Paid parental leave, mothers' education, and children's scholastic performance*

by

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Abstract

We study how the duration of paid parental leave affect children's accumulation of cognitive skills. Using a Swedish benefit extension from 12 to 15 months which covered children born during the second half of 1988 we evaluate the effects of prolonged parental leave spells on children's test scores and grades at age 16. We show with considerable precision that the reform had no effect on the average child's scholastic performance. We do however find positive effects for children to well-educated mothers, a result that is robust to a number of different specifications. We find no corresponding heterogeneity relative to parental earnings or fathers' education, or relative to other predictors of child performance. In addition we investigate the effects on mothers' subsequent earnings, child health, parents' fertility, divorce rates and the rate of maternal depressions, again finding no effects. The result suggests positive causal interactions effects between mothers' education and the amount of time mothers spend with their children. Since the institutional context is one where the alternative is subsidized day care, the results suggest that subsidizing longer parental leave spells rather than day care reinforces the relationship between maternal education and school outcomes.

Keywords: Maternal employment, education, human capital, cognitive skills
JEL-codes: I21, J13, J24

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1 Introduction

Researchers and policymakers tend to agree that the early childhood environment is important for the long run development of children.[1] Arguably, parent's labor supply decisions and the ensuing day care arrangements are key parameters in the child environment. However, the effects of these choices are ambiguous *a priori*. Even though parents in general, and perhaps mothers in particular, are likely to be important for child development it is not clear that more time with the parents is always good, or even that it is good on average. The effects are likely to depend on both the quality of care provided by the parents when on leave, and the quality of alternative child care options. In addition, indirect effects via the income, health, and fertility of parents may affect future child development in ambiguous directions. From a policy perspective it is noteworthy that Article 3 of the UN Convention on the Rights of the Child, which is legally binding is ratified by most UN member states,[2] requires that the best interest of the child should be the primary consideration when designing policies that affect children. This suggests a need for credible evidence on the impact on child development of policies targeted towards parents' labor supply.

This paper studies how extensions of parental leave benefits affect children's accumulation of cognitive skills. We study a reform in 1989 which extended the duration of parental leave entitlements for Swedish children born from August 1988 from 12 to 15 months of paid absence. Using data on all Swedish children born in 1988 we study how our outcome measures differ depending on the duration of parental leave entitlements which in turn depend on the month of birth. Children born in adjacent years (1987 and 1989) are used to remove the direct influences of months of birth. We use compulsory school grades and national tests taken at age 16 as our primary outcomes. In addition, we study indicators of child health and a number of other intermediate outcomes measured at the parental level that has been suggested to be important by the

[1] For example, Heckman (2000) claims that early successful interventions in the life cycle of learning lead to higher overall achievement, and in the motivation for a recent extension of Swedish parental leave policies it is claimed that extended benefits allow parents to make choices that increase the welfare and development of the children (proposition 2007/08:91, page 66–67). Dustmann and Schonberg (2008) present evidence of similar motivations of German reforms.

[2] The US is a rare exception. It has signed, but not ratified the Convention, see www.unicef.org.

previous literature. As we show, the main alternative to parental leave in the Swedish context is subsidized day care. Thus, our study can be interpreted as estimating the effects of shifting time from subsidized child care to subsidized parental leave.

Previous literature (see Boeri et al, 2005 for a thorough survey) often presumes that parents provide a higher quality of child care than available alternatives and that the emotional bond between mother and child grows stronger when the mother is at home.[3] A generous parental leave system should therefore have positive effects on children's wellbeing via increases in the quantity of time the parents spend with their children. In addition, it is possible that children under home care benefit from extended breast feeding and are less exposed to health hazards (e.g. infections) which may be detrimental to child development.[4] On the other hand, maternal employment can also be positively related to child development. Employment may lead to increased family income which may be used to for alternative human capital investments, or affect the frequency of depression among mothers in an ambiguous direction.[5] In addition, it is conceivable that the alternative day care is of better quality than the care provided at home. Parental leave extensions may also affect fertility decisions which may lead the indirect effects on siblings if family size and/or child spacing affect child outcomes.[6]

Boeri et al (2005) present a fairly comprehensive review of results from earlier empirical studies of how mothers employment decisions affect early cognitive and non-cognitive skills, later educational attainment, and many other behavioral and earnings related measures realized in adulthood.[7] The reviewed studies do however struggle with potential selection and endogeneity problems so it is uncertain whether the results should be interpreted as causal.

[3] We will follow the convention in the literature and not discuss the role of *paternal* care. Although paternal leave is a growing phenomenon in some countries, including Sweden, it was of minor importance for the cohorts studied in this paper and remains a marginal phenomenon in most countries today.
[4] See e.g. Berger et al (2005), Gregg et al (2005), Ruhm (2000) and Tanaka (2005).
[5] See discussions in Parcel and Menaghan (1994) or Moore and Driscoll (1997).
[6] See e.g. Lalive and Zweimüller (2008) for evidence on fertility effects of extended parental leave duration, Black et al (2005) or Åslund and Grönqvist (2007) for estimates of the effect of family size on child outcomes, and Petterson-Lidbom and Skogman-Thoursie (2007) for estimates of child-spacing effects on child outcomes.
[7] See e.g. Hill and Duncan (1987), Haveman et al (1991), Harvey (1999), Ruhm (2000), Parcel and Menaghan (1994), Han et al (2001), Waldfogel et al (2001), Chase-Landsdale et al (2003), Ruhm (2004), Liu et al (2007), Bernal (2008).

A few recent papers have used policy reforms as natural experiments to address the long-term effects of parental leave on children's outcomes. Dustmann and Schonberg (2008) evaluate three waves of maternity leave coverage reforms in Germany in the late 1970s, 1986 and 1988 and find no evidence that the reforms improved the children's future wages or high school attendance.[8] Wurtz (2007) uses a regression discontinuity approach to study a Danish increase in parent's leave from 14 to 20 weeks during 1984. The paper finds no significant evidence on long term effects on children's educational outcomes as measured by high school completion and, for a small subsample, test scores.[9]

Our main contribution relative to the recent literature is threefold: First, we use population wide data on test scores so that we can measure effects on school performance with considerable precision. Second, the large data set also allows us to study how the effects vary with characteristics of the parents. Of particular interest is the role of mothers' education. It is well known that parents' education is one of the strongest predictors of children's scholastic performance. Recently substantial effort has been devoted into separating out the "causal" effect of parental education as apart from other correlated inheritances (e.g. genes).[10] However, the exact mechanisms by which parents' education affect their children are still not very well understood. In particular, it is possible that educated parents provide better care when at home, but equally plausible that they value, and therefore arrange for, better quality care when they are working.[11] By separately estimating the effects of prolonged leave spells for high- and low-educated mothers we are able to distinguish between these two hypothesizes. Third, we provide evidence on the impact of the reform on several mechanisms suggested by the previous literature. We analyze how the reform affected child health, parent fertility and

[8] Horisch (2008) provides a complementary analysis in the German case, adopting models of sibling differences, again finding no evidence of long run effects.
[9] Other credibly identified studies include Baker et al (2005) who study an extension of Quebec's kindergarten system, which could be seen as a close substitute to parental care. Using other provinces as a control group, they found that extensions of childcare which increased maternal labor supply lead to worse outcomes for the children. In addition, Baker and Milligan (2008) study an expansion of parental leave benefits in Canada from 25 weeks to 50 weeks but find little effect of the reform on children's early development.
[10] See Björklund et al (2006) and references therein.
[11] As suggested by results reported in Schonkoff and Phillips (2000).

sibling spacing, as well as parental well-being as indicated by divorces and the mental health of the mothers.

Our results suggest that the duration of parental leave benefits, on average, have no effect on children's average school performance. Our heterogeneity analysis show positive and statistically significant effects for children of mothers with tertiary education, suggesting that the parental leave extensions strengthened the relationship between maternal education and scholastic performance of the children. We find no effects on mothers' future labor earnings, fertility, child health, parental separations or indicators of the mothers' mental health.

The paper is structured as follows, in section 2 we discuss the institutional background. In section 3, we present the data. Section 4 presents the empirical models. Section 5 shows the results and section 6 concludes.

2 Institutional background

2.1 Swedish parental leave policies and the 1988–89 reform[12]

The Swedish parental leave system was established in 1974. Originally parents were entitled to 6 months of paid leave at a compensation rate of 90 percent of previous earnings. Those with no prior earnings earned a lower flat rate. It is not required that parents to use their entitled days in sequence and parents can use the benefits at half or quarter of full time.[13] This flexible structure has remained intact over time.

