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The effect of own and spousal parental leave on earnings

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The effect of own and spousal parental leave on earnings*

by

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Abstract

This paper investigates the effect of parental leave – both own and spousal – on subsequent earnings using different sources of variation. Using fixed-effect models, and in line with previous results, parental leave is found to decrease each parent's future earnings. Also spousal leave is important, but only for mothers. In fact, each month the father stays on parental leave has a larger positive effect on maternal earnings than a similar reduction in the mother's own leave. Using two reforms of the parental leave system as exogenous sources of variation yields only imprecisely estimated effects, even though the reforms had a strong effect on parental leave usage. However, the point estimates tentatively suggest effects in the same range or larger than the fixed-effects model found.

Keywords: parental leave, gender equality, earnings
JEL-codes: J13, J16, J24

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1 Introduction

The last decades have seen a convergence in the labor market behavior of males and females, where the male-to-female ratio of educational levels, participation rates, hours worked and hourly earnings have declined (Lundberg and Pollak, 2007; Lundberg, 2005). Despite this, females continue to take the lion's share of housework, child minding and parental leave (Evertsson and Nermo, 2007; Gershuny and Robinson, 1988; Halleröd, 2005; Lundberg and Pollak, 2007), and it is sometimes argued that this is one potential explanation for the remaining, unexplained earnings gap (Datta Gupta et al, 2008; Lundberg and Pollak, 2007). For example, being on parental leave for young children may reduce future earnings through a number of channels such as human capital losses during the absence period or signaling effects (Albrecht et al, 1999; Mincer, 1974; Mincer and Polachek, 1974; Mincer and Ofek, 1982; Stafford and Sundström, 1996).

An additional mechanism, generally ignored in previous work, is the effect via future division of intra-household labor and child care. If parental leave today affects child care and household labor tomorrow, also spousal parental leave may be an important determinant of future earnings. For example, if a fathers' parental leave helps him acquire skills useful for taking care of children, this may affect future division of housework and child care within the family, and hence feed back onto maternal labor market behavior. This paper investigates the effect of parental leave on earnings.[1] It fits into a broader literature on the effects of career interruptions on earnings. However, the present paper departs from previous studies in several ways. First, it explicitly investigates the effect of not only own, but also spousal parental leave, an issue generally ignored in previous work. Second, it utilizes several sources of variation to identify effects. Besides cross sectional (CS) and fixed-effects (FE) models, it utilizes two policy

[1] The present study also serves to evaluate the Swedish daddy month reform. The main goals of the Swedish parental leave system are, as described in a government bill from 1993, gender equality, the child's right to both parents, child development and equal opportunity for both males and females to combine parenthood with a career (The Swedish Government, 1994). To my knowledge, there are no studies on how the daddy month affected parental labor market behavior.

reforms of the Swedish parental leave system that produced arguably exogenous variation in parental leave. The reforms reserved one and two months of leave for each spouse, which in practice decreased mothers' leave (the first reform) and increased fathers' leave (both reforms). Since the new rules applied to parents with children born after certain dates, the effect of reform exposure can be estimated using a difference in differences (DD) or triple differences (DDD) strategy. Finally, the register-based data set encompasses the entire Swedish population and is virtually free from missing-variables problems, attrition and self-report errors.

Previous studies have mostly found negative effects on earnings of absence in general and parental leave in particular (see for example Albrecht et al, 1999; Datta Gupta and Smith, 2002; Gangl and Ziefle, 2009; Görlich and De Grip, 2009; Mincer, 1974; Mincer and Polachek, 1974; Mincer and Ofek, 1982; Ruhm, 1998; Skyt Nielsen, 2009). In general, regression adjustment approaches are used for identification, sometimes with fixed effects to control for unobserved but time-invariant heterogeneity (Skyt Nielsen, 2009, is an exception using a reform of parental leave schemes among Danish publicly employed as exogenous variation). Regarding the effect of spousal parental leave, this issue is mostly ignored (one exception is Pylkkänen and Smith, 2003, who find that an increased parental leave period for fathers ("fathers' quota") reduces the job absence time of mothers, even when the days available for mothers are left unchanged). However, there are indications that early paternal involvement in childcare has effects on their involvement also later on. For example, Nepomnyaschy and Waldfogel (2007) find that fathers who take longer leave in connection to the birth of the child are more involved in child-caring activities 9 months later. On the other hand, Ekberg et al (2004) find no effects of ordinary parental leave on later care for sick children.

This paper shows that both own and spousal parental leave is potentially important for future earnings. Using the fixed effects model to control for unobserved but time-constant heterogeneity, the results show that each parent's own leave has a significant and negative effect on own future earnings. However, and more interesting, also spousal leave is important, but only for mothers. Each month the father stays on parental leave has a larger positive effect on maternal earnings than a similar reduction in the mother's own leave. Using the reforms as exogenous variation in parental leave yields imprecise estimates, despite the fact that both reforms strongly affected parental leave usage. However, the point estimates tentatively suggest larger effects than what was found using the fixed effects model.

2 The Swedish parental leave system and the reforms

The modern Swedish parental leave system was introduced in 1974, when both parents were given equal rights to use the system. It consists of several parts, the most important one being the governmentally paid cash benefit for parents staying home to care for their child. Most days (360 or 390, depending on child birth date) are reimbursed as a percentage of the previous wage, while a smaller amount of days (90) are reimbursed on a low flat rate. For individuals without the required previous labor market attachment, all days are replaced on a fixed (low) flat rate. The number of days on cash benefits as well as the reimbursement level has varied slightly over time; see Appendix for more details. There is great flexibility in the parental leave cash benefits; they can be used until the child turns eight years old and the parents can also choose to stay home part-time. The leave is also job protected. For more information on the Swedish parental leave system, see Berggren (2005), Duvander et. al. (2005) or The Swedish Social Insurance Agency (2002).

The overwhelming majority of parental leave is taken by mothers (Batljan et al, 2004). To increase the fathers' take up of parental leave benefits, two so called "daddy months" were introduced, the first in 1995 and the second in 2002. Before 1995, each parent were given half of the cash benefits days, but were free to transfer days to each other. But for those with children born from the 1st of January, 1995, 30 days of cash benefits are set aside for each parent and cannot be transferred. If those days are not used, they are simply lost. The 1st of January, 2002, an additional daddy month was introduced, making 60 days non-transferable. An important difference between the reforms is that in 1995, the total number of days was held constant, which meant that in practice mothers lost one month of parental leave. In 2002, the total number of days increased by one month so that mothers' maximum number of days was left unchanged.

It is important to note that the new rules apply according to the birth date of the child. There are also other changes in the parental leave system and in the social insurance system in general imposed from the 1st of January 1995 and the 1st of January 2002, but they generally apply equally to all individuals regardless of child birth dates. Hence, they affect both treatment (born after the turn of the year) and control (born after the turn of the year) groups equally. There are, however, some exceptions. The reimbursement rate was lowered from 90 to 80 percent in 1995. Although this affected all families equally in the long run, parents with children born before 1995 were given a respite and could keep their previous, higher replacement rate until the end of 1996. However, the 30 days set aside for each parent were excluded from this change and still replaced as 90 percent of previous wage. In 2002, the reimbursement rate for the flat rate days was doubled and this only applied to children born after 1st of January, 2002.

The daddy month legislation applies only to parents with shared custody of the child. Married parents are automatically given shared custody, while non-married parents must apply for shared custody. However, the overwhelming majority of families have shared custody. Within our sample (described below) 93 percent of all children had cohabiting parents at the time they turned one, and among cohabiting parents shared custody is very common. For example, 96 percent of all cohabiting parents of 1-5 year old children had shared custody in 1999 (Statistics Sweden, 2000). In the data, there is no information on custodial arrangements.

3 Identification

Theoretically, career interruptions and parental leave could affect an individual's own future earnings through three main channels. First is the effect via decreased market human capital (Mincer, 1974; Mincer and Ofek, 1982). This loss in market human capital may arise for different reasons such as a) forgone experience, b) skill depreciation during the leave, and c) effects ex ante via sorting into different types of jobs because of anticipated future career interruptions (Gronau, 1988). Second, career interruptions may work as a negative signal of work commitment (Albrecht et al, 1999; Datta Gupta and

Smith, 2002). Third, there may be statistical discrimination against high absence groups (Gangl and Ziefle, 2009; Spence, 1973).

In addition, it is possible that not only the individuals' own but also *spousal* parental leave affects earnings. This possibility has generally been ignored in previous work. If we consider a standard model for intra-family division of labor, it implies that increasing returns to specialization, along with (possibly small) initial differences in (different types of) human capital endowments will induce females to at least partly specialize in home production and males in market work. This in turn lowers female annual earnings primarily via the direct effect on hours worked, but also via the effect on hourly earnings, as housework is assumed to lower hourly earnings through different channels (less effort left for work, less experience and human capital accumulation when working part-time or because of periods of job absence[2]) (Albrecht et al., 1999; Becker, 1991; Datta Gupta et al, 2008; Lundberg, 2005: Lundberg and Pollak, 2007, Mincer and Ofek, 1982; Stafford and Sundström, 1996). If the division of parental leave affects spousal relative human capital endowments, it could also affect earnings. For example, fathers on parental leave could acquire child care human capital if the parental leave implies a period of learning to take care of a child (this is especially likely if we focus on the first-born child) making him more likely to take part of child care also in the future, which in turn could feed back to mothers' labor market behavior.

