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The Impact of a Large Parental Leave Benefit Reform on the Timing of Birth around the Day of Implementation

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Abstract. The introduction of the German parental leave benefit (Elterngeld) applied to all children born on January 1st, 2007 or later. The Elterngeld considerably changed the amount of transfers to families during the first two years postpartum. We show that the incentives created by using a cut-off date led more than 1,000 parents to postpone the delivery of their children from December 2006 to January 2007. Concerning potential adverse impacts on health outcomes of children we find a slight increase in average birth weight and the rate of children with high birth weight (>4,000 grams).

JEL classification: H31, J13.

Keywords: Cut-off date effect, fertility, parental leave, policy evaluation.

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I. Introduction

Displaying one of the lowest fertility rates among European countries for several decades and relatively long out of job periods of women after child birth, Germany experienced the implementation of a new system of parental leave benefit in the year 2007. The preceding benefit, which was means-tested and provided for up to two years after child birth, was replaced by a wage-dependent benefit provided for up to 14 months which was applied to all parents of children born on January 1st, 2007 or later. Depending on pre-birth labour market participation (and earnings) and planned post-birth labour market activities households might receive up to 25,200 Euro more or up to 3,600 Euro less under the new regulation than under the preceding one. The main aims of this reform were to reconcile the work-family balance, to increase labour market participation of mothers with young children, to increase fathers' involvement in child care and to improve the financial situation of households with young children. Several politicians also expressed their hope for an increase of fertility rates.

At the current point in time an evaluation of the long-term impact of the reform on birth rates is not feasible. Instead this paper is concerned with the impact on short-term behaviour, specifically the timing of delivery of newborn children during December 2006 and January 2007. We analyze whether the use of a cut-off date (January 1st) for allocating parents to the old or to the new benefit system and the differences in transfer payments between the two systems had an impact on the probability that a child was born in the last days of December rather than in the first days of January.

Analyzing these short-term effects is interesting for several reasons. Firstly, the analysis provides evidence on the power that parents have on the timing of birth. Overall there is ample evidence that a certain number of deliveries are timed. For example, using our data from birth registers we observe that the number of births taking place on weekends or holidays is lower by around 21% than on regular working days. Yet, it is generally not clear whether this timing is due to doctors and hospitals or due to parents. Any timing effects

around the cut-off date will be informative on the relative power of parents vis-à-vis doctors/hospitals concerning the choice or refusal of inducement and caesarean section and the timing of such a delivery. Besides choosing the mode of delivery, mothers expected to give birth at the margin might opt for more or less stressful, i.e. labour-inducing behaviour. Secondly, timing effects represent revealed preferences and indicate whether parents or sub-groups of parents are in favour of the reform (i.e. benefiting from the reform) or not. This is interesting from a policy perspective as there has been uncertainty on the share of parents that might be disadvantaged by the reform. Clearly, revealed preferences are stronger indicators for preferences than results from opinion polls.

Thirdly, the use of birth related cut-offs to determine eligibility for policy programs is quite common and there is a small but growing literature analyzing incentives created by birth related cut-off dates and their impact on the timing of delivery. In the literature very different settings and institutions that rely on cut-off dates that were known to parents before delivery have been analyzed. For example some cut-offs are associated with direct financial transfers, e.g. with birth related benefits or tax reductions. Other cut-offs are related to parental leave regulations which might have an indirect impact on income (or opportunity costs) if they alter work incentives. Furthermore there are cut-offs that are related to school or kindergarten entry regulations which might have an impact if they reduce child care costs or the opportunity cost to take care of the child by oneself. For some of these cut-offs birth shifting was found (e.g. Dickert-Conlin and Chandra 1999, Gans and Leigh 2009) but not for others (e.g. Dickert-Conlin and Elder 2010, Dustmann and Schönberg 2011, Lalive and Zweimüller 2009 and McEwan and Shapiro 2008). Dickert-Conlin and Chandra (1999) show that incentives provided by the US tax system¹ induce parents to shift delivery towards December rather than January and that shifting is larger among high income parents. Gans and Leigh (2009) analyze the Australian ‘Baby Bonus’, which was implemented in July 2004 and increased in July 2006

¹ Parents receive a full annual tax exemption for a given year if the child is born until December 31, while for children born after December 31 parents receive the tax exemption for the following years only.

and gives one-time cash transfers to parents with babies born after these dates. These bonuses heavily influenced parents' behaviour and induced postponements of deliveries by more than 14 days. In contrast, Lalive and Zweimüller (2009) do not find that extending the leave duration from 12 to 24 months (including the job-protection period and a monthly cash transfer of 340 Euro) in July 1990 affected birth timing of Austrian women. Similarly, Dustmann and Schönberg (2011) do not find that an expansion of job-protected leave from 18 to 36 months implemented in January 1992 influenced birth timing in Germany. Concerning school entry related cut-offs McEwan and Shapiro (2008) find no evidence of timing effects in Chile and Dickert-Conlin and Elder (2010) do not find evidence for the US. Given these mixed results this paper provides evidence for an additional setting and country to help shedding some light on what types of birth related cut-off dates influence the timing of birth.

Finally, we are able to analyze whether timing of birth has any detrimental impact on health outcomes of children. We do so by looking at birth weight and length at birth. This is of particular importance as there is evidence that health at birth has consequences for medium- and long-term outcomes of children (e.g. Black et al. 2007, Oreopoulos et al. 2008, Royer 2009 and Johnson and Schoeni 2011). We are aware of only one previous paper by Gans and Leigh (2009) analyzing the impact of cut-off induced birth timing on health outcomes.

In the next section we describe the reform of the benefit system in more detail. Section III provides information on the data. In Section IV we present results on the timing of deliveries as well as results for health outcomes of newborn children that might be affected by shifts in delivery. The final section provides a discussion and conclusions.

