J Popul Econ (2013) 26:87–108
DOI 10.1007/s00148-012-0420-1

ORIGINAL PAPER

Economic incentives and the timing of births: evidence from the German parental benefit reform of 2007

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Received: 16 February 2011 / Accepted: 18 April 2012 /
Published online: 1 June 2012

Abstract Economic theory suggests that incentives matter for people's decisions. This paper investigates whether this also holds for less self-evident areas of life such as the timing of births. We use a natural experiment when the German government changed its parental benefit system on January 1, 2007. The policy change strongly increased economic incentives for women to postpone delivery provided that they were employed. Applying a difference-in-difference-in-difference approach, we find very strong evidence that women with an employment history near to the end of their term indeed succeeded to shift births to the New Year and, therefore, could benefit from the new and more generous parental benefit system. Suggesting a model of chain reactions, we also report evidence that some women with due dates earlier in December tried but did not succeed to shift births to the New Year.

Keywords Timing of births · Economic incentives · Parental benefits · Policy reform

JEL Classification J130 · J180 · H530 · D190

Responsible editor: Junsen Zhang

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Principle #4: People respond to incentives.
N.G. Mankiw, *Principles of Economics*. Fort Worth: Dryden Press, 1998

1 Introduction

On New Year's Day 2007, a new parental benefit system was enacted in Germany. At the extreme, a woman giving birth 1 min before midnight instead of giving birth in the New Year would have lost, depending on her previous net income, several thousands of euros in transfers from the government. This is an exceptional policy change close to a natural experiment. We exploit it to study whether monetary incentives matter also in less apparent areas of daily life such as the timing of births. As they do, concerns may arise with respect to unintended consequences of such major reforms—here, in particular, health issues of the newborns.

The nature of the policy change makes it possible for us to, compared to existing studies, not only conduct a before/after comparison against previous reference years. We also exploit an identification possibility along women's employment status. We believe that this allows us an even more credible inference on whether incentives drive the timing of births or more generally whether incentives drive people's behavior.

A second contribution of this paper is that we estimate the effect of the policy change not only close to the key date but for a fairly wide time window before and after the policy change. Thus, we can show that such kind of policy reforms do not only have an impact on people's behavior close to the time of their implementation but initiate behavioral changes well in advance, and have follow up effects. A chain reaction model suggests that there were unsuccessful shifts of births occurring before the policy change and crowding out of births after the policy change. An unnatural increase of births before the policy change driven by unsuccessful shifts of births and capacity constraints potentially restricting births after the key date affect the size of the estimated effect of the policy change.

Our analysis draws on data covering *all* births occurring in Germany during the 7 days before and after the change of the parental benefit system in 2006/2007 and the 2 weeks centered around the turn of the year of the previous 2 years for comparison. Overall, our base sample consists of up to 74,000 observations including, in addition to the date of birth, various socioeconomic characteristics of the childbearing mother and the father of the child. Most importantly, we have information on the employment status of the mother. This allows us to estimate the effect of the policy change on the timing of the births applying a difference-in-difference-in-difference approach.

The *first* difference is the before/after comparison of births around the policy change in the year 2006/2007. If economic incentives matter, we expect a dip in births during the days preceding the policy change and a peak for the days after the policy change. The *second* difference arises from comparing this difference with the difference in births around the turn of the year of the

preceding 2 years. *Finally*, we bring into the picture the difference along the dimension of whether the childbearing woman has an employment history or not. The enacted change in the parental benefit system was advantageous for employed women while hardly changing the incentives for women without an employment history preceding the childbearing. This suggests a positive effect along the employment dimension.

It is in particular this third difference, which we can exploit due the nature of the policy change, and the estimation of shifts in births taking place several weeks before and after the policy change by which we substantially differentiate from previous studies of the incentives to time birth. The third difference may address the problem that one mistakenly attributes the shifts in births to calendar effects which may not be appropriately modeled by weekday or holiday indicators. Such issues may, in particular, arise when key date regulations fall on a New Year's Day which is surrounded by holidays (and weekends). Day patterns around these key dates vary. It is, therefore, very difficult to appropriately model hospital activity which is a major constraint on child bearing activity. While we know that there are, on average, fewer babies born on weekends and holidays, activities for, e.g., bridging days are difficult to control for. Identifying incentives along the third dimension, which is employment status in our case, circumvents the inference problem if employed and not employed women are affected by varying day patterns for treatment and nontreatment years in the same way. And we believe that this is the case.[1]

The actually measured size of the effect around the date of the policy change may be biased if women unsuccessfully tried to shift births from weeks well before the policy change. Furthermore, as hospitals face capacity constraints, it is also possible that some births that were actually due in the early days after the policy change were shifted to later days. Again, this would have an impact on the actually measured size of the effect. Therefore, and on top of previous attempts to find evidence for economic incentives for the timing of births, we model and estimate the behavior of women in a larger time window of in total 8 weeks around the policy change.

The change in the parental benefit system received an extensive press coverage in Germany during the last days of the year 2006 ranging from the major newspapers to the tabloid press, and even foreign newspapers which makes it very likely that pregnant women were aware of the economic consequences of shifting birth to the New Year. Besides the legislative innovations and what this meant in terms of transfers under the new regime as opposed to the old one, a hot topic was whether women were ready to shift birth to increase the transfer. On December 31, 2006, *RP.Online* quotes Björn Brunke, assistant medical director, saying "... on Thursday we had a delivery by an employed woman. She actually was a bit aggravated." Joachim Dudenhausen, chief obstetrician

[1]Note that from the perspective of using control years which show the same day pattern in a time window 7 days before and 7 days after the policy change, the 2000–2001 pair of years would have been particularly interesting as in this respect it is equivalent to the treatment years 2006 and 2007. However, it happens that in these years, a legislative change in educational benefits occurred.

at Charité Berlin, is quoted in *Der Tagesspiegel Online* on December 31, saying that "About a third of those wanted to speed up the birth. Two thirds wanted to retard it. These are people looking forward to the 12 or 14 months during which they get 67% of their current net income."

