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ORIGINAL PAPER

Announcement effects of health policy reforms: evidence from the abolition of Austria's baby bonus

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Abstract We analyze the short-run fertility and health effects resulting from the early announcement of the abolition of the Austrian baby bonus in January 1997. The abolition of the benefit was publicly announced about 10 months in advance, creating the opportunity for prospective parents to (re-)schedule conceptions accordingly. We find robust evidence that, within the month before the abolition, about 8 % more children were born as a result of (re-)scheduling conceptions. At the same time, there is no evidence that mothers deliberately manipulated the date of birth through medical intervention. We also find a substantial and significant increase in the fraction of birth complications, but no evidence for any resulting adverse effects on newborns' health.

Keywords Baby bonus · Scheduling of conceptions · Timing of births · Policy announcement · Announcement effect · Birth complications · Medical intervention

JEL Classification H31 · J13

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Introduction

Forward looking behavior of individuals has potentially important implications for the implementation, as well as for the evaluation, of health policy reforms (and beyond). Indeed, if individuals are forward looking and if policy reforms are publicly announced in advance, individuals are likely to adapt their behavior even before the effective implementation of any given policy reform. As Malani and Reif [16, p. 2] put it, "anticipation is a reasonable diagnosis if individuals are forward looking, have access to information on future treatment, and there is a benefit to acting before the treatment is adopted". Clearly, policy makers need to be aware of potential announcement effects resulting from anticipatory behavior whenever they are planning to implement major health policy reforms and to announce them beforehand. It is also evident that anticipatory behavior should be factored in when evaluating specific health policy reforms. In fact, ignoring announcement effects may lead to misleading conclusions regarding the impact of the policy reform under study [5, 16]. Moreover, we may be especially concerned about potential negative health effects if individuals anticipate even small changes in financial incentives, but are unable to foresee all potential health effects resulting from specific behavioral changes. We believe that such a situation is especially common in health policy contexts where it is very difficult, even for experts, to plausibly assess all potential health effects resulting from any given change in health-related behavior.

Some of the most compelling empirical evidence on announcement effects of health policy reforms available comes from the recent experience of introducing baby bonuses in Australia and Germany, respectively. Indeed, a couple of recent empirical studies has convincingly shown that the introduction of such policy measures is usually associated with considerable behavioral responses in the short-run, potentially resulting in negative health effects for the mother and/or her newborn child.[1] In the case of Australia, the government publicly announced on May 11, 2004, that it would pay 3,000 $ (about 2,190 €) to each family of a newborn child born on or after July 1, 2004.[2] The introduction of the bonus was thus announced in advance of the effective policy change, creating an incentive to delay births. In their empirical evaluation of the policy change, Gans and Leigh [12] find that 6 % of the births (more than 1,000 births) expected to happen in the 28 days preceding the actual policy change were moved to July 1st 2004 or later to become eligible for the baby bonus.[3] They find that most of the effect is due to a corresponding timing of induction and cesarean section procedures. Consequently, they also find that children who were moved into the eligibility period were more likely to be of high birth weight.[4] Tamm [26] analyzes a similar reform in the system of family benefits in Germany, the introduction of parental leave benefits ("Elterngeld") as of January 1, 2007.[5] As in Australia, the announcement happened only a few weeks in advance, meaning that the policy change could only affect the timing, but not the number of births. Similarly to Gans and Leigh [12], Tamm finds that a substantial number of births were delayed and moved into the eligibility period for the new benefit system. Specifically, he concludes that almost 8 % of births (around 1,000 births) that could have been expected in the last week of December were shifted to the first week of January 2007. He also finds a slight increase in birth weight among the births most likely to have been shifted (i.e., January vs. December births).

In this paper we study the fertility effects, as well as the potential health consequences for both mother and newborn child, following the announcement of the abolition of the Austrian baby bonus as of January 1, 1997. The Austrian baby bonus amounted to a maximum of 1,090 € per child in 1996, the year before the abolition, and was paid conditional on medical examinations of both mother and newborn child. The unique feature of this policy change is that the elimination of the benefit was announced about 10 months prior to enactment, creating the potential for an announcement effect because prospective parents had both an incentive and the opportunity to move their baby plans forward. Although the response window in order to qualify for the birth benefit before its abolition was only limited to 3 weeks, the early announcement could have increased the number of babies born in the month prior to the policy change. On top of this, pregnant women with a due date close to the date of the policy change might have manipulated the exact day of birth by means of a medical intervention (i.e., cesarean section). In the second part of the analysis, we will explore whether the early announcement of the policy reform had any negative health effects for mothers and/or her newborn children.

The remainder of this paper is organized as follows. In section "The Austrian baby bonus" we provide some background information on the baby bonus in Austria. This is followed by a short discussion of the data source and some descriptives in section "Data and descriptives". We present our estimates of the fertility response following the announcement of the abolition of the baby bonus in section "The fertility response". Section "Taking risks for the bonus?" examines whether mothers (un)consciously take increased health risks for themselves and/or their babies when rescheduling the timing of conception or birth. Section "Conclusions" concludes.

The Austrian baby bonus

Institutional background

The Austrian baby bonus ("Geburtenbeihilfe") was first introduced on January 1, 1968, as an untaxed single payment per live birth. In 1975 the payment of the bonus was made conditional on medical examinations both during pregnancy and after childbirth, and the payment of the bonus was consequently partitioned. After the last expansion of the birth benefit in January 1987, the maximum benefit amounted to 1,090 € per child and was paid in five

[1] Substantial effects on birth timing are also found by Dickert-Conlin and Chandra [10] and Chen [7] who study the effects of tax incentives on the timing of births in the US and France, respectively. Other studies have found that taxes distort other types of individual behavior such as marriages [1] or even deaths [14].

[2] More precisely, the baby bonus replaced another policy previously in force. In the previous system, the bonus was dependent on the income of the primary caregiver in the year the child was born and was in the form of a refundable tax offset. Most, though not all, households had an incentive to move births to July 1, 2004, or later. See Gans and Leigh [12] for details.

[3] Drago et al. [11] also analyze the introduction of the birth benefit in Australia, but use a different data source. They find that the birth benefit had both a positive effect on women's fertility intentions and one of modest size on the effective birthrate. Positive fertility effects from the Australian policy change are also reported in Lain et al. [15].

[4] This in turn may imply long-run effects of short-run behavioral responses, since birth weight is suspected to be causally related with later labor-market outcomes (e.g. Black et al. [3]).

[5] As in the case of Australia, the German policy changed incentives differently for households with different characteristics. Generally, households with women working before giving birth, those planning to work shortly following birth, or those with high income received higher benefits after the reform and thus had an incentive to delay their births. See Tamm [26] for details. Neugart and Ohlsson [18] provide an alternative evaluation of the German parental benefit reform (with similar conclusions).

consecutive rates. The first rate of the bonus was paid immediately after birth (145.3 €), the second rate 1 week after birth (218 €), and the remaining three rates were paid after the child's first (363.4 €), second (218 €), and fourth (145.3 €) birthday.