II. Institutional background

Parental leave regulations and the system of benefits to parents with young children underwent several reforms in Germany during the last two decades. While most of the changes have been minor, the reform in 2007 constitutes a complete turn-around of the benefit

system. The new *Elterngeld* was announced in September 2006 to replace the preceding *Erziehungsgeld* for all children born on January 1st, 2007 or later. The new *Elterngeld* offers a 67% replacement rate of previous labour earnings (from employment or self-employment) for either father or mother for up to 12 months postpartum. If both father and mother participate, they can receive an extra 2 months and the resulting total leave of 14 months can be freely distributed between the two parents. Single parents can receive a total of 14 months alone. The transfer is truncated at a maximum of 1,800 Euros per month and a flat rate minimum of 300 Euros per month, which also applies to parents without previous earnings. Additional bonuses are given to parents with multiples or with siblings below age four. Almost 100% of families receive the benefit for some months after birth (RWI 2008) as eligibility for the new *Elterngeld* only depends on not working full time, i.e. less than 30 hours per week, during the period of receipt.

In contrast to this, the old *Erziehungsgeld* was means-tested. Parents could choose between two versions of *Erziehungsgeld*, the first consisting of monthly benefit of up to 300 Euro payable for a maximum of 24 months postpartum, the second consisting of monthly benefit of up to 450 Euro payable for a maximum of 12 months postpartum. Around two thirds of families received the 300 Euro version and one out of ten families received the 450 Euro version (RWI 2008). In both groups, families experienced reductions in transfers following month six if household income exceeded specific thresholds. The remaining 23% of families did not receive any type of *Erziehungsgeld* due to high household income.² Besides means-testing, eligibility for the old *Erziehungsgeld* also depended on not working full time during the period of receipt.

The outline of the reform had different impacts on households in terms of cumulative benefit payments mainly depending on pre-birth labour market participation (and earnings) and

² The income threshold (after accounting for several deductibles) was 30,000 Euro per year for couples and 23,000 Euro for single parents. In terms of annual gross income this corresponds to approximately 40,400/31,200 Euro, respectively.

planned post-birth labour market activities. Households with women who were working before child birth, those planning to take up full time work relatively early (i.e. one year after child birth or earlier) or high income households were generally receiving more transfers after the reform than before. In contrast, households with women who were not working before child birth or those with low pre-birth earnings planning to take up full time work relatively late experienced a decrease in cumulative transfers in some cases. At the one extreme, households might gain up to 25,200 Euro overall (i.e. $(12+2) \times 1,800$ or even more in the case of bonuses for multiples or siblings), at the other side they might lose up to 3,600 Euro (i.e. $(24-12) \times 300$).³ These differences in benefit transfers created different incentives for birth timing as well as political discussion on the share of families that would be disadvantaged by the reform. Table 1 shows estimates of the average Elterngeld transfer cumulative over the period of receipt for selected subgroups of mothers. Reliable estimates of average transfers of the old Erziehungsgeld are unfortunately not available for subgroups. Based on results in RWI (2008) cumulative transfers of Erziehungsgeld for an average family were around 5,000 Euro.

TABLE 1
Average Elterngeld transfers

	<i>Average transfer of Elterngeld cumulative over period of receipt</i>
All mothers	6,730
Mothers working before delivery	9,002
Mothers not working before delivery	4,181
Mothers age ≤ 20	3,808
Mothers age 21-25	5,201
Mothers age 26-30	6,185
Mothers age 31-35	8,130
Mothers age ≥ 36	8,219

Notes: Estimates are based on survey data on parents with delivery during the first quarter of 2007. (For more details on this data see RWI 2008.)

³ Because of differences in the treatment of the two transfers for income taxation the maximum loss from the new regulation might slightly exceed 3,600 Euro. Spiess and Wrohlich (2008) calculate that any losses resulting from the tax treatment are moderate.

III. Data

In the empirical analysis we use data from birth registers which have been provided at the research data centre of the statistical offices of the federal regions (*Forschungsdatenzentrum der Statistischen Ämter*). The data includes information on gender, multiples, length, weight and date of birth for all children born in Germany. In addition it provides information on mother's age, citizenship, working status⁴ and state of residence (*Bundesland*). Father's age and citizenship is only recorded for married couples; for cohabiting couples information on the father is incomplete. Parity and age of previous children is restricted to those born within the present marriage. The data is of very high quality as reporting is mandatory and generally done by hospitals or in case of home delivery it is based on certificates from midwives (Statistisches Bundesamt 2010a). In the analysis we use data going back to mid of 2001 only, because there have been several reforms of the old *Erziehungsgeld* and to parental leave regulations during earlier years, all of them relying on birth specific cut-off dates that were used to determine eligibility (cf. Schönberg and Ludsteck 2008). The last of these previous reforms took place in January 2001.⁵

Unfortunately the birth register data does not include information on several variables that influence the incentive structure, for example wages of women. We try to take these factors into account by using region specific data in the second sub-section of Section IV. At the level of the 16 federal states we know average female wages. These are taken from Statistisches Bundesamt (2010b) and refer to annual gross wages of women in the manufacturing sector in 2006. Between states average wages vary between 25,200 Euro and 42,272 Euro. Since these

⁴ Information on maternal working status refers to the time of delivery. The definition is thus not fully identical with benefit criteria for the new *Elterngeld* transfer which are based on labour earnings during the 12 months preceding child birth. Due to temporary employment spells some mothers recorded as not working in the birth register data might have been so at some point preceding delivery. Overall, the difference between both definitions of working status is relatively small. In the birth register data 49.0% of mothers with child birth in 2007 are recorded as working while according to the *Elterngeld* transfer applications 52.7% are (Statistisches Bundesamt 2008).

⁵ On January 1, 2002 and on January 1, 2003 the income thresholds for the old *Erziehungsgeld* transfer increased somewhat for families with siblings. We consider these changes as minor. Sensitivity checks show that none of our main results changes when excluding information from 2002 and 2003 from the analysis.

wages refer to all women and not only to women giving birth and because there is no information for one of the states (Bremen) we also use administrative records on all applications for Elterngeld of parents with child birth in 2007 (Statistisches Bundesamt 2008). These records do not include wages but the level of Elterngeld transfer. From these we generate the regional share of women who receive relatively high transfers (1,500 Euro per month or more) among all those receiving Elterngeld. Between states this share varies between 3.3% and 14.0%.