Besides whether women were actually ready to shift births, another issue was whether women would try to cheat in order to qualify for the new parental benefit system. Grid Rademacher, midwife, had a clear stance on this as reported in *Spiegel Online* on December 28: "But we do not cheat here." Others spoke out less clearly: "Key date regulations are always inequitable... However, all the watches in the corridors of our hospital run slightly differently." (Boris Gabriel, assistant medical director, *FAZ.NET*, December 31, 2006). Suggestions were passed in the media by midwives and doctors that stress, sport, or sex may spark contractions—somewhat a point of no return for a woman who wanted to shift birth to the New Year.[2]

There are a couple of previous studies on the timing of births.[3] Dickert-Conlin and Chandra (1999) study the impact on the timing of births of tax changes in the USA in 1979–1993. The tax changes were such that there were incentives to hasten births. The authors find that almost 14% of the births were shifted from the first week of January each year to the last week of December. It should be noted that the tax changes were known well in advance so that both conceptions and births potentially could be affected by the policy changes. In a complementary study to Dickert-Colin and Chandra, Maghakian and Schulkind (2010) find tax effects of similar size, based on a broader dataset with the possibility to also look into the mechanisms with which birth dates were changed and the health consequences of it.

Gans and Leigh (2009) use Australian data on the introduction and the increase of a Baby Bonus in the years 2004 and 2006. These allowances created incentives for all women to delay births. The authors find that 16% of the births were shifted in 2004 while 9% of births were shifted in 2006. The introduction in 2004 was not known enough ahead to affect conceptions while conceptions potentially could have been affected in 2006.

Finally, Tamm (2009) also uses German data stemming from the parental benefit reform. He finds that around 8% of births were shifted from the last week before the policy change to the week after. This analysis differs from ours as he additionally looks into potential health effects for the newborn babies arising from the shifting of births. Most importantly, however, Tamm (2009) does not use the employment status of women as an explanatory factor for the shifting of births.[4]

[2] All quotes were translated by the authors.

[3] There is also a related literature on the timing of death with contributions from Kopczuk and Slemrod (2003), Gans and Leigh (2006), and Eliason and Ohlsson (2008, 2010).

[4] While we study incentives to shift birth for women already being pregnant having an employment history or not, there is also a literature studying fertility in relation to women's employment. See, e.g., Gutierrez-Domenech (2008).

There are two results we would like to stress. First, economic incentives did matter leading to a shift of births for employed women. Assuming that biological constraints imply a fairly small time window for postponing births, we find a marginal effect of approximately five percentage points for employed women in an analysis focusing on the 7 days before and the 7 days after the policy change.

Second, however, there occur to have taken place unsuccessful shifts in births during the earlier weeks in December to the last days of December and some shifts of births possibly driven by capacity constraints of hospitals from the first days of January to later days. Taking these chaining effects into account, the magnitude of births that shifted following the German legislative change is in the ballpark of what previous studies find on other data, applying different estimation techniques.

We proceed by a description of the policy change in the following section and the comparison group in Section 3. Section 4 reports on the timing of births. In Section 5, we present our findings. Finally, we conclude in Section 6.

2 The policy change

As of January 1, 2007, German legislation with respect to parental benefits changed. What was formerly known as the educational benefit (*Erziehungsgeld*) became the parental benefit (*Elterngeld*). Babies born until midnight of the New Year's Eve were still subject to the educational benefit. However, any baby born on January 1, 2007 or later would make parents eligible for the parental benefit. Table 1 summarizes the core of the legislative changes.[5]

Under the old law, parents could opt for a monthly payment of EUR 300 for 24 months or a payment of EUR 450 for 12 months. No employment history was required in order to qualify for the educational transfer. However, income limits were applied so that transfers would not be paid or reduced if net income earned in the previous year was above certain thresholds depending on family status and number of children.[6] With the new law, two major changes were introduced: (a) transfers could now be in a range between EUR 300 and EUR 1,800 per month and (b) transfers were made conditional on the employment history of the last 12 months of the parent applying for the transfer.

In particular, from January 1, 2007 onwards, the transfer to the parent is calculated as 67% of the average net monthly income of the 12 months before the delivery of the baby. Thus, depending on the previous income, a parent may get up to EUR 1,800 per month for a duration of 12 months under the parental

[5]The corresponding bills from which this information is taken are the *Bundeserziehungsgeldgesetz* (BErzGG) and the *Bundeselterngeld- und Elternzeitgesetz* (BEEG).

[6]See Appendix for more details on income limits.

Table 1 The policy change

	Before	After
	Educational benefit (*Erziehungsgeld*)	Parental benefit (*Elterngeld*)
Monthly benefit, EUR	300 (450), income limits applied	67% of average net monthly income during the previous 12 months, min EUR 300, max EUR 1,800
Maximum benefit duration, months	24 (12)	12 (14)
Employment condition	No employment history was required	Employment history is required for payments above minimum
Maximum total benefit, EUR	7,200 (5,400)	With employment history 3,600–21,600 (4,200–25,200) Without employment history 3,600 (4,200)

transfer regime. A lower bound of EUR 300 per month provides transfers even to those who would fall below based on their previous net income including those parents who did not receive any income at all in the previous 12 months.

The mother or the father is eligible for applying for the parental leave payment. The applicant's employment history is taken into account when calculating the transfer. Payments are conditional on the applicant not working or receiving an income of less than 300 euros during the time of support. It is possible to extend benefit duration by another 2 months. If, for example, the mother was taking care of the child during the first 12 months, the father could apply for an extension of 2 months if he wants to take a leave from his job and take care of the child.[7]

For a parent without an employment history during the 12 months before the delivery of the baby who would have gone for the EUR 450 under the old law, the introduction of the new law constitutes a (relatively) small loss of EUR 150 per month. Most importantly, for our analysis, however, a parent with an employment history who would have chosen the 12-month option under the old regime may gain considerably under the new law. For example, should the parent qualify for a transfer of EUR 863 per month (which corresponds to a net monthly labor income of ≈EUR 1,288), the increase in transfers in comparison of the two schemes amounted to EUR $12 \cdot (863 - 450) = 4{,}956$. At the extreme, if a "high-income" parent in the old regime would have collected EUR 1,733 by staying at home for 1 year after giving birth,[8] under the new

[7]Kluve and Tamm (2012) evaluate the taking up of leaves of the German parental benefit reform.
[8]This assumes a net yearly income of 32,239 in which case no transfers would have been paid for the first 6 months under the old regime and from month 7–12 transfers would have been cut by 7.2% in relation to the income exceeding a threshold of 30,000 euros.