In the following, we focus on the effect of parental leave on mothers' earnings in a setting with panel data on families with their first child born in December or January around the reform cutoff or one year earlier.[3] Each family is observed twice, one year before birth and four years later. A flexible structural model for the effect of parental leave on mothers' earnings may be written

$$\ln E_{itcm} = \beta^0 + m^0 MPL_{itcm} + f^0 FPL_{itcm} + \alpha_c + \alpha_t + \alpha_m + \alpha_{mt} + \alpha_i + e_{itcm}$$

where the subscripts denotes family (i), time in terms of (approximate) child age (t=0 or t=4), cohort group (c=1 if the child is born around the reform cutoff) and month-of-birth (m=1 if born in January).

[2] Empirical support for this hypothesis is found in Hersch and Stratton (1994, 1997, 2000).

[3] Models for fathers' earnings may be written in an equal fashion but since the parameters may differ by gender the models need to be estimated separately for mothers and fathers.

The dependent variable measures log earnings, MPL and FPL measures the mother's and the father's cumulative parental leave and the α:s denotes time (α_t), cohort (α_c), month-of-birth (α_m) and family (α_i) fixed effects. The interaction term α_{mt} allows the effect on earnings to vary between children born in December or January over time. This is potentially important, since we measure outcomes at the end of each calendar year. This means that children born in January are, by construction, on average one month younger when outcomes are measured than children born in December (remember that t denoted average child age; at t=4 children born in December are on average 4 years and 0.5 month old while children born in January are on average 3 years and 11.5 months old). This could imply that parents of January-born children are less likely to work or to work full-time and that those who do work are drawn slightly more from the upper end of the income distribution (the idea being that the reservation wage is higher, the younger the child is). This effect is also likely to vary over time – before birth (t=0) it is likely zero, while if we looked at t=1 it could be a sizeable effect and at t=4 it is probably smaller but perhaps not zero. Another example, which might produce systematic differences for parents of children born around the turn of the year, relates to the school starting age legislation. When children reach school starting age, there is a cutoff at the turn of the year, making children in the control group start school one year earlier than children in the treatment group which in turn could affect parent's labor market behavior[4]. However, this is probably a small concern since we measure outcomes for children below school starting age.

Since the family fixed effects are unobserved, we may rewrite $v_{itcm}= \alpha_i +e_{itcm}$ i.e. replace the error term and the family fixed effect with the composite error term v_{itcm}. For ease of exposition, control variables are omitted but can easily be added to the model. For simplicity we also disregard the fact that the number of parental leave days may enter nonlinearly; the intuition still holds for the more general case. Naturally, we would expect $|m|>|f|$, i.e that a mother's own parental leave have a larger effect on earnings

[4] In Sweden, the mandatory school starting time is in August the calendar year when the child turns seven years old. One year earlier all children are offered to participate in a voluntary pre-school class during some hours each day. The pre-school classes are intended as a bridge between ordinary preschool and compulsory school (Swedish National Agency for School Improvement, 2007).

than spousal parental leave. Previous research has generally ignored the spousal effects. However, here we have the explicit aim to estimate also the effect of spousal parental leave on own earnings.

First, if we only had cross-sectional data at t=4 the model would reduce to a standard cross-sectional (CS) model,

$$\ln E_{icm} = \beta^1 + m^1 MPL_{icm} + f^1 FPL_{icm} + \alpha_c + \alpha_m + \alpha_i + e_{icm} \tag{1}$$

which is consistently estimated by ordinary least squares as long as $v_{icm}=\alpha_i+e_{icm}$ is uncorrelated with *MPL* and *FPL.* This assumption is unlikely to hold. For example, if parents who take more (less) parental leave also are less (more) career oriented and for that reason have lower (higher) earnings, this assumption is clearly violated. These differences in preferences for children versus market work may be difficult to proxy by including standard control variables and the resulting estimates will reflect selection rather than causal effects. In such case, the estimates will be biased downwards. Another possible story, potentially most applicable for fathers, is that fathers on leave – i.e. "responsible fathers" –are fathers with high earnings capacity. This interpretation is similar to the male marital wage premium found in earlier literature, where married men and/or fathers have higher earnings than non-married/non-fathers (Datta Gupta et. al, 2007; Gray, 1997). This story would lead to an upward biased estimate of the effect of parental leave on earnings among fathers.

Previous studies have used individual/family fixed effects to control for unobserved but time-invariant heterogeneity. If the endogenous variables – such as family preferences or "responsibility" – are constant over time, this approach yields unbiased estimates. Given our panel data, we can estimate a dummy-variable fixed effects (FE) model,

$$\ln E_{itcm} = \beta^2 + m^2 MPL_{itcm} + f^2 FPL_{itcm} + \alpha_c + \alpha_t + \alpha_m + \alpha_i + e_{itcm} \tag{2}$$

where we have assumed that $\alpha_{mt}=0$.[5] Note that *MPL* and *FPL* are always zero before birth so the main difference from model (1) above is that the dependent variable is measured as first differences. Now, the family unobserved effect can be controlled for

[5] Of course, we cannot distinguish between the different time-constant fixed effects, α_c,α_m and α_i, they are estimated simultaneously.

so this model is consistently estimated by OLS under the weaker assumption $E[X_{itcm}*\Delta e_{itcm}]=0$, where X=*MPL*, *FPL*. In particular, the model allows for fixed family characteristics that are correlated with the dependent and independent variables.

However, to the extent that fertility (number, timing and spacing of children) is endogenous, also fixed-effects models may yield biased estimates (Browning, 1992; Lundberg, 2005). This could happen if, for example, fertility and/or parental leave respond to income shocks. If so, we need some kind of exogenous variation in parental leave to identify causal effects. This paper utilizes the daddy-month reforms as such plausibly exogenous variation and compares children born just around the reform cutoffs. If we continue to assume $\alpha_{mt}=0$ – i e. that there are no time-varying systematic differences between children born in December and January - we may restrict focus to children born around the reform cutoff only (and exclude families with children born the preceding year). Then a difference-in-differences (DD) model is given by

$$\ln E_{itm} = \beta^3 + rREFORM_{itm} + \alpha_t + \alpha_m + \alpha_i + e_{itm} \tag{3}$$

where REFORM is an indicator variable for being exposed to the reform. Note that this variable is exactly the same as the interaction term between month-of-birth and time, α_{mt}, from above. This is why we need the $\alpha_{mt}=0$ assumption to hold in order for the REFORM coefficient to measure the effect of the reform (rather than the effect of differences between children born in December and January). If there are no such differences between children born in December and January, this model is consistently estimated by OLS as long as $E[REFORM_{itm}*e_{itm}]=0$. In particular, exposure to the reform should be exogenous and uncorrelated with for example income shocks. This specification identifies the intention to treat (ITT) effect – the effect of the reform on all families regardless of whether they comply or not – and as such, it mat be viewed as giving a lower bound on the "true" effect of a month increase/decrease in parental leave for fathers/mothers. In the absence of extra control variables, the reform coefficient equals the difference between different group means, see *Table 1*.

If there are normal-year systematic differences between children born in December and January ($\alpha_{tm}\neq 0$), for example because children in the group exposed to the reform are slightly younger when earnings are measured, we would need to include also families from a comparison year and estimate a difference-in-difference-in-differences (DDD) model,

$$\ln E_{itcm} = \beta^4 + r' REFORM_{itcm} + \alpha_c + \alpha_t + \alpha_m + \alpha_{mt} + \alpha_{ct} + \alpha_i + e_{itcm} \quad (4)$$

where REFORM=α_{ctm} now is an indicator for children born in January during reform year at time t=4 (for completeness also the second "baseline" interaction effect α_{ct} is added to the model). In the absence of control variables, also this REFORM coefficient is given as a difference between group means; see *Table 1* below.

Table 1 DD and DDD estimates

	Comparison group (child born one year before reform cutoff)		Reform group (child born around reform cutoff)	
Child's month of birth	**December**	**January**	**December**	**January**
lnE at t=0	a'	b'	a	b
lnE at t=4	c'	d'	c	d
Difference	c'-a'	d'-b'	c-a	d-b
DD estimate	(d'-b')-(c'-a')		(d-b)-(c-a)	
DDD estimate	[(d-b)-(c-a)]-[(d'-b')-(c'-a')]			

The models using the reforms as exogenous sources of variation (eq. 3-4) identifies the joint effect of MPL and FPL for the first reform, and the effect of FPL for the second reform. Remember that the second reform affected only fathers' parental leave while holding mothers' available parental leave days constant. In contrast, the first reform affected both parents' leave; given that mothers before the reform used virtually all parental leave, MPL was reduced by one month, while FPL was increased by a similar amount for the compliers.

Using the first reform, and without further assumptions about the parameters (m and f) we cannot identify whether the effect runs through own or spousal uptake of parental leave; we have only one instrument and two endogenous variables. But since we have two reforms, it is, in principle, possible to calculate instrumental variables estimates of the effect of each parent's parental leave (rather than the "reduced form" reform effects). However, such a strategy requires that there are no structural changes over time and since it is seven years between the first and second reform, this assumption may be questioned. We may also note that by using the reforms as exogenous variation and comparing families around the reform cutoffs our identification strategy isolates the direct and individual-level effect of parental leave on earnings. In particular, the estimated effect does not include long-term equilibrium effects, such as statistical discrimination, sorting into different types of job because of anticipated future job absence, or increased female investments in market human capital due to changed expectations of a future partner's share of housework.