IV. Results

In this section we evaluate the impact of the reform in January 2007. First, we analyze whether there have been any shifts in the date of delivery at the national level and by subgroup. Then, we analyze regional differences in birth shifting. Finally, we present results for health outcomes which might be affected by such shifts. In order to disentangle the impact of the reform from variation that might be due to the festive season around the end of year we use data from preceding years as reference.

Timing of delivery

For the case of timing of delivery we focus on live births only. Figure 1 displays the number of births per week for several years starting in mid of 2001.⁶ The black solid line indicates the period mid of 2006 to mid of 2007 and largely displays the same seasonal variation observable during preceding years. In contrast to other years, however, there is a very sharp decrease during the last weeks of 2006 and a strong increase of births during the first week of 2007. The number of births during the last week of 2006 (10,907) is clearly the lowest among all weeks in the period under investigation and the number of births during the first week of 2007 (13,305) is the highest among all first weeks of a year.

⁶ In this paper weeks are defined as seven day intervals. The first week of each year is defined to start on January 1st and the last week is defined to end on December 31. In order to obtain exactly 52 seven day intervals July 2nd (and in leap years also July 1st) is not assigned to any week.

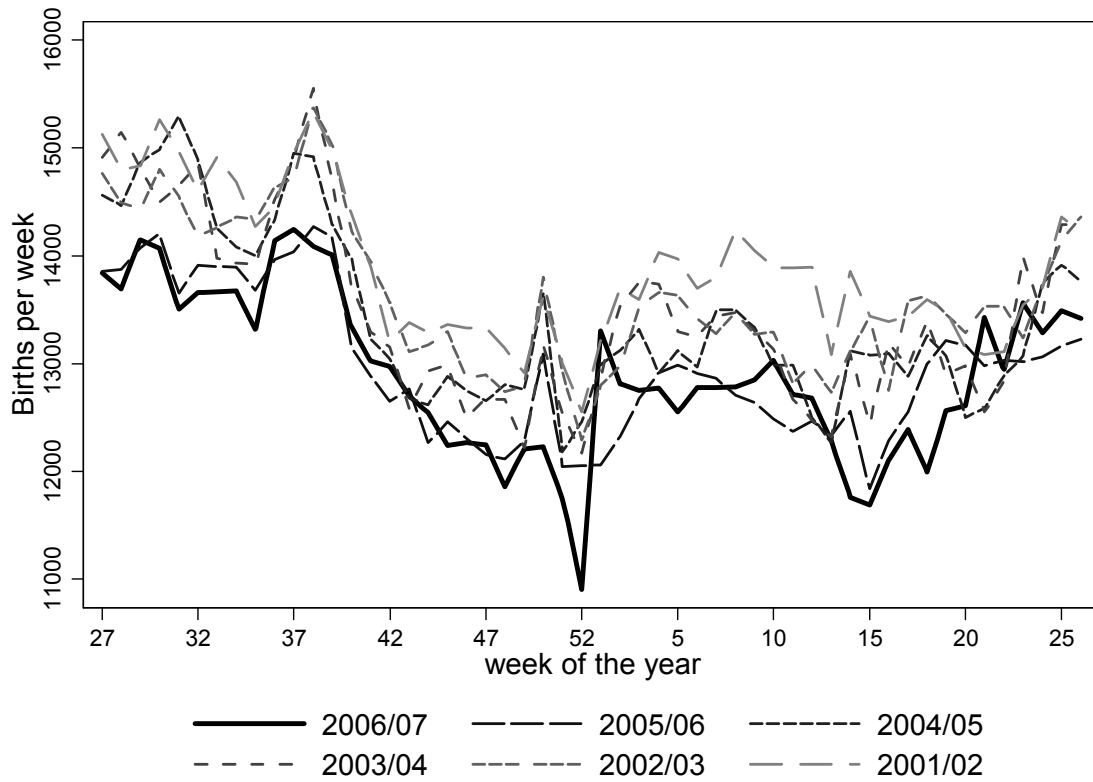
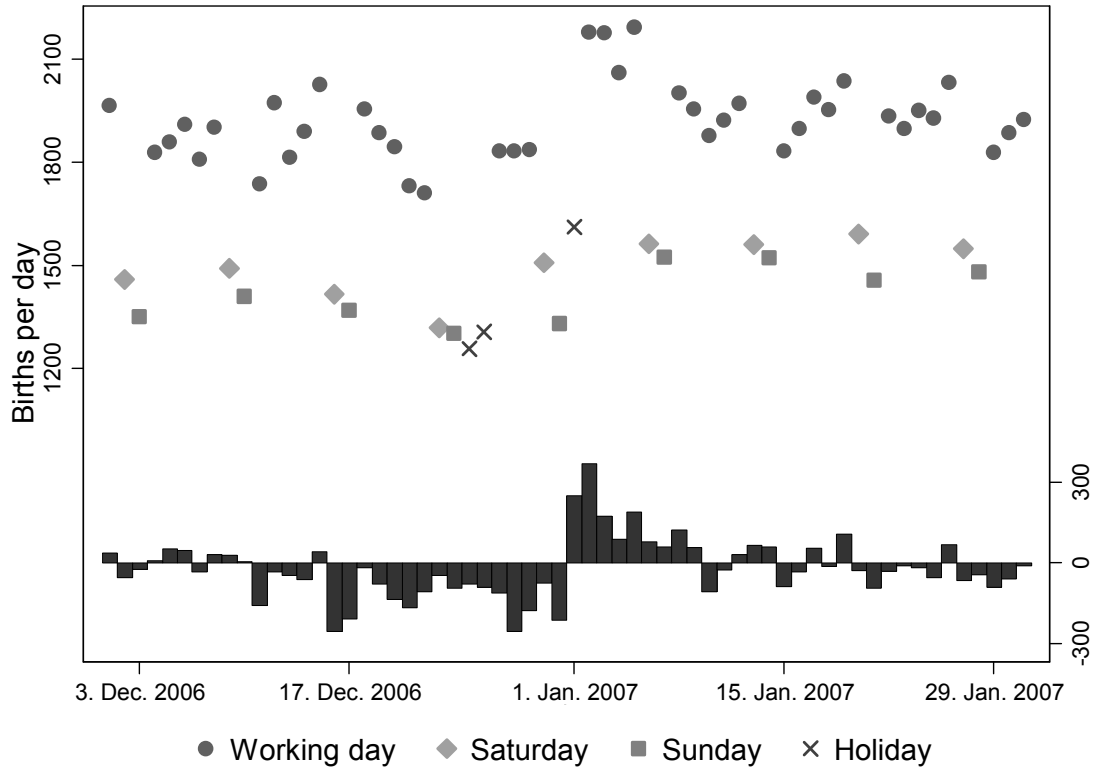