regime the same parent is eligible for up to EUR 21,600, resulting in a gain of EUR 19,867.[9]

3 Comparison group

Around 93% of all approved requests within the first 3 months after the policy change were submitted by women although either parent may apply. This indicates that it is the mother's employment status and not the father's employment status which is driving the incentives to time the birth. Our data records the woman's employment status before giving birth only. Accompanying sources of information, however, suggest that there is overall a large overlap between the employment status before birth and the employment history within the 12 months preceding the birth. Survey evidence presented in Bundesministerium für Familie, Senioren, Frauen und Jugend (2008) states that there is only a 5% point lower employment rate for women at the time immediately before birth is given if compared to the employment history in the previous 12 months. Thus, conditional on working immediately before the birth is given, most women also worked in the 12 months before the birth. This suggests that we are confronted with a homogenous study group. Women were employed when giving birth and have continuous employment histories by definition. It is very unlikely that any of these women would not benefit from the reform.

By contrast, the comparison group is heterogenous. These women were not employed when giving birth. One can imagine that these women had no employment history at all and, therefore, did not gain from the reform. Others may have had an interrupted employment history and may still have gained from the reform. Approved requests for the first 3 months after the policy reform clearly indicate different payments for employed and not employed women (see Table 2). For women with an employment history more than 80% received more than EUR 450 in transfers. Contrarily, 98.9% of the women without an employment history received between EUR 300 and EUR 375 with the EUR 75 on top of the EUR 300 being the transfer ascribed to women who already have a child. Note also that no matter how high transfers are or whether women have an employment history or not, average applied for and

[9]Two examples taken from the household income records of the *Statistische Bundesamt* may help bring the transfers from the parental benefit into perspective with gross incomes and net incomes after tax and social security payments. According to Statistisches Bundesamt (2008), a single parent's gross monthly labor income was EUR 1,255 on average in year 2006. He or she had to pay EUR 151 taxes and EUR 251 in social security contributions. Thus, the base for calculating the parental benefit would have been EUR 853. As a second example, take the average gross monthly household labor income of a couple with children. This amounted to EUR 3,719 with tax and social security contributions being EUR 537 and EUR 606, respectively. If both partners contributed equally to the household income, then the base for calculating the 67% of transfers is EUR 1,288.

Table 2 Approved requests for parental benefit payments for women, January 2007–March 2007

Monthly benefit, EUR	With employment history		Without employment history	
	Share, %	Ave. dur., months	Share, %	Ave. dur., months
299–375	12.9	11.64	98.9	11.61
375–450	6.4	11.82	–	–
450–525	6.6	11.73	–	–
525–600	8.2	11.80	0.6	11.54
600–700	18.6	11.80	0.5	11.58
700–800	9.8	11.75	–	–
800–900	9.0	11.76	–	–
900–1,000	7.6	11.73	0.0	12
1000–1,100	5.5	11.71	0.0	12
1,100–1,200	3.8	11.60	–	–
1,200–1,300	2.4	11.60	–	–
1,300–1,400	1.7	11.53	–	–
1,400–1,500	1.5	11.50	–	–
1,500–1,600	1.3	11.36	–	–
1,600–1,700	1.2	11.57	–	–
1,700–1,800	3.0	11.58	–	–
1,800–more	0.3	11.17	–	–
Total	100	11.70	100	11.61

Numbers as calculated by the Statistische Bundesamt

approved duration of payments is almost 12 months.[10] Overall, these numbers suggest that incentives for women to shift or not to shift birth indeed differed along the employment dimension warranting the use of women's employment dimension as a third difference.

The first serious step to change the German parental benefit system was taken by the partners of the coalition government (composed of the CDU/CSU and SPD parties) in June 2006. A draft law on parental benefits was presented and published as a *Bundestagsdrucksache* framing the following discussion.[11] After several committee hearings and statements received by the second chamber, the proposal by the *Bundesregierung* followed at the end of August 2006.[12] Finally, the new law "Gesetz zur Einführung des Elterngeldes" came into effect on December 5, 2006.[13]

Why are we presenting this short history of the genesis of the law? We do so as one might be concerned about possible endogeneity of conceptions and the employment status of the woman and her partner. Given the short time period elapsed between the first proposal of the new law and the new government benefits becoming operative on January 1, 2007 relative to gestation periods, we believe that this is a minor issue. Furthermore, it is also unlikely that women or men could considerably increase net income earned in the 12 months

[10]The employment rate of women for the data on which Table 2 is based is 39.8% compared to 45.5% in our sample. Sample sizes are available from the authors upon request.

[11]See the *Bundestagsdrucksache* 16/1889 dated June 20, 2006.

[12]C.f. *Bundestagsdrucksache* 16/2454 dated August 25, 2006.

[13]See the *Bundesgesetzblatt* Jahrgang 2006 Teil 1 Nr. 56, issued in Bonn on December 11, 2006.

preceding birth by starting to look for a job as a reaction to the announced policy change, given an average job search duration of 40.1 weeks in 2006 (Bundesagentur für Arbeit 2010). Putting it differently, the legislative change was truly exogenous to the pregnant woman and her partner.

Furthermore, it occurs that the broad public became aware of the legal change with the intense media coverage during Christmas holidays which we already described earlier on. Comparing the number of babies born in Germany in the first quarter of 2007 (164,683) with the number of households that received parental benefits (163,372)—after having filed an application—further strongly indicates that the new policy was known to the eligible households right from the beginning (Bundesministerium für Familie, Senioren, Frauen und Jugend 2008). Finally, we are not aware of any other policy change during our treatment and control years that would potentially contaminate our data.

4 Timing of births

Economic theory suggests that economic incentives matter for the decision on the timing of births. Contrary to other fields of investigation such as the decision to work or not, getting married, inherit, or residence choice as a response to tax or transfer changes, women's decisions in our case are restricted by biology. The exact timing of birth is not feasible. However, by means of Cesarian section or the inducement of labor the timing of the delivery of a child can be manipulated and postponed.