Benefit levels are calculated as a fraction of earnings from the 240 days preceding child birth. The replacement rate remained at 90 percent of earnings until 1995 and the maximum benefits were only adjusted according to inflation during this period.[14] The

[12] This section draws on Sundström and Duvander (2002) and Riksförsäkringsverket (2002). See also Björklund (2006).

[13] From 1987, parental leave days should be used before the child turns 4, in 1989 rules changed back to the pre-1987 rules of usage before age 8, but the evidence suggest that very few days are used after age 4.

[14] Since 1986, special rules pertain for time spent in parental leave so that benefit levels pertaining to the first child birth can be saved and used also for the next child. Maximum amounts are calculated as 7.5 "base amounts", where base amounts are the concept used to rescale all benefit systems in Sweden. In 1999 the cap was at annual earnings of USD 32,300 (Sundström and Duvander, 2002). From 1995 onwards there has been a series of reforms altering the replacement rate and parts of the leave entitlement have been earmarked for the father (1 month from 1995 and 2 months from 2002).

lower flat rate for those without previous earnings remained at 60 SEK (10.1 USD in 1990) per day between 1987 and 2002.

As shown in Table 1 the implementation of the policy was followed by a series of extensions between 1975 and 2002.[15] This paper studies the reform of 1989. This extension from 12 to 15 months was implemented from July 1st 1989 but retroactively covered parents to children born from October 1988 onwards. In addition, parents to children born in August (September) of 1988 received 1(2) extra month(s) of leave entitlement.[16]

Table 1 Reforms in the parental leave benefit system

Reform year	Total number of remunerated days	whereof days only compensated at lower flat rate
1974	180	0
1975	210	0
1978	270	30
1980	360	90
1989	450	90
2002	480	90

[15] The first number in the parenthesis indicates the number of months with 90% of previous labor earnings, while the second number indicates the number of months with lowest flat rates. Multiple births give an extra 180 days per child.

[16] See the 1989:100 act on changes in the Public Insurance Act (”Lagen om allmän försäkring”) available at www.notisum.se.

Our empirical model (see section 4) studies children born in different months of 1988, comparing them to children born in 1987 and 1989. Given our empirical strategy, the most important features of the reform are that

- it was implemented with respect to birth dates so that differences in entitlement between children are well-defined
- it covered only parts of the cohort born in 1988 so that we can compare outcomes within a birth cohort
- it was launched after the births of the 1988-children (and the conception of the 1989-children) so that the composition of children is unaffected by the policy and
- no other relevant reforms took place during the period.

2.2 Parental leave usage around the 1988-89 reform

To assess the impact of the reform we first use aggregate data from the National Insurance Board and display the number of days of paid parental leave by year, normalized by the number of children born the year before (Figure 1). The measure is somewhat crude since parents not always use their entitled leave in the calendar year after birth. The usage starts at the level of the old entitlement (360 days) and then converges towards the new level of entitlement (450 days) by 1991. The pattern is entirely consistent with a report from the National Insurance Board,[17] which uses detailed data from the late 1990s showing that most days of parental leave are used during the child's first 2 years, with some non-trivial usage for an additional half year (see also Sundström, 1996, for similar conclusions regarding the 1980s). The report also show that at least 97 percent of the 360 parental leave days reimbursed at the replacement rate and between 85 and 90 percent of the 90 flat-rate days were used during all years of the 1990s. Since this implies that parents used nearly all the benefit days they were entitled to *after* the extension it is likely that the effect in terms of benefit usage was nearly complete.

[17] SOU (2005:73), page 142.

Additional statistics show that approximately 90 percent of used days are by parents with labor earnings during the 240 days qualifying period and that 93 percent of parental leave days were used by mothers. Both of these numbers hold before as well as after the reform (see e.g. Sundström and Duvander, 2002).

In Figure A1 in appendix A we use data from the Swedish Labour Force Survey and show that total (paid or unpaid) absence "for child care reasons" during the survey week among employed women with small children increased from 20 to 25 percent around the time when the first cohorts received extended benefits. This relative increase of 25 percent is exactly at par with the extension of entitled benefits. The rate of absence for other reasons was unchanged during the same period for the same population. This suggests that the increased use of paid benefits did not serve as a subsidy of previously unpaid absence.

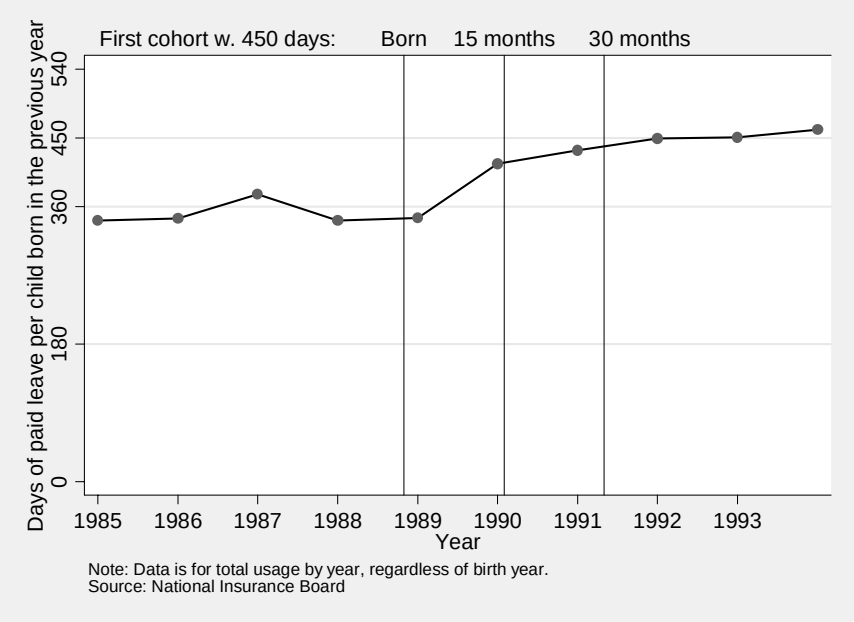

Figure 1. Full-time equivalent days of benefits per child born in the previous year.

2.3 The impact of transition rules

The identification used in this paper relies on using the transition rules applying to children born during 1988. Although the numbers presented above suggest a full *long-run* pass-through of the reform, they do not asses whether the impact was immediate. In

figure 2 we show how real labor earnings during the calendar year after child birth depended on the month of birth for children born in the years 1987 to 1989. Not surprisingly earnings are lower the later in the year the child was born, independent of the year of birth. Importantly, we also see a linear convergence during the transition months (August to October 1988) when behavior smoothly adjusted from the pre-reform (1987) trajectory to the post-reform (1989) trajectory. This pattern suggests that the transition rules had a real "bite" and that the impact of the reform on the mothers' behavior was immediate. Figure A2 in Appendix A show that the pattern is equally clear independent of the mothers' levels of education. Figure A3 show a similar pattern for the second year after child birth.

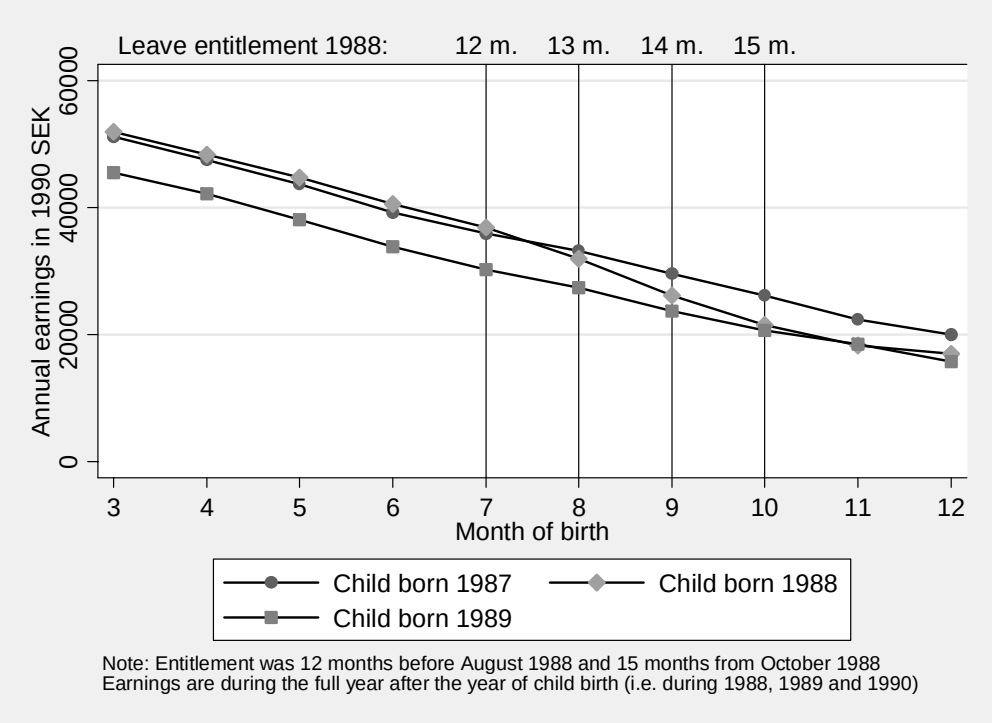

Figure 2. Mothers real labor earnings during the full year following child birth – averages by month of child birth.[18]

[18] The average exchange rate during 1990 was 5.92 SEK/USD.