Policy makers wanted to sustain the incentive for mothers to continue with medical examinations for themselves and their newborn children even after the abolition of the baby bonus; they thus introduced an alternative incentive, the so-called "Mutter-Kind-Pass", which is still in place today. It consists of a single bonus of 145 € per birth; the payment is conditional on both mother and child undergoing specific medical examinations, and it is paid on the child's first birthday. Furthermore, eligibility to the new bonus is confined to mothers who are the child's primary caregiver and to households whose income does not exceed a given threshold in the year of birth.[6] Thus, depending on household income, the abolition of the birth benefit meant a cut in cash benefits amounting to either 945 € or 1,090 € (equivalent to a cut in benefits of 87 or 100 %, respectively). For a full-time employee (not household) with median labor earnings in 1996 equal to 20,991 €, the full amount of the baby bonus was worth approximately 4.5 % of his or her annual earnings (equivalent to about 2.3 weeks' income).

Compared to other family benefits, the baby bonus was rather modest in size. Aside from the birth benefit, three basic other types of family benefits existed (and still exist today). The most important (i.e., substantial) benefit is the family benefit ("Familienbeihilfe"), which is paid until the child's 18th birthday at the minimum. In 1997 it amounted to 95–134 € per month, depending on the child's age. Parental leave benefits were paid over a period of 1.5 years at that time, and amounted to 340 € per month. However, until another major reform in 2002, these benefits were tied to the mother's employment before giving birth. Finally, there is a monthly tax allowance for children who live in the same household as the parent filing the tax report. The tax allowance amounted to 25–51 € per month in 1997, depending on the child's parity. Taken together, the baby bonus accounted for roughly 9 % of all benefits (excluding tax allowances) accruing within the first 4 years of a child's life.[7]

[6] Specifically, the maximum household income in order to qualify for the "Mutter-Kind-Pass" bonus is defined as 11 · HBGr, with HBGr ("Höchstbemessungsgrundlage") denoting the upper income threshold above which the maximum pension benefit accrues. The threshold varies over time and amounted to 2,965 € in 1997. Thus, annual household income had to be lower than 32,616 € in 1997 to qualify for the "Mutter-Kind-Pass".

[7] Neglecting tax deductibles, $[1{,}090\text{ €}/(4\cdot 12\cdot 94.5\text{ €} + 1.5\cdot 12\cdot 338.6\text{ €} + 1{,}090\text{ €})] \simeq 0.093$.

The abolition of the baby bonus

The structural deficit of the federal budget was the ultimate reason for the abolition of the baby bonus. Generous social benefits combined with a deterioration of the labor market caused the ratio of social expenditure to GDP to skyrocket in the early 1990s. In spite of a temporary strengthening of the economy in 1994, social expenditures still rose, resulting in an overall increase of 36.5 % between 1991 and 1996 [2]. To decelerate rising social spending, the governing coalition between the conservatives and the social democrats finally passed an encompassing austerity package ("Strukturanpassungsgesetz") on July 1, 1996. Savings in family policy should be achieved by reducing maternity leave duration by half a year (from 24 to 18 months) and by abolishing the baby bonus. In terms of our identification strategy, it is important to stress that, except for the birth benefit, all reforms decided on within the framework of the austerity program came into effect on July 1, 1996—half a year before the abolition of the baby bonus.

Our review of newspapers suggests that the abolition must have been known by February 2, 1996, when the coalition between the conservatives and the social democrats first announced their agreement on the austerity package. There was extensive press coverage, but there was also confusion about the exact date of abolition, and the media initially discussed July 1, 1996 as the effective date of elimination. By the first week of March, however, shortly before the coalition's agreement on the structural adjustment law was signed (March 11, 1996), it must have been evident that the birth benefit would be canceled for all children born on January 1, 1997 or later.

The window of opportunity

From what we have said above, it follows that there was a time gap of nearly 10 months between the definitive announcement and the effective date of the policy change. Because the abolition of the baby bonus implies an increase in the price of a further child, prospective parents had a financial incentive to move their baby plans forward. From the time of the announcement of the elimination of the birth benefit, the time window during which a baby would have to be conceived in order to still get the birth benefit was very short, however.

In fact, we can be quite precise regarding the length of this time window because the duration of gestation is recorded in the birth statistics (more details are given in section "Data and descriptives" below). In the time period considered (i.e., the period from July 1990 until December 2006), the length of a pregnancy shows an approximately normal distribution, with a mean duration of 276 days and a standard deviation of about 14 days. The abolition of the bonus was definitely announced on March 7, 1996. After a mean pregnancy duration of 276 days, birth would take place on December 8 at the earliest. The potential response time for women with average pregnancy duration therefore lasts 23 days (i.e., December 31–December 8)—a little bit more than 3 weeks. The corresponding 90 % confidence interval ranges from 266 to 287 days, implying that approximately 90 % (10 %) of all conceptions from March 19 (April 9) can be expected to be born before January 1, 1997.[8] These simple calculations make it clear that the window of opportunity was short, and that prospective parents thus had to respond quite immediately if they wanted to still be eligible for the bonus after the abolition had been made public.

Data and descriptives

Data source

Our empirical analysis relies on individual birth records from the Austrian birth statistics ("Geburtenstatistik"), covering all births from 1971 until 2006. In addition to information on year and month of birth, the data also contains some information on parental characteristics (such as age, education, marital status, labor market status, religion, and nationality) and, beginning in 1984, some health measures for the newborn child (such as weight, length, and Apgar score).[9] Moreover, information regarding the implemented birth procedure is recorded in the data from 1995 onwards.

Sample period(s)

Our baseline sample period basically covers the period from July 1990 until December 2006. We start the sample period in July 1990 because another major family policy reform took effect on that specific date (the reform basically involved a massive extension of the duration of parental leave benefits).[10] The sample period is considerably shorter, however, when we focus either on newborn's health or on birth procedure due to data availability.

As we will show below, however, the exact length of the sample period does not appear to have any substantial impact on our results. In fact, our estimates of the policy impact turn out to be robust across a wide range of alternative sample periods (as will be shown in section "Robustness" below).

The monthly birth count, 1990–2006

Figure 1 shows both the observed and the de-trended absolute number of monthly births from January 1990 to December 2006.

Panel (a) shows the absolute number of monthly births, with dots (triangles) indicating the number of births in December (January) in any given year. Two specific features stand out clearly. First, there is a strong non-linear trend in the number of births, with a pronounced hump shape in the 1990s (presumably reflecting the large immigrant influx from the Balkan countries at that time) and a flattening afterwards. The number of monthly births increased from about 7,500 births per month in the early 1990s to a high of somewhat more than 8,000 births per month in the mid 1990s. The number of births began to decrease again at the end of the 1990s, when the number of births seems to have stabilized at about 6,500 births per month. The second outstanding feature is the existence of a pronounced cyclical pattern within any given year. Within each year, many more children are born in the middle rather than at the end of the year.[11] Even more striking is the fact that the number of children born in December over the whole period considered never exceeds the number of

[8] Note that it is possible that some couples already tried to conceive after February 2, 1996, even though there was confusion about the exact date of abolition until March 7, 1996. It is, therefore, still possible to find an increase in births before December 1996. See also footnote 12.

[9] The Apgar score is used to assess the health of a newborn immediately after birth. In our data, the Apgar score one, five, and 10 min after birth is recorded. The Apgar score assesses five different categories (heart rate, breathing, muscle tone, reflex response, and skin color) with a score between zero and two each, where the scores are simply added up. Low values on the score are indicative of poor health. In the regression reported in section "Newborns' health" below, we use the average of a child's score 1, 5, and 10 min after birth.