Figure 1. Births per week



Notes: Bars indicate difference between actual and predicted number of births (axis on right-hand side). Predictions are based on births in 2001/02 to 2005/06 controlling for day of the year, weekend, holiday and year effects.

Figure 2. Births per day around January 1st, 2007

A closer look at births around the cut-off date is provided in Figure 2 that displays the number of births per day during the period December 1st to January 31. The individual points report the number of birth during December 2006 and January 2007 with the shape of the point indicating the respective day of the week. The bars below indicate deviations from cyclical trends, which are based on the number of births occurring on each specific day of the year during the period mid of 2001 to mid of 2006 accounting for weekend and holiday effects and yearly level effects. Besides confirming large differences between weekends/holidays and regular working days the figure shows that all of the numbers after December 15, 2006 are below the cyclical trend with differences being especially large after Christmas. On New Year's Day 2007 and during the following week, however, numbers clearly exceed the cyclical trend.

Following Gans and Leigh (2009) we estimate two models to determine the statistical significance of the findings analytically and to obtain estimates for the number of births shifted. In the first model we regress the number of births per day B_t on indicators for Saturdays and Sundays,⁷ a set of year indicators,⁸ a set of indicators for the day of year and a reform dummy indicating those days after the beginning of the reform. This OLS specification is estimated based on data including 7 days before and 7 days after the turn of year 2006/07 and the turns of year in our reference period (i.e. 2001/02 to 2005/06).⁹

$$B_t = \alpha + \beta_1 \text{ saturday} + \beta_2 \text{ sunday} + \sum_{y=02/03}^{06/07} \beta_y \text{ year} + \sum_{d=1}^6 \beta_d \text{ day} + \sum_{d=359}^{365} \beta_d \text{ day} \quad (1) \\ + \delta \text{ reform} + u_t$$

⁷ None of the results for models 1 and 2 changes when including a full set of indicators for day of week instead of including indicators for Saturdays and Sundays only.

⁸ The year indicators are defined to run from July to June, e.g. $\text{year}_{06/07}$ includes July 1st, 2006 to June 31, 2007. They are included to account for time trends in the overall number of births.

⁹ Model 1 is equivalent to a difference-in-discontinuities model. There is an independent parallel paper by Neugart and Ohlsson (2009) who analyze the same reform we do. Based on the argument that mothers without employment history did not gain from the reform, Neugart and Ohlsson (2009) use non-working mothers as control group and estimate a difference-in-difference-in-discontinuities model. They find significant effects as well, but slightly smaller ones than we do. Yet, we think that the reduction in transfer for non-working mothers is too large in order to use them as control group unaffected by the reform and restrict our analysis to a difference-in-discontinuities model.

From the estimates we can calculate the net-number of births shifted, $B_{shift} = 0.5 \cdot \delta \cdot 7$. As sensitivity check we replicate this analysis based on data including alternative time windows of different size. The respective windows cover 7, 14, 21 and 28 days on both sides of each turn of year.

In a second model we allow for asymmetric shifts taking place on both sides of the cut-off. This is done by including separate indicators for each of the four weeks before the reform and the four weeks after. This specification is estimated by including all days within the window of eight weeks before and after each turn of year.

$$\begin{aligned}
B_t = & \alpha + \beta_1 \text{ saturday} + \beta_2 \text{ sunday} + \sum_{y=02/03}^{06/07} \beta_y \text{ year} + \sum_{d=1}^{55} \beta_d \text{ day} + \sum_{d=310}^{365} \beta_d \text{ day} \\
& + \delta_{-4} \text{ week49}_{06} + \delta_{-3} \text{ week50}_{06} + \delta_{-2} \text{ week51}_{06} + \delta_{-1} \text{ week52}_{06} \\
& + \delta_{+1} \text{ week1}_{07} + \delta_{+2} \text{ week2}_{07} + \delta_{+3} \text{ week3}_{07} + \delta_{+4} \text{ week4}_{07} + u_t
\end{aligned} \tag{2}$$

In this model the net-number of births shifted in each week is given by

$$B_{shift}^w = \delta_w \cdot 7 \quad \forall w = -4, \dots, 4.$$

Panel A in Table 2 reports the reform effect and the calculated number of net-shifts¹⁰ for all births as well as births of several subgroups of mothers based on model 1. Results show that the introduction of the Elterngeld led to a significant net-shift towards the year 2007 of more than 900 births from the last week of 2006 towards the first week of 2007 (row 1, columns 1 and 2). Taking into consideration more days around the turn of year, i.e. widening the size of window, the number of net-shifts increases to slightly more than 1,400 (columns 5 and 6). Doing the analysis for subgroups of mothers (row 2 and the following) we find that the net-shift towards January 2007 is much larger among working mothers than among non-working mothers. The shift is significant only among mothers age 26 and older. Given that age is highly correlated with labour market attachment and labour earnings these findings are clearly

¹⁰ Instead of being able to calculate the absolute number of shifts the data only allows for calculating the number of net-shifts from December to January, i.e. the number of births delayed to January minus the number of births brought forward to December.