For simplicity the discussion above did not include control variables. Given exogeneity of treatment status, control variables X are unnecessary; the inclusion of control variables may, however, increase precision and is also an informal way of testing exogeneity. Note, however, that the control variables are always measured prior to the child's birth and never in first differences even in the fixed-effects or DD/DDD models. (In the standard fixed-effects setting, non-variant control variables drop out; however, assuming that predetermined control variables can have different impact at different times/child ages allows us to include interactions between time and the pre-determined control variables.[6])

In the estimations, parental leave is measured only up to child age three (instead of four). The reason is that the outcome is annual earnings (as compared to wages or hourly earnings) and the prime purpose is to investigate the long-term effects of previous leave on future earnings (and not the obvious and immediate effect of parental leave today on earnings today). See Section A2 in Appendix for more details on the timing of variable collection.

As usual in earnings regressions, the problem of zeroes due to non-participation arises since we only observe earnings for individuals who participate in the labor market. Different processes may be at work on the extensive and intensive margin, and including observations with value zero and using a linear estimation model may induce specification bias due to nonlinearity. Focusing on individuals who do work necessarily implies conditioning on an endogenous variable which yields a selected sample of participants (Wooldridge, 2002). Throughout the paper OLS is used on log annual earnings in SEK+1 to include also non-participants but results on the participation decision as well as results for the participants only are shown in the Appendix.

[6] Note that we never want to include control variables measured at t=4 since they may be affected by treatment and as such are part of the outcome.

4 Data

This section describes the data. It also describes how the reforms affected parental leave usage and discusses issues of exogeneity.

4.1 Data and estimation

The panel data set is based on register information (created by combining the LISA data base and the so called multigenerational registry, provided by Statistics Sweden, with data on parental leave provided by the Swedish Social Insurance Agency) encompassing the entire Swedish population. It contains high-quality, individual level information on all children and their family members, including information on annual earnings (from the tax registers), parental leave usage and standard covariates such as age, educational levels and marital status. There is in principle no attrition or missing variables problem.

The samples consist of native Swedish families[7] whose first child[8] was born one month before or after each reform cutoff or the preceding year. Families whose first birth was a multiple birth (approximately 3 percent) are excluded since the parental leave rules for these families are slightly different. This leaves us with 9007 families for the first reform sample and 8301 families for the second reform sample. In the main analysis, most variables are observed both one year prior to the child's birth (t=0) and when the child is on average four years old (t=4); see Section 3 above and Section A2 in Appendix for more details on the timing of data collection.

The dependent variable measures log annual earnings (in SEK + 1 to include zero-earners). The (possibly endogenous) independent variables of interest are the mother's and father's total parental leave up to child age three. These are measured in days in the descriptive section to give a precise picture of how the reforms affected parental leave, but for readability they are rescaled to months (by dividing by 30) in the regressions. These variables are used in models (1) and (2). The exogenous reform indicator, used in

[7] For immigrants, there are around 20% missing observation due to lack of educational information. However, including immigrants in the estimations does not change the results.

[8] Only children who are *both* parents first-born child are included to avoid bias from previous children and their parental leave days.

models (3) and (4), is 1 for children born in January 1995 (first reform sample) or January 2002 (second reform sample) and 0 for all other children. The models also include the other indicator variables mentioned in Section 3 (cohort, month-of-birth, time and their interactions). A number of control variables are also available, including parental age and educational levels, marital status and child gender.

Table 2 shows descriptive statistics for the samples. There are relatively small differences in terms of control variables both between comparison and reform periods and between children born in December and January. Most individuals have either a high school degree (around 60 percent) or a university degree (almost 30 percent). Fathers are older and have higher earnings than the mothers. A relatively small proportion (20 percent) is married and this is explained by the fact that marital status is measured one year prior to the child's birth.

Regarding parental leave, the reforms seem to have had a strong effect. The first reform decreased mothers' leave by around one month (27.8 days) and increased fathers' leave by almost 8 days. This is in clear contrast to the comparison period, where the number of parental leave days is quite similar for children born in December and January; slightly fewer days have been used for children born in January and that is probably because of the small difference in age. The second reform is associated with a decrease in mothers' leave by 10 days; however, in the comparison period mothers' days decreased by even more (14 days), which again suggests that this is due to the fact that children born in January are slightly younger than December-born children when parental leave is measured. Fathers' parental leave increased by 9 days after the second reform, while it remained virtually unchanged during the comparison period. These reform effects are slightly smaller than the ones estimated by Ekberg et al. (2005) and the reason is our focus on parental leave during the child's first three years of life.

Table 2 Descriptive statistics for the samples

	Comparison cohort		Reform cohort	
		Panel a) First reform sample		
	Dec93	**Jan94**	**Dec94**	**Jan95**
Mother's earnings	117.8	118.8	112.5	111.3
(thousands SEK)	(63.2)	(64.2)	(71.1)	(71.5)
Father's earnings	143.8	145.9	140.7	142.1
(thousands SEK)	(89.5)	(93.3)	(105.3)	(101.0)
Mother’s PL (days)	460.4	457.8	467.1	439.3
	(160.6)	(154.6)	(159.7)	(152.5)
Father’s PL (days)	50.1	47.5	40.4	47.9
	(69.4)	(69.7)	(71.2)	(62.0)
Mother's age	25.6	25.6	25.6	25.5
	(4.55)	(4.40)	(4.43)	(4.42)
Father's age	27.7	27.6	27.8	27.7
	(4.96)	(4.98)	(4.94)	(4.85)
Mother w. high school educ.	0.60	0.59	0.59	0.61
Father w. high school educ.	0.58	0.60	0.60	0.58
Mother w. university educ.	0.29	0.29	0.29	0.28
Father w. university educ.	0.27	0.26	0.27	0.28
Married	0.18	0.17	0.18	0.18
Son	0.51	0.51	0.53	0.51
N	2135	2520	2115	2237
		Panel b) Second reform sample		
	Dec00	**Jan01**	**Dec01**	**Jan02**
Mother's earnings	155.3	155.8	170.0	170.4
(thousands SEK)	(95.1)	(99.4)	(104.5)	(105.7)
Father's earnings	205.4	209.3	226.9	222.1
(thousands SEK)	(128.4)	(134.4)	(176.7)	(168.1)
Mother’s PL (days)	408.1	394.4	405.3	395.2
	(142.5)	(142.6)	(146.9)	(138.8)
Father’s PL (days)	56.6	57.3	62.5	71.6
	(69.2)	(68.5)	(67.9)	(69.7)
Mother's age	26.8	26.9	27.3	27.0
	(4.50)	(4.58)	(4.71)	(4.55)
Father's age	28.9	28.9	29.2	28.8
	(5.01)	(4.92)	(5.04)	(4.97)
Mother w. high school educ.	0.50	0.48	0.54	0.56
Father w. high school educ.	0.54	0.53	0.66	0.64
Mother w. university educ.	0.38	0.40	0.38	0.36
Father w. university educ.	0.34	0.35	0.24	0.26
Married	0.21	0.20	0.20	0.18
Son	0.53	0.52	0.53	0.51
N	1848	2174	1944	2335

Notes: All variables except the parental leave variables and child gender are measured one year prior to the child’s birth. Earnings are measured in thousands SEK, including zeroes. Standard errors in parenthesis.

4.2 How the reforms affected parental leave use

As a start, it is illuminating to look at how the reforms affected parental leave use from different angles. *Figure 1* starts by plotting the mean number of parental leave days (measured at the end of the calendar years three years after the birth-turn of the year) for different child birth month cohorts (December- or January-born children from different years). This shows the development of parental leave over time. Clearly, fathers' parental leave increased at both reform cutoffs, while mothers' parental leave decreased only at the first reform cutoff. However, mothers with children born in January seem to always have used slightly fewer parental leave days, most likely because their children are on average one month younger when outcomes are measured. This small difference in child age does not seem to affect fathers' parental leave during non-reform years.

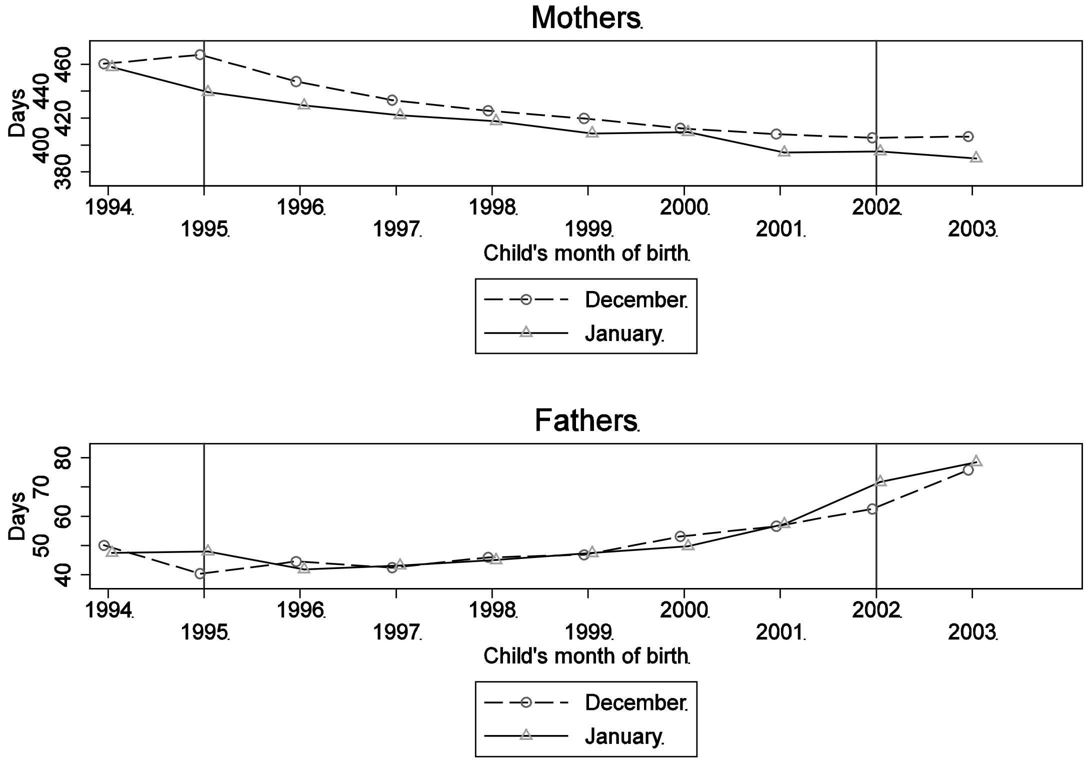

Figure 1 Mean parental leave for different child mob-cohorts

Second, *Table 3* shows the results when parental leave days are regressed onto reform exposure status with and without control variables (i.e. the DDD model (4) above but with mothers' and fathers' days on parental leave instead of earnings as dependent variable). Clearly, the reforms effectively increased fathers' leave by around 9-10 days each, and the first reform decreased mothers' leave by almost 26 days. The reform coefficients do not change much when control variables are added, which indicates that the reforms were exogenous to the parents. However, this issue is more deeply investigated in the Section 4.3 below.