2.3.1 The alternative: Publicly provided child care

The effects of extended parental leave policies are likely to depend on what the children would have done if their mothers had not stayed at home. In the Swedish context the most frequent alternative is participation in publicly provided, and heavily subsidized, day care. Figure 3 uses survey data on the type of child care in January of 1989 to 1992.[19] The figure shows that most children younger than 11 months were in parental care, both before (88 percent) and after (95 percent) the benefit extension. Among children aged 12-23 months, roughly half (40 percent before and 50 percent after) were at home with a parent.

The figure also shows that, regardless of age, very few children were catered for by alternative private child care arrangements (such as grandparents, neighbors etc). The vast majority of children who were not at home with their parents were either at a public day care centre or at a "family centre". The latter category captures day care which is organized and provided by the municipalities although located in a care giver's home. Starting in the mid 1980s day care centers have increasingly replaced family centers; a trend which is also visible in the graph. Private care has remained rare.

[19] Corresponding data were not published before 1989.

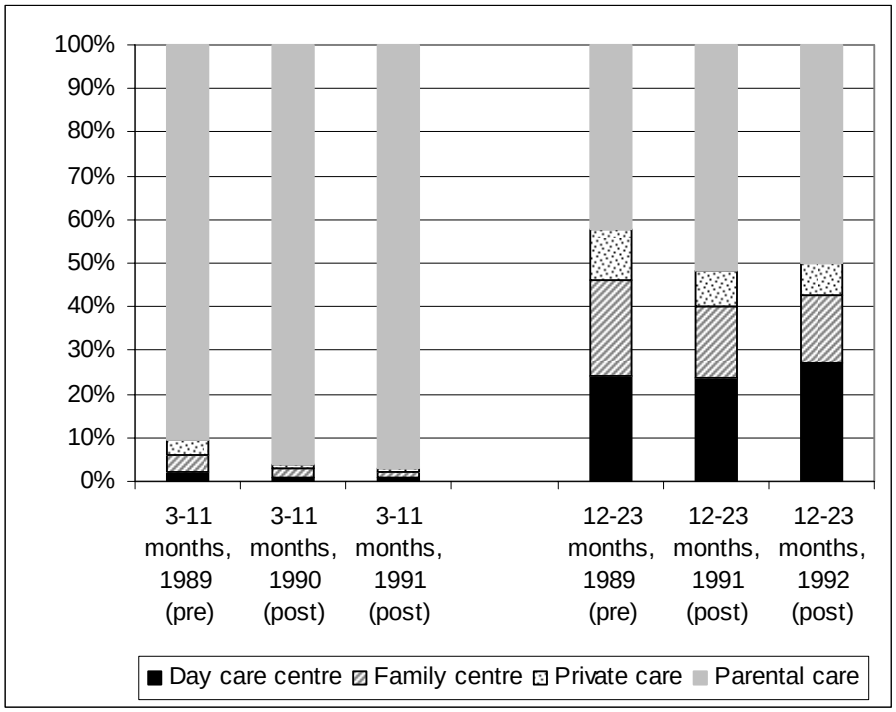

Figure 3. Care for children aged 3-11 months and 12-23 months born before (pre) and after (post) the reform.
Source: Statistics Sweden's Child Care Survey ("Barnomsorgsundersökningen") 1989-92, table S2.
Note: Data is for January each year. The bar for children aged 12-23 months in 1990 is not shown since some of the children are pre-reform and some are post-reform (46 percent were in parental care).

2.3.2 Female employment and the business cycle

During the late 1980s to the early 1990s Sweden had a remarkably low unemployment rate. Between 1987 and 1991 the unemployment rate for females was between 1.5 and 3 percent in all years. The female employment rate of around 80 percent was only 4 percentage points lower than that of males. This means that the cohorts we are interested in, i.e. children born in 1987 to 1989, were born in a period where females in general were employed. Furthermore, since unemployment was so low it is reasonable to assume the decision whether to work or not while the children were small was a pure supply decision.[20]

[20] There was a substantial negative shock to the Swedish economy in the early 1990s where the unemployment rate rose rapidly to 8.2 percent for females in 1993 and employment rates plummeted, reaching 72 percent for females in 1993. But these events happened later than the relevant leave entitlement periods.

3 Data

Our analysis is based on population wide registers from the IFAU-database. The data base combines data from various sources such as Statistics Sweden and the National Board of Education. Our primary sample consists of all children born in Sweden between 1987 and 1989.

We assign parental leave entitlement according to the month of birth of the child in accordance with transition rules that were in place due to the extension of parental leave entitlements during 1989. These rules meant an extension of 30 days (60 days) for children born in August (September) of 1988 and 90 days thereafter. We measure the duration of parental leave entitlements in months.[21]

3.1 Scholastic performance

Our main outcomes are scholastic performance indicators at age 16. Swedish children start school during August of their 7th year and are mandated to participate in 9 year of schooling. Thus, compulsory schooling ends during the child's 16th year since birth (i.e. 2003 to 2005 for the relevant cohorts). Fredrikson and Öckert (2006) present a detailed description of the Swedish institutions that are relevant when estimating month-of-birth-effects on Swedish data. The main complication is that some children born early in the year start (and thus complete) school one year earlier (i.e. at age 15) and that some children, most notably those born late in the year, postpone their school start.[22] Grade repetition is extremely rare in Swedish schools. To reduce the impact of early school start we remove all children born in January or February each year. This comes at a small cost since the reform started in the third quarter of 1988. Since we have relatively few months after the reform we keep children born in December but we have verified that the reform did not have any impact on delayed school starts (results are available on request).

Our main outcome is test-scores from national tests during the final compulsory school year. These tests are intended to be used by teachers as a part of their assessment

[21] We define a month as having 30.5 days. Thus, those born before August 1988 are assigned 11.8 months of paid leave, those born in August 1988 have 12.8 months, those born in September 1988 have 13.8 months and those born in October 1988 or after have 14.8 months of paid leave entitlement.

when allocating the final grades. Tests are marked locally according to a national standard and then collected by the National Board of Education.[23] The national tests measure skills in Mathematics, Swedish and English. These three subjects are the three "core subjects" in compulsory school. The Math score is further divided into one basic and one advanced part and although we only report the results for a combined score, we have verified that the results are consistent over both parts.[24] We transform all test results to mean zero and standard deviation 1 within each cohort (before applying any restrictions) in order for the estimates and descriptive statistics to be readily interpretable. We have verified that all results hold also when using outcomes scaled in terms of percentiles.

We also look at compulsory school grades which are the selection tool when students apply for upper secondary education. Grades are set by the teacher in each subject using a four-tiered scale in each subject where "fail", the lowest grade, indicate inadequate performance. We use two variables based on these grades. First, the official grade point average (GPA) which is calculated by allocating 0 to each failed course and otherwise 10, 15 or 20 depending on the grade on a maximum of 15 courses. Second, we construct a "core GPA", as the average grade in the three core subjects (Math, English and Swedish).

3.2 Other outcomes

In order to assess the importance of some mechanisms suggested by previous literature we study how the reform affected a host of intermediate processes. We analyze the timing and number of future siblings (on the mother's side) using the mother-child links. We use data on parental separation where parents are considered as separated if they are neither married nor residing at the same address at the end of the child's 14th year. We use health data which cover admittances to hospitals between 1987 and 2005. These data records the date of admission and the diagnosis according to the

[22] 8 percent of those born in December, 4 percent in total.
[23] Tests are supposedly not marked by the grading teacher, but we cannot verify that schools comply with this rule.
[24] We aggregate the tests by giving the advanced score the weight of 1.5 times the basic part using the same logic as when the official GPA is calculated. The tests in Swedish in English are based on three subtests, each of which is graded on the four-tiered Swedish grade scale. We also aggregate these tests according to the metric used when calculating the official GPA.

international standards ICD9 (until 1996) or ICD10.[25] We construct two types of outcomes from these data. For children we look at the probability of being admitted to a hospital for any reason within a number of months after birth. For mothers, we are primarily interested in the effects on mental health and therefore include all instances of admittances due to mental health reasons.[26] The health data are drawn from an alternative data base and the samples are therefore marginally different but the deviations are trivial (details are available on request). The estimated models will also have a slightly different set of control variables as indicated below the relevant tables.

