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ABSTRACT

The Impact of Eliminating a Child Benefit on Birth Timing and Infant Health

We study the effects of the cancellation of a sizeable child benefit in Spain on birth timing and neonatal health. In May 2010, the government announced that a 2,500-euro universal "baby bonus" would stop being paid to babies born on or after January 1st, 2011. We use detailed micro data from birth certificates from 2000 to 2011, and find that more than 2,000 families were able to anticipate the date of birth of their babies from (early) January 2011 to (late) December 2010 (for a total of about 9,000 births a week nationally). This shifting of deliveries led to a significant increase in the number of low birth weight babies, as well as a peak in neonatal mortality. These results suggest that announcement effects are important in shaping economic decisions and outcomes. They also provide new, credible evidence highlighting the negative health consequences of scheduling births for non-medical reasons.

JEL Classification: H00, H30, J00, J13, J17

Keywords: incentives, policy change, fertility, child health

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1. Introduction

We analyze the short-term effects of the elimination of a large child benefit in Spain in 2010, on the timing of births and the health of newborn babies. The benefit used to pay 2,500 euros to all mothers, right after giving birth. Its cancellation was announced seven months in advance, which generated incentives for families with a due date near the cutoff to anticipate their delivery date in order to qualify for the benefit.

Taxes and benefits have been shown to affect many individual decisions, including labor supply, consumption, and even fertility. Economists have also paid attention to the effects of economic incentives on the timing of events, such as charitable contributions (Randolph, 1995), bequests and inter-vivos transfers (Bernheim et al., 2004), and even marriage (Alm & Whittington, 1997). However, not so much attention has been paid to incentives potentially affecting the timing of births, even though any distortions in this dimension could have important welfare consequences.

There is a consensus in the medical literature that babies that are born "too early" face higher health risks. Low birth-weight and prematurity are associated with a number of negative long-term health, and even economic, outcomes.[1] In normal pregnancies, the delivery date is typically determined by the mother going into labor naturally. However, the timing of birth can be anticipated in at least two ways: a pre-programmed caesarean section, or labor induction.

The early work of Dickert-Colin & Chandra (1999) provided some evidence that economic incentives may indeed affect the timing of births. Using NLSY data, they exploited variation in demographic characteristics as well as changes in the tax system over time, and showed that families with more to gain tax-wise were more likely to give

[1] See for example Currie & Hyson (1999), Behrman & Rosenzweig (2004), Black et al. (2007), Crump et al. (2011), Johnson and Shoeni (2011), and Datta Gupta et al. (2013).

birth in December rather than January. Their results were confirmed in more recent work by LaLumia, Salle and Turner (2013) and Schulkind and Shapiro (2014), although the magnitude of the effect appears to be very small. Schulkind and Shapiro (2014) also look at health outcomes. However, they only have monthly-level data and therefore cannot focus on the days and weeks right around the New Year. In addition, they do not observe how much each family benefits from a December versus a January birth, and need to impute it based on demographic characteristics.

Several recent papers have studied benefit reforms in different countries that generated incentives either to anticipate or to postpone the date of birth. Tamm (2011) and Neugart & Ohlsson (2012) analyze the effects of a reform in the parental leave system in Germany in 2007. The German reform, however, was complicated, with several different components, and generating incentives for some families to postpone and others to anticipate birth, depending on factors not perfectly observable to the researcher. Brunner & Kuhn (2011) study the cancellation of a baby-bonus in Austria, with two caveats. First, their data is aggregated at the monthly level, so that they cannot focus on the dates close to the reform. Second, the benefit elimination was announced 10 months in advance, so that they cannot separate timing-of-birth from fertility effects.

The closest paper to ours is the one by Gans & Leigh (2009), who analyze the increase in the generosity of a child benefit in Australia. With detailed data on births (similar to ours), they find that some families did manage to postpone their birth date in order to qualify for the new benefit. We instead evaluate a *reduction* in a child benefit, larger in magnitude, and generating incentives to *anticipate* rather than postpone births, thus with a larger potential for negative health consequences. Crucially, we have much richer data on family characteristics and health outcomes, which allows us to credibly estimate the health impact of the reform. In particular, they only observe health variables the same year of the reform, so that they cannot use previous years as a benchmark.

Using birth-certificate micro data from 2000 to 2011, we focus on the days and weeks surrounding December 31, for the reform year (2010-11) and using the previous ten years as a benchmark. We find that more than 2,000 births were anticipated in order to receive the benefit (out of about 9,000 weekly births nationally), at least a quarter of them by more than two weeks. This makes it relevant to evaluate any potential health effects as a result of such significant shifting.

We find that, on average, the babies whose delivery was anticipated due to the benefit cancellation were born about 315 grams (10 percent) smaller as a result. In addition, we observe a significant spike in neonatal mortality, with almost 80 newborn babies "too many" not surviving the first 24 hours after delivery as a result of the reform.

We also find that native, older mothers were more likely to react to the benefit cancellation, suggesting that the effect was not driven by families with low socio-economic status. The shifting was also more prevalent for higher-order births and twins. These heterogeneous effects are consistent with the evidence on delivery type. We show that the scheduling of births took place via both c-sections and inductions, and the effect was probably driven by private hospitals.

The remainder of the paper is organized as follows. Section 2 provides more details about the child benefit and its cancellation, and describes the empirical strategy and the data sources. Section 3 presents the results, starting with the timing of births, and following with the effects on neonatal health. Section 4 concludes.

2. Empirical strategy and data

2.1 The benefit cancellation

In 2007, facing a budget surplus and an upcoming election, the Spanish government introduced a new, universal child benefit, which would pay 2,500 euros to all mothers right after giving birth. The new baby-bonus was to be paid to mothers who gave birth from July 1, 2007 onwards. The size of the benefit was large, amounting to almost 5 times the monthly minimum wage of a full-time worker, and more than twice the median monthly earnings of employed women.[2]

Three years later, on May 10, 2010, the benefit was eliminated in one of the first rounds of budget cuts as a result of the recession. The baby-bonus, it was announced, would stop being paid for babies born after December 31, 2010. Thus, the announcement pre-dated the effective cancellation date by almost 7 months.[3]

The elimination of the child benefit could have a range of short and longer-term effects. In particular, it may have discouraged fertility in the medium-term. However, any reduction in fertility as a result of the announcement would have led to fewer births starting 9 months after the announcement at the earliest (February 2011). For ongoing pregnancies, however, the pre-announced cancellation created an incentive for those families with a due date close to the threshold to anticipate their delivery date in order to qualify for the 2,500 Euros.[4]

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[2] González (2013) evaluates the effects of the introduction of the benefit, finding positive fertility effects and a small negative effect on maternal labor supply.