Cesarean sections are invasive surgeries requiring an abdominal incision. Typically, Cesarean sections are made if the conditions of the mother or the child prevent a vaginal delivery. But women may also decide for a Cesarean section if a natural delivery would have been possible. An inducement of labor is a stimulus to the uterus sparking contractions in order to achieve the delivery.

When a woman and the obstetrician have decided that birth will not be given naturally, it is, within limits, up to them to arrange for a date for the Cesarean section or the inducement of labor. Very often, the day of delivery is driven by the organizational and capacity constraints of the hospital and may be postponed for a few days.

Rule of thumb suggestions given by midwives indicate that avoidance of physical or mental stress may delay the onset of labor. In fact, there is a literature that tries to causally link maternal stress with preterm delivery. The majority of these studies finds a significant association between these two variables (see, e.g., Khashan et al. 2009), supporting the individual views of the midwives expressed in the public at the eve of the policy change.

During the days preceding the policy change, a midwife was cited in one of the major German newspapers[14] who claimed that drugs were traded on the

[14]See *Süddeutsche Zeitung, Regionalausgabe*, December 30, 2006.

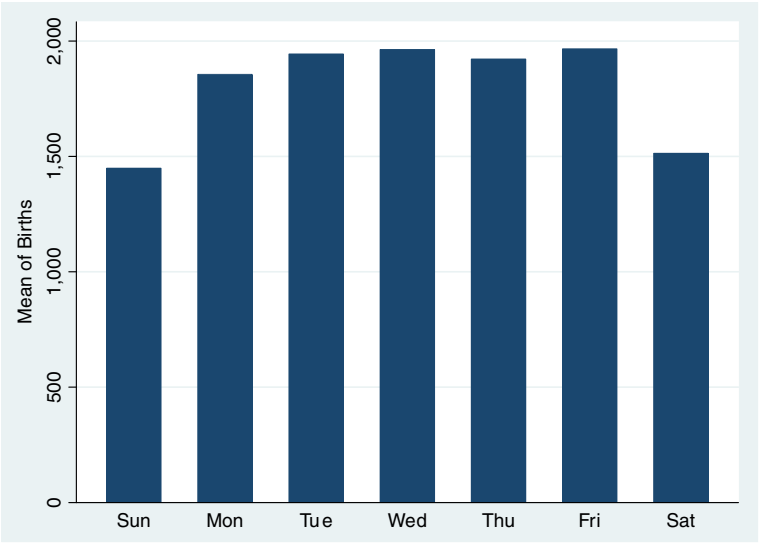

Fig. 1 Average number of births by weekday for all December and January months, December 2004–January 2007

Internet which may help postponing birth. These tocolytics such as Nifedipine or Atosiban are typically given to women in preterm labor and are apt to delay delivery by 48 h as reported in a meta-analysis in Coomarasamy et al. (2003). Due to these drugs being traded illegally, there is no way to assess if women actually were using this possibility to postpone birth.

Perhaps the most convincing argument for the feasibility of the timing of births comes from Fig. 1. It shows average births by weekday for all December and January months from December 2004 to January 2007. The striking feature is that on Saturdays and Sundays, overall births are lower than at any other day of the week which would not be the pattern if deliveries could not be shifted away from weekends. This piece of evidence is in line with previous findings on weekday effects, see Chandra et al. (2004) and Gans and Leigh (2009).

While we lack the data that would allow us to split up births by way of delivery, these figures still suggest that organizational considerations on the side of the hospitals may play a role in the timing of births, and, hence, there must be ways of medically manipulating the timing of the delivery of a child at least for a few days. The same figure, furthermore, illustrates the importance of weekday patterns warranting an analysis of the role of incentives for the timing of births along a third dimension as what we intend to do.

5 Empirical evidence

5.1 Descriptive evidence

If we expect economic incentives to matter, then we should be able to observe a drop in birth counts in the last days of December 2006, before the policy

change was implemented, and an increase in birth counts in the first days of January 2007. Birth statistics of the German Federal Statistical Office counts all births taking place and furthermore give information on socioeconomic characteristics of the childbearing mother and the father of the newborn. In addition to the day of birth, we have information on the place of birth, and mothers' and fathers' characteristics such as citizenship, religion, age, and marital status. We furthermore know the baby's sex and whether birth was given to one or more babies. It is on these microdata that we base our analysis. Note, however, that while the data is rich in many respects, we do not have information on people's income, nor do we have information on the mode of delivery, i.e., whether the baby was born with a Cesarean section.

As shown before, incentives arising from the policy change for women with an employment history before pregnancy were different from incentives for women without an employment history. Thus, on top of the before/after comparison, we should also be able to see different birth counts comparing the two groups. The dataset allows us to differentiate along women's self-reported employment status. This information is not on the employment history of the last 12 months but information related to the time immediately before birth was given. As argued before, however, there is a large correspondence between reported employment at time of birth and an employment history during the preceding 12 months.

There is a strong weekday pattern with birth counts being lower on Saturdays and Sundays as compared to the other weekdays. For that reason, we ran regressions of births by day on weekday indicators. Figures 2 and 3 show