[10] It is worth mentioning that our basic sample period covers, besides the austerity package and the abolition of the baby bonus, two other major policy changes in family law that were made public in August 2001. First, parental leave duration was extended from 18 to 30 months for all mothers who were on maternity leave during August 2001, gave birth after July 2000, and earned no more than 14,600 € per year. A second reform was enacted in January 2002 and decoupled eligibility to maternity leave benefits from any prior work requirement, thus extending eligibility to self-employed women and mothers not in the labor force. We control for these policy changes by including appropriately defined indicator variables in the regressions that are based on sample periods covering these policies.

[11] There are basically two explanations for the seasonal pattern in birth timing. First, there are seasonal fluctuations in marriages which may lead to fluctuations in births. In fact, marriage seasonality in Austria matches the seasonal pattern in births if newlywed couples immediately stop using contraceptives with the intent of conceiving. A second explanation are parental preferences regarding the month of birth [25]. See also Buckles and Hungerman [6] for a detailed discussion of both causes and consequences of seasonality in births.

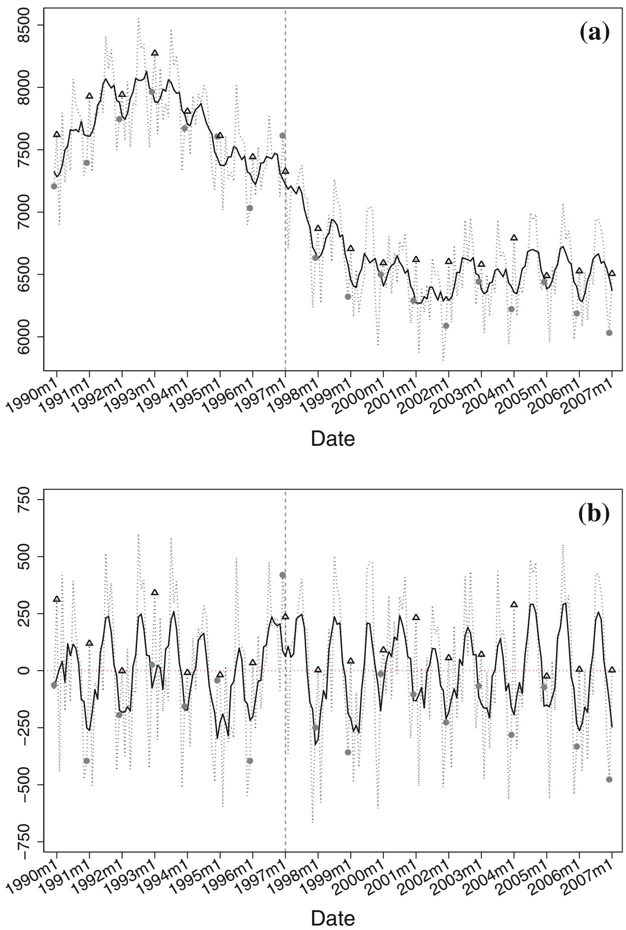

Fig. 1 Actual and de-trended number of monthly births **a** observed absolute number of monthly births, **b** de-trended number of monthly births. *Notes*: **a** shows the observed number of monthly births. The *dotted line* shows the actual number of monthly births, the *solid line* displays the 3-month moving average (we average over the current observation as well as three lags and leads). **b** shows the de-trended number of monthly births (de-trending is done using a Hodrick–Prescott filter with default smoothing parameter). The *solid line* represents the 2-month moving average. The *dots* (*triangles*) indicate the actual or de-trended number of births in December (January) of each year

births in January—except in December 1996, the month just before the birth benefit was effectively abolished.

We remove the time trend from the data in panel (b) and, thus, make the monthly cyclicality in births more evident. In each year, the number of births is lowest at the end of the year and highest in the middle. The year 1996 clearly stands out regarding the number of children, especially those born in December. In fact, the de-trended number of births in December 1996 (463) corresponds to the 95th percentile of the distribution of de-trended number of births over the entire period and to the 1st percentile of the distribution of the de-trended number of births in December.

Maternal characteristics

Table 1 shows maternal characteristics for five different subsamples (i.e., sample periods), including the subsample of mothers giving birth in December 1996 which appears in the last column (the asterisks denote significant differences between the last and the first, second, third, or fourth column, respectively).

Three features are noteworthy. First, most maternal characteristics are trending over time. For example, there are substantial shifts in mothers' age at birth (i.e., mothers become older over time). Second, there are cyclical patterns within any given year, consistent with the evidence on seasonal patterns in births presented by Buckles and Hungerman [6]. That is, the table shows that mothers giving birth in December are different from mothers giving birth in any month on most dimensions considered. For example, mothers giving birth in December appear to be slightly younger than the average mother. Third, mothers giving birth in December 1996 differ from mothers giving birth in December (in years other than 1996). However, due to the trending nature of the variables, it is difficult to tell whether this difference is due to compositional changes related to the abolition of the baby bonus.

The fertility response

Estimating the fertility response

A couple of issues have to be considered when estimating the fertility response following the announcement of the abolition of the baby bonus. First, note that we have to rely on the longitudinal patterns in the number of births to estimate the policy impact on the birth count. Because there is no control group available, the outcome in the absence of the policy change needs to be predicted using regularities in the data before and/or after the policy change. Another issue is that there may be a permanent effect from the abolition of the baby bonus on fertility behavior. This implies that we should be cautious when using, or potentially try to do without, data from after the baby bonus has been abolished (i.e., data after December 1996). Second, Fig. 1 suggests that we should try to model a flexible time trend in the number of births when using a longer sample period. However, it turns out that fitting a flexible time trend becomes somewhat difficult when only using data from before the policy change because observations at the boundary of the sample period have a strong impact on the estimated time trend in case of a nonlinear trend (and thus on the estimate of the fertility response as well). We use two distinct empirical strategies to cope with these issues in the following.