3.3 Control variables

Our model (see below) includes a number of control variables. The variables measure maternal and paternal employment during November the prior to child birth, and labor earnings in the year of, and the year prior to, child birth as well as the immigration status, age, county, and dummies for the highest attained education of the mother and father measured in the year of child birth, as well as the gender and birth order of the child and the number of siblings born until 2003. In addition we include a dummy for whether the father and mother is residing in the same parish at the end of the child's birth year, serving as a proxy for being an (initially) intact family. All of these variables are drawn from population wide registers.

3.4 Restrictions

We place the following restrictions on our main data: Children in the data:

- are born in Sweden between March and December any of the years 1987 to 1989,
- have a mother who was a Swedish resident during the year of birth and the year before that (ensuring that all are entitled to the benefits),
- are not twins or triplets (since special rules apply for these),

[25] See http://www.who.int/classifications/apps/icd/icd10online/

[26] From 1997 this is straightforward, we use all ICD10 codes in the F00-F99 interval which means "Mental and behavioural disorders". Since there is no one-to-one link between ICD9 and ICD10 (the revisions were substantial) we proceed bluntly with the older data. We use a translator which include all combinations of ICD9 and ICD10 codes and, code as mental disorders all ICD9 codes (on the 3-digit level) where the majority of the corresponding ICD10 codes (which may be many) are in the F00–F99 interval. More than ¾ of these codes have an unambiguous match and the results are not sensitive to different versions of this procedure (i.e. using all codes with some F00–F99 match, or only using codes that fully match into F00–F99).

- participate in regular Swedish compulsory schooling at age 16 (i.e. are not enrolled in special schools for disabled children),
- complete a national test, or receive grades from compulsory school, depending on specification (i.e. non-missing outcomes).

3.4.1 Data limitations

Our data have two main limitations which are important for the set-up of our study. First, we cannot measure time of birth at a more exact frequency than months. Since the treatment intensity increases linearly with month of birth between August 1988 and October 1988 we are thus not able to use a meaningful regression discontinuity set up. Second, we are not able to measure the impact on parents' time off from work other than through annual labor earnings. Using annual earnings is problematic in several dimensions. For example, we are not able to quantify the impact in terms of hours worked so we cannot estate a "first stage" for an IV-strategy. Neither can we differentiate between potential effects on hours and effects on wages. In addition, it is a complicating factor that the impact of the reform will appear during different outcome years depending on the month of birth. This creates a mechanical interaction between the reform (benefit durations) and month of birth at the annual frequency. This effect is visible both in figure 2, where the lines for 1987 and 1989 slowly converges (parents to children born in December work very little in the following year both before and after the reform), and more clearly in figure A3 which studies the second year after birth where the slopes are very different depending on when during a year the child is born. Although one could, in principle, look at the full impact by pooling data from a sufficient number of outcome years, this would require looking so far ahead that the estimates will be very imprecise, and potential wage responses are likely to bias the estimates.[27] Hence, all of our estimates will focus on the effect of the policy variable, i.e. *leave entitlement*. However, since the evidence presented in Section 2 suggests a nearly full long-run impact of the reform the differences in interpretation should be small.

3.5 Descriptive statistics

Table 2 displays summary statistics, all of which show expected patterns: Approximately forty percent are first-born children; half of the sample consists of males; the average age of mothers at child birth is around 28 years old (31 for fathers). Almost 90% of the mothers (over 90% of the fathers) were employed the year before children's birth which is consistent with the high female labor market participation rate in the late 1980s. Around three quarters of both mothers and fathers had completed at least a 2-year upper secondary education and one quarter had at least some tertiary education. The descriptive statistics suggest that there is little variation between years in the characteristics of the mothers or the fathers. Most importantly for identification purposes, nothing suggests that the *seasonality* differ between years.

The most notable observations regarding outcomes is that those who were born in the early half of the year have on average much better scholastic performance, both in terms of test scores and grades. This is in line with results in Fredriksson and Öckert (2006) showing that the early-born children do better in school since they are older when they start.

4 Identification

The parental leave reform affected parents to children born in 1988 depending on when the children were born. Our identification cannot solely rely on the variation between children born 1988 since such an analysis would be confounded by generic season-of-birth effects. None of the children born in 1987, but all children born in 1989, were affected by the reform buts since all our outcomes may vary between years for other reasons (e.g. because tests are different) we do not wish to rely on direct comparisons between years for identification.

We therefore employ a model which relies on the within-1988 variation in entitlement and use data from 1987 and 1989 in order to purge the model of the season-of-birth effects. To this end we first include 9 seasonal month-of-birth dummies, one for

[27] Using two years after the reform (i.e. combining figures 2 and A3) we find a total difference between post and pre-reform earnings of 9,996 SEK in real currency for children born in December. This corresponds to 16.6 percent (i.e.

each included month (except one), assuming that the effect of seasonality is equal between years. In addition we have a dummy for each birth cohort which removes year-of-birth effects that are independent of birth season. Thus, denoting the outcomes by *Y*, entitlement by *E*, dummies for season (month of birth) and year respectively by *D*:s we estimate:

$$Y_i = \gamma E_i + D_i^{Birth\_season(month)} + D_i^{Year\_of\_birth} + \varepsilon_i \qquad (1)$$

The parameter of interest γ is properly identified if the pure effects of seasonality are the same in 1988 as in the comparison year(s). In order to assess the robustness of this assumption we apply the model to three different samples: the first one studying only the years 1987 and 1988, the second one studying the years 1988 and 1989, the third one studying all three years.

To further strengthen the identification we also estimate a model with a linear time trend (*T*) for each month (where time is measured in years). In addition, we add a set of covariates (*X*) attempting to capture potential pre-birth differences between parents. Thus, our richest model can be written as:

$$Y_i = \gamma E_i + D_i^{Birth\_season(month)} + D_i^{Birth\_season(month)} \cdot T_i + D_i^{Year\_of\_birth} + \beta X_i + \varepsilon_i \qquad (2)$$

The model of equation (2), which is estimated on 1987–89 data, allows for changes over time in seasonality, as long as the changes are linear in years. Thus the model is identified as long as there are no month-specific events in 1988 that are unrelated to trend changes in seasonality.

The covariates we include are measured before the relevant child birth and capture both parents' ages (with squares), education (8 dummies), employment status in the pre-birth year, labor earnings if employed (with square and cube), labor earnings in the previous year if employed (with square and cube), immigration status, a dummy for

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two months) of average annual earnings three years after child birth for this cohort.

intact families and county of birth dummies. In addition we control for the gender of the child, birth order and the complete number of siblings.

4.1 Effects on the composition of children

In order to test the identifying assumptions we have estimated the "effects" of the reform on predetermined variables. In Table 3 we present estimates of how the reform affected the composition of children in terms of a number of observable characteristics. We estimate equation (1) above on data from 1987 to 1989 using birth-order as well as parental age, employment and education as outcomes. The presented estimates are for one month extension of parental leave benefits. Since all estimates are miniscule and statistically insignificant we conclude that the reform did not have any compositional effects, which is consistent with our identifying assumptions as well as with the fact that the reform was implemented *after* the conception of the children as described in Section 2.

5 Results

5.1 Main results

Table 4 shows the estimated effects of parental leave entitlement on children's scholastic performances using data on children born in the months March to December 1987 to 1989. The results suggest that access to longer parental leave does not have any long-term impact on the children's average scholastic performances as measured by standardized test scores in Swedish, English, or Mathematics nor as measured by GPA or GPA in the three core subjects.

The results from five different models based on equations (1) and (2) are consistent. All estimated coefficients are small and insignificant. We have verified that the results hold also if the outcomes are percentile-ranked instead of measured in terms of standard deviations. The Math test has two parts, one basic and one advanced and we have verified that the result hold for both parts. In addition, we have estimated the effect on the probability of a delayed school-start and the probability of getting better than a

"fail" grade in all three core subjects but the results (available on request) show that these margin are not affected either.