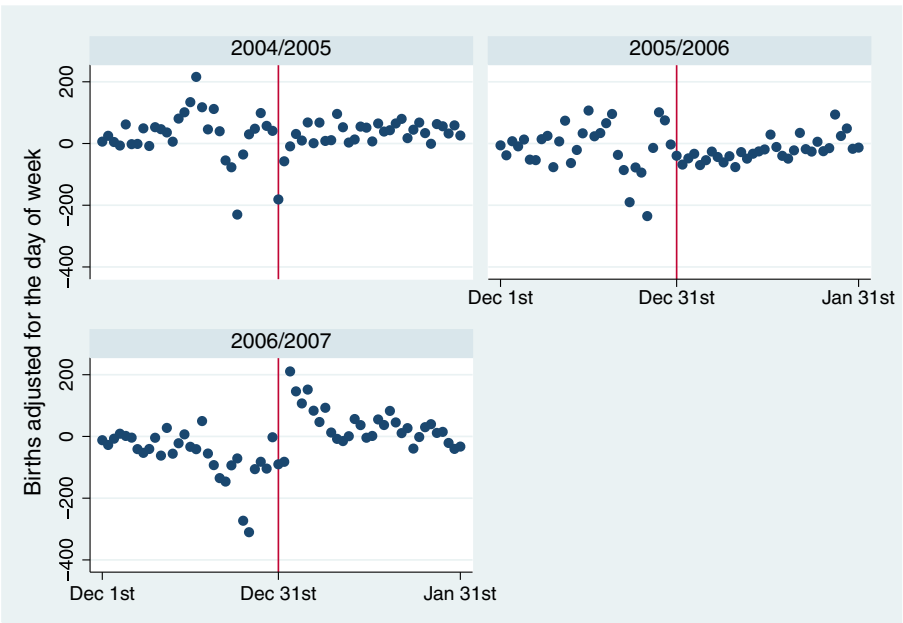

Fig. 2 Residuals of births by day and year, mother employed

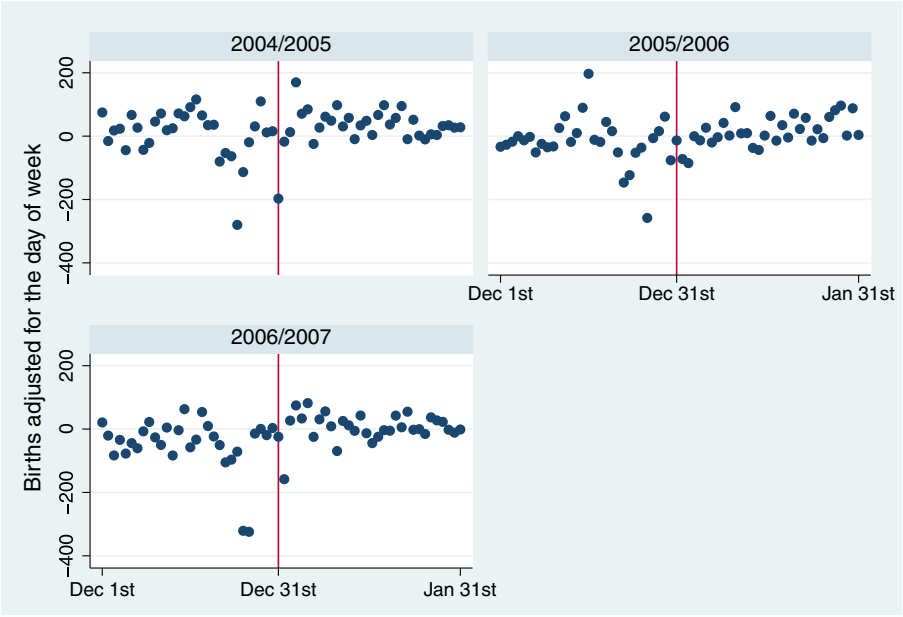

Fig. 3 Residuals of births by day and year, mother not employed

births adjusted for the day of the week for employed and not employed women, respectively, for all days in December and January in the years 2006/2007, 2005/2006 and 2004/2005.

Around the policy change, we observe a remarkable increase in births in the first days of 2007. No such pattern is observable for the preceding two New Year holidays. Detecting an effect is less straightforward for the sample of not employed women. Perhaps, there is a small change in birth counts around the policy change, again with no dips and peaks for the comparison years.

The data at hand allow us to disentangle the shares of births before and after the turn of the year by various socioeconomic characteristics. For this purpose (and the regression analysis following later on), we define a time window of 7 days before and after the turn of the year creating an indicator variable for each birth that took place within this fortnight:

$$\text{birth} = \begin{cases} 0, \text{ if birth given in 7 days } \textit{ending} \text{ the Old Year} \\ 1, \text{ if birth given in 7 days } \textit{beginning} \text{ the New Year} \end{cases} \quad (1)$$

The choice of a fortnight day window is driven by the consideration that for biological reasons, it will be difficult for a woman to postpone birth longer than a week. Later on, in our robustness analysis, we vary the time window. We also reestimate our models applying a sliding fortnight window over an 8-week period in order to evaluate how unsuccessful shifts and potential capacity constraints at hospitals may impact on the size of the effect of the shifting of births arising from the change in the economic incentives.

Table 3 Shares of births in the beginning of the New Year by employment status, marital status, and age

	2004/2005	2005/2006	2006/2007	Treatment effects		"Placebo" effects
				Difference 2006/2007, 2004/2005	Difference 2006/2007, 2005/2006	Difference 2005/2006, 2004/2005
Mother						
Employed	50.2	49.4	57.0	6.8	7.6	−0.8
Not employed	51.8	50.6	53.0	1.2	2.4	−1.2
Difference	−1.6	−1.2	4.0	5.6	5.2	0.4
Mother						
Married	50.7	49.6	54.8	4.1	5.2	−1.1
Not married	51.9	50.9	55.3	3.4	4.4	−1.0
Difference	−1.2	−1.3	−0.5	0.7	0.8	−0.1
Age mother						
Above median age	50.5	50.1	55.6	5.1	5.5	−0.4
Below median age	51.7	49.9	54.1	2.4	4.2	−1.8
Difference	−1.2	0.2	1.5	2.7	1.3	1.4
Age father						
Above median age	50.7	49.5	56.2	5.5	6.7	−1.2
Below median age	51.4	50.6	53.6	2.2	3.0	−0.8
Difference	−0.7	−1.1	2.6	3.3	3.7	−0.4

Table 3 reports the shares of total births during the fortnights occurring in the beginning of the New Year. This is done for 2006/2007, when the policy change occurred, and for the two preceding comparison years.[15]

Looking into the 14-day window defined around the New Year holiday in 2004/2005, we find that a share of 50.2% of employed women gave birth in the beginning of the year 2005. Among the not employed women, 51.8% of those who gave birth within this fortnight did so during the first days of the New Year. The share for employed women was, therefore, 1.6 percentage points lower than the share for not employed women in 2004/2005. The numbers for the following year are as follows: The share of births in the New Year given by employed women is 49.4%, while the corresponding share for not employed women is 50.6%. The share for employed women is, therefore, 1.2 percentage points lower than the share for not employed women. This is not very different compared to the change for the not employed women to the previous year. Still, the variation over the years may arise as the traditional weekday pattern that we observe during the year does not apply during the holiday season around Christmas and New Year each year with very few nonholiday

[15]No standard errors are reported as they directly follow from the means given that we have dummy variables.

weekdays. Applying the third difference in our more advanced specification later on exactly addresses this issue.[16]

For the year in which the policy change occurred, the share of employed women giving birth after the turn of the year increases to 57.0%. Compared to the previous 2 years, there is also a slight increase for the share of births given by not employed women up to 53.0%. The share for employed women now is 4.0 percentage points higher than the share for not employed women. This difference is 5.6 percentage points higher than the corresponding difference in 2004/2005 and 5.2 percentage points higher than the corresponding difference in 2005/2006.