It is also interesting to investigate if there are heterogeneous responses to the reform, i.e. to examine the compliers. *Table A1* and *Table A2* in Appendix show the reform effects for subgroups with different levels of maternal and paternal education. Although the patterns are not so clear it does seem like both reforms had relatively smaller effects on fathers' leave among families with a low maternal level of education.

Table 3 The effect of the reforms on parental leave use

	Mothers' days	Mothers' days	Fathers' days	Fathers' days
		Panel a) First reform sample		
REFORM	-25.304**	-25.792**	10.144*	10.025*
	9.383	9.352	4.075	4.046
Controls	No	Yes	No	Yes
R2	0.905	0.905	0.595	0.602
F	9537.692	3222.543	537.277	190.664
N	18014	18014	18014	18014
		Panel b) Second reform sample		
REFORM	3.522	1.537	8.444*	9.019*
	8.910	8.817	4.289	4.213
Controls	No	Yes	No	Yes
R2	0.899	0.901	0.648	0.660
F	8188.341	2822.574	859.009	305.184
N	16602	16602	16602	16602

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

Next, we take a closer look at the behavior around the reform cutoffs. *Figure 2* shows the timing of parental leave for mothers and fathers in the reform cohorts (January 1995 versus December 1994 and January 2002 versus December 2001). More specifically, it shows the number of parental leave days each month during the child's first 6 years of life. Note that parental leave days from younger siblings show up in this figure since the parents get additional leave entitlements for each child.

Clearly, most days are used before the child turns two years old. For mothers, there are no clear seasonal patterns and no differences between the January (solid) and December (dashed) group except that the graph for January-mothers in the first reform sample lies slightly below the graph for December-mothers, a natural result of the reform. For fathers, we may note several interesting features. First, the graph for the January group mostly lies above that for the December group, which indicates that the January group indeed used more parental leave. Second, there are clear seasonal trends – fathers seem to use more parental leave during holidays, primarily during the summers but also in connection with Christmas and New Year. That is also the most likely explanation for the small difference in timing between January and December groups – fathers in the January group are on parental leave slightly earlier and this may be because of the timing of holiday breaks. Apart from that, the differences between December and January groups are small.

Finally, *Figure 3* shows the distribution of the amount of parental leave for January (solid) and December (dashed) group, respectively. (In this figure, parental leave is measured up to child age three – the variation that is used in the main analysis - but looking at longer run parental leave does not change the overall picture). For the first reform sample, the distribution of fathers' days is clearly shifted to the right as a result of the reform, with a new peak at around 30 days. The distribution of mothers' days is likewise shifted to the left (the peaks for mothers are located at or slightly below the maximum available days on benefits, with and without the flat rate days). In this picture, the second reform does not seem to have affected mothers' distribution of leave, but fathers' leave was again shifted to the right with a new peak at 60 days.

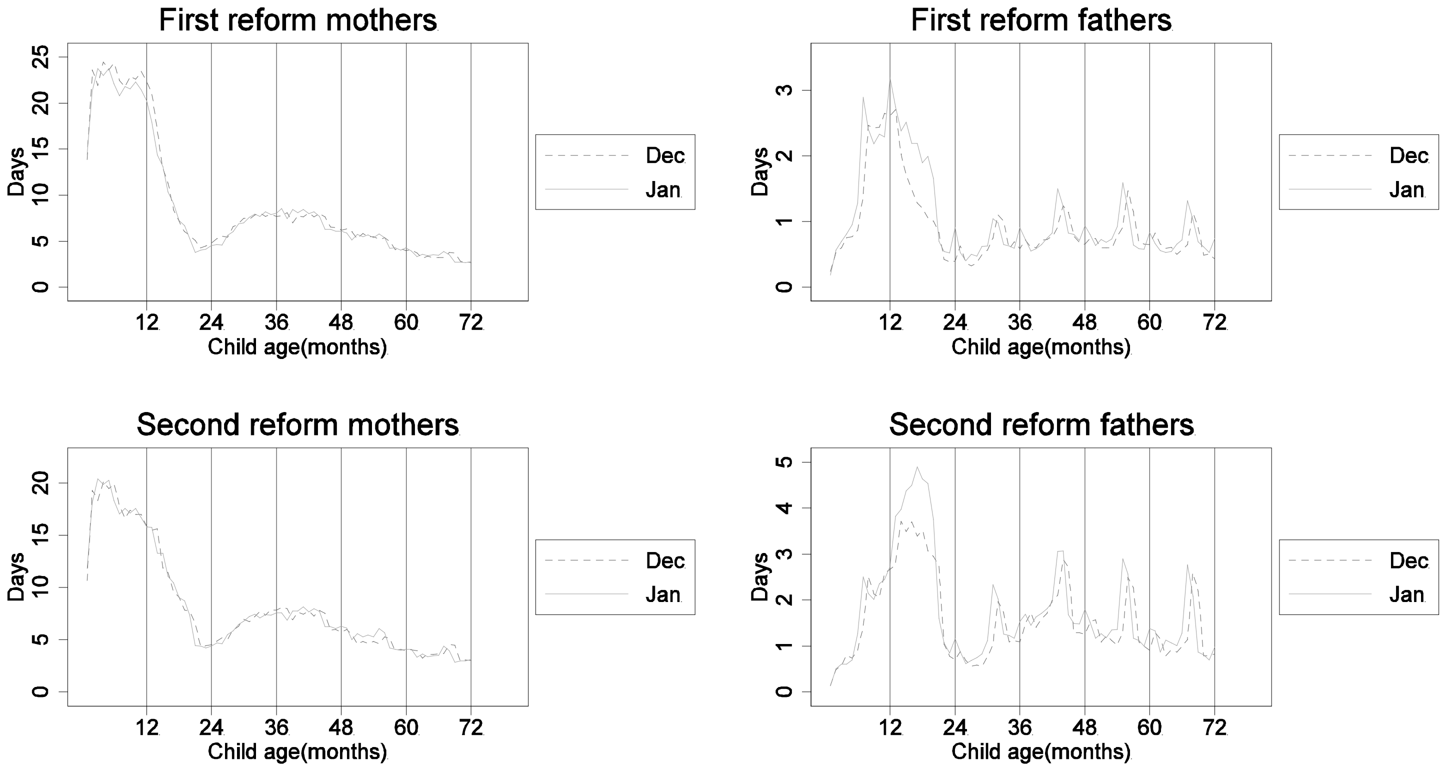

Figure 2 The timing of parental leave for reform cohorts by child month-of birth (December/January)