TABLE 2
Shifts in date of delivery (model 1)

<i>Size of window</i>	± 7 days		± 14 days		± 21 days		± 28 days	
Panel A	<i>Coeff.</i>	<i>Births shifted</i>	<i>Coeff.</i>	<i>Births shifted</i>	<i>Coeff.</i>	<i>Births shifted</i>	<i>Coeff.</i>	<i>Births shifted</i>
All births	261.3 (50.1)	915	164.7 (35.4)	1,153	139.3 (28.8)	1,463	94.0 (24.2)	1,315
By subgroup								
Working mothers	211.3 (31.1)	739	139.9 (22.3)	979	115.7 (17.2)	1,215	81.1 (14.6)	1,136
Nonworking mothers	<i>50.1</i> (26.7)	175	24.7 (18.7)	173	23.6 (15.6)	248	12.8 (13.1)	179
Mothers age ≤ 20	-3.6 (6.5)	-13	-2.1 (4.7)	-15	-2.6 (3.8)	-27	-1.9 (3.4)	-27
Mothers age 21-25	-0.7 (11.9)	-2	6.1 (8.5)	42	12.6 (7.0)	132	7.1 (6.2)	100
Mothers age 26-30	110.2 (18.5)	386	68.3 (13.7)	478	49.7 (10.5)	522	31.6 (9.1)	442
Mothers age 31-35	84.4 (20.1)	296	52.4 (14.5)	367	49.7 (11.9)	522	39.3 (9.9)	550
Mothers age ≥ 36	71.0 (14.9)	249	40.0 (9.8)	280	29.8 (8.1)	313	17.9 (6.9)	251
Panel B	<i>Coeff.</i>	<i>Births shifted in %</i>	<i>Coeff.</i>	<i>Births shifted in %</i>	<i>Coeff.</i>	<i>Births shifted in %</i>	<i>Coeff.</i>	<i>Births shifted in %</i>
All births	0.151 (0.026)	7.8	0.098 (0.018)	5.0	0.082 (0.015)	4.2	0.056 (0.013)	2.9
By subgroup								
Working mothers	0.251 (0.031)	13.4	0.172 (0.023)	9.0	0.141 (0.018)	7.3	0.100 (0.016)	5.2
Nonworking mothers	0.059 (0.028)	3.0	0.031 (0.020)	1.6	<i>0.030</i> (0.016)	1.5	0.017 (0.014)	0.8
Observations	84		168		252		336	

Notes: Sample comprises births during end of 2001 to beginning of 2007 within specific window of days around each turn of year. Outcomes are number of births per day (in Panel A) and log of number of births per day (in Panel B). Regression includes indicators for Saturdays and for Sundays, year indicators, day of the year indicators and a dummy indicating the reform. Results of the reform dummy are reported in the table together with the net number of births shifted. The latter equals 0.5 times the estimated reform coefficient times the size of window in days (Panel A). Proportion of births shifted equals $\exp(0.5 \cdot \delta) - 1$ (Panel B). **Bold** indicates significance at 5%-level, *italics* indicates significance at 10%-level. Standard errors in parentheses.

in line with the structure of incentives (see Section II). Even among those women who are more likely to lose from the reform, e.g. very young women (age 20 and below), we do not observe any significant shift toward December 2006. Panel B in Table 2 largely confirms the findings using the log of the number of births as dependent variable. Using the log number of births provides a measure of the proportion of births shifted, i.e. it relates the number of shifts

with the number of births overall. In the entire population almost 8% of births were shifted from the last week of 2006 to the first week of 2007. Among working mothers the share is 13%.

Results of model 2 show a certain asymmetry of shifts over time (Table 3). The overwhelming majority of shifts took place from the last three weeks of 2006 to the first week of 2007. For those benefiting from the reform this is in line with incentives as each mother expected to give birth during December had incentives to delay delivery. In 2007, however, there was no incentive to delay delivery further than January 1st. The fact that the reduction in the number of births is not only restricted to the last one or two days before the implementation of the reform but is observable over a period of up to three weeks fosters the conclusion that deliveries were actually shifted and that our findings are not simply the result of fraud.¹¹

As the reform was decided on in September 2006, i.e. only three months before its implementation, we would not expect the reform to have any impact on the aggregate number of births, only on the timing.¹² Results of model 2 show that the number of ‘additional’ births in January might be somewhat larger than the number of ‘missing’ births in December and that this is almost exclusively due to non-working mothers. The difference is statistically insignificant, however, so that we cannot reject the hypothesis that the overall number of births was unaffected.

¹¹ For children born in a hospital it is the hospital that reports the information to the birth register. For hospitals it is very unlikely to misreport the time of delivery by more than few minutes. Only for children born at home it is the parents who report the information to the birth register (based on a certificate obtained from the midwife). Yet, on average less than 1.5% of children are not born in a hospital. Even if delivery took place at home, we consider it unlikely that parents can misreport the day of delivery by more than one or two days.

¹² There was a discussion on the implementation of the reform already before the final passing of the law in September 2006. The rough outline of the reform and the day of implementation were discussed by the governing parties in May 2006 (see Kluge and Tamm 2012). That is, even if parents started to adapt fertility decisions in May this should not affect deliveries in January.

TABLE 3
Shifts in date of delivery (model 2)

	<i>All Births</i>			<i>Births by working mothers</i>			<i>Births by non-working mothers</i>		
	<i>Coef.</i>	<i>Births shifted during week</i>	<i>Cumulative births shifted</i>	<i>Coef.</i>	<i>Births shifted during week</i>	<i>Cumulative births shifted</i>	<i>Coef.</i>	<i>Births shifted during week</i>	<i>Cumulative births shifted</i>
Week49/06	37.8 (29.9)	265	-968	12.1 (18.2)	85	-1,103	25.7 (18.3)	180	135
Week50/06	-85.0 (29.9)	-595	-1,233	-56.0 (18.2)	-392	-1,188	-29.0 (18.3)	-203	-45
Week51/06	-20.2 (29.9)	-142	-638	-42.6 (18.2)	-298	-796	22.4 (18.3)	156	158
Week52/06	-70.9 (29.9)	-496	-496	-71.1 (18.2)	-498	-498	0.2 (18.3)	2	2
Week1/07	190.2 (29.9)	1,331	1,331	140.4 (18.2)	983	983	49.8 (18.3)	348	348
Week2/07	47.1 (29.9)	330	1,661	25.5 (18.2)	179	1,161	21.6 (18.3)	151	500
Week3/07	4.7 (29.9)	33	1,694	11.6 (18.2)	81	1,243	-6.9 (18.3)	-48	451
Week4/07	-4.3 (29.9)	-30	1,664	-10.3 (18.2)	-72	1,171	6.0 (18.3)	42	494
Observations		672			672			672	