The total number of births during the fortnight around the turn of the year 2006/2007 given by employed women was 11,580. This suggests that slightly more than 600 births were shifted from the end of 2006 to the beginning of 2007.

Note that if we use the two control years to construct a "placebo" experiment, the difference shrinks to 0.4 percentage points. This further strengthens the policy change as a sound treatment along the employment dimension of the childbearing women.

We can also look at the share of births in the beginning of the New Year within these 14-day windows along other characteristics. The share of births in the beginning of the New Year for married women was 54.8% during the year of policy change. The corresponding share for not married women was 55.3%. The difference between married and not married women was also small during the comparison years.

The picture is somewhat different if we consider the age of the mother and the age of the father. For women older or equal to the median age, the share of birth in the beginning of the New Year of the policy change is 55.6% while the corresponding share for women younger than the average is 54.1%. The difference between the year of the policy change and the comparison years is larger for older women than for younger women.

A similar age pattern emerges for the fathers. Comparing the difference between old and young fathers during the year of policy change and the comparison years reveals that the birth share of old fathers is 2.2 and 3.0 percentage points higher, respectively.

The dependence of the share of birth on these demographic and other covariates[17] suggests that it might be important to control them when studying the impact of the policy change. This is also the objective of the following section.

[16] A referee suggested that the differences between employed and not employed women might be driven by differing access to health care. But this occurs unlikely to us given the egalitarian German health-care system under which 85% are covered by the public health insurance (Hajen et al. 2010) which provides equal service by law.

[17] Additional descriptive statistics is provided in the Appendix.

5.2 Regression analysis

We estimate a probit model with the dependent variable being *birth* as defined above. The full model as given in Table 4 in column 5 writes

$$P(\text{birth} = 1|\mathbf{x}) = G(\beta_0 + \beta_1 \cdot d_{0506} + \beta_2 \cdot d_{0607} + \beta_3 \cdot \text{emp} + \beta_4 \cdot \text{emp} \cdot d_{0607} + \boldsymbol{\gamma}\mathbf{z}), \quad (2)$$

where birth was defined in Eq. 1; G is the standard normal cumulative distribution function; d_* is an indicator for the pairs of years; emp is an employment indicator; $\mathbf{z}$ is a vector holding the control variables which are state indicators, community indicators, mother's age, citizenship, religion, and marital status, a multiple birth indicator, child's sex, and father's age, citizenship, and religion; and $\boldsymbol{\gamma}$ as the corresponding vector of parameters.

Columns 1 to 5 report the estimation results when we build up the model step by step. Our results are based on up to 74,000 observations which are births given in the fortnight window around the turns of the years 2004/2005 and 2005/2006, and the New Year Eve of the policy change 2006/2007.

The estimated year effects are such that women were less likely to give birth in the beginning of the New Year during the turn of the year 2005/2006 compared to the previous turn of the year. The estimated year effects for 2006/2007 are, on the other hand, positive although not significantly so when adding more controls. Moreover, employed women are less likely than not

Table 4 Giving birth in the beginning of the New Year rather than in the end of the Old Year, probit models

	(1)	(2)	(3)	(4)	(5)
The New Year holiday					
2004/2005, reference					
2005/2006	−0.026*	−0.026*	−0.025*	−0.034**	−0.033**
	(0.011)	(0.011)	(0.011)	(0.012)	(0.012)
2006/2007	0.033*	0.033*	0.013	0.016	0.017
	(0.015)	(0.015)	(0.015)	(0.016)	(0.016)
Employed	−0.034**	−0.032**	−0.034**	−0.030*	−0.027*
	(0.011)	(0.011)	(0.012)	(0.013)	(0.013)
Not employed, reference					
Interaction	0.136**	0.135**	0.139**	0.141**	0.143**
Employed 2006/2007	(0.020)	(0.020)	(0.020)	(0.021)	(0.021)
Control variables					
State	No	Yes	Yes	Yes	Yes
Community	No	No	Yes	Yes	Yes
Mother and baby characteristics	No	No	No	Yes	Yes
Father characteristics	No	No	No	No	Yes
Number of observations	74,012	74,012	73,686	67,110	66,319
Pseudo R^2	0.0018	0.0020	0.0067	0.0079	0.0081

Standard errors are within parentheses. All regressions use a dependent variable indicating whether a birth took place 7 days ending the Old Year or beginning the New Year as defined earlier on

*$p < 0.05$; **$p < 0.01$ (statistical significance)

employed women to give birth in the beginning of the New Year rather than at the end of the Old Year.

The key variable of interest is the interaction indicator for employed women giving birth during the turn of the year 2006/2007. The estimated coefficients for this variable are positive and strongly significant in all the specifications.

Columns 2 to 5 in Table 4 show the estimates as we step by step include additional controls finally arriving at the model specified in Eq. 2. In the model shown in column 2, we added dummies for the 16 states of Germany. In a second step, we added dummy variables for the more than 300 communities taking care of community-related fixed effects. Finally, we control mothers' and fathers' characteristics which are citizenship, religion, age, and marital status; baby's sex, and whether more than one baby was given birth to. Adding the controls hardly changes our parameter estimate on the interaction effect. In all specifications, significance is at $p < 0.01$.

The number of observations drops slightly as we include community dummies because births in some communities are so few that the community dummies fully explain the pre- or post-turn of the year births. Furthermore, when the birth is registered with the local authority, age, religion, and citizenship of the father are not a mandatory piece of information when the couple is not married. This explains the additional slight drop in observations as we include fathers' characteristics.