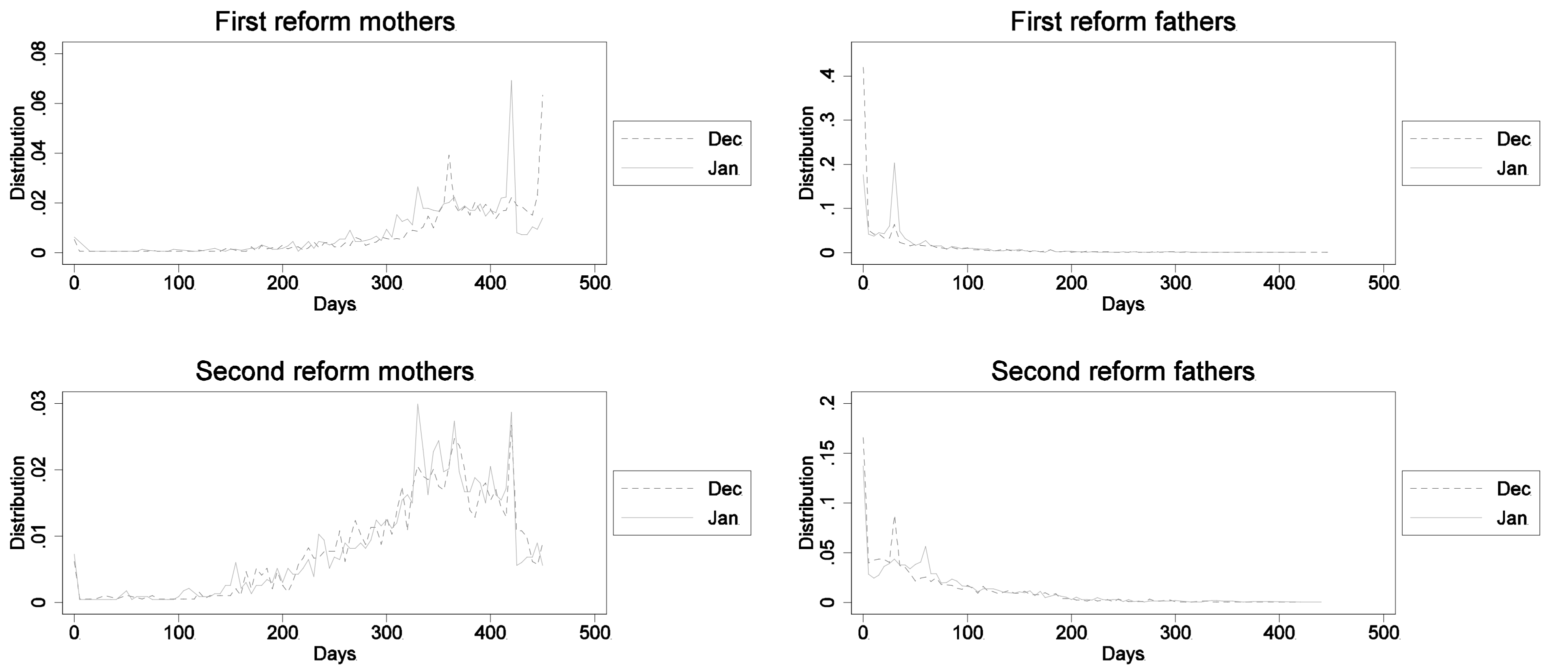

Figure 3 The distribution of parental leave days for reform cohorts by child month-of birth (December/January)

Note: for visibility, the graph is cut at the one-child maximum of 450 days; however, a smaller amount of parents have used slightly more days than this since they had another child before the first child turned three.

4.3 Exogeneity of reform exposure

The parental leave reforms in 1995 and 2002 are used as exogenous sources of variation in order to estimate the causal effect of parental leave on earnings. This identification strategy requires that a) no other change, affecting treatment and control groups differently, occurs at the same point in time as the reforms, and b) there is no endogenous sorting at the reform thresholds.

Regarding (a), are the reforms the single changes affecting January and December groups differently? Again, there were other changes in the social security system passed the 1st of January in 1995 and 2002, but they generally affected both groups equally. Only the daddy-month introduction along with some smaller changes in the reimbursement rate for the transferable days (*not* the daddy-month) was tied to the birth date of the child.

Regarding (b), is there any endogenous sorting at the reform thresholds? We start by investigating static sorting, although it is worth noting that fixed individual characteristics are allowed to be correlated with the probability of reform exposure (the individual fixed effects are differenced out; see Section 3 above). However, if there are static sorting it is also possible that there are sorting in terms of time-varying variables as well.

The first reform gave incentives for parents to induce an earlier birth, both to avoid the daddy month restriction and because of the slightly higher replacement rate for children born before 1995. The second reform reversely gave incentives to postpone birth since the parental leave rules were strictly better for children born after the reform. These incentives may have caused informed parents to fine-tune delivery. Are there such indications?

The first reform was difficult to anticipate at the time of conception. Although the daddy-month debate had been going on for years, it was unclear whether, when and how it should be implemented. As late as the 26th of April, 1994, three parties from the governing coalition threatened to vote against any such proposal (Karlsson, 1994a) and the reform proposition was not passed until 30th of May, 1994 (Karlsson, 1994b) when the turn of the year babies 1994/1995 were already conceived. Even so, parents could of course plan an earlier birth just in case. In addition, although the exact natural birth date is a random process it is in principle possible to induce an earlier birth by medical means, for example by using a caesarian section. The second reform had been known long in advance (TT, 2001) and informed parents may have chosen to postpone child-bearing.

Since there may be room for sorting around the reform cutoffs, we investigate this issue a little deeper. First, *Figure 4* below plots the number of first births in December and January over time. There clearly seem to be large variations over time, and possibly some tendencies of sorting in the anticipated direction – the difference in births between January and December are relatively small in 1994/1995 and slightly larger in 2001/2002. However, such tendencies exist also at other points in time. In 1999/2000, for example, the difference is even smaller than in 1994/1995.

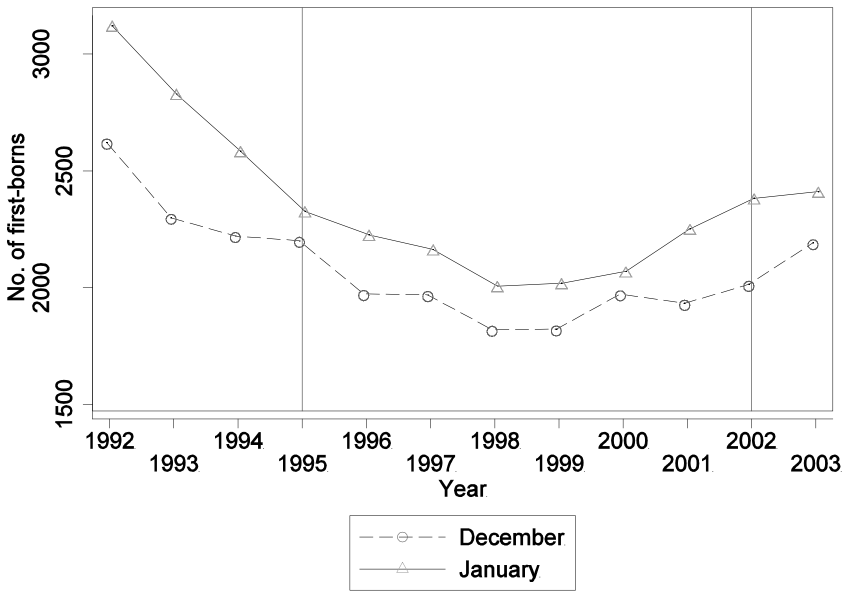

Figure 4 Number of first births in December and January over time

Next, we investigate whether observables can explain treatment status. This may show if there are indications of endogenous sorting at the reform cutoffs or if the pattern in *Figure 3* above is merely the result of random variation. (Of course, there could be endogenous sorting that does not show up in terms of observables, but that is impossible to investigate). *Table 4* shows regression results when an indicator variable for being exposed to the reform is regressed onto some arguably exogenous covariates (i.e. model

(4) above but where the outcome variable is REFORM status and this is regressed onto all other fixed effects and the control variables).

Clearly, there are no statistically significant differences in parental characteristics[9] between children born in January and December and all point estimates are small in magnitude[10]. However, even if each single coefficient is statistically non-significant, they could have explanatory power together. In fact, F-tests between these models and similar models without control variables (only the fixed effects for cohort, time, month-of-birth and their pairwise interactions are included) returns test statistics of 2.77 (first reform sample) and 2.58 (second reform sample) which is statistically significant and rejects the null hypothesis that the added control variables have no explanatory power. So, there may be some static sorting in terms of observable characteristics. This suggests that there could also be sorting in terms of unobservables. However, as noted above, static sorting is in itself not problematic (since we have panel data and can estimate the family fixed effects).

Next, we investigate the more important issue, if there seems to be time-variant sorting. In particular, we do not want reform exposure to be correlated with income shocks. Instead, January and December groups should follow the same wage growth paths over time. *Table 5* investigates this issue by regressing the probability of reform exposure (being born in January around the reform cutoff) on the fixed effects, the control variables and different earnings lags (maternal and paternal earnings two and three years before the birth of the child). This is necessarily done on a slightly smaller sample since these earnings lags are not available for all individuals. At most, we lose 74 individuals from the first reform sample and 75 individuals from the second reform sample. Clearly, none of the earnings lags are statistically significant and they are also small in magnitude. Hence, the groups exposed to the reforms seem to follow the same earnings pattern over time as the comparison groups.

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[9] See also Ekberg et. al. (2005) who compare the number of births each *day* around the turn of the year 1994/1995 and other years and find no systematic pattern. In addition, they compare parental age distributions for children born two *weeks* before and after the reform and find no evidence of differences in parental characteristics.

[10] All variables except the child gender variable are measured prior to the birth of the child.

Table 4 The effect of exogenous characteristics on prob(reform exposure)

	First reform	**Second reform**
Mother’s lnE	(-0.002	0.002
	(0.001)	(0.002)
Father’s lnE	0.001	-0.001
	(0.001)	(0.001)
Father's age	-0.000	-0.000
	(0.001)	(0.001)
Mother's age	-0.000	-0.001
	(0.001)	(0.001)
Father w. high school educ.	-0.012	0.003
	(0.011)	(0.013)
Mother w. high school educ.	0.011	0.001
	(0.013)	(0.015)
Father w. university educ.	-0.000	0.015
	(0.014)	(0.015)
Mother w. university educ.	0.005	-0.012
	(0.015)	(0.016)
Married	0.002	-0.001
	(0.010)	(0.010)
Son	-0.005	-0.003
	(0.007)	(0.008)
R2	0.857	0.872
F	1031.9	1329.9
N	18014	16602

Notes: All variables (except child gender) are measured one year before the birth of the child. Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

Table 5 The effect of income lags on prob(reform exposure)

	Prob (reform exposure)	Prob (reform exposure)	Prob (reform exposure)	Prob (reform exposure)
		Panel a) First reform sample		
Mother's lnE, lag2	-0.001			
	(0.002)			
Mother's lnE, lag3		-0.000		
		(0.002)		
Father's lnE, lag2			0.002	
			(0.001)	
Father's lnE, lag3				0.001
				(0.001)
Controls	Yes	Yes	Yes	Yes
R2	0.857	0.857	0.857	0.857
F	1110.1	1089.9	1114.3	1111.0
N	17970	17866	17998	17970
		Panel b). Second reform sample		
Mother's lnE, lag2	0.002			
	(0.001)			
Mother's lnE, lag3		0.001		
		(0.001)		
Father's lnE, lag2			-0.000	
			(0.001)	
Father's lnE, lag3				-0.000
				(0.001)
Controls	Yes	Yes	Yes	Yes
R2	0.872	0.872	0.872	0.872
F	1429.7	1403.4	1434.8	1427.8
N	16522	16452	16565	16533

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

4.4 Preview of results – simple cross-tabulations

Without control variables, the REFORM-coefficient in the difference-in-differences (DD) and triple differences (DDD) models can be calculated as simple differences between group means. *Table 6* and *Table 7* below shows these estimates for mothers and fathers; both estimates are also shown for different placebo years and the DDD-estimates are calculated using different comparison years. For ease of exposition, standard errors are omitted but as will be clear from Section 5.1 the standard errors are indeed huge and none of the differences below are statistically significant.