Notes: Sample comprises births during end of 2001 to beginning of 2007 within eight weeks before and after each turn of year. Outcome is number of births per day. Regression includes indicators for Saturdays and for Sundays, year indicators, day of the year indicators and dummies indicating each of the last four weeks before and the first four weeks after the reform. **Bold** indicates significance at 5%-level, *italics* indicates significance at 10%-level. Standard errors in parentheses. Number of births shifted during each week is equal to the estimated coefficient times 7 (days). Cumulative number of births shifted is the respective sum of births shifted between New Years Eve/Day and the respective week.

As further exercise we analyze individual birth decisions instead of aggregate outcomes per day. Results are based on a regression (linear probability model) of being born in January instead of December on a set of control factors conditional on being born between December 11 and January 21 of the years 2001/02 to 2006/07.¹³ That is, for all those children born during the last three weeks of a year or the first three weeks of the following year, we analyze whether maternal working status, citizenship or age are the driving factors that lead to a delivery in January rather than December. Besides these individual characteristics we control

¹³ Given that the calculation of marginal effects for interaction terms (Ai and Norton 2003) is not trivial when the non-linear model includes multiple interaction terms, we present results of a linear probability model. Results of a Probit model that does not take account of the Ai-Norton problem are almost identical with those of the linear probability model.

for a dummy indicating the year 2006/07 and, most importantly, a set of interaction variables. The latter include interactions between the indicator for the year 2006/07 and the individual characteristics of the mother.

$$\Pr(1 = \text{born in January}) = \alpha + \sum \beta X + \delta \text{ year}_{06/07} + \sum \gamma (X \cdot \text{year}_{06/07}) + u \quad (3)$$

Results in Table 4 show that during years not affected by any reform foreigners and mothers in the highest age group are significantly more likely to deliver in January and that working mothers and mothers in the lowest age group are significantly more likely to deliver during December. The year 2006/07 indicator shows that children in the baseline group (mother not working, age group 26-30 and German citizen) were significantly more likely to be delivered in January after the Elterngeld reform. For this group the probability for a delivery in January rather than December is higher by 1.2 percentage points than during previous years.

The interaction effects show that among working mothers the probability to shift was significantly higher than for mothers in the baseline group, while for mothers age 20 and below it was significantly lower than for mothers in the baseline group. More specifically, for mothers working, age group 26-30 and German citizen the probability of delivery in January rather than December was higher by 3.7 percentage points than during previous years.¹⁴ In contrast, for mothers not working, age group 20 or below and German citizen the probability of delivery in January rather than December was lower by (insignificant) 0.9 percentage points than during previous years.

Given that scheduling births forward (e.g. by inducement or elective caesareans) is generally easier than delaying birth, these results are highly remarkable as there is no group of women displaying significant net-shifts towards December 2006. Even among those women who are more likely to lose from the reform, those actually disadvantaged by the reform do not prevail or at least do not act accordingly (maybe partly because the maximum loss is much smaller

¹⁴ For women differing from the baseline group the reform effect is given by the sum of the interaction term and the year 2006/07 indicator.

than the maximum gain). An alternative explanation might be that these women do not have the power to convince their obstetrician to time delivery accordingly.¹⁵

TABLE 4

Multivariate regression of the probability of delivery during January rather than December

<i>Linear probability model (1=born in January)</i>	<i>Marginal effect</i>	<i>Std. error</i>
Year 2006/07	0.0118	0.0045
Working × year 2006/07	0.0253	0.0042
Foreigner × year 2006/07	-0.0031	0.0053
Mother age ≤20 × year 2006/07	-0.0210	0.0092
Mother age 21-25 × year 2006/07	<i>-0.0099</i>	0.0059
Mother age 31-35 × year 2006/07	0.0016	0.0053
Mother age ≥36 × year 2006/07	-0.0005	0.0061
Working	-0.0046	0.0017
Foreigner	0.0083	0.0022
Mother age ≤20	-0.0072	0.0036
Mother age 21-25	-0.0036	0.0023
Mother age 31-35	0.0020	0.0021
Mother age ≥36	0.0067	0.0026
R ²	0.0005	
Observations	461,725	

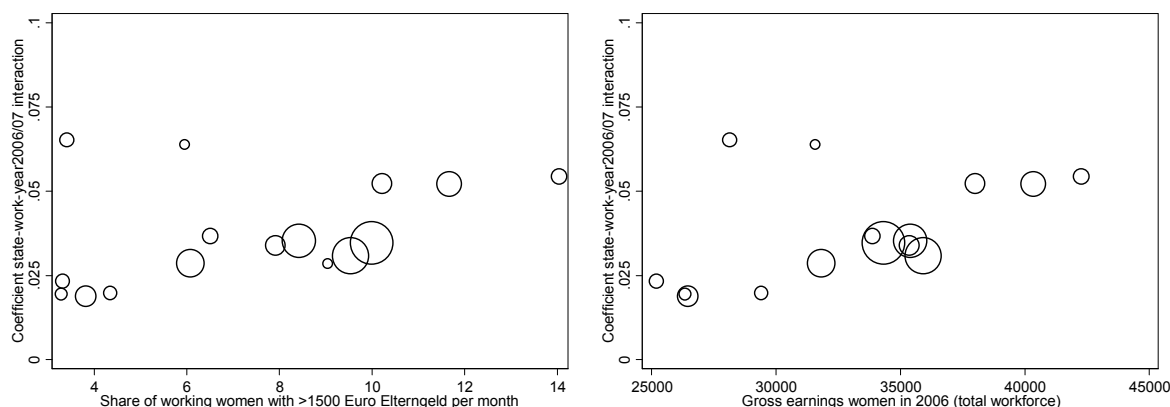
Notes: Sample comprises births during end of 2001 to beginning of 2007 within three weeks before and after each turn of year. Outcome of the linear probability model is birth during January. Reference group of regression is years before 2006/07, mother not working, age group 26-30 and German citizen. **Bold** indicates significance at 5%-level, *italics* indicates significance at 10%-level.