The marginal effect for the model in column 1 of Table 4 is highly significant at 5.4 percentage points. We chose to calculate the marginal effect based on the most parsimonious specification as the more elaborate models did not indicate major changes in the size of the estimated parameters, and by doing so, we could also avoid making further assumptions on the other variables that would have had to be included into the calculation of the marginal effect. Generally, as the estimated coefficient for the interaction term does not give the complete marginal effect of being employed in 2006/2007 for a nonlinear model, we calculated it according to Ai and Norton (2003) and Norton et al. (2004) which writes

$$\frac{\Delta^2 F(u)}{\Delta d_{0607} \Delta emp} = G(\beta_0 + \beta_2 + \beta_3 + \beta_4) - G(\beta_0 + \beta_2) - G(\beta_0 + \beta_3) + G(\beta_0), \tag{3}$$

where $F(u)$ is the probability that birth $= 1$ as a nonlinear function of the interacted variables and the intercept (model 1 in Table 4).

5.3 Shifting as a chain reaction

Given the biological constraints, there is little reason to believe that women actually shifted births for more than 7 days. However, what might have occurred is that employed women with due dates earlier in December were (unsuccessfully) trying to postpone birth to the New Year. Consider that women with babies due in the third week of December wanted to postpone

Table 5 Estimates using a sliding fortnight window

Weeks:	December			Turn of the year	January		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	−3 and −2	−2 and −1	−1 and 0	0 and 1	1 and 2	2 and 3	3 and 4
Interaction employed 2006/2007	−0.009	−0.046*	−0.004	0.136**	−0.046*	0.006	−0.043*
Marginal effect	−0.004	−0.018*	−0.002	0.054**	−0.018*	0.002	−0.017*
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)
Total number of obs. in estimation	76,448	75,165	71,619	74,012	76,872	77,260	77,595

Results refer to same specification as in model 1 in Table 4. Each column is the result of a separate regression. Within parentheses are the standard errors for the marginal effect.
*$p < 0.05$; **$p < 0.01$ (statistical significance)

birth to the New Year but actually gave birth in the last week of December. In this case, the dip in births during the last days in December would not be so large as when these women would not have (unsuccessfully) tried to shift birth. Also, if capacity constraints are binding, leading to a shift of some births that would have taken place in the first days of January to a later time, the peak that we expect at the beginning of January would be lower, again leading to a lower estimated effect.[18]

In order to investigate the impact of the reform along these lines, we replicated the type of regression analysis by applying a sliding fortnight day window. The results as shown in Table 5 stem from the same regression already stated in Eq. 2. The only difference now is that the 14-day window is not centered around New Year Eve but moves from the first and second week of December to the second and third week of December and so forth.

Using the marginal effects as shown in Table 5, one can come up with an evolution of the total numbers of shifted births during these 8 weeks applying some additional definitions and assumptions. Let us define the ratio of actual births during a week t to the sum of actual births week t and week $t-1$ as

$$b_t = \frac{B_t}{B_t + B_{t-1}} \tag{4}$$

where B_t is the actual number of births in week t. The accounting identity suggests that the number of births due week t, D_t, and the number of births shifted from the previous week, S_{t-1}, are identical to the actual number of births and the number of births shifted to the next week

$$B_t + S_t \equiv D_t + S_{t-1}. \tag{5}$$

[18]Readers might be concerned about an analysis of delays of birth by 3 weeks given that births may not be shifted by more than a few days. While we, in principle, agree on the impossibility to postpone birth by more than a few days, one has to take into account uncertainty related to conception and gestation periods. These uncertainties combine to uncertainty regarding the natural delivery date and warrant, from our point of view, such an analysis.

In a steady state, we would expect that there would be no shifting. If, in addition, the number of births due is constant, we will have $b_t = 0.5$.

Table 6 contains some illustrative calculations. The calculations are made assuming a steady state before and after the weeks in the table and that the number of birth due is the same for all weeks, $D_t = \bar{D}$. The first row in the table reports the birth ratios implied by the estimated marginal effects from Table 5. From this, we can calculate the number of actual births and the number of shifted births, see the second and the third rows in the table.

We need some additional assumptions if we want to calculate how births due on a certain week are distributed between births during the week and births the following weeks. On one extreme, we can assume the shifting is only possible to the following week. Then, a measure of the share of births shifted would write

$$a_t^{\max} = \frac{S_t}{\bar{D}}. \tag{6}$$

A consequence of this assumption is that it is only possible to successfully shift births due the last week of December to the first week in January.

On the other extreme, we can assume that the possibilities to shift births are the same regardless of when the birth originally was due and write:

$$a_t^{\min} = \frac{S_t}{\bar{D} + S_{t-1}}. \tag{7}$$

We arrive at a share of 17–19% of the births due the last week of December 2006 being shifted to the first week of January 2007. This estimate considerably differs from the 5–6% of births shifted according to our calculations in Table 6. In our interpretation, the difference arises because there is an unduely high number of births in the last week of December driven by unsuccessful shifts

Table 6 Shifted births using a sliding fortnight window

	December				January			
t	−3	−2	−1	0	1	2	3	4
b_t	0.5	0.497	0.482	0.498	0.554	0.482	0.502	0.483
$B_t/\bar{D}$	1	0.986	0.916	0.910	1.131	1.051	1.061	0.991
$S_t/\bar{D}$	0	0.014	0.098	0.188	0.058	0.007	−0.054	−0.045
Shifting only possible to the following week:								
$a_t^{\max}$	0	0.014	0.098	0.188	0.058	0.007	−0.054	−0.045
Births due week t, percent								
Not shifted	100	98.6	90.2	81.2				
Unsuccessfully shifted	0	1.4	9.8	–				
Successfully shifted	–	–	–	18.8				
The same possibilities to shift regardless of when the birth was due:								
$a_t^{\min}$	0	0.014	0.097	0.171	0.049	0.007	−0.054	−0.047
Births due week t, percent								
Not shifted	100	98.6	90.3	82.9				
Unsuccessfully shifted	0	1.4	8.0	–				
Successfully shifted	0	0.0	1.7	17.1				

Table 7 Estimated interaction effects for different window sizes

Window size, ± days	1	2	3	4	5	6	7
Interaction employed 2006/2007	0.143*	0.166**	0.174**	0.175**	0.169**	0.156**	0.136**
Marginal effect	0.057*	0.065**	0.069**	0.069**	0.067**	0.062**	0.054**
	(0.022)	(0.015)	(0.012)	(0.010)	(0.009)	(0.008)	(0.008)
Total number of obs. in estimation	8,742	19,475	31,555	43,591	55,498	65,017	74,012

Results refer to same specification as in model 1 in Table 4. Within parentheses are the standard errors for the marginal effect

*$p < 0.05$; **$p < 0.01$ (statistical significance)

from earlier weeks and relatively lower births in the first week of January possibly driven by crowding out of births due to capacity constraints at hospitals.