Although most of the point estimates are positive, it should be stressed that the precision around zero is good—the largest estimate using the most tightly specified model (column 5) is for Math and Swedish (0.002) and the confidence interval around this estimate only allow for effects of at most 0.8 percent of a standard deviation from a one month longer leave entitlement. As a comparison, being born one month earlier (results are available upon request) would on average improve the test scores by about 2 percent of a standard deviation. Thus, the point estimates suggest an effect in the order of (at most) being born approximately 3 days earlier and a 95 percent confidence interval (at most) allows for an effect corresponding to being born 2 weeks earlier.[28]

An alternative is to compare the results to the educational differences depending on mothers' education. Cross-sectional estimates (results are available upon request) show that children to mothers with a 3-year upper secondary diploma have 30 percent of a standard deviation better result than children to mothers with a 2-year (vocational) diploma. These estimates cannot be given a causal interpretation but nevertheless provide an interesting baseline. An extra month of leave entitlement is in the order of half a week of maternal education and the confidence interval at most allows one month extra leave to affect the test scores by the same amount as 1.2 weeks of maternal education.[29]

5.2 The role of mothers' education

In this subsection we estimate the effects of prolonged parental leave durations for high- and low-educated mothers respectively. By doing so, we are able to provide evidence on why children of highly educated parents perform better at school. Since the extension of parental leave entitlements shifts time from public child care to maternal care we are able to see if high- (or low-) educated mothers provide more human capital accumulation at home relative to that of the public day care system.

[28] Each day increases test scores by 0.02/(30.5) so for the point estimates 3 days*0.02/30.5=0.002. The upper bound for the confidence interval is 0.002+2*0.003=0.008 and 12 days*0.02/30.5≈0.008.
[29] Assuming that one year in upper secondary education corresponds to 45 weeks of education, each week improves the test scores of children by 0.3/45=0.0067 and 1.2*0.0067≈0.008.

Table 5 shows results for the sample divided into three education groups, only compulsory school, upper secondary, and at least some tertiary, where the first and last group includes approximately one quarter of the sample each. The results show that the duration of parental leave does have a positive and statistically significant effect for children to well-educated mothers. One-month additional parental leave entitlement improves the test results by 2 percent of a standard deviation for these children. This is in the order of being born almost one-month earlier. On the other side of the scale, all coefficients are negative but insignificant for the quarter of children whose mothers lack education beyond compulsory school. We have verified that these results hold in all models shown in Table 3 and also when looking at percentile ranked test scores.[30] In the second part of Table 3 we show estimates from samples split according to fathers' education, but all estimates are small and insignificant.

One possible concern is that low educated mothers might have been less informed about the transition rules. However, in Figure A1 in Appendix A we display the impact of the reform in terms of real annual earnings for both the highest and lowest educated mothers during the year after each cohort was born, depending on month of birth.[31] As shown in the figure, the transition rules seem to have had a full impact on both the low and high educated groups—the behavior of the mothers to children born in 1988 does in both cases fully converge from the pre-reform to the post-reform behavior during the transition months. We have also verified that the result hold if we remove the two months (August and September) that were only partially covered by the extension.[32] Thus, the differences in outcomes between the education groups do not seem to be the result of differences in information regarding rights to benefits during the transition months.

One possible interpretation of the estimates is that maternal education serves as a proxy for mothers' earnings which in turn determine benefit levels. Since benefits levels are determined by earnings in the last 240 days before child birth we have analyzed how the outcomes vary with mothers' annual earnings in the pre-birth year. The estimates

[30] An estimate of 0.02 standard deviations correspond to approximately half a percentile rank.
[31] For the impact on the whole sample see figure 3, section 2.

presented in Table 6 do not support the notion that the effects are larger for mothers with high pre-birth labor earnings (and therefore also high benefits). We find similar results when splitting the sample according to the fathers', or both parents', earnings percentiles or when using residual earnings instead (results available on request).

An alternative explanation is that the pattern we find is due to the quality of the children. As noted, children to highly-educated mothers are on average better students and it is possible that the effects of parental time differ depending on the general ability of the students. Partly, the absence of heterogeneity with respect to fathers' education speaks against this explanation but to further test the hypothesis we have split the sample based on predicted school performance using all available characteristics except mothers' education. The estimates for different samples of predicted performance shown in the second part of table 6 are all insignificant and small, suggesting that the positive effects for children with highly educated mothers is not driven the overall quality of the children.[33]

5.2.1 Heterogeneity in other dimensions

We have investigated a number of other dimensions but find no evidence of heterogeneity with respect to mothers' or fathers' immigration statuses, pre-birth employment statuses, whether the family is intact at the end of the birth year, the gender of the child, or whether the child is first or last born in the family. The only exception is for the small sample of lone children where we find significant positive results. However, this result is not entirely robust, and we do not have any straightforward explanation for it.[34]

[32] In addition we have removed December where delayed school starts are more frequent, again finding very similar results.

[33] If we include mothers' education in the prediction, we find some positive estimates, but smaller than if we only split the sample according to the mothers' education.

[34] A possible interpretation is that these children are constrained in terms of maternal time since they cannot "free ride" on parental leave related to siblings. Note thought that this interpretation is inconsistent with the perceived negative effects of family size (see the discussion in Black et al, 2005). The estimates are however not significant in models that do not include covariates and they do not stand for the test we show for mothers education in Section 5.3 below.

5.3 Mechanisms

Here we provide evidence related to mechanisms suggested by the earlier literature such as future labor earnings, child health, fertility and parental well-being. We do this separately for the full sample as well as for the lowest and highest educated groups.

We start by looking at long run effects on mothers' earnings. Results are shown in Table 7. Here we use real annual earnings and find no evidence of long-run effects on the labor market outcomes of mothers which is consistent with results found by Lalive and Zweimuller (2008).[35] Figure 3 below we show the impact 6 years after child birth (further results are available on request) for the groups with highest and lowest education. Due to lack of appropriate data we are unfortunately not able to properly estimate the effects on working hours and wages.

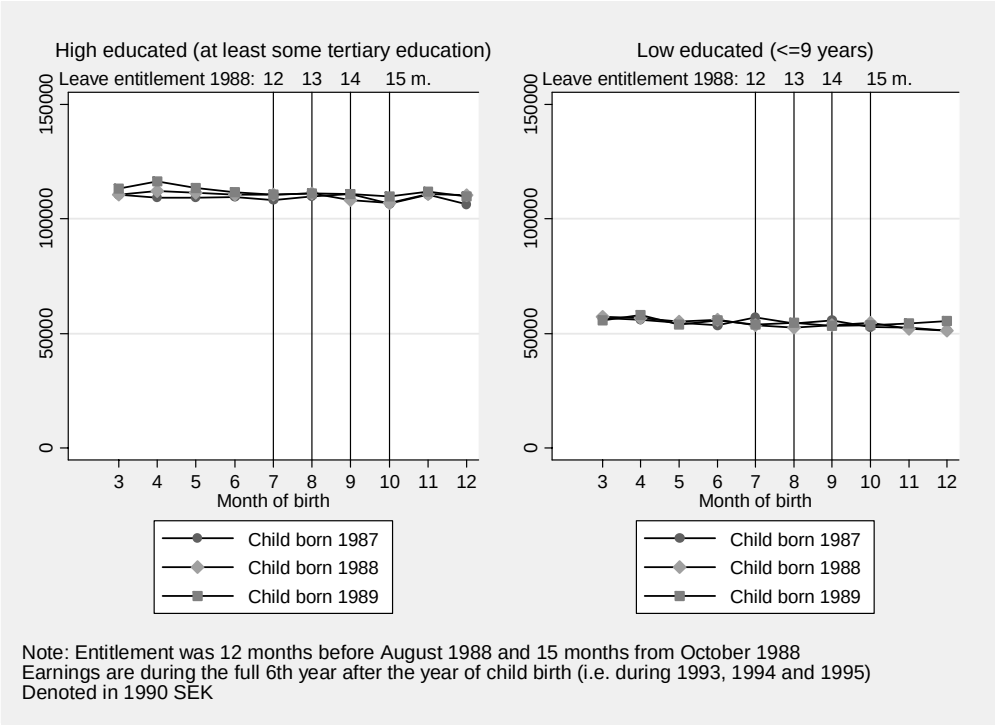

Figure 3. Month of birth and mothers' real annual earnings 6 years later, averages by birth month.

The second panel looks at indicators of child health. We use data on admittances to hospitals and our model estimates the probability of being admitted to a hospital at least once within 3, 6 and 16 years since birth. We find no evidence of health effects of increased parental leave at any of the durations or any of the subgroups.

The third panel looks at fertility and child "spacing" (the distance between siblings) to see whether the extended leave spells affected the competition for time within the family. Note that the analysis is partial in the sense that we estimate the effect on fertility of having a longer leave period for the previous child and not the, potentially large, anticipatory effects of more generous benefits on fertility (see Björklund, for such analysis on Swedish data). The only significant effect we find is a small (0.2 percentage points) increase in the probability of having an additional child very fast (within 18 months) which appears to be driven by high educated mothers. The effect is vanishes if we include births up to 24 months. The effect is interesting since it suggest that mothers may reduce child spacing if parental leave spells become sufficiently long to facilitate sequential births without intermediate return to work. However, the effects are unlikely to contribute to our results regarding cognitive skills. The effects are too small to be of any important magnitude and if anything they should provide a negative effect on the children to well-educated mothers if we trust the evidence in the existing literature (see Petterson-Lidbom and Skogman-Thoursie, 2009).