Regional differences in timing of delivery

To investigate the importance of factors not included in the birth register data we next consider regional differences in birth shifting and their correlation with variables measured at the regional level. Information on region specific birth shifting for working and for nonworking mothers is generated from a regression similar to model 3 that additionally includes separate indicators for the 16 federal states (each of them interacted with the indicator for maternal working status) and interaction terms between these state-(non)working indicators and the year 2006/07 dummy. The coefficients of the latter interaction terms are our estimates of region specific birth shifting. They are used as dependent variable and regressed on several region specific factors like the share of women receiving high amounts of

¹⁵ Further results show that controlling for marital status and number of siblings (within the present marriage) does not challenge these findings. While married mothers with three or more previous children are less likely to deliver during January 2007 than mothers in the baseline group, there is no significant difference between these mothers and married mothers with three or more children during previous years.

Elterngeld, average female wages and a dummy indicating supplementary benefits for parents that are available in some regions but not in others.¹⁶



Figures 3a, 3b. Regional differences in birth shifting among working women and (a) share of recipients with high Elterngeld and (b) average female earnings

Results (available upon request) indicate that there are no significant correlations between regional differences in shifting and the dummy indicating supplementary benefits for parents. In contrast, there are significant correlations with the income variables that reflect the incentives created by the reform. Figure 3a plots state specific shifting among working mothers against the share of working mothers who receive more than 1,500 Euro Elterngeld per month and Figure 3b plots state specific shifting among working mothers against average gross earnings of all women in the labour market. Each circle represents one of the states and the size of the circle indicates the importance of the state (measured by the overall number of deliveries during the year 2007). There is a slight positive correlation between the share of recipients with high Elterngeld and shifting as well as between average female earnings and shifting.¹⁷

¹⁶ In the four states Bavaria, Baden-Württemberg, Saxony and Thuringia parents with children born up to 2006 could receive a means-tested benefit during months 25 to 36, i.e. after the old Erziehungsgeld was terminated, the so-called Landeserziehungsgeld. After the implementation of the new Elterngeld regulations for the regional benefit were amended. Now parents receive this benefit during months 13 to 24 but means-testing remains similar.

¹⁷ Regressing the state-work-year2006/07 coefficients against the share of recipients with high Elterngeld results in a positive point estimate that is significant at the 7%-level. Per one percent more women receiving 1,500 Euro Elterngeld the probability to shift was higher by 0.19 percentage points among working women. A regression against average gross earnings of women results in a positive point estimate that is significant at the 3%-level. Per 1,000 Euro higher average annual income the probability to shift was higher by 0.16 percentage points

Health outcomes

After having shown that there were sizeable shifts in deliveries, we now analyze health outcomes. It is a well known fact that most births do not exactly occur on the expected day of delivery. Instead they are approximately distributed normally around the due day with a standard deviation of around 10 days. As such, a shift in date of birth is nothing uncommon. However, one might fear that a shift of birth for non-medical reasons might lead to complications. In addition to birth related complications there might in principle also be problems with congestion or overcrowding in hospitals. In our case, however, the latter should not be responsible for any health problems among those being shifted as birth numbers during the first week of January (even including those who were delayed) were still smaller than birth numbers in average weeks of July, August or September.

The data allows for analyzing several health outcomes: birth weight in grams, length at birth in centimetres and several health indicators derived from weight, e.g. low birth weight (<2,500 grams), very low birth weight (<1,500 grams) and high birth weight (>4,000 grams). In the literature weight and length are used extensively and according to Almond et al. (2005) birth weight is the measure of baby's health that receives most attention.¹⁸

Using the micro data available in the birth registers we regress each of the health outcomes on a set of day of the year indicators, indicators for each day of the week and year indicators. These are included to control for weekly and seasonal variation. In addition we include

among working women. Surprisingly, when regressing the state-nonwork-year2006/07 coefficients against the share of recipients with high Elterngeld we also find a positive point estimate that is significant at the 8%-level, while regressing the state-nonwork-year2006/07 coefficients against average gross earnings of women gives insignificant effects.

¹⁸ In the economic literature (low) birth weight is used in several contexts (Almond et al. 2005). Firstly, it is used as output in analyzing infant health production functions, e.g. with regards to the impact of maternal behaviour but also with regards to evaluate the impact of social policy. Secondly, birth weight is used as input in production functions for adult health or human capital in its broadest sense. For example, Black et al. (2007) show that birth weight does not simply matter in the short-run but also in the long-run affecting adult height, BMI, IQ, educational attainment and labour earnings. Oreopoulos et al. (2008) find additional effects on death rates between age one and 17, grade completion at high school and welfare take-up. Other studies are Royer (2009) and Johnson and Schoeni (2011). While most of the literature focuses on low birth weight, some studies also focus on high birth weight and sometimes also on length at birth. For example, The et al. (2010) find that babies with more than 4000g are more likely to become obese in adolescence and Pietiläinen et al. (2001) show that height in adolescence is related to length and weight at birth.

dummy variables indicating each of the last three weeks before the reform and an indicator for the first week after the reform. These are supposed to indicate differences during the turn of year 2006/07 when the reform was implemented and when birth shifting took place.