5.4 Changing the window size

In addition to running the regression with a large selection of control variables and with sliding fortnight windows, we have also checked for the robustness of our estimates by changing the window size centered around the turns of the years. Table 7 shows the parameter estimates of the interaction effect in the probit regressions as in column 1 of Table 4. We vary the window size from 1 day before and after the turn of the year to 2 days before and after the turn of the year, and so on. Window size does not affect our results. The parameters stay in a fairly small range of what we previously estimated for the fortnight window. This also suggests that our model—due to the third difference—is robust against day pattern effects potentially driven by varying degrees of hospital activity that may only inadequately be captured by indicator variables. Furthermore, all estimates on the interaction effect are highly significant again.

6 Conclusions

Economic theory suggests that economic incentives matter for people's decisions. While this may be self-evident in many areas of daily life, we showed that even in less apparent areas such as childbearing monetary incentives do play a role. Overall, our estimates suggest that a significant share of employed women was giving birth in the New Year instead of the Old Year. Based on a fortnight window around the policy change, which for biological reasons seems to be the appropriate time frame within which to expect shifting to take place, we estimate a marginal effect of approximately five percentage points. This result stems from employing a difference-in-difference-in-difference estimator as, contrary to existing studies on the timing of births, the data at hand allow us to make causal inference on whether the reform gave incentives for women to postpone birth along their employment status.

We also provide evidence that some women with due dates earlier in December were trying to postpone births unsuccessfully. There might also have been capacity constraints at hospitals in the first days of January 2007 which led to a shift of some births to later days that otherwise would have taken place earlier. As we take into account these chaining effects, the estimated magnitude as compared to only looking into a fortnight window around the policy change increases by a little more than a factor of three.

These findings related to biased estimates stemming from looking into time windows of different size may appear at first glance specific to the type of policy change that we analyze. However, we believe that they are more general in the sense that a quantitative analysis of changes in incentives on many other outcome variables might be flawed by such effects. One may think, for example, about the purchase of durable goods as a response to a change in tax or transfer policies. People may try to postpone the purchase until the key date but fail to do so as the good needs to be substituted earlier. Similarly, as households postpone the purchase after the key date, these goods may have gone out of stock which again would flaw estimates if only data were analyzed close to the policy change.

Due to the number of births shifted and the change in transfers involved, budgetary consequences for the government are less important in the policy change which we study. Still, there is a more general lesson to be learned. Changes in government policies aimed at the longer run—which was the case in the parental benefit reform which targeted relatively low fertility in Germany—may result in short-run distortions. These distortions, in our case, may have unintended consequences for the mothers' and infants' health. But we cannot say anything on this, nor did we come up with an answer to whether fertility actually increased as a result of the policy change.

Acknowledgements This work started when Neugart was affiliated with the Wissenschaftszentrum Berlin für Sozialforschung which generously supported data purchase. Some of the work was done when Ohlsson enjoyed the hospitality of LEM, Université Panthéon-Assas, Paris II. This project was financially supported by a grant of the Central Research Committee of the Free University of Bozen/Bolzano. We would also like to thank the Forschungsdatenzentrum der Länder im Amt für Statistik Berlin-Brandenburg, in particular Matthias Klumpe, for their support in handling the data. Helpful comments and suggestions from Mikael Elinder, Jochen Kluve, Håkan Selin, and seminar participants at Bielefeld, London, and Växjö are gratefully acknowledged.

Appendix

- Before (*Erziehungsgeld*):
  - In *300 euro option*, no transfers were paid within the first 6 months if the yearly income of couples was above 30,000 euros and of singles above 23,000 euros; from the seventh month onwards, transfers were paid at a reduced amount if the income was above 16,500 for couples or 13,500 singles. The reduced amount was calculated as 300 euros minus 5.2% of the income exceeding the income limits.

Table 8 Descriptive statistics

	2004/2005		2005/2006		2006/2007	
	Last week	First week	Last week	First week	Last week	First week
Share of mothers employed	48.02	46.45	47.47	46.32	45.50	49.57
Share of newborn boys	51.40	50.96	51.17	51.26	52.42	50.78
Share of multiple births	3.42	3.22	3.66	3.31	2.94	3.34
Share of married couples	70.98	70.01	70.68	69.56	69.07	68.59
Religion of father (three largest shares)						
Protestant	25.39	24.91	24.12	23.40	23.21	24.48
Catholic	27.72	27.55	28.04	28.31	26.23	27.57
No denomination	27.36	27.99	28.56	28.15	28.64	28.50
Religion of mother (three largest shares)						
Protestant	28.23	27.69	27.58	27.47	26.06	27.70
Catholic	29.03	29.21	29.14	29.50	27.97	29.26
No denomination	25.42	25.16	26.01	25.17	26.03	25.23
Share of newborns with German citizenship	95.28	95.16	95.45	95.28	95.39	95.64
Share of mothers with German citizenship	82.65	80.91	81.71	80.98	80.87	82.30
Mean age of mothers (years)	30.0	29.9	30.2	30.1	30.0	30.4
Mean age of fathers (years)	33.8	33.7	34.0	34.0	34.0	34.2

  - In *450 euro option*, no transfers were paid within the first 6 months if the yearly income of couples was above 30,000 euros and of singles above 23,000 euros; from the seventh month onwards, transfers were paid at a reduced amount if the income was above 16,500 for couples or 13,500 singles. The reduced amount was calculated as 300 euros minus 7.2% of the income exceeding the income limits.
  - In both options, income limits increased by 3,140 euros for every additional child living in the household.

- After (*Elterngeld*):
  Transfer is 67% of the average net income of the 12 months preceding the birth of the baby up to a maximum of 1,800 euros. A minimum payment of 300 euros irrespective of the employment history is guaranteed.

The dataset comprises 16 states and 451 communities (Table 8).

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