The first reform increased mothers' subsequent earnings by 9 percent using the DD approach and by 10-15 percent using the DDD approach with different comparison years. Hence, it is a sizeable positive effect of the first reform on mothers' earnings, and the point estimate also seems robust to different comparison years. In addition, the DD- and DDD-estimates from different placebo years are all much smaller and mostly of the reverse sign, which further indicates that the reform indeed had an effect on maternal subsequent earnings. However, turning to the second reform, the results are less robust. The coefficients from DD and DDD-models vary in both sign and size (from -5 percent using the DD model to between 1 and 11 percent using DDD-models) and the result are not very different from estimates in different pre-reform placebo years.

This could indicate that it is mothers' own leave (which was affected by the first but not the second reform) that is important. (Another possible story is that there could be differences in parental leave timing between the reforms. Potentially the first reform induced fathers to take more "non-holiday" parental leave, since otherwise the total expected leave was reduced, while the second reform was less strict in the sense that the families were given an additional month of leave, implying that fathers could more freely choose the timing of the parental leave. If so, and if "holiday"-parental leave is less helpful for maternal labor market behavior, this could explain the difference in effects between the first and second reform.)

Regarding the fathers, both reforms seem to have had a negative effect on subsequent earnings. The first reform's estimates range from -18 to -34 percent, indeed huge effects but suprisingly robust to the choice of comparison year and also more negative than any of the pre-reform placebo estimates. The second reform's estimates are much smaller, -5 to 5 percent, and also quite similar to the pre-reform placebo estimates.

Table 6 Cross tabulations with DD and DDD estimates, mothers

	Comparison cohort 3		Comparison cohort 2		Comparison cohort 1		Reform cohort	
	Panel a) First reform sample							
	Dec 91	**Jan 92**	**Dec 92**	**Jan 93**	**Dec 93**	**Jan 94**	**Dec 94**	**Jan 95**
LnE at t=0	11,21	11,20	11,11	11,05	10,96	11,00	10,65	10,50
LnE at t=4	9,29	9,24	9,39	9,32	9,49	9,48	9,64	9,57
Diff	-1,93	-1,96	-1,72	-1,73	-1,47	-1,53	-1,02	-0,93
DD estimate		-0,03		-0,01		-0,06		**0,09**
DDD estimate1				0,02		-0,05		**0,15**
DDD estimate2						-0,03		**0,10**
DDD estimate3								**0,12**
	Panel b) Second reform sample							
	Dec 98	**Jan 99**	**Dec 99**	**Jan 00**	**Dec 00**	**Jan 01**	**Dec 01**	**Jan 02**
LnE at t=0	10,65	10,65	10,93	10,95	11,08	11,05	11,25	11,31
LnE at t=4	10,16	10,00	10,22	10,18	10,18	10,01	10,06	10,07
Diff	-0,49	-0,65	-0,71	-0,77	-0,91	-1,05	-1,19	-1,24
DD estimate		-0,16		-0,06		-0,14		**-0,05**
DDD estimate1				0,10		-0,08		**0,09**
DDD estimate2						0,02		**0,01**
DDD estimate3								**0,11**

Table 7 Cross tabulations with DD and DDD estimates, fathers

	Comparison cohort 3		Comparison cohort 2		Comparison cohort 1		Reform cohort	
	Panel a) First reform sample							
	Dec 91	**Jan 92**	**Dec 92**	**Jan 93**	**Dec 93**	**Jan 94**	**Dec 94**	**Jan 95**
LnE at t=0	11,21	11,24	11,14	11,15	10,87	10,82	10,39	10,55
LnE at t=4	10,98	11,10	11,04	11,10	11,18	11,05	11,26	11,18
Diff	-0,24	-0,14	-0,10	-0,04	0,30	0,24	0,88	0,63
DD estimate		0,09		0,06		-0,07		**-0,24**
DDD estimate1				-0,04		-0,12		**-0,18**
DDD estimate2						-0,16		**-0,30**
DDD estimate3								**-0,34**
	Panel b) Second reform sample							
	Dec 98	**Jan 99**	**Dec 99**	**Jan 00**	**Dec 00**	**Jan 01**	**Dec 01**	**Jan 02**
LnE at t=0	10,93	10,91	11,05	11,16	11,29	11,34	11,51	11,38
LnE at t=4	11,58	11,48	11,46	11,59	11,50	11,58	11,68	11,53
Diff	0,64	0,57	0,42	0,43	0,21	0,24	0,17	0,15
DD estimate		-0,07		0,01		0,03		**-0,02**
DDD estimate1				0,09		0,01		**-0,05**
DDD estimate2						0,10		**-0,04**
DDD estimate3								**0,05**

5 Results

5.1 Main results

Table 8 and show estimation results for mothers and fathers for the first and second reform sample separately and using the different models (cross-section, fixed effects, DD and DDD).

There are several things to note. First, there are clear differences between the cross-sectional model and the fixed-effects model, which suggest selection of families into different levels of parental leave usage. Second, using the fixed-effects model, own parental leave do seem to reduce subsequent earnings – each month of own parental leave lowers mothers' earnings by 4.5 percent (in the first reform sample) and fathers' earnings by around 7.5 percent. The magnitude of these effects is far larger than previous studies – for example, Albrecht et al (1999) found wage reductions of 0.1-0.5 percent for each month of parental leave. This can be explained by the fact that here, annual earnings are used which reflect both wages and hours worked, while most previous studies have focused on wages. In addition, our focus is on the relatively short run effect on earnings four years later, when some parents could still be on parental leave (and parental leave up to child age 3 may be correlated with later parental leave). In addition, the longer-run effects are usually found to be smaller due to rebound effects and catching-up of human capital.

The differences in effects between males and females could be due to nonlinearities, if the first months of leave are more important for earnings than later parental leave. It could also be a signaling effect. As suggested by Albrecht et al (1999), parental leave could have a stronger signaling value for males since so few fathers stay on parental leave compared to virtually all mothers.

Third, and more interesting, spousal parental leave has no effect on father's earnings but do seem important for mother's labor market behavior. Each additional month that the father stays on parental leave increases mothers' earnings by 6.7 percent in the first reform sample (the effect in the second reform sample is not statistically significant). This is a large effect, even larger than the effect of a mother's *own* parental leave. This indicates that paternal (lack of) involvement in parental leave and child care may in fact be one important explanation for the male-to-female earnings gap. Another story could be a “reverse signaling” story – while most mothers take all available parental leave, a shorter period of leave could work as a positive signal of work-commitment.

These causal interpretations rest on the assumption of no time-variant unobserved heterogeneity, and in particular that fertility and parental leave is not endogenous. For example, if parents who experience an income shock becomes more (less) likely to have children and/or stay on parental leave, this assumption is clearly violated. Using the reforms as exogenous variation in parental leave do, unfortunately, yield very imprecise estimates that are not statistically different from zero. We can note, however, that this is not because of a weak effect on parental leave use. As we saw in Section 4.2, the reform effectively changed the parents’ time on parental leave. Instead, it could be that the normal-year variation in earnings depending on child birth dates is too large to enable precise estimation.

However, we may still make some comparisons of the point estimates across models. The tables also report the predicted reform effect for the CS/FE-models, which is a calculation of the predicted effect of the reform if the assumptions underlying the CS or FE models are fulfilled. This effect is calculated as the mean change in mothers’ and fathers’ time on parental leave as induced by the reforms (see the reform-coefficient from *Table 2* above, columns 2 and 4), multiplied by the coefficient on each month of leave as estimated by the CS/FE models.[11]

For example, if the fixed-effects results are true, we would expect the first reform to increase maternal earnings by 6.1 percent; both because of the decrease in own leave and because of the increase in spousal leave. This effect is well within the 95 percent confidence interval of both models using the reform as exogenous variation. The most flexible model, DDD, tentatively suggests even larger effects – the point estimate is 14.9, albeit very imprecisely estimated. The same pattern is found also for the second reform sample and among fathers – model (4) always returns larger point estimates than model (2). This tentatively suggests that the “true” effect is in the same range or larger

[11] The standard error of this estimate is calculated assuming that the underlying variables are independent random variables.

than suggested by the fixed-effects specification.

Finally, we can note that these estimates are quite similar to the estimates without control variables (see the cross-tabulations above), which further indicates that the reforms are indeed exogenous.

