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

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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 make use of a natural experiment when the German government changed its parental benefit system January 1, 2007. The policy change strongly increased economic incentives for women to postpone delivery to the new year provided that they were employed. The incentives for women not employed were not the same, they could gain slightly from giving birth before the policy change. 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 and became subject to the new and more generous parental benefit system. We estimate the quantitative impact to correspond to a 5–6 percentage points increased probability to give birth the first seven days of 2007 rather than the last seven days of 2006 for employed women.

Keywords: timing of births, economic incentives, parental benefits, policy reform
JEL-Classification: J130, J180; H530, D190

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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 women giving birth one minute 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 in order to investigate whether monetary incentives matter also in less apparent areas of daily life such as the timing of births.

Our analysis draws on data covering *all* births occurring in Germany in the seven days before and after the change of the parental benefit system in 2006/07 and the two weeks centered around the turn of the year of the previous two years for comparison. Overall, our sample consists of up to 74,000 observations including in addition to the date of birth various socioeconomic characteristics of the child giving 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 2006/07. 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 two 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, that allows us to substantially differentiate from previous studies on 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 dummies. 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 kids 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 non-treatment years in the same way. And we believe that this is the case.

The change in the parental benefit system received extensive press coverage in Germany and even abroad. 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 in order to collect the money.

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 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 percent 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 midwifes and doctors that stress, sport, or sex may spark contractions – somewhat a point of no return for a women who wanted to shift birth to the new year. Some even claimed that: "In the internet drugs are traded" (Margit Reitmayer, midwife, *Süddeutsche Zeitung, Regionalausgabe*, December 30, 2006) that may help postponing the delivery.[1]

There are a couple of previous studies of the timing of births.[2] Dickert-Conlin

[1] All quotes were translated by the authors.

[2] 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, 2009).

and Chandra (1999) study the impact on the timing of births of tax changes in the United States 1979–1993. The tax changes were such that there were incentives to hasten births. All women faced the same incentives. The authors find that almost 14 percent 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.

Gans and Leigh (2009) use Australian data on the introduction and the increase of a Baby Bonus in the years 2004 and 2006, respectively. These allowances created incentives for all women to delay births. The authors find that 16 percent of the births were shifted in 2004 while 9 percent 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 percent 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 kids arising from the shifting of births. Most importantly, however, Tamm (2009) does not use the employment status of women as a causing factor for the shifting of births. Due to the nature of the policy change we do, as compared to this and the other existing studies, not only conduct a before/after comparison against previous reference years, but 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 are also relevant in less apparent areas of life.

Our main result is that we estimate that the probability of employed women to give birth after the reform was introduced increased by 5–6 percentage points. This corresponds to more than 600 births shifted from the last seven days of 2006 to the first seven days of 2007.

The estimated impact reported here is lower than what has been found in previous studies of the timing of births in other countries as well as in the study by Tamm (2009) based on the German policy reform. This might be because we, as opposed to previous studies, also can exploit that the reform did not affect all women in the same way.

We proceed by a description of the policy change in the following section. Section 3 reports on the timing of births. In Section 4 we present our findings.

Table 1: The policy change

	Before Educational benefit (*Erziehungsgeld*)	*After* Parental benefit (*Elterngeld*)
Monthly benefit, EUR	300 (450), income limits applied	67 percent 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)

Finally, we conclude in Section 5.

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*). Kids born up until midnight of the New Year's Eve were still subject to the educational benefit. However, any kid born on January 1, 2007 or later would make parents eligible for the parental benefit. Table 1 summarizes the core of the legislative changes.[3]

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 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 kids.[4] With the new law two major changes were introduced: a) now transfers could be in a range between EUR 300 and EUR 1,800 per month, b) transfers were

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

[4]For example, for a couple with one kid the annual net income earned in the previous year to the birth had to be lower than EUR 30,000 to qualify for the educational benefit for the first six months after the kid was born.

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 percent of the average net monthly income of the 12 months before the delivery of the kid(s). Thus, depending on the previous income, parents may get up to EUR 1,800 per month for a duration of 12 months under the parental 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. There is the possibility to extend benefit duration by another 2 months if there is an income loss associated with a leave, for example, by the partner.