[3] None of the other measures and cuts announced at that time of afterwards would affect children born in January 2011 differently from those born in December 2010, at least not in any obvious way.

[4] In fact, there was quite a lot of discussion in the media at the time regarding this possibility. For example, a headline in the national newspaper ABC on December 30,

2.2 Empirical strategy

Our identification strategy relies on comparing the timing of births and the health of newborns around December 31, 2010, using the previous ten years as a benchmark. If the cancellation of the benefit had an effect on the timing of births, we expect to observe "too many" births in December 2010, and "too few" in January 2011, relative to previous years, as well as possibly "too many" low birth-weight or premature babies close to the turn of the year, and possibly higher neonatal mortality.

Birth timing

We start by evaluating the effect on the timing of births. The analysis is performed both at the aggregated, daily level, and at the individual level. We focus on births taking place in December or January of years 2000-01 to 2010-11. The main equation is the following:

$$(1) \quad Births_{jt} = \alpha + \beta Dec2010_{jt} + \delta_{dw} + \phi_{dy} + \mu_h + \lambda_t + \varepsilon_{jt}$$

Where *Births* is the number (or the log number) of births taking place on day j of year t. Our explanatory variable of interest is a dummy indicating December 2010 births. We include a set of dummies for each day of the week (δ), as well as dummies for day of the year (ϕ), holidays (μ), and year (λ), the year dummies being in fact indicators for each December-January pair. Our full specification, which closely follows Gans and Leigh's (2009), also includes interactions between year and day of the week. The

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2010 read: "*High-risk baby bonus: The end of the 2,500-euro baby bonus raises controversy about mothers seeking to anticipate births*" (own translation). The same newspaper interviewed Beatriz, a mother-to-be due in January 2011, who explained: "*I think it's risky to anticipate your due date, but I understand if a woman with a c-section programmed for January 1, 2, 3 or 4, as long as her doctor approves, asks to have it brought forward a few days. Given the current economic crisis, I understand that people consider it.*" The article also quoted health professionals, with statements such as: "*Anticipating birth is (...) absolutely not advised, since it generates risks for the health of the baby*."

coefficient of interest, β, captures any "extra" births taking place in December 2010, compared with January 2011, and relative to the previous 10 years.[5] If the benefit cancellation affected the timing of births, we expect β to be positive, capturing the number of "extra" daily births.

We estimate this regression on four different samples. First, we limit the sample to only the seven days before and after the turn of the year. Given the potential negative health consequences of manipulating the birth date, we expect most of the action to take place the days immediately surrounding the cutoff date. We then extend the window to two, three, and four weeks before and after.

Additionally, we run the analysis on the same four samples at the individual (instead of daily) level. This allows us to include individual-level demographic controls, as well as to interact the effect of interest with individual characteristics, in order to identify the types of families most likely to have reacted to the reform. The individual-level specification, similar to those in Dickert-Colin & Chandra (1999) and Tamm (2012), is the following:

$$(2) \quad December_{it} = \alpha + \beta Reform_{it} + \gamma X_{it} + \delta t + \varepsilon_{it}$$

The dependent variable is now binary, taking value 1 if birth i in December-January pair t took place in December, and 0 for January births. We expect this variable to average 0.5 in "normal" years. We control for demographic characteristics *X*, and include a linear trend in t. The main explanatory variable, *Reform*, takes value 1 for the reform period, December-January of 2010-11. Thus, a positive β would indicate that there were too many December (versus January) births in 2010-11, compared with the previous ten

[5] Since we have day of the year dummies, no December dummy is needed.

years. In an additional specification, we interact the reform dummy with all the demographic characteristics X.

Neonatal health

Once we have established that many births were in fact anticipated as a result of the reform, we move on to estimating any health effects resulting from the shifting in delivery dates. The health specifications are equivalent to the one shown in equation 2, except that the dependent variable is now one of a range of measures of the health of the newborn.

$$(3) \quad Health_{it} = \alpha + \beta Reform_{it} + \gamma X_{it} + \delta t + \varepsilon_{it}$$

As health indicators, we use the available information on newborns' weight at birth, as well as the number of gestational weeks at birth, and whether the baby survived 24 hours after delivery. On top of birth-weight in grams and its log, we construct several indicators of low birth-weight, as well as a prematurity dummy (for babies born before 37 gestational weeks), and a mortality indicator. All specifications include demographic controls as well as a linear time trend.

The coefficient of interest, β, captures the difference between the average health status of babies born close to December 31, 2010 (the reform period), and that of babies born around the same dates during the previous ten years, allowing for a long-term (linear) trend. Crucially, we are comparing the health of babies born in December-January of 2010-11 with respect to the previous 10 years, as opposed to the health of December versus January babies, since doing so would conflate the impact of the reform with composition effects, due to the differential characteristics of families who switched birth from January to December.

For instance, suppose that only the healthier babies were switched and that they suffered no health effect. Then, December 2010 newborns would be on average healthier than January 2011 ones, giving the impression that the reform improved babies' health. If instead we compare babies born in December 2010 or January 2011 to those born in the same period in previous years, we would rightly conclude that the reform had no effect.[6]

2.3 Data

We use micro data from birth certificates from the Spanish National Statistical Institute. These population-level data provide detailed information on all births taking place in Spain, as recorded in the official national registry. All families are supposed to register their newborn babies within 10 days of birth. The variables made available come from a standardized form that families fill out at the time of registration.

We supplement the publicly available files with information on the exact date of birth for each baby (only month of birth is available in the public files), purchased from the National Statistical Institute for years 2000 to 2011. We focus only on births taking place in December or January.

Descriptive statistics for the main variables of interest are reported in table 1. The main sample includes all births in the last 4 weeks of December or the first 4 weeks of January, for the eleven December-January pairs from 2000-01 to 2010-11. The first panel refers to the data set aggregated by exact date of birth. The number of observations is 616 (28 days, times 2 months, times 11 years). There were on average 1,229 births per day, with a standard deviation of 181.[7]

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[6] The specification used by Schulkind & Shapiro (2014) is similar to ours and addresses composition concerns, while the one in Gans & Leigh (2009) does not.

[7] We also re-do all of the analysis for singleton births only, with all results essentially unchanged.