Finally, we look at indicators of general parental wellbeing. As proxies we use the probability that parents separate and mothers' admittances to hospitals due to mental disorders. Admittedly, these two measures are based on rather extreme outcomes and may be only loosely related to general well-being. However, taken at face value the estimates provide no indications that parental well-being, as measured by these variables, is affected by the duration of parental leave for any of the education groups.

5.4 Robustness: Benefit extensions and the relative school performance of children to educated mothers

Since we have analyzed a number of subsamples, a possible concern is that the significant effects found for highly educated mothers may be spurious. Although

[35] Effects for other years than those shown in the table are available upon request.

maternal education is a first candidate for theoretical reasons, we would still like to corroborate our results using some alternative identifying variation. We have therefore estimated a Difference-in-Difference (DD) model describing how parental leave entitlement affect children to high educated mothers *relative* to children of low educated mothers. We exclude all observations from 1988 so as to purge the analysis of the within-year variation we have relied on so far. Instead we only use data from the last complete pre-reform year (1987) and the first complete post-reform year (1989). Formally, we estimate:

$$Y_i = \lambda E_i * D_i^{High\_Educated} + D_i^{High\_Educated} + \varphi E_i + \beta X_i + \varepsilon_i, \qquad (3)$$

where the notation is as before. Since entitlement do not vary within year in this model, the entitlement variable captures common (i.e. education invariant) differences between years. We only include children to mothers with *either* less than upper secondary education *or* at least some tertiary education. Estimates of λ show how the benefit extensions changed the association between mothers' education and children's cognitive skills. The results found in the first column of Table 8 show positive and significant estimates on most outcomes suggesting that the benefit extension improved the relative performance of children to well-educated mothers.

We do not have any test score data on cohorts born before 1987. However, assuming that the reform only affected the level of relative performance and not the trend, we can purge the analysis of the influence of trends by using an additional post-reform year (i.e. children born 1990). This model includes year dummies as well as an interaction term between time (linearly in years) and maternal education. The results, shown in the second column of Table 8, become less precise in this specification, but the point estimates are almost not affected at all. Overall, the results in table 3 and table 8 therefore consistently suggest that longer parental leave spells improve the relative performance of children to high educated mothers.

6 Conclusions

The paper studies how extensions of paid parental leave affect the children's scholastic performance. We use a quasi-experimental strategy to isolate the effects of benefit entitlement and our results are robust to a number of variations regarding the identification strategy. Overall, the evidence suggests that a prolonged parental leave system do not affect average scholastic performance. This result is in accordance with two recent studies using similar strategies on Danish (Wurtz, 2007) and German (Dustmann and Schonberg, 2008) data to identify the long-run effects of parental leave, although on slightly different outcomes. [36]

Using population wide data allows us to study the effects separately for different parts of the population. We do find significant positive effects for children to well educated mothers, and negative but insignificant results for children to low educated parents.

We study a number of intermediate outcome variables which are related to processes discussed previously in the literature. We study long-run earnings effects for mothers, child health, fertility decisions, parental separations and indicators of the mothers' mental health. None of these outcomes appear to have been affected by the reform for any of the education groups with the exception for a small reduction in subsequent child spacing among well-educated mothers.

Given that we i) do not find any impact on intermediate outcomes, ii) we fail to find any heterogeneity with respect to fathers' education, parents' pre-birth earnings nor with respect to other indicators of children's scholastic quality, and iii) the difference in impact is corroborated by means of a between-cohort comparison in the relative performance of children, we interpret the estimates as suggesting that there are positive interaction-effects between time-at-home and maternal education. [37] The results are

[36] Ongoing work (parallel to this paper) by Carniero et al (2009) find enormous effects of a Norwegian leave extension in the mid 1970s. The results suggest that high school drop out rates were reduced by 3 percentage points (and height at age 18 reduced by nearly a centimeter) due to an extension of parental leave from 4 to 6 months. One possible explanation for why these results deviate from the rest of the literature is that the paper studies shorter spells (with a possible effect through early breast feeding), and under a very different institutional setting. The bulk of the effect appears to be driven by low educated parents in rural areas whose children otherwise where catered for by their grandparents.

[37]

consistent with an interpretation where the Swedish public child care system provides less human capital accumulation than well-educated mothers, but more than low-educated mothers, when these mothers stay at home.

From a policy perspective it is noteworthy that the results suggest that expanding parental leave entitlements does not improve schooling results on average nor has any beneficial distributional effects. Thus, a reasonable conclusion is that extensions of parental leave policies, at least in countries that already have quite generous benefits, cannot be motivated from the perspective of child outcomes as captured by performance at school. Naturally, such extension may still be motivated by reasons related to the general well-being of children or parental outcomes such as fertility, female labor force participation or gender equality.

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Table 2 Summary Statistics of the used data set.

	1987 March-July	Aug-Dec	1988 March - July	Aug-Dec	1989 March - July	Aug-Dec
GPA	0.070	0.008	0.074	0.008	0.076	0.010
Complete	0.916	0.909	0.917	0.909	0.913	0.905
Test results						
Swedish	0.066	-0.007	0.038	-0.027	0.040	-0.035
English	0.035	-0.008	0.031	-0.006	0.036	0.001
Math-Basic	0.053	0.010	0.063	0.010	0.063	0.004
Math-Advance	0.046	-0.005	0.046	-0.005	0.051	-0.015
Math	0.054	0.001	0.055	0.001	0.059	-0.008
First born child	0.407	0.427	0.406	0.428	0.414	0.430
Male child	0.510	0.510	0.514	0.513	0.515	0.512
Intact family*	0.950	0.939	0.947	0.934	0.943	0.932
Maternal char.						
Age*	28.7	28.4	28.7	28.4	28.7	28.3
Employment**	0.872	0.863	0.877	0.873	0.893	0.887
Education*						
<=9 yrs schooling	0.23	0.24	0.22	0.23	0.22	0.23
Upper secondary	0.52	0.51	0.54	0.53	0.54	0.54
Tertiary	0.25	0.25	0.25	0.24	0.24	0.23
Paternal char.						
Age*	31.6	31.3	31.6	31.3	31.6	31.2
Employment**	0.915	0.903	0.922	0.908	0.922	0.909
Education*						
<=9 yrs schooling	0.26	0.27	0.25	0.25	0.24	0.24
Upper secondary	0.50	0.50	0.52	0.52	0.53	0.54
Tertiary	0.24	0.23	0.23	0.23	0.23	0.22
Average month of birth	4.95	9.92	4.93	9.89	4.96	9.91
# Births/month	9,320	7,794	9,855	8,357	10,057	8,749
N	46,600	38,970	49,276	41,783	50,285	43,747

Note: Child outcomes are measured at age 16. * Measured the year of child birth. ** Measured in November the year before child birth. For definitions, see the text.

Table 3. Effects of paid parental leave entitlement on the composition of children.

Outcome	1	2	3	4
	All	Mother compulsory schooling	Mother upper secondary schooling	Mother at least some tertiary schooling
Mother's age	0.003	-0.017	0.002	-0.015
	(0.017)	(0.039)	(0.022)	(0.030)
Mother employed	0.001	0.005	-0.001	0.002
	(0.001)	(0.003)	(0.001)	(0.001)
Father's age	0.017	0.005	0.020	-0.030
	(0.019)	(0.045)	(0.026)	(0.037)
Father employed	-0.000	0.003	-0.000	-0.001
	(0.001)	(0.002)	(0.001)	(0.002)
Order of child	-0.000	-0.005	0.002	-0.008
	(0.003)	(0.008)	(0.004)	(0.006)
Mother's education				
Compulsory education	-0.000	--	--	--
	(0.001)			
Tertiary	0.002	--	--	--
	(0.001)			
N	252,393	57,370	134,106	60,917
Years	1987-89	1987-89	1987-89	1987-89
Trend in month effects	No	No	No	No
Covariates	No	No	No	No

Note: Estimates are for one month extension of leave entitlements. All models include 9 month of birth dummies and a dummy for each (but one) birth year. Robust standard errors are in parenthesis. *(**, ***) Significant at the 10 (5,1) percent level.

Table 4. The effects of paid parental leave entitlement on scholastic performance.