For a parent without an employment history in the 12 months before the delivery of the kid who would have gone for the EUR 450 Euros 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 that 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 $\approx$ 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 one year after giving birth[5], under the new regime the same parent is eligible for up to EUR 21,600, resulting in a gain of EUR 19,867.[6]

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.[7] After several com-

[5]This assumes a net yearly income of 32,239 in which case no transfers would have been paid for the first six months under the old regime and from month 7 to 12 transfers would have been cut by 7.2% in relation to the income exceeding a threshold of 30,000 Euros.

[6]Two examples taken from the household income records of the *Statistische Bundesamt* may help bringing 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 kids. 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 percent of transfers is EUR 1,288.

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

mittee hearings and statements received by the second chamber the proposal by the *Bundesregierung* followed end of August 2006.[8] Finally, the new law "Gesetz zur Einführung des Elterngeldes" came into effect on December 5, 2006.[9]

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 women. 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 and job search durations, we believe that this is a minor issue.

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 kids born in Germany in the first quarter of 2007 (164,683) with the number of households that received parental benefits (163,372) 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).

Although the new system was deliberately designed as a parental benefit system, 84 percent of the months of child care provided within families under the new benefit system was done by mothers (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2008). This indicates that it is the mother's employment status, and not the father's employment status, driving the incentives to time the birth.

3 Timing of births

Economic theory suggests that economic incentives matters 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, there are means of medically manipulating the timing of the delivery of a child. These are primarily Cesarean sections and the inducement of labor. Cesarean sections are invasive surgeries requiring an abdominal incision. While Cesarean sections are made if the conditions of the mother or the child prevent a vaginal delivery, women may also decide for a Cesarean section if a natural delivery

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

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

would have been possible. When a women has taken the decision not to naturally give birth, it is, within limits, up to the obstetrician and the women to arrange for a date for the Cesarean section. Very often the day of delivery is driven by the organizational and capacity constraints of the hospital and may be shifted for a few days.

An inducement of labor is a stimulus to the uterus sparking contractions in order to achieve the delivery before the natural onset of labor. Similarly to Cesarean sections there may be medical reasons for the inducement of labor but it allows, again within limits, the shifting of births.

Besides medical considerations and rule of thumb suggestions given by midwifes such that physical or mental stress may lead to the onset of labor, perhaps the most convincing argument for the feasibility of the timing of births comes from Figure 1. It shows average births by weekday for all December and January months from December 2004 to January 2007. On Saturdays and Sundays overall births are lower than at any other day of the week. This is in line with previous findings on weekday effects, see Chandra et al. (2004) and Gans and Leigh (2009).

While we lack 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 we intend to do.

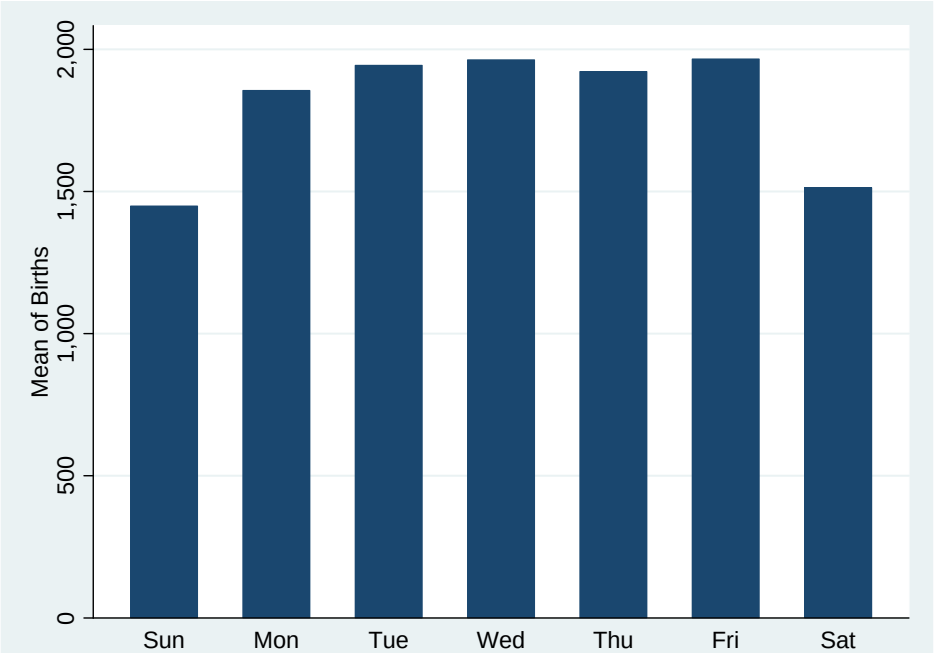
4 Empirical evidence

4.1 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 count all births taking place and furthermore give information on socio-economic characteristics of the childbearing mother and the father of the newborn. It is on these micro data that we base our analysis.