The second panel reports summary statistics for the individual-level data set. The number of observations (births) is 756,855. On average, almost exactly 50% of all births in the sample took place in December. Average weight at birth is 3,203 grams, with about 8% of babies below 2,500. Also about 8% of babies are born prematurely (before the 37th gestational week), and only about 4 in 1,000 babies do not survive the first 24 hours after delivery. Regarding family characteristics, mothers are on average 31 years old, while fathers' average age is 33.5. About 16% of mothers are born outside of Spain. Almost 56% of births are first-born babies for the mother, and 2% are multiple births.

3. Results

3.1 Birth timing results

We start by providing some graphical evidence on the impact of the benefit cancellation on the timing of births around the cutoff date. Figure 1 displays the weekly number of registered births in Spain during the last four weeks and the first four weeks of the year, for December-January pairs 2008-09, 2009-10 and 2010-11. In the two years before the reform, between 8,800 and 8,900 births took place during the last week of December, down to about 8,650 during the first week of January. In comparison, a week before the benefit was eliminated in December 2010, the number of births was almost 10,000, down to less than 7,850 the first week of January 2011. The gap in the number of births between those two weeks, which was only about 200 births during "normal" years, increased to more than 2,000 surrounding the benefit cancellation. Note that the second-to-last week of the year 2010 was also above the "normal" number of births, while the second week of 2011 was also below the level in previous years. These numbers suggest that there was probably some shifting of births from early January 2011 to late December 2010.

We now formalize this observation with our regression analysis. Table 2 shows the results of estimating equation 1 on the four samples, extending the window from one to four weeks before and after the cutoff date for benefit eligibility, in order to capture possible effects as far from the cutoff as four weeks.

The first column includes only the 7 days before and after the cutoff, thus the number of observations is 14 days times 11 years (N=154). The first row uses daily number of births as the dependent variable. The result suggests that there were 294 "extra" daily births in the last week of December 2010. The coefficient is estimated with high precision, and it translates into more than 1,000 births shifted from January to December.[8] The second row uses the natural log of the number of births as a dependent variable, and it estimates that about 12% of births were anticipated from the first week of January to the last week of December 2010.

The second column expands the window to two weeks before and after the cutoff date for benefit eligibility. The daily number of "extra" December births goes down, suggesting that most of the shifting took place within the 14 days around the cutoff, but the estimated total number of births moved increases to 1,491. This indicates that quite a number of births (more than 400) were shifted by more than one week. Once we include all four weeks before and after (last column), we estimate that about 2,086 births were anticipated from January 2011 to December 2010, or about 6% of all January births.

[8] The coefficient is multiplied by 7 since there are 7 days pre-cutoff in the sample, and then it is divided by 2 since each extra birth in December is "counted twice" as it corresponds to one less birth in January.

These results are not overly sensitive to the set of dummy variables included as controls. Table A1 in the appendix shows the results of several alternative specifications for the one-week window sample. The estimated number of births moved fluctuates only between 987 and 1029 (11-12%).

The daily number of births in December and January, for the reform year as well as the previous one, can be seen in figure 2. In 2009-10 (first panel), daily births fluctuated between 1,100 and 1,500, with a minimum of 999 on December 25 and a maximum of 1,540 on December 29. There are fewer births during weekends, especially Sundays (around 1,100). The second panel corresponds to the reform period. It is easy to see that the number of births was unusually high during the last two weeks of December, reaching almost 1,700 on some days (except for Sundays, which remained around 1,100), while there were clearly "too few" births during the first two weeks of January, reaching a minimum at 877 on January 2. All Sundays in January 2011 were lower than usual, at around 1,000 births.

The dynamics of the shifting of births are better appreciated when we estimate the regression described in equation 1, but instead of a single December 2010 dummy, we include four dummies for the last 4 weeks of December 2010, as well as four dummies for the initial 4 weeks of January 2011.[9] The results of these specifications are reported in table 3. It is now evident that the "extra" December births took place during the last two weeks of the year, while there were significantly "too few" births extending up to the third week of January. This implies that, while most of the shifting probably involved no more than a few days, at least some births were anticipated by as much as

[9] In these specifications, the sample includes all births in December and January of the eleven December-January pairs, i.e. 31 days before and after the cutoff. Thus, the reference period includes December 1-3 and January 29-31 for the eleven sample years.

three weeks. According to the results in table 3, around a quarter of all shifted births ((72.8*7)/2000) were anticipated by more than two weeks.

Before turning to the health effects, we present the results for the effect on birth timing using the individual-level sample. We estimate equation 2, where the dependent variable is an indicator for births that took place in December (rather than January). Again, the regressions are estimated for the four samples progressively widening the window around the cutoff date for benefit eligibility. The average value of the dependent variable for each of the 11 years is displayed in figure 3. During the decade 2000-2010, between 49% and 51% of births taking place within a week of the New Year happened in December, as expected. The reform year is clearly an outlier, with 56% of births taking place in December.

This observation is confirmed by our regression results. Table 4 reports the results of estimating equation 2 for the four different samples. The first row includes demographic controls and a linear time trend. The controls included are: mother and father's age, mother's immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, and dummies for first-borns, female babies, and multiple births (see table 1 for descriptive statistics and the appendix for details on variable construction).

The results confirm the graphical evidence: as shown in the first panel of table 4, in the reform year, there were about 6 percentage points more births in the last week of December compared to the first of January, relative to the previous 10 years. The magnitude of the effect goes down as we widen the window, but it remains statistically significant and sizeable: more than 3 percentage points even when we consider four weeks before and after.

The second panel of table 4 shows the results of interacting all the control variables with the reform dummy, in order to detect the characteristics of the families that were "over-represented" in the December births of 2010. We report only the interactions that are significantly different from zero. The results suggest that mothers older than 35 were more likely to react to the benefit cancellation (by 2 percentage points, relative to younger women). The shifting also appears less common among immigrant mothers (by almost 3 percentage points). These two results together suggest that the scheduling of births was not driven by women with low socio-economic status.

First births react less than higher-order ones, and singleton births less than multiples. These effects are probably driven by twins and higher-order births being more likely to be scheduled (in normal times) than singletons and first births, which would make it easier to manipulate the delivery date.[10]

3.2 Health results

We have estimated that more than 2,000 births were anticipated in order to receive the 2,500-Euro benefit. Most were probably shifted by no more than a few days, but at least some were moved by as much as three weeks. We next evaluate the impact of this tinkering with the timing of births on the health of the newborns. The birth-certificate data provide us with three variables related to babies' health: their weight at birth, the number of gestational weeks at birth, and whether the baby survived the first 24 hours after delivery.

[10] Previous work (Lalumia et al. 2013, Aron et al. 1998, Patel et al. 2005) has documented a higher incidence of c-sections among older women, higher-order births, and multiples, from which they infer a higher incidence of scheduled labor among these groups.