Table 1 Maternal characteristics

Sample period:	1992.11−1996.10	1994.11−1996.10	Dec 1992−1995	Dec 1994−1995	Dec 1996
Age					
Aged below 20	0.046**	0.041	0.048***	0.042	0.040
	(0.208)	(0.198)	(0.213)	(0.201)	(0.196)
Aged between 20 and 24	0.253***	0.236***	0.256***	0.244***	0.211
	(0.434)	(0.424)	(0.436)	(0.429)	(0.408)
Aged between 25 and 29	0.382	0.384	0.381	0.385	0.381
	(0.485)	(0.486)	(0.485)	(0.486)	(0.485)
Aged between 30 and 34	0.232***	0.247***	0.227***	0.236***	0.266
	(0.422)	(0.431)	(0.418)	(0.424)	(0.441)
Aged between 35 and 39	0.075***	0.079**	0.076***	0.079**	0.087
	(0.262)	(0.27)	(0.264)	(0.269)	(0.281)
Older than 40	0.012*	0.013	0.012	0.013	0.014
	(0.109)	(0.113)	(0.108)	(0.114)	(0.118)
Child order					
First child	0.447	0.445	0.451	0.446	0.450
	(0.497)	(0.496)	(0.497)	(0.497)	(0.497)
Second child	0.362	0.364	0.355	0.360	0.359
	(0.48)	(0.481)	(0.478)	(0.48)	(0.479)
Third or higher order child	0.191	0.191	0.194	0.193	0.192
	(0.392)	(0.392)	(0.395)	(0.394)	(0.393)
Parity	1.829	1.830	1.833	1.837	1.825
	(0.978)	(0.975)	(0.994)	(0.993)	(0.969)
Marital status					
Single	0.238***	0.243**	0.236***	0.247	0.253
	(0.425)	(0.428)	(0.424)	(0.431)	(0.434)
Married	0.726***	0.720***	0.727***	0.714*	0.703
	(0.445)	(0.449)	(0.445)	(0.451)	(0.457)
Divorced, widowed	0.036***	0.038***	0.037***	0.039**	0.045
	(0.187)	(0.19)	(0.188)	(0.192)	(0.206)
Citizenship					
Native	0.813	0.809	0.814	0.811	0.809
	(0.389)	(0.393)	(0.388)	(0.391)	(0.393)
Formal education					
Mandatory school	0.243***	0.229***	0.250***	0.243***	0.205
	(0.428)	(0.42)	(0.433)	(0.429)	(0.403)
Vocational school for apprentices	0.378	0.377	0.379	0.374	0.373
	(0.485)	(0.484)	(0.485)	(0.483)	(0.483)
Intermediated technical or	0.174	0.173	0.174	0.171	0.179
vocational school	(0.379)	(0.378)	(0.379)	(0.376)	(0.383)
Higher technical or	0.112*	0.117	0.108***	0.114	0.119
vocational school	(0.315)	(0.321)	(0.31)	(0.317)	(0.323)
University or university college	0.076**	0.080	0.072***	0.075**	0.083
	(0.264)	(0.271)	(0.258)	(0.263)	(0.275)
Unknown education	0.017***	0.024***	0.016***	0.023***	0.041
	(0.128)	(0.154)	(0.124)	(0.149)	(0.198)
Employment status					
Employed before birth	0.738***	0.737***	0.739***	0.735**	0.719
	(0.439)	(0.44)	(0.439)	(0.441)	(0.449)

Table 1 continued

Sample period:	1992.11–1996.10	1994.11–1996.10	Dec 1992–1995	Dec 1994–1995	Dec 1996
Not employed before birth	0.245	0.239	0.245	0.242	0.240
	(0.43)	(0.426)	(0.43)	(0.428)	(0.427)
Labor market status unknown	0.017***	0.024***	0.016***	0.023***	0.041
	(0.128)	(0.154)	(0.124)	(0.15)	(0.198)

*, **, and *** denote statistical significance of the difference between the first (second, third, or fourth column, respectively) and the last column at the 10, 5, and 1 % level, respectively

Our first empirical strategy only uses data from before the policy change until (and including) October 1996, but refrains from fitting a flexible time trend.[12] At the same time, we want to focus on a time period within an approximately linear time trend in the number of births. A simple visual inspection of the observed number of births, as in Fig. 1, suggests that there is a linear time trend in the monthly number of births from about 1992 onwards. We thus regress the absolute (or, alternatively, the log) number of births on a linear time trend and a series of dummies for calendar month, denoted by γ_m, on a sample period of varying length

$$b_t = \alpha + \beta t + \gamma_m + \varepsilon_t, \tag{1}$$

with $t \in \{T, T+1, \ldots, 1996.9, 1996.10\}$. We let T be equal to either 1992.11, 1993.11, or 1994.11. Thus the sample period consists of either 48, 36, or 24 months (in section "Robustness", we show that our results are also robust to alternative sample periods).

In the second strategy, we use observations from both before and after the policy change, implicitly assuming that only those parents move birth forward who originally wanted to give birth in the time period that we omit from the sample period. Using observations from after the policy change as well makes it possible to use a much more flexible form for the time trend in the number of births

$$b_t = \alpha + \beta\kappa(t) + \gamma_m + \delta 1(t \geq 1997.1) + \varepsilon_t, \tag{2}$$

with $t \in \{1990.7, 1990.8, \ldots, 1996.9, 1996.10, T, T + 1, \ldots, 2006.11, 2006.12\}$. That is, t runs from July 1990 to December 2006 in this case, but we leave out a period of varying length in the middle of the sample period, running from 1996.11 to T. We let T equal either 1997.1, 1998.1 or 1999.1, and, thus, the period that is left out from the analysis correspond to either 2, 14, or 26 months. In this second scenario we allow for a flexible time trend in the number of births, using a fourth-order polynomial in t, denoted by $\kappa(t)$. We also allow for the possibility of any permanent effect of the abolition of the bonus on the number of births by including a dummy variable that takes on the value of 1 if t is equal to or >1997.1. Thus δ will capture any permanent fertility of the abolition of the bonus (as well as differences in the number of births between the two time periods for any other reasons).[13]

For either strategy, we then use the estimates from the above regressions in a second step to make an out-of-sample prediction of the number of babies that would have been born in December 1996 in the absence of the policy change, denoted by $\widehat{b}_{1996.12}$. The difference between the observed and the predicted number of births in December 1996,

$$b_{1996.12} - \widehat{b}_{1996.12}, \tag{3}$$

is our estimate of the impact of the (public announcement of the) abolition of the baby bonus on the number of children born in December 1996, relative to the number of children we would have expected in the absence of the policy change (or, alternatively, in the case that the abolition were not publicly announced in advance).

Table 2 shows results for both the absolute number of births and the log number of births and for the two different strategies outlined above.[14] Panel A shows the resulting estimates when using data from before the policy only, but for three different sample periods, combined with a simple linear time trend in each case. Depending on the length of the sample period, estimates of the additional number of births in December 1996 range from 487 to 592 births. In

[12] November 1996 births are excluded as well because, according to the distribution of the pregnancy duration, about 5 % of responding mothers who conceived immediately after announcement delivered before November 28, 1996. This follows from the 90 % confidence interval that ranges from 266 to 287 days of pregnancy. More importantly, it turns out that the initial confusion about the exact date of the abolition was less pronounced than our reading of the newspapers suggested (see section "The abolition of the baby bonus" again) and that many prospective parents must have known the date of the abolition already before the first week of March. Indeed, we already find a substantial, and statistically significant number of additional births in November 1996 (results not shown). This implies that our main estimates based on births in December 1996 unambiguously represent a lower bound on the overall fertility effect.

[13] Similarly, we also include additional dummies for the other major policy changes that were implemented during the sample period (see footnote 10 for details).

[14] We also re-ran our baseline regressions using the total fertility rate as dependent variable. Results turn out to be qualitatively similar.

all three cases, the estimate of the extra births turns out to be statistically significant. In relative terms, the estimates imply that about 6.8 % ($= 100\,\% \cdot [487/(7{,}613 - 487)]$) to 8.4 % ($= 100\,\% \cdot [592/(7{,}613 - 592)]$) additional children were born due to the announced abolition of the bonus. We get very similar estimates when using the log number of births as the dependent variable, as shown in the lower part of panel A. Relative effects in this case range from about 6.8 % ($= 100\,\% \cdot [\exp(0.066) - 1]$) to 8.1% ($= 100\,\% \cdot [\exp(0.078) - 1]$).