Outcome	1	2	3	4	5
Test results					
Swedish	0.004	0.004	0.004	0.004	0.002
(se)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
English	0.003	-0.001	0.001	0.001	0.000
(se)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Math	0.001	0.006	0.004	0.004	0.002
(se)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)
Grades					
GPA	0.000	0.001	0.001	0.001	0.001
(se)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
GPA Core	0.002	0.004	0.003	0.003	0.003
(se)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
R2	0.00	0.00	0.00	0.00	0.16–0.21
N	134,584–163,396	158,549–170,853	215,783–249,731	215,783–249,731	184,491–212,292
Years	1987-88	1988-89	1987-89	1987-89	1987-89
Trend in month effects	No	No	No	Yes	Yes
Covariates	No	No	No	No	Yes

Note: Estimates are for one month extension of leave entitlements. Outcomes except "Complete grades" are standardized within birth cohort (standard deviation = 1). All models include 9 month of birth dummies and a dummy for each (but one) birth year. Other covariates are included in column 5. These characterize the mother and the father by: age, age squared, education, county dummies (for mother the year before child birth), employment status in the previous year, real labor earnings (with square and cube), real labor earnings in the previous year (with square and cube), immigration status as well as a dummy variable for intact families (at time of birth), and the child by gender, number of siblings and order of birth. The number of observations differs between outcomes due to non-attendance at tests (the largest numbers pertain to the grade outcomes) and between columns 4 and 5 due to some missing information on background characteristics (mostly missing fathers). Robust standard errors are in parenthesis. *(**, ***) Significant at the 10 (5,1) percent level.

Table 5. Heterogeneity: Mothers' and fathers' education.

	Mothers Education			Fathers Education		
	<=9 yrs	Upper secondary	Some Uni	<25%	[25%--75%]	>75%
Test results						
Swedish	-0.001	-0.001	0.013	0.004	0.005	-0.005
	(0.008)	(0.005)	(0.006)**	(0.007)	(0.005)	(0.006)
English	-0.006	-0.004	0.016	-0.002	0.003	-0.003
	(0.008)	(0.005)	(0.006)***	(0.007)	(0.005)	(0.006)
Math	-0.007	-0.001	0.016	-0.003	0.005	0.003
	(0.007)	(0.005)	(0.007)**	(0.007)	(0.005)	(0.007)
Grades						
GPA	0.003	-0.002	0.006	-0.003	0.003	0.001
	(0.007)	(0.004)	(0.006)	(0.006)	(0.004)	(0.006)
GPA Core	-0.002	-0.000	0.016	0.001	0.005	0.001
	(0.007)	(0.004)	(0.006)**	(0.007)	(0.004)	(0.006)
R2	0.06–0.11	0.09–0.15	0.09–0.14	0.07~0.14	0.10~0.16	0.08~0.14
N	37,622–44,501	99,757–113,906	47,112–53,885	44,340~51,575	96,809~111,075	43,342~49,642
Years	1987-89	1987-89	1987-89	1987-89	1987-89	1987-89
Trend in month effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates are for one month extension of leave entitlements. The models are identical to the last column of Table 3, including covariates. The number of observations differs between outcomes due to non-attendance at tests (the largest numbers pertain to the grade outcomes). Robust standard errors in paranthesis. *(**, ***) Significant at the 10 (5,1) percent level.

Table 6. Heterogeneity: Mothers' earnings and children's predicted performance.

	Mothers Earnings Percentiles			Predicted GPA		
	<25%	[25%–75%]	>75%	<25%	[25%–75%]	>75%
Test results						
Swedish	-0.007	0.004	0.008	0.005	-0.000	0.006
	(0.007)	(0.005)	(0.006)	(0.007)	(0.005)	(0.006)
English	-0.005	0.001	0.004	0.005	-0.001	0.004
	(0.007)	(0.005)	(0.006)	(0.007)	(0.005)	(0.006)
Math	-0.004	0.006	0.003	-0.003	0.006	0.004
	(0.007)	(0.005)	(0.007)	(0.007)	(0.005)	(0.007)
Grades						
GPA	-0.001	0.002	0.002	0.002	0.001	0.000
	(0.007)	(0.004)	(0.005)	(0.007)	(0.004)	(0.006)
GPA Core	-0.003	0.005	0.006	0.001	0.005	0.003
	(0.007)	(0.004)	(0.006)	(0.007)	(0.004)	(0.006)
R2	0.15–0.20	0.12–0.18	0.14–0.21	0.04–0.06	0.07–0.08	0.08–0.10
N	39,682–46,260	94,754–108,735	50,055–57,297	41,952–49,435	94,303–107,775	48,087–55,082
Years	1987-89	1987-89	1987-89	1987-89	1987-89	1987-89
Trend in month effects	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

Note: Estimates are for one month extension of leave entitlements. Earnings percentiles are calculated among all mothers, using data for the year before birth. The number of observations differs between lowest and highest quartile due to differences in the number of missing values on the outcomes. The GPA predictions are based on a regression using 1987 data. All covariates described below Table 3 except mothers' education are included in the prediction. The models are identical to the last column of Table 3. The number of observations differs between outcomes due to non-attendance at tests (the largest numbers pertain to the grade outcomes). Robust standard errors in paranthesis. *(**, ***) Significant at the 10 (5,1) percent level.

Table 7. Effects on earnings, fertility, parental well-being and health.

		Estimated effects of entitlement		
Dependent variable	Mean of dependent variable: All [low ed./high ed.]	All	Low educated (<=9 years)	High educated (Tertiary)
Mothers labor earnings (100s of 1990s SEK)				
After 3 years	684	0.248	0.466	2.781
	[534/957]	(1.695)	(3.213)	(3.967)
After 7 years	998	0.833	1.057	5.108
	[713/1449]	(2.185)	(4.334)	(4.961)
Child admitted to hospital (any reason)				
Within 3 years	0.330	0.000	-0.004	0.004
	[0.365/0.302]	(0.002)	(0.003)	(0.003)
Within 6 years	0.384	0.000	-0.001	0.001
	[0.416/0.353]	(0.002)	(0.003)	(0.003)
Within 16 years	0.539	0.000	-0.001	-0.000
	[0.575/0.505]	(0.002)	(0.004)	(0.003)
Younger siblings				
Within 18 months	0.058	0.002**	0.002	0.003*
	[0.067/0.045]	(0.001)	(0.002)	(0.001)
Within 24 months	0.163	0.002	0.000	0.001
	[0.161/0.155]	(0.001)	(0.003)	(0.003)
Within 3 years	0.337	0.001	-0.004	0.001
	[0.309/0.337]	(0.002)	(0.003)	(0.003)
Within 6 years	0.512	0.001	0.001	0.000
	[0.491/0.493]	(0.002)	(0.003)	(0.003)
Within 16 years	0.577	-0.000	-0.002	0.000
	[0.577/0.528]	(0.001)	(0.003)	(0.003)
Total number	1.767	0.001	0.002	-0.002
	[2.001/1.633]	(0.003)	(0.007)	(0.005)
Parents separated				
At age 14	0.357	0.001	-0.002	0.003
	[0.485/0.232]	(0.002)	(0.004)	(0.003)
Mother admitted to hospital for mental health disorders				
Within 3 years	0.008	-0.000	0.000	0.000
	[0.014/0.004]	(0.000)	(0.001)	(0.000)
Within 6 years	0.012	0.000	0.001	0.001
	[0.023/0.007]	(0.000)	(0.001)	(0.001)
Within 16 years	0.038	-0.000	0.000	0.001
	[0.068/0.021]	(0.001)	(0.002)	(0.001)
N		227,697	49,105	57,257
Years		1987-89	1987-89	1987-89
Trend in month effects		Yes	Yes	Yes
Covariates		Yes[A]	Yes[A]	Yes[A]

Note: Estimates are for how one month extension of leave entitlements affects the outcomes of children to mothers with at least tertiary education relative to children with mothers having less than upper secondary education. Covariates are as in Column 5 of Table 3 for the first two panels. [A] Reduced covariate set for the health outcomes including only mothers age and square, order of child, county dummies (at birth of the child), dummies for the education of mother as well as the month and year variables. Robust standard errors are in parenthesis. The number of observations differs between outcomes due to minor differences in the sample construction for the health outcomes. *(**, ***) Significant at the 10 (5,1) percent level.

Table 8. Before and after estimates of the effects of paid parental leave entitlements on the relative performance of children to high educated mothers.