We estimate equation 3, where the dependent variable is a measure of newborn's health, on the individual-level data set that includes only births taking place near December 31, during the eleven years of data. All specifications include demographic controls as well as a linear time trend (see table 1 for descriptive statistics). As mentioned, the coefficient of interest β captures any deviations from trend in the average health of newborns around the New Year in the reform period.

We first report the results for birth-weight. Previous research has found that low birth-weight has important long-term consequences on both health and economic outcomes (Behrman & Rosenzweig 2004, Black et al. 2007, Johnson and Shoeni 2011, Figlio et al. 2013). The results are shown in table 5 for the four different samples, from 1 to 4 weeks away from the threshold. The dependent variable in the first row is just the continuous birth-weight variable. When looking at the 7-day window, we find that newborns were on average 18 grams smaller in the reform period.

Although this effect may seem small, it is worth remembering that only 12% of babies in this sample were "affected" by the benefit cancellation (see table 2). Thus, an 18-gram average effect for all newborns implies that affected babies were on average around 315 grams smaller (about a 10% effect).[11] The estimated magnitude of the effect is very similar if we take the two-week window sample, while it becomes smaller as we widen the window.

We also find significant results when we use the natural log of birth-weight as the dependent variable (second row of table 5). Birth-weight in logs is the variable that

[11] According to the results in table 2, 1,029 births were anticipated in the +/- 1-week window, out of a total of 17,791 births in the 14-day period (5.78%). Thus, for those babies who were shifted, the effect was -18.2/0.0578 = -315 grams. For the 2-week window, the estimated effect for shifted babies is -12.97/0.041 = -316 grams.

other papers typically use when studying the medium- and long-term effects of birth-weight (Black et al. 2007, Figlio et al. 2013).

The medical literature seems to agree that babies born below 2,500 grams face significant health risks (Hack et al. 2003, Elgen et al. 2005). Thus, we also use as a dependent variable an indicator for babies born below 2,500 grams. The results are reported in the third row of table 5. We do find that there were more low-birthweight babies in the reform period, but the results are not statistically different from zero at standard confidence levels (for the one-week window sample, the effect is significant at 90%).

Next, we define two additional thresholds (2,750 and 3,000 grams), which we call "borderline low birthweight". The results of using those thresholds are reported in the last two rows of table 5. We do find that the reform led to a significant increase in the number of babies born below 2,750 and 3,000 grams (for a mean birth-weight of 3,203). In addition, we estimate quantile regressions for birth-weight, and report the results in appendix table A2. The negative effect on birth-weight is found across all deciles of the distribution, but it is only weakly significant for the first decile.

Taken together, the results suggest that the shifting of birth dates led to a significant reduction in birth-weight for the affected babies, although most of the shifting did not take place at the bottom end of the weight distribution, where it probably would have been the most harmful.

This is confirmed by our results on weeks of gestation. Babies are considered "full-term" at 37 gestational weeks, and prematurity is associated with negative health outcomes (Crump et al. 2011). We create a prematurity indicator, taking value 1 for babies born before week 37 of the pregnancy. The regression results when using prematurity as a dependent variable are reported in the first row of table 6. We find that there was no significant increase in the number of premature babies around the reform date.[12]

Finally, as an extreme measure of health, we also evaluate the effect of the reform on neonatal mortality, defined as the newborn not surviving the 24 hours after delivery. This is the case for about 4 in 1,000 babies (see table 1), and it includes deaths that take place during delivery.

The results of the mortality regressions are reported in the second row of table 6. We find that the reform was associated with a significant increase in neonatal mortality. The magnitude of the estimated effect is large: about 1 in 1,000 babies, more than a 20% increase. Considering that there were almost 73,000 babies born in the eight weeks surrounding the benefit reform, the results for the 4-week window (column 4) suggest that the benefit elimination led to an additional 78 newborn deaths.[13]

We can use our mortality estimates to calculate the implied "value of a statistical life" (Blomquist 2001, Viscusi and Aldy 2003). Receipt of a 2,500-euro benefit led to an increase in the mortality risk of 1 in 1,000. This implies a value of a statistical life of 2.3 million Euros (using the 4-weeks window results).[14] This figure is in the ballpark of previous estimates for children. For example, Blomquist et al. (1996) provide an estimate of €3.7 million ($2.9 million in 1991 dollars), estimated from willingness to pay for children's safety seats. Similarly, Mount et al. (2000) report between €2.6

[12] Note, however, that our gestational age variable is based on the reported date of the mother's last menstrual period, so that it is most likely subject to substantial measurement error (Lynch & Zhang 2007, Hall et al. 2013).

[13] 72,771 births, times 0.00107.

[14] 2500/0.00107. Note that we are using a willingness to accept measure of the value of a statistical life, which is usually found to be higher than willingness to pay measures (Guria et al. 2005).

and €7.8 ($2.6 and $7.7 million 2000 dollars), estimated from willingness to pay for cars' safety features.[15]

In order to lend support to the credibility of our health results, we run additional "placebo" regressions, where we exclude the reform period from the estimation sample and re-estimate all regressions in tables 5 and 6, defining a fake "reform" dummy for December-January of the previous year, 2009-10. The results of the placebo regressions are reported in appendix table A3. We find no significant "effects" on any of the birth-weight variables (except for a small negative effect when using the 3- or 4-weeks samples for the continuous weight variables). The prematurity coefficient is significantly negative for the two-week sample and non-significant for the remaining three windows. Finally, the mortality coefficient is half the size as in the main regressions, and not statistically different from zero (except for the last two columns).

Overall, the results suggest that the anticipation of birth dates as a result of the benefit cancellation led to significantly smaller babies, as well as a significant spike in neonatal mortality.[16]

3.3 Channels: Delivery method and private hospitals

In this section we try to provide more insight into how families managed to bring forward their delivery date. In order to do so, we explore how many of the shifted births

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[15] All figures in 2013 Euros.

[16] We also estimate the health effects (equation 3) with interactions between the reform dummy and family characteristics. The results are presented in appendix table A4, and are roughly consistent with the timing results in table 4. The effect on birth weight is stronger among married, urban women. We find an increased likelihood of a premature birth among twins. The mortality increase is more pronounced for women over 25 and higher-order births.

took place via a c-section, and whether public versus private health centers were more likely to agree to tinkering with the timing of childbirth.

Inductions and c-sections

The date of birth for a pregnant woman can be anticipated medically either by inducing birth or via an elective c-section. Both of these methods appear to have become more popular in recent years. Labor induction consists of administering the pregnant woman certain hormones (prostaglandin, oxitocin) that trigger childbirth. This is usual practice when the woman has reached the end of the 42nd week of pregnancy. In the US, induced births reached 23% of all births in 2010, up from 9.5% in 1990 (US Census Bureau).