As shown before, incentives arising from the policy change for women with an

Figure 1: Average number of births by weekday for all December and January months, Dec 2004 – Jan 2007



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 data set allows us to differentiate along women's self-reported employment status. This is not information on the employment history of the last 12 months but information related to the time immediately before birth was given. Therefore, one might be concerned to which extent the self-reported employment status before birth, on which we draw, corresponds with the 12 month employment history which is constitutional for the incentives to shift birth.

Accompanying sources of information 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 percentage 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, we are confronted with a homogenous study group. These 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. Very likely some of 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. Generally, the incentives for the comparison group as a whole are unclear.

However, looking at approved requests for parental benefit payments for women along employment history shows strikingly different applied for and approved benefits (Table 2). For women with an employment history approximately more than 80 percent received more than EUR 500 in transfers. Contrarily, 63 percent of the women without an employment history received EUR 300. The relatively large share in the income class between EUR 300–500 is mainly driven by a parental benefit regulation ascribing women who already have a kid an additional EUR 75. Note also, that no matter how high transfers are or whether women have an employment history or not, average applied for and approved duration of payments is almost 12 months. Overall, Table 2 suggests that incentives for women to shift or not to shift birth indeed differed along the employment dimension.

Looking into the birth statistics along these lines reveals the following picture

Table 2: Approved requests for parental benefit payments for women, January 2007 – March 2008

	with employment history		without employment history	
monthly benefit, EUR	share, percent	ave. dur., months	share, percent	ave. dur., months
300	4.5	11.68	63.1	11.49
301–500	15.7	11.72	35.5	11.56
501–750	32.1	11.77	1.3	11.40
751–1,000	21.8	11.73	-	-
1,001–1,250	12.1	11.61	-	-
1,251–1,500	5.5	11.43	-	-
1,501–1,800	4.5	11.35	-	-
1,801–more	3.7	10.91	-	-
total	100	11.66	100	11.51

Source: Statistisches Bundesamt, Statistik zum Elterngeld, Sonderauswertung zur Erwerbsbeteiligung vor der Geburt, 2008.

as shown in Figures 2 and 3. In both figures we plotted not only birth counts for all days in December and January in the years 2006/2007 but also of the 31-day time windows around the two previous turns of year.

As already noticed before there is a strong weekday pattern with birth counts being lower on Saturdays and Sundays as compared to the other weekdays. Around the policy change we observe a remarkable drop in births given by employed women in the last days of December. Furthermore, there is an increase in births in the first days of 2007. The weekend births given by employed women after the policy change lift up to a level comparable to the weekday births the days before the policy change occurred. 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 allows 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 for each birth that took place within