It turns out that our alternative strategy yields very similar estimates, as shown in panel B. Depending on the sample period, estimates range from a low of 664 (9.5 %) to a high of 698 (10.1 %) extra births in December 1996. Using data from after the policy change as well thus yields somewhat larger estimates than those we obtain when we only use data from before the policy change, but the point estimates based on the two strategies are in fact not statistically different from each other. Again, using the log number of births yields very similar quantitative implications.

Table 2 Fertility responsiveness

Panel A: Observations from before the abolition only, linear time trend			
Sample period	1994.11–1996.10	1993.11–1996.10	1992.11–1996.10
Number of births			
Residual December 1996	486.625***	562.583***	591.333***
	(46.648)	(109.883)	(114.649)
Number of births December 1996	7613	7613	7613
Number of observations	24	36	48
Adjusted R^2	0.739	0.718	0.838
p value(*F* statistic)	0.000	0.000	0.000
Log number of births			
Residual December 1996	0.066***	0.075***	0.078***
	(0.006)	(0.014)	(0.014)
Number of log births December 1996	8.938	8.937	8.938
Number of observations	24	36	48
Adjusted R^2	0.732	0.715	0.839
p value (*F* statistic)	0.000	0.000	0.000
Panel B: Observations from before and after the abolition, nonlinear time trend			
Sample period	1990.7–1996.10 & 1997.1–2006.12	1990.7–1996.10 & 1998.1–2006.12	1990.7–1996.10 & 1999.1–2006.12
Number of births			
Residual December 1996	698.290***	678.489***	663.846***
	(129.264)	(124.48)	(123.07)
Number of births December 1996	7613	7613	7613
Number of observations	196	184	172
Adjusted R^2	0.956	0.960	0.962
p value (*F* statistic)	0.000	0.000	0.000
Log number of births			
Residual December 1996	0.097***	0.094***	0.091***
	(0.018)	(0.018)	(0.017)
Number of log births December 1996	8.938	8.937	8.938
Number of observations	196	184	172
Adjusted R^2	0.952	0.957	0.960
p value (*F* statistic)	0.000	0.000	0.000

*** denotes statistical significance at the 1 % level. In Panel A, the time trend is assumed to be linear and the underlying sample period varies between two (column 2) and four (column 4) years prior to the effective policy change. In Panel B, the time trend is assumed to follow a fourth-order polynomial in calendar time, and a dummy variable for each major policy change within this period is included (1997.1, 2000.7, 2002.1). The underlying sample period basically covers observations from 1990.7 to 2006.12, but leaves out a period in between which is potentially affected by the policy change. The period left-out varies between 2 months (i.e., November and December 1996) and 3 years and 2 months (i.e., the period from November 1996 to December 1998)

Robustness

We first test the sensitivity of our baseline results with respect to (additional) variations in the sample period. Remember that when only using observations from before the abolition of the bonus, our baseline model uses either 24, 36, or 48 months prior to the policy change in order to predict the December 1996 birth count. Panel A of Table 3 shows the resulting minimum and maximum estimate of the fertility response when we vary the length of the observation period, in steps of 1 month, from 24 to 48 months. Estimates turn out to be robust to this variation in the sample period. The resulting minimum (maximum) estimate equals 417 (622) births, an estimate well within the range of our baseline estimates. The same conclusion applies to the range of estimates when using the log number of births as the dependent variable.

When using observations from both before and after the policy change, our baseline result basically relies on the whole observation period from July 1990 to December 1996, but excludes a period in between of varying length. In contrast to the baseline specification, panel B of Table 3 holds the omitted period fix (1997.1–1999.1), but varies the length of the sample period before and after the omitted period, from a minimum of 24 months to a maximum of 76 months. Again, estimates turn out to be surprisingly robust across the various sample periods. The minimum (maximum) estimate among all estimates is equal to 309 (684) additional births in December 1996. As above, we find a quantitatively similar pattern of estimates when modeling the log number of births instead of the absolute number of births.

As an additional robustness check, we apply an alternative two-step procedure. In a first step, we de-trend the whole time series using a conventional Hodrick–Prescott filter. We then regress the de-trended number of births on a series of monthly dummies in the second step.[15] As in the baseline model, we use the four foregoing years to predict the de-trended number of births in December 1996. Moreover, this exercise is not only done for the real policy change but also for hypothetical policy changes in December 1993, 1994 and 1995. Panel C of Table 3 presents the results. The first column shows that the impact of the abolition is estimated to amount to 455 additional births. This estimate is slightly smaller than the estimate obtained by the baseline model, but it is well within the estimated range of estimates from panel A above (i.e., the estimates are not significantly different from each other).[16] The results presented in the subsequent columns show estimates of the residual number of births in the hypothetical scenario that the policy change happened one, 2 or 3 years earlier than it actually did. It is immediately evident that none of the placebo regressions yields a residual that is statistically different from zero, underlining our argument that the announced abolition of the baby bonus increased fertility in the short-run.

[15] Specifically, we run the following regression: $\ddot{b}_t = \alpha + \gamma_m + \varepsilon_t$, where $\ddot{b}_t$ denotes the de-trended number of monthly births and γ_m denotes the inclusion of a full set of monthly dummies.

Alternative estimation approach

As a final robustness test we present results based on a slightly different estimation approach than before. In contrast to our baseline estimates, the impact of the policy change in this case is estimated by including a simple binary indicator that takes on the value of one for the observation from December 1996, and zero otherwise (see footnote 10 again). That is, we estimate the parameters of the following regression model:

$$b_t = \alpha + \beta\kappa(t) + \beta' x_t + \gamma_m + \delta 1(t \geq 1997.1) + \psi 1(t = 1996.12) + \varepsilon_t, \quad (4)$$

with t running from 1994.11 to 1999.12, from 1992.11 to 1999.12, or from 1990.7 to 2006.12.[17] The alternative approach thus uses data from both before and after the abolition of the baby bonus without excluding any observations after the policy change, in contrast to our previous estimates presented in Tables 2 and 3. One advantage of this alternative approach is that we can additionally control for observed maternal characteristics, and, thus, Table 4 shows results with and without controlling for average maternal characteristics x_t [i.e., age, schooling, child parity, marital status (married), and employment status (employed)]. In either case, the estimated fertility response is simply given by $\widehat{\psi}$, the estimated coefficient on the dummy variable indicating that t equals 1996.12.

It is immediately evident that this alternative strategy yields estimates that are similar (in fact, statistically identical) to our baseline estimates, with estimates ranging from about 595 to 697 additional number of births (alternatively, in the case of using the log number of births as dependent variable, with an additional 8.4–9.6 % of births in December 1996).

[16] It is actually quite intuitive that the estimate based on the de-trended number of births is smaller because the filter fits the time trend using all observations—including the extra births in December 1996. As a consequence, the time trend is biased upward around the date of the true policy change. This in turn results in a downward biased estimate for the fertility response in December 1996.

[17] The first (second) sample period covers the same observations as our baseline estimates (cf. column 1(3) in panel A of Table 2), but includes 1996.11 and 1996.12 and extends the sample period until 1999.12. The third sample period uses the maximum number of observations (similar to panel B of Table 2).