An elective caesarean section can take place for medical reasons or on maternal request, which has also become more frequent, as the procedure has become safer. The rate of c-sections as a fraction of all births increased from 21 to 33% in the US between 1996 and 2009 (US Census Bureau), and from 19 to 25% in Spain (Spanish Ministry of Health).

Either of these two methods could have been used by women to bring forward their date of birth. Both are considered risky if performed for non-medical reasons (Thomson et al. 2002, Belizán et al., 2007). Note also that elective induced labor can lead to an unanticipated c-section (Stock et al. 2012), so these two procedures are not exclusive.

Our birth-certificate data do not provide information on whether the birth was spontaneous or induced. We do observe whether delivery took place via a c-section, starting in 2007. However, at first this variable had many missing values and it was imputed by the National Statistical Institute (NSI). The NSI refused to provide us with an indicator for observations with imputed delivery method, or with information regarding the imputation procedure. For these reasons, the analysis of c-sections should be treated with caution.

We estimate equation 3 using an indicator for c-sections as the dependent variable, on the sample including only 2007-08 to 2010-11 December-January pairs. The results can be found in the first row of table 7. We find that the incidence of c-sections was significantly higher during the reform period (December 2010-January 2011), compared with the previous three years, by more than 5 percentage points (for an average of 22%). The benefit cancellation thus seems to have increased the number of babies born via c-section.

We also expect that some families that would have had a c-section in any case (such as many of those expecting twins) may have shifted it from January to December. Thus, we re-estimate the regressions, adding a dummy for December 2010. The coefficients for this "difference-in-differences" analysis are reported in the second row of table 7. They suggest that in fact there was some shifting of c-sections from January to December in the reform year.

We thus confirm that some of the changes in the timing of births as a result of the benefit cancellation can be attributed to an increase as well as a shifting of c-sections. However, c-sections cannot account for all of the effect, since we observe the same pattern over time in births that did not require a c-section (see appendix figure A1). This suggests that birth induction was also used as a tool to trigger childbirth before the cutoff date.[17]

Public versus private hospitals

It appears that private clinics in Spain are more open to scheduling childbirth for non-medical reasons compared to public hospitals, as reflected in their higher c-section rates (Redondo et al. 2013). In 2009, the c-section rate was 22% of all births in the public

[17] It is also possible that the common pattern observed in figure A1 is driven by incorrect imputation of c-sections by the Spanish Statistical Institute.

health system, versus 37% in private health centers (Spanish Ministry of Health).[18] If this is the case, then we would expect to see more shifting among births taking place in the private sector.

The birth certificate data do not contain information on the type of health center where a birth takes place. However, we obtained information from an independent data source (the National Catalogue of Hospitals, 2000-2011, from the Spanish Health Ministry) on the number of private clinic beds across the 52 Spanish provinces and over time. If the shifting took place mostly among women giving birth in private hospitals, we expect to see more action in the provinces with more private hospital beds.

In order to test this hypothesis, we re-estimate equation 2, including also the interactions between the reform variable and all the controls (as reported in the second panel of table 4). We now add province dummies, as well as a new variable measuring the fraction of private hospital beds in each province, and an interaction of the reform variable with the fraction of private beds (standard errors are now clustered by province). The results are reported in table 8.

We find that the increase in December 2010 relative to January 2011 births was significantly more pronounced in provinces with more private hospital beds, even after controlling for province fixed-effects and interactions between the reform and individual characteristics. The results suggest that a province in the 75th percentile of private hospital beds (about 35%) had a spike in December 2010 births about 2.3 percentage points higher than a province in the 25th percentile (10% of private hospital

[18] In a context of public, universal healthcare, higher rates of c-sections in private hospitals in Spain is consistent with the lower incidence of c-sections among the uninsured in the US (Aron et al., 2000).

births). These results are consistent with private hospitals being more willing to adjust the date of birth on parental request.

All in all, our results suggest that the timing effect (and the resulting health impacts) took place at least in part via scheduled c-sections in private hospitals. This is consistent with the heterogeneous effects presented in table 4. Private hospitals are used to a greater extent by native women of higher socio-economic status, and both c-sections and inductions are more common among older mothers, higher-order births, and twin births.

4. Conclusions

We analyze the effects of the cancellation of a child benefit in Spain on the timing of births and the health of newborns. We exploit individual-level birth certificate data, focusing on births very close to the cutoff date. We find that many families were able to bring forward their date of birth in order to qualify for the 2,500-Euro benefit. Although most of the shifting was probably just a few days, some births were shifted by as much as three weeks.

We also find that this bringing forward of delivery dates had significant health consequences for the affected babies. Strikingly, we find a significant spike in neonatal mortality right around the benefit cancellation date, leading to almost 80 deaths “too many”, relative to the same dates in previous years. We also find that babies who were born earlier as a result of the reform were significantly lighter at birth, by about 315 grams (or 10 percent) on average. Previous research suggests that low birth-weight can have important long-term consequences on many relevant outcomes, such as adult height, IQ, educational attainment and earnings. The estimates by Black et al. (2007)

suggest that 10% lower birth-weight would decrease high school completion rates for the affected children by almost 1 percentage point, and lower their full-time earnings by about 1%.

Our results suggest that announcement effects are important. The government announced the benefit cancellation seven months in advance, with a single cutoff date, so that babies born on December 31, 2010 were entitled to 2,500 Euros, while those born on January 1, 2011 would receive 0. It would perhaps have been advisable to devise a not-so-steep cancellation mechanism, so that, for instance, the benefit amount could have declined more slowly over time.

These findings also highlight the fact that parents may be willing to trade-off income and health, at least to some extent. Note that the most vulnerable babies were not affected, since we find no increase in prematurity rates, and only a small increase in the number of babies with very low birth weight.

It would be interesting to know more about the interaction and bargaining process between families and health professionals. Media reports at the time make it clear that doctors were well aware of the health risks associated with shifting the timing of births. How did (some) families manage to convince their health providers to anticipate childbirth?[19] Did doctors have any economic incentive to do so? These are all interesting questions that are yet to be answered.

[19] Faking the date of birth in the birth certificate would be difficult. However, families could have convinced hospitals to change the exact time of birth reported, for babies born close to midnight on the cutoff date. However, this seems unlikely to have happened in practice, since the spike in births in December 2010 did not take place exactly on December 31-January 1, but was instead quite spread over the two weeks before and after.