We start with looking at the impact on birth weight. Results in Table 5 show that children born during the first week of 2007 were heavier by around 11 grams (significant at the 10%-level). Related to the 8% of babies that have been shifted this would imply that babies that were actually delayed were heavier by around 130 grams, which is equal to the average weight gain of a foetus at term over a four to five day period.¹⁹ Correspondingly, the proportion of high birth weight babies is higher by around 0.5 percentage points (significant at the 10%-level). In addition, similar increases in birth weight and the share of high birth weight babies are observed during the last week of 2006, which might result from shifts from earlier weeks in December that were not successful in delaying until January. For length at birth Table 5 shows that shifting had no significant influence. This also holds when using various binary indicators for small or large length at birth (not reported in the table).

Given that shifts in date of delivery were not random, the weight differences in comparison with preceding years might directly result from shifts in the date of delivery but might as well be due to selective shifting. Therefore we also estimate a specification that controls for selectivity based on observable factors, i.e. we additionally include individual-specific information on the child (i.e. gender, multiples) and its mother (i.e. age, region, citizenship, working and marital status, parity) which are important determinants of health outcomes. Results (available upon request) show that the effects on weight only change slightly.

¹⁹ Hardlock et al. (1991) report an average weight gain per day of between 25 and 30 grams during the 38th to 40th week of gestation.

TABLE 5
Health outcomes

	<i>Weight</i>	<i>lbw</i> (<i><2,500 g</i>)	<i>vbw</i> (<i><1,500 g</i>)	<i>hbw</i> (<i>>4,000 g</i>)	<i>Length</i>
Average in population	3,335 g	6.94 %	1.18 %	10.16 %	51.09 cm
	Marginal effect				
Week50/06	0.21 (6.02)	0.066 (0.259)	-0.117 (0.102)	-0.201 (0.309)	-0.01 (0.03)
Week51/06	-2.11 (6.14)	-0.051 (0.257)	0.047 (0.110)	-0.351 (0.315)	-0.01 (0.03)
Week52/06	<i>10.78</i> (6.34)	-0.250 (0.268)	-0.033 (0.113)	<i>0.657</i> (0.338)	0.01 (0.03)
Week1/07	<i>10.50</i> (5.86)	-0.238 (0.248)	0.107 (0.114)	<i>0.516</i> (0.308)	0.02 (0.03)
Observations	1,237,902	1,237,902	1,237,902	1,237,902	1,233,645

Notes: Sample comprises births during end of 2001 to beginning of 2007 within eight weeks before and after each turn of year. Regression includes day of the year indicators, indicators for each day of the week, year indicators and dummies indicating each of the last three weeks before the reform and for the first week after the reform. For weight and length estimates are based on OLS, for other health outcomes are based on Probit models. Marginal effects and standard errors of Probit models are multiplied by 100. **Bold** indicates significance at 5%-level, *italics* indicates significance at 10%-level. Standard errors in parentheses.

V. Discussion and conclusion

The introduction of the parental leave benefit (Elterngeld) applied to all children born in Germany on January 1st, 2007 or later. The new Elterngeld considerably changed the amount of transfers to families during the first two years postpartum. Especially households with women who were working before child birth, those planning to take up full time work relatively early and high income households were receiving more transfers after the reform than before. In contrast, households with women who were not working before child birth or those with low pre-birth earnings planning to take up full time work relatively late experienced a decrease in cumulative transfers in some cases.

We show that the incentives created by using such a cut-off date led more than 1,000 parents to postpone the delivery of their children from December 2006 to January 2007. That is, around 8% of all births were shifted from the last week of December to the first week of January and several deliveries were moved by more than one week. Concerning health outcomes of children we observe an increase in average birth weight and the rate of children

with high birth weight ($>4,000$ grams) during the last week of December and the first week of January.

From a policy perspective one might learn that (i) families do react to short-term incentives. Taking into account findings from the preceding literature on significant birth timing effects (Dickert-Conlin and Chandra 1999, Gans and Leigh 2009) as well as insignificant ones (e.g. Dickert-Conlin and Elder 2010, Dustmann and Schönberg 2011, Lalive and Zweimüller 2009 and McEwan and Shapiro 2008) a conclusion might be that parents do react to incentives if the financial reward is immediate but do not or are less likely to do so if it only affects opportunity costs at some future point in time. (ii) While those groups advantaged by the reform display large movements in the date of delivery and thus indicate a strong preference for the new Elterngeld, those groups expected to lose from the reform did not show any response in timing of delivery. This is especially remarkable given that forwarding birth is generally easier than delaying birth. Those groups expected to lose from the reform do thus not show any clear preference for either new or old benefit or have less power to convince their obstetrician about the 'right' timing of the delivery. (iii) Comparing differences in birth timing between states we find that effects were slightly larger in states where more mothers received high Elterngeld transfers. (iv) Even though we find only a slight increase in the rate of high birth weight babies, we would recommend politicians to use cut-off dates that are very close to or identical with the date of announcement of a reform or maybe even precede that date to avoid unintended response in the future. Also note that the Elterngeld was implemented in January, which generally is a month with relatively few deliveries in Germany. We cannot rule out that using a month with a higher number of deliveries as cut-off date would have led to different health consequences. Using as cut-off date a month with a relatively high number of deliveries might have created problems due to overcrowding of hospitals.

Finally, our findings have consequences for researchers trying to evaluate the impact of the reform on parental behaviour, like e.g. maternal labour market participation (see Kluge and Tamm 2012, Bergemann and Riphahn 2011a and 2011b) or on long-term outcomes of children. In principle, the application of a sharp discontinuity in policy (i.e. the cut-off date) provides researchers with an excellent opportunity to analyze the impact of the reform by comparing outcomes of those close to both sides of the discontinuity (i.e. those born just before the cut-off date with those just born after) using a regression discontinuity design (Imbens and Lemieux 2008). However, this is a valid identification strategy only if individuals are either not able to determine the variable that determines whether they are on one side or the other at all or if individuals have only imperfect control over that variable (Lee 2008, Lee and Lemieux 2010). As we have shown, parents did influence the date of birth and shifts strongly differ between socioeconomic groups. Thus the socioeconomic background of children born shortly before and shortly after largely differs. That is, there is a problem of selectivity that might contaminate a comparison of those born just before the cut-off date with those just born after (Urquiola and Verhoogen 2009).

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