Figure 2: Number of births by day and year, mother employed

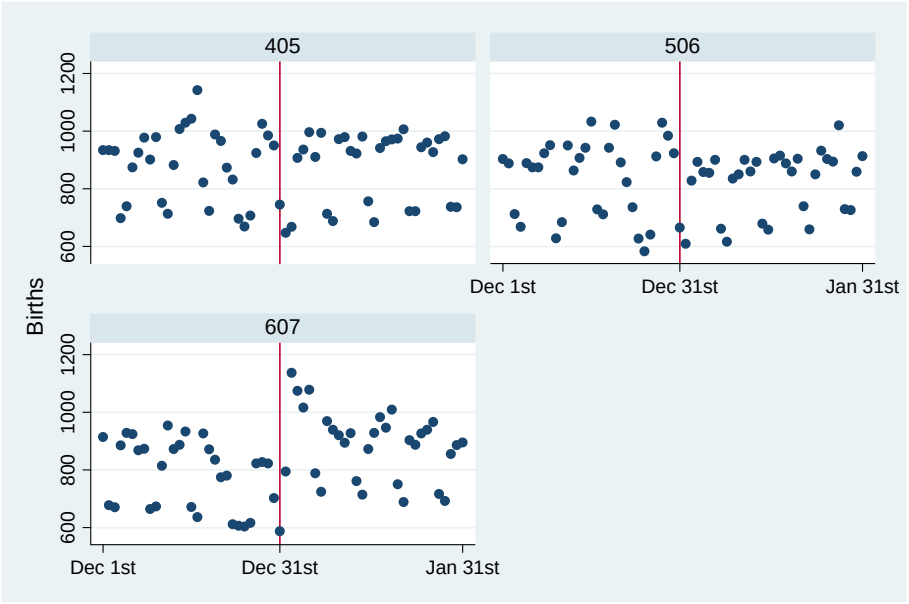
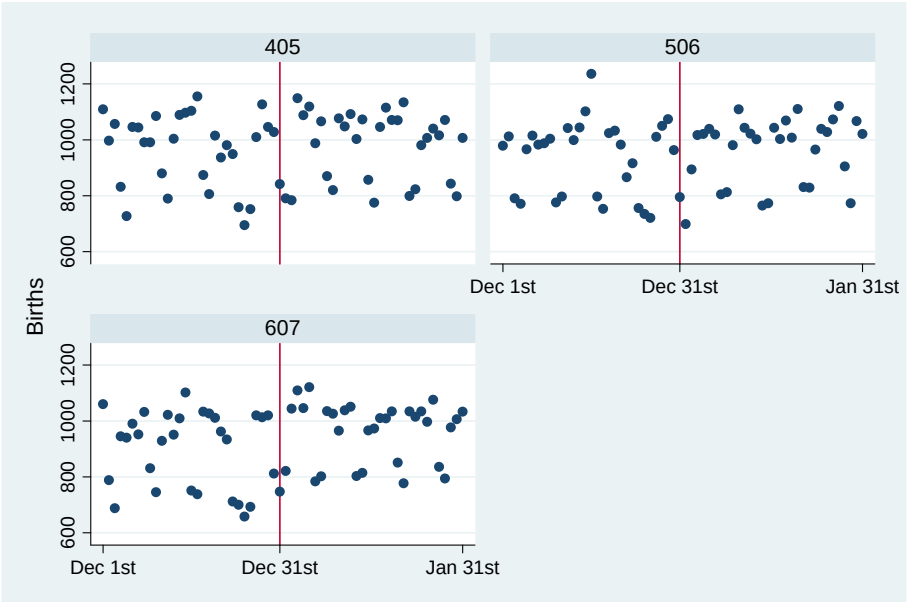


Figure 3: Number of births by day and year, mother not employed



these 14 days a dummy variable:

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

The choice of a 7 day window is driven by the consideration that for biological reasons it will hardly be feasible for a women to postpone birth longer than a few days. Later on, in our robustness analysis we vary the time window and re-estimate our models. There we find that after six days the number of births shifted levels off which suggests that the size of the time window is somewhat chosen appropriately for the analysis.

Table 3 report the shares of total births during the fortnights occurring in the beginning of the New Year. This is done for 2006/07, when the policy change occurred, and for the two preceding comparison years.

Looking into the 14-day window defined around the New Year Holiday 2004/05, we find that a share of 50.2 percent of employed women gave birth in the beginning of New Year Eve. Among the not employed women, 51.8 percent of those who gave birth within this fortnight did so in the beginning of the New Year. The share for employed women was, therefore, 1.6 percentage points lower than the share for not employed women in 2004/05.

The numbers for the following year are as follows: The share of births in the New Year given by employed women is 49.4 percent, while the corresponding share for not employed women is 50.6 percent. 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 previous year.

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 percent. Compared to the previous two years there is also a slight increase for the share of births given by not employed women up to 53.0 percent. 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 2004/05 and 5.2 percentage points higher than the corresponding difference in 2005/06. The total number of births during the fortnight around the turn of the year 2006/07 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

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

				treatment effects		"placebo" effects
	2004/05	2005/06	2006/07	difference 2006/07, 2004/05	difference 2006/07, 2005/06	difference 2005/06, 2004/05
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

change as a sound treatment along the employment dimension of the child bearing 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 percent during the year of policy change. The corresponding share for not married women was 55.3 percent. 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 Years of the policy change is 55.6 percent while the corresponding share for women younger than the average is 54.1 percent. 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.

This suggests that it might be important to control for demographic and other covariates when studying the impact of the policy change. This is also the objective of the following section.