Finally, our results provide new, credible empirical evidence supporting the widely held belief in the medical community that scheduling births for non-medical reasons can have important health consequences for babies.

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Data appendix

Here we provide some details on the construction of the control variables in the individual-level analysis. The averages reported are for the sample of all births in the last 4 weeks of December or the first 4 weeks of January, for December-January pairs from 2000-01 to 2010-11 (as in table 1).

Marital status: We include a dummy for married mothers (71%), and a dummy for babies with no registered father (1.7%).

Urban: We construct an indicator that takes value 1 if the birth is registered either in the main city (the capital) of each province, or in a town larger than 100,000 inhabitants (43%).

Occupation: We include 6 occupation dummies for each of the parents. The categories include: homemakers, managers, high-skill professionals, administrative jobs, service occupations, and production jobs.

Female: 48.5% of babies in the sample are female.

Table 1. Descriptive statistics

	Average	Stdev.	Min	Max
Daily data				
(N=616)				
N. births per day	1229	181	806	1683
Year	2005.5	3.2	2000	2011
Individual-level data				
(N=756855)				
December birth	0.4995	0.5	0	1
Year	2005.7	3.16	2000	2011
Birth weight	3203	540	4	6560
BW<2,500	0.081	0.273	0	1
BW<2,750	0.157	0.36	0	1
BW<3,000	0.297	0.457	0	1
Premature (<37w.)	0.081	0.273	0	1
Mortality (24h.)	0.0041	0.0637	0	1
Mother's age	30.82	5.33	12	55
Father's age	33.49	7.26	14	83
Mother<25	0.127	0.333	0	1
Mother >35	0.186	0.389	0	1
Immigrant mother	0.161	0.367	0	1
First birth	0.558	0.497	0	1
Twins	0.0197	0.139	0	1

Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011. The sample includes all births in the last 4 weeks of December or the first 4 weeks of January, for December-January pairs from 2000-01 to 2010-11.

Table 2. The effect of the benefit cancellation on the timing of births

	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Dep. var.: Number of births								
	294	***	213	***	182	***	149	***
	(32,7)		(23,4)		(18,9)		(16,9)	
Number of births moved	1029		1491		1911		2086	
Dep. var.: ln(number of births)								
	0,228	***	0,163	***	0,139	***	0,115	***
	(0,026)		(0,019)		(0,015)		(0,013)	
Share of births moved	12%		8%		7%		6%	
N	154		308		462		616	
Year dummies	Y		Y		Y		Y	
Day of week dummies	Y		Y		Y		Y	
Holiday dummy	Y		Y		Y		Y	
Year*day of week	Y		Y		Y		Y	
Day of year dummies	Y		Y		Y		Y	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. An observation is a day. The sample includes all births in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2010-11. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). Standard errors are shown in parentheses.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table 3. Medium-run effects of benefit cancellation on the timing of births

Dep. var.	**Number of births**		**ln(number of births)**	
December 4-10	0,747		0,0063	
	(33,98)		(0,03)	
December 11-17	45,25		0,0362	
	(33,98)		(0,03)	
December 18-24	60,58	*	0,045	*
	(33,98)		(0,03)	
December 25-31	160,7	***	0,1206	***
	(33,98)		(0,03)	
January 1-7	-123,75	***	-0,0992	***
	(33,99)		(0,03)	
January 8-14	-80,38	**	-0,0614	**
	(33,98)		(0,03)	
January 15-21	-72,8	**	-0,0555	**
	(33,98)		(0,03)	
January 22-28	-50,22		-0,0361	
	(33,98)		(0,03)	
N	682		682	
Year dummies	Y		Y	
Day of week dummies	Y		Y	
Holiday dummy	Y		Y	
Year*day of week	Y		Y	
Day of year dummies	Y		Y	

(*** 99%, ** 95%, * 90%)

Note: Each column corresponds to a different regression. An observation is a day. The sample includes all births in December and January, for December-January pairs from 2000-01 to 2010-11. The coefficients shown correspond to a set of binary explanatory variables indicating the week of birth for 2010-11 births (the period right around benefit cancellation). Standard errors are shown in parentheses.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table 4. Heterogeneous effects of benefit cancellation on the timing of births

	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Baseline model								
Reform	0,0619	***	0,0486	***	0,0403	***	0,0338	***
	(0,0046)		(0,0032)		(0,0026)		(0,0023)	
Model with interactions								
Reform	0,0755	***	0,0575	***	0,0495	***	0,0372	***
	(0,0123)		(0,0086)		(0,0070)		(0,0061)	
Reform*	0,0233	**	0,017	**	0,0105	*	0,0131	***
Mom over 35	(0,0097)		(0,0068)		(0,0055)		(0,0048)	
Reform*	-0,0261	***	-0,0161	**	-0,012	**	-0,0073	
Immigrant mom	(0,0101)		(0,0071)		(0,0058)		(0,0051)	
Reform*	-0,0227	***	-0,0118	**	-0,0084	*	-0,0027	
First birth	(0,0082)		(0,0057)		(0,0047)		(0,0041)	
Reform*	0,0651	**	0,0264		0,0261	*	0,0181	
Twins	(0,0267)		(0,0185)		(0,0152)		(0,0133)	
N	181626		375269		569316		756855	
Year trend	Y		Y		Y		Y	
Controls	Y		Y		Y		Y	

(*** 99%, ** 95%, * 90%)

Note: Each column in each of the two panels comes from a different regression. An observation is an individual birth. The sample includes all births in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2010-11. The dependent variable is a binary indicator for December births. "Reform" is a binary explanatory variable indicating December 2010- January 2011 births (the weeks right around benefit cancellation). Standard errors are shown in parentheses. Control variables include mother and father's age, mother's immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table 5. The effect of benefit cancellation on birth-weight

	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Dep. var.: Birth weight	-18.18	***	-12.97	***	-6.712	**	-4.528	*
	(4.782)		(3.319)		(2.690)		(2.334)	
Dep. var.: Birth weight (in logs)	-0.00664	***	-0.00483	***	-0.00261	***	-0.00187	**
	(0.00178)		(0.00123)		(0.00099)		(0.00086)	
Dep. var.: BW<2,500	0.00414	*	0.00206		0.00112		0.00068	
	(0.00238)		(0.00166)		(0.00134)		(0.00117)	
Dep. var.: BW<2,750	0.00827	***	0.00398	*	0.00311	*	0.00205	
	(0.00318)		(0.00221)		(0.00179)		(0.00155)	
Dep. var.: BW<3,000	0.01030	**	0.00632	**	0.00448	*	0.00223	
	(0.00407)		(0.00283)		(0.00230)		(0.00199)	
N	175751		363355		551738		733657	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. An observation is an individual newborn baby. The sample includes all babies born in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2010-11. The coefficients correspond to a binary explanatory variable indicating December 2010- January 2011 births (the weeks right around benefit cancellation). Standard errors are shown in parentheses. Control variables include mother and father’s age, mother’s immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table 6. The effects of the benefit cancellation on prematurity and neonatal mortality