Table 3 Robustness

Dependent variable	Number of births		Log (number of births)	
	Min	Max	Min	Max
Panel A: Observations from before the abolition only, linear time trend				
Residual December 1996	416.800***	622.250***	0.056***	0.082***
	(65.384)	(114.179)	(0.008)	(0.015)
Number of (log) births December 1996	7613	7613	8.937	8.937
Number of observations	25	40	25	40
Adjusted R^2	0.667	0.814	0.661	0.812
p value (F statistic)	0.000	0.000	0.000	0.000
Panel B: Observations from both before and after the abolition, nonlinear time trend				
Residual December 1996	308.955***	683.588***	0.041***	0.095***
	(102.14)	(140.048)	(0.015)	(0.019)
Number of (log) births December 1996	7,613	7,613	8.937	8.937
Number of observations	61	81	61	65
Adjusted R^2	0.941	0.909	0.937	0.863
p value (F statistic)	0.000	0.000	0.000	0.000

	True policy change	Placebo regressions		
	Y: 1996	Y: 1995	Y: 1994	Y: 1993
Panel C: Placebo regressions, de-trended number of births				
Residual December Y	455.025***	−182.784	72.090	68.241
	(143.074)	(150.167)	(135.771)	(145.206)
Number of births December Y	7,613	7,232	7,605	7,672
Number of observations	48	48	48	48
Adjusted R^2	0.648	0.669	0.732	0.741
p value (F statistic)	0.000	0.000	0.000	0.000

*** denotes statistical significance at the 1 % level. Panel A uses a linear time trend with a sample period that varies between two and four years prior to the policy change. The model in panel B assumes a time trend that follows the fourth polynomial. The sample period varies between 24 and 76 months before and after the policy change, while the omitted period in between is held fixed (1997.1–1999.1). Panel C shows results from several placebo regressions based on the de-trended series of monthly births. See main text for details

Conception (re-)scheduling versus timing of births

Thus far we have ignored the fact that we expect to see extra births in December 1996 for two very distinct reasons. First, as we have discussed above, there was a short window of opportunity of about 3 weeks during which prospective mothers could try to get pregnant in order to give birth before January 1, 1997 and still get the birth benefit. A second reason, however, may be that women with a due date close to the date of abolition could have manipulated the exact day of birth by means of a surgical intervention (i.e., cesarean section). We now try to gain some insight into the effective source of the additional births that we observe in December 1996. To distinguish between the two channels, we now focus on the date of conception, which can easily be derived from the available information on the duration of pregnancy and the date of birth. Note that, because the abolition of the birth benefit was announced after the first week of March, the 3 week response window falls entirely into the month of March. Hence, the comparison of the impact on the number of conceptions in March with the impact on the number of births in December 1996 is insightful in terms of whether conception (re-)scheduling or birth timing is the primary cause of the extra births in December 1996. Analogous to the baseline model, we use data from the preceding 48 months to make a simple prediction of the number of babies that would have been conceived in March 1996 in the absence of the policy change.

The resulting estimates, shown in Table 5, imply that 631, or about 9.1 % $(= 100\,\% \cdot 631/(7{,}547 - 631))$,

Table 4 Alternative estimation approach

Dependent variable:	Number of births			Log (number of births)		
Sample period	1994.11–1999.12	1992.11–1999.12	1990.7–2006.12	1994.11–1999.12	1992.11–1999.12	1990.7–2006.12
Panel A: without controls for maternal characteristics						
1(t = 1996.12)	616.558***	601.603***	672.943***	0.088***	0.084***	0.094***
	(196.818)	(179.079)	(150.400)	(0.029)	(0.026)	(0.022)
Number of observations	62	86	198	62	86	198
Adjusted R^2	0.854	0.915	0.954	0.845	0.908	0.951
p value (F statistic)	0.000	0.000	0.000	0.000	0.000	0.000
Panel B: with controls for maternal characteristics						
1(t = 1996.12)	594.613**	624.208***	697.095***	0.085**	0.086***	0.096***
	(225.229)	(194.750)	(154.823)	(0.034)	(0.028)	(0.023)
Number of observations	62	86	198	62	86	198
Adjusted R^2	0.840	0.909	0.953	0.830	0.903	0.949
p value (F statistic)	0.000	0.000	0.000	0.000	0.000	0.000

** and *** denotes statistical significance at the 5 and 1 % level, respectively. The overall time trend is assumed to follow a first-order (fourth-order) polynomial in the first and second (third) column. All specifications include a full set of monthly dummies. We also include a dummy variable for each major policy change (i.e., 1997.1, 2000.7, 2002.1) if covered by the sample period. Panel B includes additional controls for maternal characteristics [i.e., age, schooling, child parity, marital status (married) and employment status (employed)]

additional children were conceived in March 1996 on top of what would have been expected in the absence of the policy change. Remember that our baseline model yields an estimate of 591 additional births in December 1996 (see Table 2)—almost the same number as our estimate for the additional number of conceptions in March 1996. The fact that both results are very much in line with each other suggests that conception (re-)scheduling, rather than birth timing by medical intervention, is the underlying cause of the observed fertility response. Section "Birth complications" below provides additional evidence in line with this result, showing that there is no impact on the fraction of cesarean sections conducted in December 1996. Finally, note that it is likely that the number of couples trying to move baby plans forward is likely to be higher than those 616 who finally succeeded.[18]

[18] A rough approximation of the total number of responding couples is obtained by multiplying the number of extra births with the probability of becoming pregnant within 3 weeks. Gnoth et al. [13] study the likelihood of spontaneous conception in subsequent cycles for a random sample of German women and find that cumulative probabilities of conception at one, three, six and twelve cycle(s) are, respectively, 38, 68, 81 and 92 %. A linear interpolation between month 0 and 1 one yields a cumulative probability of conception of 29 % at week three, which implies that approximately 2038 (=591/0.29) couples were induced to bring their baby plans forward. Relative to the December 1996 birth count that would have occurred in the absence of the policy change, the responsive sample thus amounts to as much as 29 % [=2038/(7613 − 591)].

Table 5 Conception (re-)scheduling versus birth timing

Dependent variable	Number of conceptions	Log number of conceptions
Residual March 1996	631.041***	0.083***
	(135.143)	(0.017)
Number of (log) conceptions March 1996	7,547	8.928
Number of observations	48	48
Adjusted R^2	0.783	0.784
p value (F statistic)	0.000	0.000

*** denotes statistical significance at the 1 % level. The dependent variable is the number of conceptions in March 1996. The regression specification assumes a linear time trend. The sample period covers all conceptions within the four years preceding March 1996

Taking risks for the bonus?

Birth complications

We now try to understand whether mothers (un)consciously take health risks for themselves and/or their newborn child when trying to obtain the bonus. We start looking at birth complications. In the following we consider instrumental vaginal delivery mechanisms (forceps delivery, vacuum extraction, and breech delivery) as indication of birth complications, as all three delivery methods involve potential health risks for mother and/or child and are, thus, applied in emergency situations only. While the former two types of assisted deliveries are used in the case of maternal exhaustion, fetal distress, or a combination of both, the latter method is used in labor with a baby in head-down position. For simplicity, we will refer to these instrumental vaginal delivery mechanisms as "birth complications" in what follows. Cesarean section is considered separately because it has been performed upon request more recently for deliveries that could otherwise have been natural, even though it is usually performed only when a vaginal delivery would put baby's or mother's life or health at risk. Accordingly, we think that a cesarean section must be viewed as an instrument for deliberate birth timing—in line with the results from Gans and Leigh [12] and Tamm [26].