	(1)	(2)
Test results		
Swedish	0.012	0.015
	(0.005)**	(0.013)
English	0.003	0.012
	(0.005)	(0.013)
Math	0.055	0.072
	(0.005)***	(0.013)***
Grades		
GPA	0.009	0.017
	(0.005)**	(0.012)
GPA Core	0.010	0.015
	(0.005)**	(0.013)
R2	0.07–0.27	0.07–0.27
N	54373–65298	85537–96509
Years	1987, 1989	1987, 1989–90
Interaction of highly-educated mothers and linear trend (year)	No	Yes
Covariates	Yes	Yes

Note: Estimates are for how one month extension of leave entitlements affects the outcomes of children to mothers with at least tertiary education relative to children with mothers having less than upper secondary education. Covariates are as in Column 5 of Table 3. Robust standard errors are in parenthesis. The number of observations differs between outcomes due to non-attendance at tests (the largest numbers pertain to the grade outcomes). *(**, ***) Significant at the 10 (5,1) percent level.

Appendix A

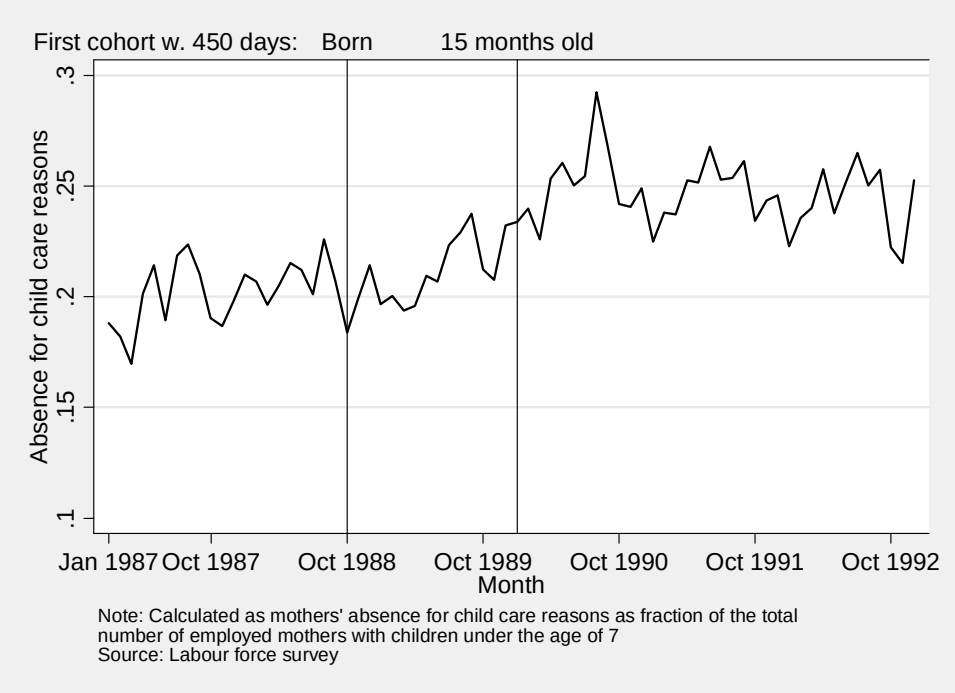
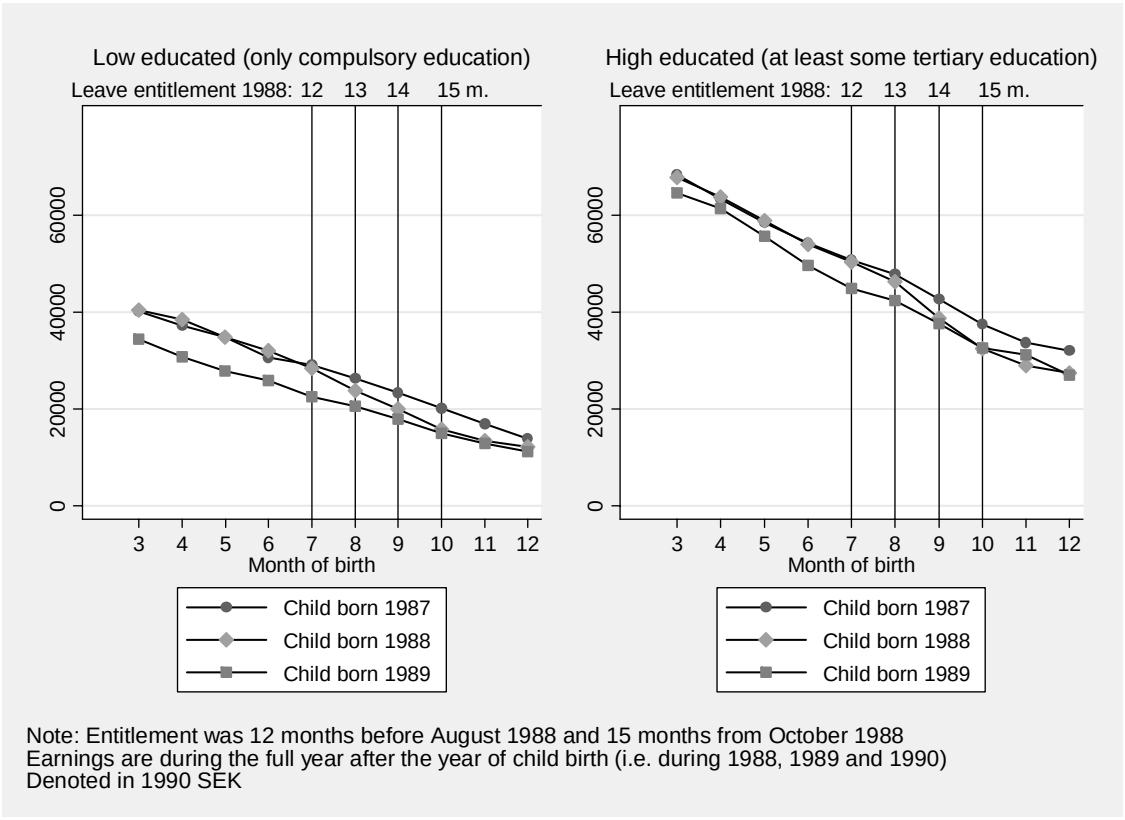


Figure A1. Percent of employed mothers with small children that were absent for child care reasons during the survey week (by month).



Note: Entitlement was 12 months before August 1988 and 15 months from October 1988
Earnings are during the full year after the year of child birth (i.e. during 1988, 1989 and 1990)
Denoted in 1990 SEK

Figure A2. Mothers real labor earnings by level of education during the full year following child birth – averages by month of child birth.

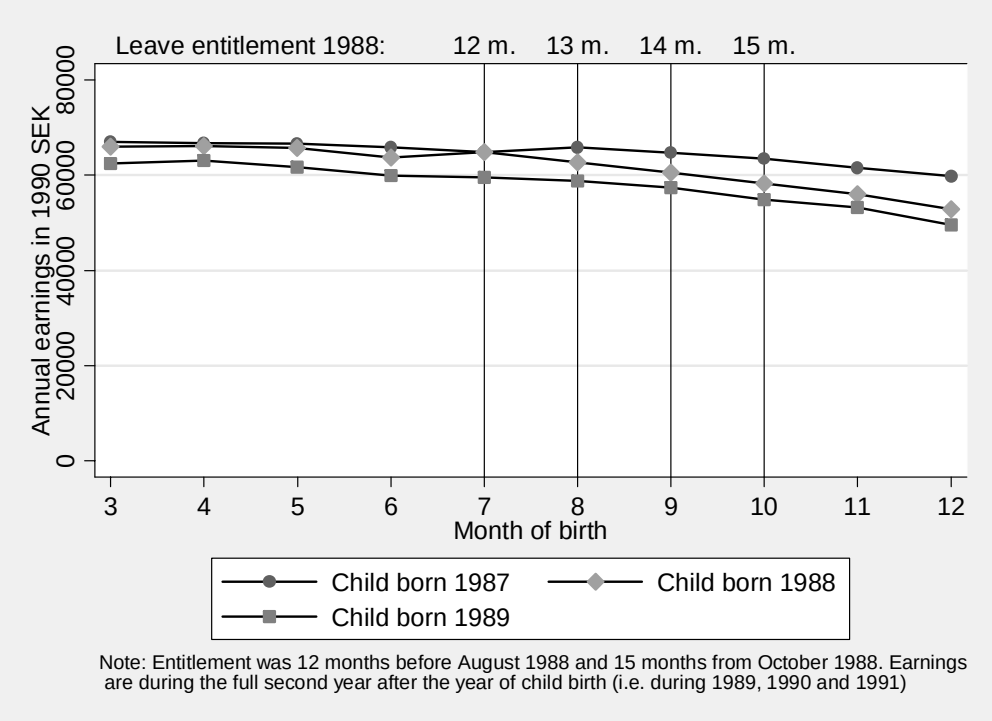


Figure A3. Mothers real labor earnings by level of education during the full second year following child birth – averages by month of child birth.