	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Dep. var.: Prematurity	0.00247		-0.00065		-0.00162		-0.00078	
(under 37 weeks)	(0.00233)		(0.00162)		(0.00131)		(0.00114)	
Dep. var.: Mortality	0.00130	**	0.00127	***	0.00118	***	0.00107	***
(first 24 hours)	(0.00057)		(0.00039)		(0.00032)		(0.00028)	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. An observation is an individual baby. The sample includes all babies born in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2010-11. The coefficients correspond to a binary explanatory variable indicating December 2010- January 2011 births (the weeks right around benefit cancellation). Standard errors are shown in parentheses. Control variables include mother and father's age, mother's immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table 7. The effect of benefit cancellation on the incidence of caesarean sections

	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Reform (Dec+Jan)	0,0545	***	0,0506	***	0,0540	***	0,0501	***
	(0,0056)		(0,0040)		(0,0032)		(0,0028)	
December 2010 (DiD)	0,0327	***	0,0391	***	0,0476	***	0,0458	***
	(0,0069)		(0,0049)		(0,0040)		(0,0034)	
N	70518		145225		220352		292828	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. An observation is an individual birth. The sample includes all births in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2010-11. The coefficients in the first row correspond to a binary explanatory variable indicating December 2010- January 2011 births (the weeks right around benefit cancellation). The coefficients in the second row correspond to a binary variable indicating December 2010 births. Standard errors are shown in parentheses. Control variables include mother and father's age, mother's immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table 8. The effect of benefit cancellation on birth timing, by availability of private health centers in the province

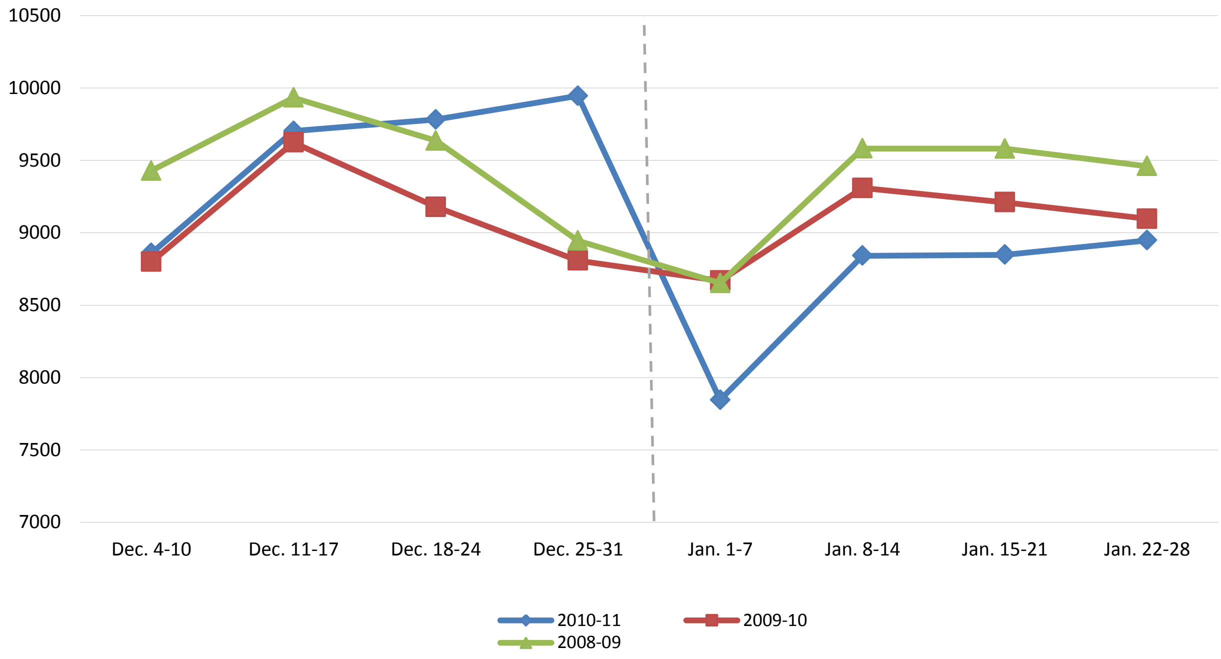
	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Reform*Fraction private beds in province	0,0918	**	0,0865	**	0,0880	**	0,0861	**
	(0,0423)		(0,0393)		(0,0372)		(0,0369)	
Province fixed effects?	Y		Y		Y		Y	
All interactions? (between "Reform" and controls)	Y		Y		Y		Y	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. An observation is an individual birth. The sample includes all births in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2010-11. "Reform" is a binary explanatory variable indicating December 2010- January 2011 births (the weeks right around benefit cancellation). Standard errors are shown in parentheses. Control variables include mother and father's age, mother's immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend. Standard errors are clustered by province

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011, and National catalogue of Hospitals, Spanish Ministry of Health, 2000-2011.

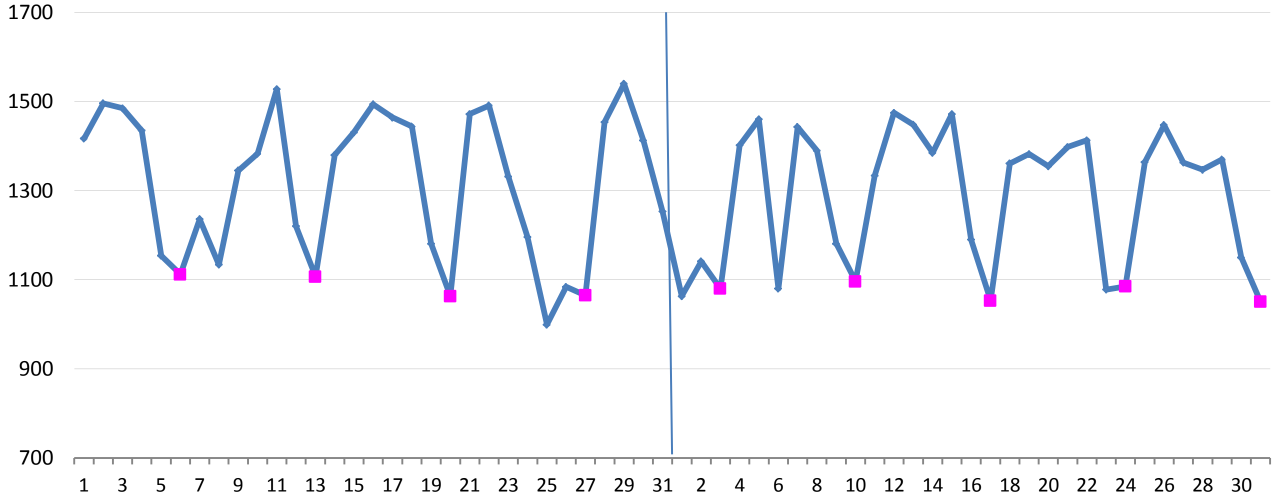
Figure 1. Number of births by week, Spain, December-January of 2008-09 to 2010-11