Table 8 The effect of parental leave on mothers' earnings at child age 4

	CS	**FE**	**DD**	**DDD**
		Panel a) First reform sample		
Mother's PL	-0.011	-0.045***		
	(0.009)	(0.013)		
Father's PL	0.021	0.067*		
	(0.019)	(0.029)		
REFORM	[0.017]	[0.061]	0.088	0.149
	[0.011]	[0.023]	(0.176)	(0.244)
Controls	Yes	Yes	Yes	Yes
R2	0.059	0.656	0.667	0.655
F	40.717	45.833	17.038	41.939
N	9007	18014	8704	18014
		Panel b) Second reform sample		
Mother's PL	0.026**	-0.023		
	(0.010)	(0.014)		
Father's PL	0.034	0.036		
	(0.022)	(0.030)		
REFORM	[0.011]	[0.010]	-0.041	0.102
	[0.012]	[0.014]	(0.164)	(0.236)
Controls	Yes	Yes	Yes	Yes
R2	0.047	0.683	0.688	0.683
F	29.497	41.427	25.744	37.474
N	8301	16602	8558	16602

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

Table 9 The effect of parental leave on fathers' earnings at child age 4.

	CS	FE	DD	DDD
		Panel a) First reform sample		
Mother's PL	0.013	0.000		
	(0.007)	(0.011)		
Father's PL	0.035	-0.076**		
	(0.019)	(0.027)		
REFORM	[0.000]	[-0.025]	-0.256	-0.186
	[0.011]	[0.018]	(0.165)	(0.221)
Controls	Yes	Yes	Yes	Yes
R2	0.058	0.706	0.706	0.706
F	39.912	11.074	10.795	11.139
N	9007	18014	8704	18014
		Panel b) Second reform sample		
Mother's PL	0.007	0.005		
	(0.008)	(0.012)		
Father's PL	0.010	-0.075**		
	(0.020)	(0.026)		
REFORM	[0.003]	[-0.022]	-0.050	-0.074
	[0.007]	[0.014]	(0.138)	(0.206)
Controls	Yes	Yes	Yes	Yes
R2	0.047	0.714	0.731	0.713
F	25.454	3.860	2.125	3.031
N	8301	16602	8558	16602

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

5.2 Robustness: other specifications

In the main analysis above, the dependent variable is defined as log(earnings+1) to include also individuals who do not participate in the labor market. As discussed above, this is not unproblematic and *Table A3* and *Table A4* in Appendix show alternative specifications for the effect of parental leave/the reforms on the probability of having nonzero earnings (the extensive margin) and on log earnings among those with earnings>0, using the FE or DDD models.

The effect of parental leave on the participation decision is mostly not statistically significant, but the effect on log earnings among those with earnings >0 follow the same pattern as above – a negative effect of own parental leave and, for mothers, a positive effect of spousal leave in the second reform sample. The magnitudes of the effects are, as expected, smaller since now zero observations are excluded and part of the effect in the main analysis above was driven by individuals with zero earnings. Again, the DDD model returns only imprecisely estimated effects. [12]

6 Extensions

6.1 Heterogeneous effects

Usually, career interruptions are believed to be more harmful for individuals in occupations requiring a high level of human capital input. Therefore, we may hypothesize that both own and spousal parental leave is more important for parents with a high level of education. Also, as we saw above, the responsiveness to the reforms differed slightly between groups. However, estimating the models (FE/DDD) separately for subgroups with different maternal and paternal levels of education yields mostly imprecisely estimated effects that are not significantly different between the groups. This is most likely because of the smaller sample sizes in the FE case.

6.2 The effect of non-holiday parental leave

If there is an effect of fathers' leave on mothers' labor market behavior, one might hypothesize that this effect should differ depending on the timing of this leave. In particular, the great flexibility of the Swedish parental leave (remember that the days can be used until the child turns eight years old) also means that parents can use parental leave instead of ordinary vacation, for example during summertime or around Christmas. Such parental leave is potentially less helpful for mothers' careers than parental leave used when the other spouse is working.

Table 10 shows the effect of non-holiday parental leave, which is defined as parental leave excluding leave in June, July or August. This is estimated using the fixed-effects specification (model 2). Indeed, and in line with the hypothesis, non-holiday parental leave seems to have a larger negative effect on own earnings than summertime leave, and father's non-holiday leave has a larger positive effect on maternal earnings than

[12] In addition, using the models above (eq. 1-4) with earnings in levels (SEK, including zeroes) instead of in logs yields similar results as when earnings in logs are used, which indicates that the results are not sensitive to the logarithmic transformation.

leave including summertime leave. For example, fathers' non-holiday leave increases maternal earnings by almost 10 percent in the first reform sample (compared to 6.7 percent for all types of parental leave; see *Table 8*).

Table 10 The effect of non-holiday parental leave

	FE: Effects on lnE mothers	FE: Effects on lnE fathers
	Panel a) First reform sample	
Mother's PL	-0.056***	0.002
	(0.017)	(0.015)
Father's PL	0.098**	-0.092**
	(0.037)	(0.035)
Controls	Yes	Yes
REFORM	[0.081]	[-0.033]
	[0.030]	[0.022]
R2	0.656	0.706
F	46.074	11.091
N	18014	18014
	Panel b) Second reform sample	
Mother's PL	-0.030	0.005
	(0.018)	(0.016)
Father's PL	0.057	-0.088**
	(0.036)	(0.032)
Controls	Yes	Yes
REFORM	[0.016]	[-0.026]
	[0.018]	[0.017]
R2	0.683	0.714
F	41.581	3.785
N	16602	16602

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

6.3 Other outcomes: fertility and marital/cohabitation status

A more equally shared parental leave could affect other outcomes than earnings. For example, previous studies have found that the amount of gender equality within a family may affect (increase) both fertility and marital happiness (Cooke 2004; Coltrane, 2000; De Laat and Sevilla Sanz, 2006; Nilsson, 2008; Oláh; 2003; Sacerdote and Feyrer, 2008; Torr and Short, 2004).

Table 11 and *Table 12* below show the effects of parental leave/the reforms on fertility and cohabitant/marital status, at child age 4. Since we focus on first-born children, the number of siblings is always zero before the child is born; hence, in the siblings regression we cannot make within family comparisons over time. Therefore, results are shown for the cross-sectional model and for a "horizontal" DD-model, where the number of siblings is compared across cohort and month-of-birth (instead of across time and month of birth in the standard DD-model). For the regressions on cohabitant/marital status, the FE and DDD-specifications are used.

Clearly, and in line with previous studies, both mothers' and fathers' parental leave have positive effects on fertility and the probability of cohabiting and being married. The coefficients in the cross-sectional and fixed-effects models are always statistically significant and very close in magnitude over time (first versus second reform sample). This suggests ambiguous expected effects of the first reform since it decreased mothers' leave while increasing fathers' leave, and positive effects of the second reform. Turning to the DD/DDD models, the results are again imprecisely estimated, but the point estimates for fertility are quite close to the predicted effects as suggested by the CS model.

Table 11 Effects on fertility (no. of younger siblings)

	CS	DD-variant
	Panel a) First reform sample	
Mother's PL	0.057***	
	(0.001)	
Father's PL	0.065***	
	(0.002)	
REFORM	[-0.028]	-0.022
	[0.020]	(0.022)
Controls	Yes	Yes
R2	0.328	0.032
F	382.805	27.570
N	9007	9007
	Panel b) Second reform sample	
Mother's PL	0.052***	
	(0.001)	
Father's PL	0.055***	
	(0.002)	
REFORM	[0.019]	0.011
	[0.017]	(0.023)
Controls	Yes	Yes
R2	0.272	0.057
F	272.253	45.510
N	8301	8301

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

Table 12 Effects on cohabitant/marital status

	Prob(cohabiting)		Prob(married)	
	FE	**DDD**	**FE**	**DDD**
		Panel a) First reform sample		
Mother's PL	0.010***		0.010***	
	(0.001)		(0.001)	
Father's PL	0.016***		0.018***	
	(0.002)		(0.003)	
REFORM	[-0.003]	-0.016	[-0.003]	-0.008
	[0.004]	(0.021)	[0.004]	(0.025)
Controls	Yes	Yes	Yes	Yes
R2	0.878	0.875	0.794	0.791
F	2017.344	1653.074	179.212	160.217
N	18014	18014	18014	18014
		Panel b) Second reform sample		
Mother's PL	0.008***		0.009***	
	(0.001)		(0.001)	
Father's PL	0.016***		0.020***	
	(0.002)		(0.003)	
REFORM	[0.005]	-0.011	[0.007]	-0.028
	[0.003]	(0.019)	[0.004]	(0.026)
Controls	Yes	Yes	Yes	Yes
R2	0.905	0.903	0.809	0.806
F	2897.678	2408.354	151.733	135.382
N	16602	16602	16602	16602

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

7 Concluding remarks

This paper investigates the effect of parental leave on earnings. In contrast to most previous studies, not only own but also spousal parental leave is considered, under the hypothesis that spousal help in child care may feed back onto each individual's labor market behavior.

Using a fixed effects model to account for time-constant unobserved heterogeneity, the results show that own parental leave is associated with earnings reductions of 4.5 percent for mothers and 7.5 percent for fathers. In terms of sign, this is in line with previous studies. The size of the effects is much larger than in previous studies, partly because the focus here is on annual earnings (which also reflect hours worked) as compared to wages, which is mostly used in other studies.

