björklund (2006) documents that such anticipatory effects appear to be substantial in Sweden. Since expectations about future leave policies should not depend on whether your previous child was born before or after the implementation of the paternity leave reform, anticipatory effects cannot be identified in our research design.

²⁸The outcomes are further described in Online Appendix B, and descriptive statistics are given in Online Appendix Table B2.

Table 4: Paternity leave and family outcomes

	(1)	(2)	(3)	(4)	(5)	(6)
	93vs91	93vs91	92vs91	92vs91	93vs92	93vs92
Mother's parity (child age 14)	0.036*	0.016	0.036*	0.013	0.001	0.003
	(0.021)	(0.017)	(0.021)	(0.017)	(0.021)	(0.017)
Father's parity (child age 14)	0.017	0.021	0.024	0.017	-0.007	0.004
	(0.023)	(0.017)	(0.023)	(0.018)	(0.023)	(0.017)
Sibling spacing (days)	0.919	-8.849	-17.555	-27.105	17.496	16.583
	(23.251)	(22.594)	(23.061)	(22.443)	(22.936)	(22.232)
Parents married (child age 14)	-0.000	-0.003	0.006	0.003	-0.006	-0.006
	(0.011)	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)
Controls	No	Yes	No	Yes	No	Yes

Note: Each cell provides ITT estimates from a regression based on Equation 1 for the different family outcomes specified in the row headings. Columns (1) and (2) estimated 1 comparing parents of children born before/after April 1 in 1993 and 1991, columns (3) and (4) compares parents of children born before/after April 1 in 1992 and 1991, and columns (5) and (6) compare parents of children born before/after April 1 in 1993 and 1992. The number of observations varies with outcomes and slightly with sample; for column (6) the number of observations is 13295 for earnings and 25991 for other outcomes. A year dummy and calendar week fixed effects are included in all specifications. Specifications (2), (4) and (6) control for a set of socio-economic background characteristics presented in Online Appendix Table B3. Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$.

V Conclusion

This paper investigates how a paternity leave reform in Norway impacts children's exam scores at 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. The effects are relatively imprecisely estimated, but do suggest 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, in line with Becker (1985, 1991) and the stated intentions 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 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 higher marital stability.

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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Appendix A: Parental leave reforms in Norway

Table A1: 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
1.7.2013	49(59)	100%(80%)	17 (3 before birth)	14

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