To estimate the impact of the abolition of the bonus on the incident of birth complications, we use basically the same regression specification as in section "Alternative estimation approach", but with the percentage share of birth complications as the dependent variable. We prefer the alternative estimation approach in this context because we focus on the composition of births now, rather than on the number of births, and, thus, it is less important not to include observations from those time periods potentially affected by the abolition of the baby bonus. Because information on birth procedure is only reported from 1995 onwards, the sample period covers the period from January 1995 to December 2006.[19]

Table 6 reports the baseline result for the percentage of overall birth complications (column 1), as well as for single birth procedures (columns 2–4). Finally, the last column shows the estimated impact on the percentage of cesarean sections. We find that there is a statistically significant and substantial increase in the percentage of overall birth complications of about 0.9 percentage points in the month prior to abolition of the baby bonus. Note that this corresponds to a relative increase in the probability of experiencing some birth complication by about 17 % ($= 100\,\% \cdot (0.967/5.587)$). In absolute numbers, the figures imply that about 74 additional complications were observed in December 1996 ($\simeq 0.009 \cdot (7{,}613 + 600)$). The following three columns show results by individual delivery method. Estimates show a significant increase for all but one of the instrumental vaginal birth procedures (forceps delivery). The overall increase in the share of labor complications is, thus, mainly driven by an increase of breech deliveries and vacuum extractions. In terms of non-vaginal instrumental delivery methods, column 5 reveals an insignificant estimate for the percentage share of cesarean sections, suggesting that women did not use this method to deliberately manipulate the date of birth.

One potential explanation of this finding is that responsive mothers are simply a selected group of mothers. If (some of) the characteristics of these mothers are associated with preexisting conditions encouraging birth complications, such as age at birth, differential fertility responsiveness may mechanically affect the incidence of birth complications.[20] To get an idea of how important compositional changes are in explaining the observed increase in the likelihood of experiencing some birth complication, we ran an additional decomposition exercise based on individual-level data (see "Appendix" for details). The decomposition results suggest that only a small fraction, about 12–13.5 %, of the observed increase in the likelihood of experiencing some birth complication can be related to observable compositional changes resulting from differential responsiveness. It is clear, however, that we cannot rule out that additional, unobserved maternal characteristics (such as mothers' health status) explain some of the increased risk of experiencing some birth complication as well.

At the same time, the fact that observable maternal characteristics explain only a minor part of the increased risk of birth complications also opens up the possibility of an alternative explanation. Specifically, we may plausibly think of the babies moved forward as mistimed pregnancies, in the sense that these pregnancies occurred earlier than initially planned or desired, and there is evidence that mistimed pregnancy is associated with increased behavioral and psychological risks.[21] First, the most important behavioral risks associated with mistimed pregnancies are smoking, drinking, and diet; and such behavior is known to be associated with complications at birth [8, 9]. Other studies have found that mistimed pregnancies are associated with psychological distress. For example, Orr [21] find that women with a mistimed pregnancy are more likely to show depressive symptoms than women with an intended pregnancy. Similarly, Cheng et al. [8] find that women with a mistimed pregnancy are more likely to suffer from postpartum depression. Increased psychological distress

[19] Running the same model, but excluding either one, 2 or 3 years in between yields very similar estimates to those reported in Table 6.

[20] For example, Rayl et al. [24] show that maternal characteristics like primiparity and older maternal age are associated with an increased risk of breech birth. The Austrian data show a very similar picture: the major determinants for both instrumental non-vaginal and instrumental vaginal delivery are primiparity and older age (results not shown).

[21] In the medical and epidemiological literature, a mistimed pregnancy is usually defined as a pregnancy that occurred earlier than desired (e.g., Cheng et al. [8]). Under normal circumstances, antedating a child is a conscious action and should not be considered a mistimed pregnancy. In our case, however, incentives to antedate a child were increased exogenously while other relevant circumstances (e.g., financial situation, health behavior, workload, size of the apartment) remained unchanged. In such a situation, one may argue that mothers are exposed to similar risks as in the case of a truly mistimed pregnancy.

Table 6 Birth complications

	Any birth complication	Vacuum extraction	Forceps delivery	Breech delivery	Cesarean section
Mean	5.587	4.365	0.698	0.524	18.588
Standard deviation	0.419	0.559	0.383	0.351	4.555
Panel A: With controls for maternal characteristics					
1(t = 1996.12)	0.967***	0.507*	0.142	0.318**	0.210
	(0.365)	(0.285)	(0.125)	(0.140)	(0.655)
Number of observations	144	144	144	144	144
Adjusted R^2	0.398	0.793	0.916	0.874	0.984
p value (F statistic)	0.000	0.000	0.000	0.000	0.000
Panel B: Without controls for maternal characteristics					
1(t = 1996.12)	0.909**	0.428	0.128	0.353**	0.370
	(0.355)	(0.281)	(0.123)	(0.137)	(0.675)
Number of observations	144	144	144	144	144
Adjusted R^2	0.407	0.791	0.915	0.873	0.982
p value (F statistic)	0.000	0.000	0.000	0.000	0.000

*, **, and *** denote statistical significance at the 10, 5, and 1 % level, respectively. The dependent variable in column 1 is the overall percentage share of birth complications (equal to the sum of columns 2–4), while columns 2–5 show the estimated impact on single delivery methods. The sample period runs from 1995.1 to 2006.12. The time trend is assumed to follow a fourth-order polynomial in calendar time. All specifications include a full set of monthly dummies as well as a dummy variable for each major policy change covered by the sample period (i.e., 1997.1, 2000.7, 2002.1). Panel A includes additional control for maternal characteristics [i.e., age, schooling, child parity, marital status (married) and employment status (employed)]

during pregnancy in turn appears to be associated with an increased risk of pregnancy complications [17, 19].

Newborns' health

We next explore the direct impact on newborns' health using several distinct health measures: the incidence of a preterm birth, low birth weight, length at birth and the Apgar score, which is a measure for quickly assessing the health of a newborn (cf. footnote 9). We expect to find differences in the health of children born in December 1996 for the same reasons as for birth complications. If newborns' health is associated with characteristics of the mother, differences in the health of newborn children may simply result from heterogeneous fertility responses. While compositional changes may have positive or negative effects on newborns' health, the additional behavioral and psychological risks potentially triggered by a mistimed pregnancy are expected to unambiguously harm the health of the newborn.[22]

Table 7 shows the resulting estimates for four different, more or less direct health measures. All estimates are derived applying the same estimation strategy as in the case of birth complications (see section "Birth complications" above). The sample period runs from 1992.11 to 2006.12. The first column shows the effect on the percentage of newborn children with a low Apgar score (i.e., a score lower than 7). The resulting point estimate is small and statistically not different from zero. Similarly, we do not find any negative effect on the likelihood of small birth length, of low birth weight or of a premature birth.[23] Overall, we thus find no statistical evidence for any (immediate) negative impact on the health of newborn children (if anything, there is a weakly significant negative effect on the probability of experiencing a premature birth; however, this effect is not robust across specifications)—despite the fact that we find evidence of increased labor complications, which would suggest that the abolition of the baby bonus put some children at risk. Of course, this finding does not rule out the existence of any health effect in the medium or the long run.