For mothers, also spousal parental leave is important for future earnings. Each month that the father stays on parental leave increases maternal earnings by 6.7 percent, which is an even larger effect than the mother's own leave. This suggests that paternal (lack of) involvement in child care and parental leave could be one factor behind the remaining, unexplained earnings gap. Among fathers, there is no effect of spousal parental leave on earnings. Even larger effects of fathers' leave on maternal earnings can be found if we restrict focus to "non-holiday" parental leave, i.e. parental leave excluding leave during the summer (June, July, or August). Such parental leave may be a better measure of spousal help than parental leave during summertime (when both spouses may be at home simultaneously because of ordinary vacation).

Finally, the fixed-effects model rests on the assumption of no unobserved, time-variant heterogeneity. In particular, it assumes that parental leave is unaffected by for example income shocks. If this assumption is violated, we need some kind of exogenous variation to identify causal effects. The two daddy-month reforms in 1995 and 2002 had a strong effect on parental leave usage. Despite that, using the reforms as exogenous variation in parental leave yields only very imprecise estimates. This is most likely due to large random variation in earnings depending on child birth dates. However, the point estimates from DD and DDD models tentatively suggests effects in the same range or larger than what was found using the fixed-effects specification.

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Appendix

A1 Additional tables

Table A1 The effect of the reform on PL usage, by mother's level of education

	Mother's PL: low educ.	Father's PL: low educ.	Mother's PL: high school educ.	Father's PL: high school educ.	Mother's PL: university educ.	Father's PL: university educ.
			Panel a) First reform sample			
REFORM	-40.484	-6.606	-31.210**	12.231*	-7.434	12.772
	(29.238)	(13.599)	(12.074)	(4.930)	(17.149)	(7.954)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.897	0.557	0.908	0.596	0.904	0.627
F	408.557	15.586	2386.775	126.709	1081.300	89.595
N	2114	2114	10754	10754	5146	5146
			Panel b) Second reform sample			
REFORM	-2.458	-4.087	2.506	13.550*	1.821	6.070
	(30.748)	(15.297)	(12.454)	(5.301)	(13.616)	(7.256)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.885	0.609	0.904	0.643	0.902	0.686
F	292.824	21.625	1822.531	165.912	1289.197	182.773
N	1730	1730	8606	8606	6266	6266

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

Table A2 The effect of the reform on PL usage, by father's level of education

	Mother's PL: low educ.	Father's PL: low educ.	Mother's PL: high school educ.	Father's PL: high school educ.	Mother's PL: university educ.	Father's PL: university educ.
			Panel a) First reform sample			
REFORM	-2.510	13.989	-35.413**	9.492	-16.395	9.721
	(27.176)	(12.088)	(11.905)	(5.106)	(18.059)	(7.803)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.896	0.574	0.909	0.594	0.903	0.632
F	476.991	22.888	2390.505	121.930	1016.760	86.887
N	2470	2470	10654	10654	4890	4890
			Panel b) Second reform sample			
REFORM	4.451	7.799	2.431	6.844	1.156	12.650
	(27.334)	(12.110)	(11.468)	(5.389)	(16.488)	(8.178)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
R2	0.900	0.616	0.905	0.642	0.894	0.695
F	371.289	27.823	2122.051	191.315	911.531	151.987
N	1826	1826	9846	9846	4930	4930

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

Table A3 Robustness, mothers

	Prob(earnings>0)		LnE given earnings>0	
	FE	**DDD**	**FE**	**DDD**
		Panel a) First reform sample		
Mother's PL	-0.003*		-0.017***	
	(0.001)		(0.005)	
Father's PL	0.004		0.024	
	(0.003)		(0.014)	
REFORM	[0.004]	0.016	[0.022]	-0.031
	[0.002]	(0.022)	[0.009]	(0.096)
Controls	Yes	Yes	Yes	Yes
R2	0.609	0.609	0.677	0.677
F	21.544	19.976	45.538	41.773
N	18014	18014	16306	16306
		Panel b) Second reform sample		
Mother's PL	-0.001		-0.011*	
	(0.001)		(0.005)	
Father's PL	-0.002		0.061***	
	(0.003)		(0.011)	
REFORM	[-0.001]	0.007	[0.018]	0.032
	[0.001]	(0.021)	[0.010]	(0.092)
Controls	Yes	Yes	Yes	Yes
R2	0.639	0.640	0.689	0.684
F	18.358	16.724	46.653	39.258
N	16602	16602	15239	15239

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

Table A4 Robustness, fathers

	Prob(earnings>0)		LnE given earnings>0	
	FE	**DDD**	**FE**	**DDD**
		Panel a) First reform sample		
Mother's PL	0.000		-0.001	
	(0.001)		(0.003)	
Father's PL	-0.004		-0.028**	
	(0.002)		(0.009)	
REFORM	[-0.002]	-0.023	[-0.009]	0.074
	[0.001]	(0.019)	[0.006]	(0.065)
Controls	Yes	Yes	Yes	Yes
R2	0.658	0.659	0.769	0.769
F	1.691	2.227	72.935	68.220
N	18014	18014	16530	16530
		Panel b) Second reform sample		
Mother's PL	0.001		-0.004	
	(0.001)		(0.003)	
Father's PL	-0.003		-0.045***	
	(0.002)		(0.008)	
REFORM	[-0.001]	-0.005	[-0.014]	-0.021
	[0.001]	(0.017)	[0.007]	(0.061)
Controls	Yes	Yes	Yes	Yes
R2	0.676	0.675	0.748	0.746
F	1.199	0.770	41.415	35.490
N	16602	16602	15570	15570

Notes: Significance levels: * 10 %, ** 5%, *** 1%. Standard errors in parentheses, clustered on family.

A2 The timing of variable collection

Figure A1 shows the timing of variable collection. All variables are collected at two points in time: one year before the birth of the child (for notational convenience this is called t=0 although it in practice means t=-1) and also at child age four (t=4). However, as is clear from the picture, this is *average* child ages. Since the variables are measured the 31st of December each year, this will mean that children born in January will on average be one month younger than children born in December when the variables are collected.

The parental leave variables are measured as the cumulative amount of parental leave up to child age three. The motivation is that it is not very interesting to estimate the direct effect of parental leave today on earnings today. Rather, the interesting relationship is that between early parental leave on future earnings.

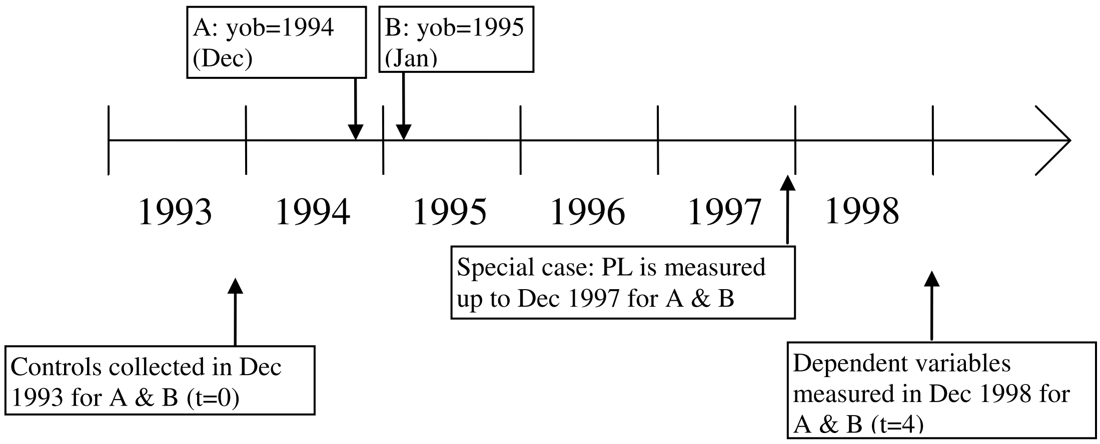

Figure A1 The timing of variable collection: example for reform cohort, first reform sample

A3 Details of the parental leave benefits over the years

Period	SGI days	% of income reimbursed	"Roof" of yearly income (SEK)	Max SEK/day, SGI days	Max SEK day if SGI=0	Flat rate days	SEK/dat, flat rate days
1990	360	90	222750	549	60	90	60
1991	360	90	241500	595	60	90	60
1992	360	90	252750	623	60	90	60
1993	360	90	258000	636	60	90	60
1994[a]	360	90	264000	651	64	90/0	60/0
1995[b]	360	80	267750	587	60	90	60
1996[c]	360	75	271500	558	60	90	60
1997	360	75	272250	559	60	90	60
1998	360	80	273000	598	60	90	60
1999	360	80	273000	598	60	90	60
2000	360	80	274500	602	60	90	60
2001	360	80	276750	607	60	90	60
2002[d]	390	80	284250	623	120	90	60
2003	390	80	289500	635	150	90	60
2004	390	80	294750	646	180	90	60
2005	390	80	295500	648	180	90	60
2006 (to June 30)	390	80	297750	653	180	90	60
2006 (from July 1)	390	80	397000	870	180	90	180
2007	390	80	398567	874	180	90	180
2008	390	80	397700	872	180	90	180
2009	390	80	415160	910	180	90	180

Notes: a) During the second half of 1994, the flat rate days were temporarily abolished for children >1 year old.
b) The first "daddy month" was introduced for children born after the 1st of january, 1995. During the 30 days set aside for each parent (the daddy month), the reimbursement level for the SGI days was still 90% of previous income.
c) During the 30 days set aside for each parent (the daddy month), the reimbursement level for the SGI days was still 85% of previous income.
d) The second "daddy month" was introduced for children born after the 1st of january, 2002.

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