4.2 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:

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

where G is the standard normal cumulative distribution function, d_* indicators for the pairs of years, emp an employment indicator, and $\mathbf{z}$ a vector holding the control variables which are state indicators, community indicators, mother's age, citizenship, religion, and marital status, a multiple births 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

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					
2004/05, reference					
2005/06	-0.026**	-0.026**	-0.025**	-0.034***	-0.033***
	(0.021)	(0.022)	(0.025)	(0.004)	(0.005)
2006/07	0.033**	0.033**	0.013	0.016	0.017
	(0.024)	(0.022)	(0.390)	(0.313)	(0.292)
employed	-0.034***	-0.032***	-0.034***	-0.030**	-0.028**
	(0.002)	(0.005)	(0.004)	(0.018)	(0.029)
not employed, reference					
interaction	0.136***	0.135***	0.139***	0.141***	0.143***
employed 2006/07	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
control variables included:					
state	No	Yes	Yes	Yes	Yes
community	No	No	Yes	Yes	Yes
mother and kid characteristics	No	No	No	Yes	Yes
father characteristics	No	No	No	No	Yes
number of observations	74,012	74,012	73,686	67,116	66,330
pseudo R^2	0.0018	0.0020	0.0067	0.0079	0.0081

Notes: p-values within parentheses.
*, **, and *** denote statistical significance at the 10, 5, and 1 percent level, respectively.

in the 14-day window around the New Year Eves in 2004/05, 2005/06 and the year of the policy change 2006/07.

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 2005/06 compared to the previous turn of the year. The estimated year effects for 2006/07 are, on the other hand, positive although not significantly so when adding more controls. Moreover, employed women are less likely than not 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 2006/07. 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 equation 1. 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 for mothers and fathers characteristics which are citizenship, religion, age, and marital status, respectively, and the kid's sex and whether more than one kid 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.

In addition to running the regression with a large selection of control variables we checked for robustness of our estimates by changing the window size centered around the turns of the years. Table 5 shows the parameter estimates of the interaction effect in the probit regressions as in column 1 of Table 4. We vary the windows size from one day before and after the turn of the year, two 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 potential day pattern effects potentially driven by varying degrees of hospital activity that may only inadequately be captured by dummy variables.

Table 5: Estimated interaction effects for different window sizes

window size, ± days:	1	2	3	4	5	6	7
interaction employed 2006/07	0.143 (0.012)	0.166 (0.000)	0.174 (0.000)	0.175 (0.000)	0.169 (0.000)	0.156 (0.000)	0.136 (0.000)
marginal effect	0.057	0.065	0.069	0.069	0.067	0.062	0.054
estimated shifted number of births given by employed women	78	210	352	482	592	635	626
total number of obs in estimation	8,742	19,475	31,555	43,591	55,498	65,017	74,012

Furthermore, all estimates on the interaction effect are highly significant again.

The estimated coefficient for the interaction term does not give the complete marginal effect of being employed 2006/07 as the estimated model is non-linear. Therefore, we also calculated the number of births shifted according to Ai and Norton (2003) and Norton et al. (2004) based on the marginal effect of two interacted dummy variables which writes:

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

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).

An estimated 78 employed women shifted giving birth from December 31, 2006 to January 1, 2007 because of the policy change. As we increase the window size the number of births shifted increases up to a window of six days before and after the turn of the year. At this point we have an estimated 635 births shifted. There is a slight decrease in estimated births shifted if we increase the window size by one additional day before and after the turn of the year. The leveling off might indicate the biological constraints for the timing of births.

5 Conclusions

On New Year Day 2007, a legislative change in the parental benefit system was enacted in Germany. This legislative change implied a generous increase in gov-

ernment benefits for parents with an employment history preceding the birth of a child. If a women managed to shift the birth into the New Year, benefits, at the extreme, increased by up to approximately EUR 20,000 for a one year leave.

We make use of this natural experiment to test whether this change in monetary incentives led women, who were already advanced in their pregnancy, to shift the birth into 2007. Overall, our estimates suggest that slightly more than 600 employed women were giving birth in the New Year instead of the Old Year.

Differently put, slightly more than 5 percent of the births given by employed women were shifted to the New Year. This magnitude is lower than the magnitudes reported in previous timing of birth studies. A possible reason might be that shifting was actually lower in Germany than in Australia and the United States. But it might also be because we in the German case can make a more precise estimation as we have two groups of women (employed and not employed) affected differently by the reform. This is not the case in the other studies.

Economic theory suggests that economic incentives matter for people's decision. While this may be self-evident in many areas of daily life we showed that even in less apparent areas such as child bearing monetary incentives do play a role. 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.

There is a more general lesson to be learned however. 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 may not only affect governments' budget but may also have unintended consequences. In our case, health considerations for the mothers and infants may have to be taken into account.

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