[22] For example, Pulley et al. [23] find that the mistiming of a pregnancy positively correlates with the probability of a preterm delivery (and low birth weight). They conclude that pregnancies that are mistimed by more than a few months may have severe health consequences for both mother and child. Similar results for unintended (i.e., both mistimed and unwanted) births are reported by Orr et al. [22].

[23] Note that it may make sense to look at the share of premature births conceived in March rather than born in December 1996. This is because babies conceived within the relevant time window of 3 weeks following the announcement, when born prematurely, would have been born at most 8.5 months later and, thus, probably already in November. However, this yields an insignificant estimate as well.

Table 7 Newborn's health

	Poor health (Apgar< 7)	Small birth length (<45 cm)	Low birth weight (<2,500 g)	Premature birth (<37 weeks)
Mean	1.369	2.876	6.330	5.883
Standard deviation	0.180	0.390	0.668	0.821
Panel A: With controls for maternal characteristics				
1(t = 1996.12)	−0.106	−0.265	−0.641	−0.638*
	(0.161)	(0.265)	(0.403)	(0.345)
Number of observations	170	170	170	170
Adjusted R^2	0.344	0.621	0.700	0.854
p value (F statistic)	0.000	0.000	0.000	0.000
Panel B: Without controls for maternal characteristics				
1(t = 1996.12)	−0.126	−0.294	−0.478	−0.539
	(0.154)	(0.256)	(0.393)	(0.335)
Number of observations	170	170	170	170
Adjusted R^2	0.360	0.624	0.697	0.854
p value (F statistic)	0.000	0.000	0.000	0.000

* denotes statistical significance at the 10 % level. The dependent variable is the percentage share of newborn children that are in poor health (indicated by a low Apgar score), of small birth length, of low birth weight or born prematurely. The sample period runs from 1992.11 to 2006.12. The time trend is assumed to follow a fourth-order polynomial in calendar time. All specifications include a full set of monthly dummies as well as a dummy variable for each major policy change covered by the sample period (i.e. 1997.1, 2000.7, 2002.1). Panel A includes additional control for maternal characteristics [i.e., age, schooling, child parity, marital status (married) and employment status (employed)]

Conclusions

We studied the fertility and health effects preceding the abolition of the Austrian baby bonus on January 1, 1997. Even though the bonus was rather small relative to other family benefits available, it was still worth about 4.5 % of the median annual labor income in the year of its abolition. Moreover, because the abolition was made public about 10 months in advance, prospective parents not only had a financial incentive but also the possibility to react without the need of medical intervention.

We find that about 8 % (roughly 600) more babies were born than in the absence of (the public announcement of) the policy change in December 1996, the month before the abolition of the baby bonus. This effect proves to be robust to a variety of robustness checks and alternative estimation strategies. Also, considering the fact that the window of opportunity was quite a short period of about 3 weeks only, the fertility response appears to be quite large. We also find (re-)scheduling of conceptions rather than direct birth timing (through medical intervention) to be the source of the fertility response. Our analysis of birth procedures further reveals a significant and substantial increase in the fraction of mothers experiencing some kind of birth complications by about one percentage point (a relative increase in the likelihood of about 17 %). We calculate that only a small fraction of this increase in birth complications can be attributed to changes in observable maternal characteristics. It thus appears plausible that some part of the unexplained increase in birth complications is caused by an underlying increase in behavioral and/or psychological risks triggered by the mistiming of pregnancies (while the other part is best viewed as being caused by unobserved maternal characteristics), even though we are not able to exactly pin down the importance of unobserved compositional changes due to selection versus the mistiming of pregnancy due to data limitations (most importantly perhaps, our data contain no information on the health status and health behavior of mothers). The increase in birth complications notwithstanding, we do not find any adverse immediate impact on newborns' health.

On a more general level, our results illustrate that announcement effects may be an important issue in health policy reforms, and the abolition of the Austrian birth benefit clearly shows that even relatively small changes in financial incentives may trigger substantial behavioral responses. Policy makers should thus be aware that not only a policy (reform) itself, but also the public announcement of its abolition (or introduction) may have an impact on individual behavior. Second, our results also suggest that policy announcements may lead some individuals to make bad choices in the sense that they unconsciously take health risks in return for a short-run financial benefit. Even though we cannot pin down the importance of this mechanism exactly, it seems fair to say that dealing with announcement effects appears to be especially important in the context of health policy, both in the planning and implementation as well as in the ex-post evaluation of specific policy measures.

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Appendix: Decomposing the increase in birth complications

To explore whether the increase in birth complications is due to unobserved stress or due to selection, we perform a simple regression-based decomposition analysis based on individual-level data [4, 20]. The goal of this exercise is to determine the impact of selective fertility responses on the likelihood of some birth complication. For the decomposition analysis we simply compare mothers who gave birth in December 1996 with mothers who gave birth in December 1995. Table 8 shows the results (note that the two columns differ only in the weighting scheme used for the decomposition).

Table 8 Oaxaca-blinder decomposition

Dependent variable	Any birth complication	
Prediction December 1996	0.0736***	0.0736***
	(0.0063)	(0.0063)
Prediction December 1995	0.0633***	0.0633***
	(0.0063)	(0.0063)
Difference	0.0104	0.0104
	(0.0069)	(0.0069)
Decomposition		
Explained	0.0014**	0.0012*
	(0.0007)	(0.0007)
	[13.517 %]	[11.359 %]
Unexplained	0.0090	0.0092
	(0.0088)	(0.0090)
Number of observations: December 1996	7,613	7,613
Number of observations: December 1995	7,032	7,032
Weights	Dec. 1995	Dec. 1996

*, **, and *** denote statistical significance at the 10, 5, and 1 % level, respectively. Robust standard errors are in parentheses. The dependent variable is a binary variable indicating any birth complication

In line with the corresponding results from Table 8, the upper part of Table 8 documents a difference in the likelihood of experiencing some birth complication of about one percentage point (in Table 8, however, the difference is not statistically significant). The lower part of the table shows the decomposition results, revealing that about 12–14 % of the observed difference in the probability of some birth complication is explained by differences in observed maternal characteristics between the two groups of mothers. Consequently, 86–88 % of the difference remains unexplained.

The extent to which the unexplained part of the increase in birth complications is driven by an underlying increase in behavioral and/or psychological risks from the mistiming of births depends on whether the included variables describe maternal characteristics comprehensively. If the omitted variables are correlated with responsiveness to the incentive and, therefore, with group affiliation, then the unexplained part of the decomposition might capture not only increased behavioral/psychological risks, but also other unobserved group differences.[24]

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[24] In particular, if responding mothers are of poorer health than average mothers, and poorer health (which is not observed) is positively correlated with the probability for labor complications, then the unexplained part of the differential would be upward biased and, therefore, not only capture increased behavioral/psychological risks triggered by mistimed pregnancies. A second issue is whether the included characteristics are affected by the behavioral/psychological risks themselves (which seems unlikely in our setup, however).

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