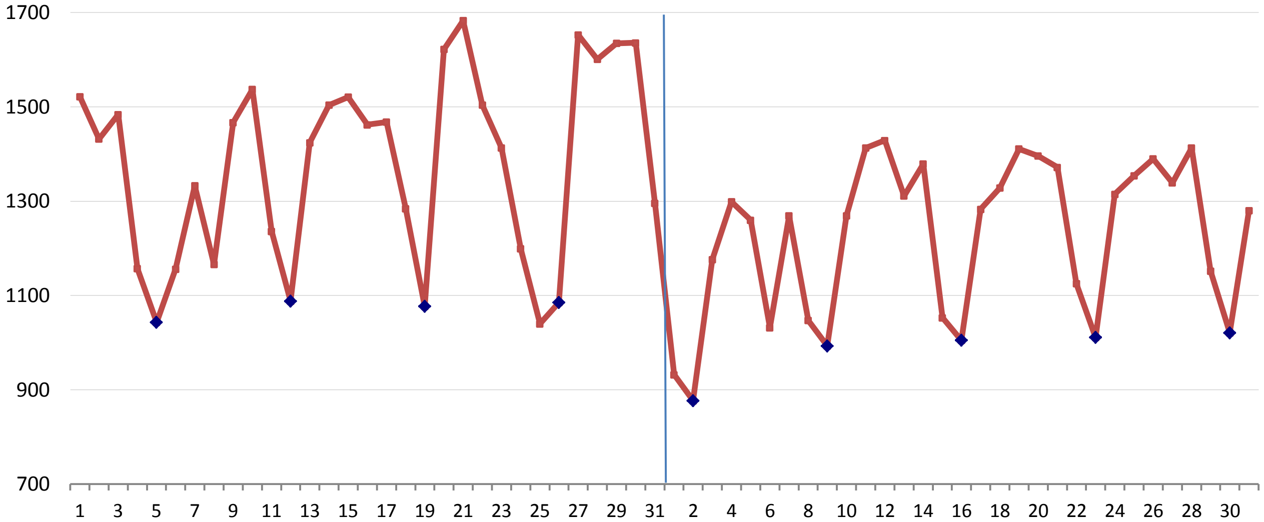
Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Figure 2. Number of births by day, Spain, December-January of 2009-10 and 2010-11

(2009-10)



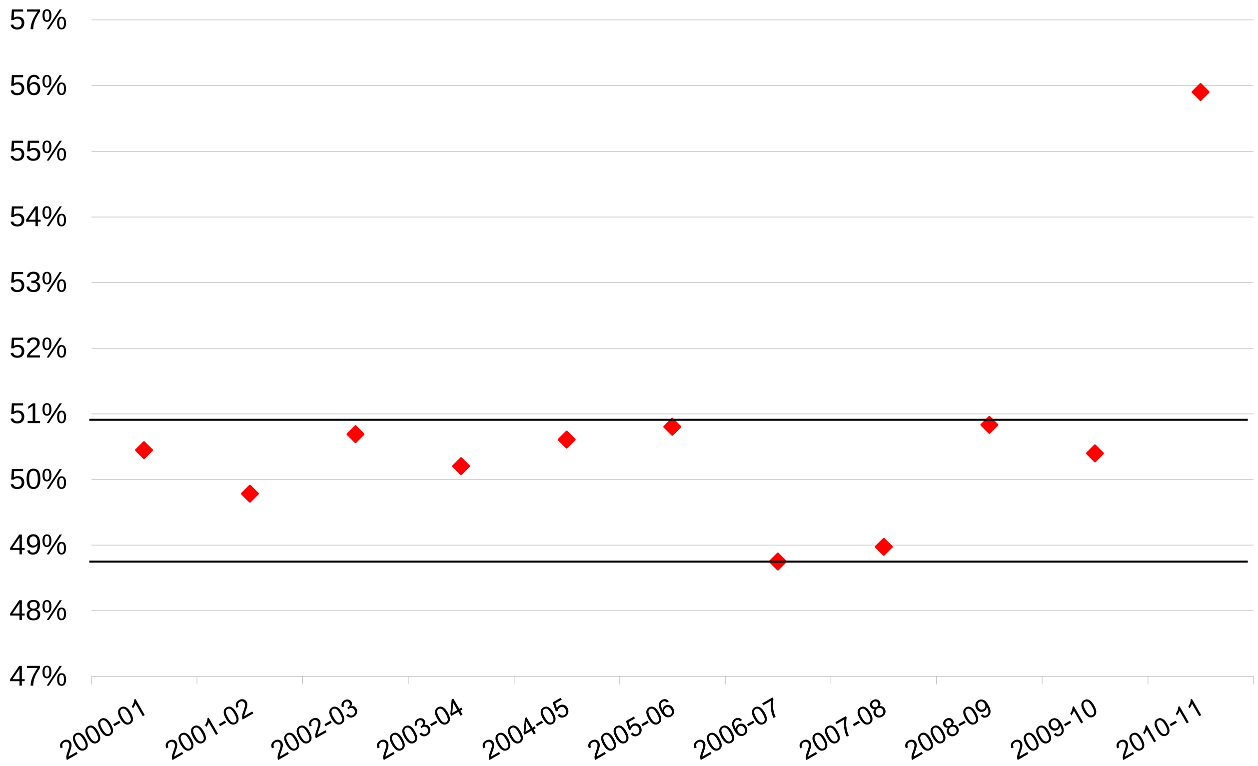
(2010-11)



Note: Sundays are highlighted.

Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Figure 3. Percentage of all births between December 25 and January 7 that take place in December, Spain, 2000-01 to 2010-11



Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table A1. The effect of benefit cancellation on the timing of births: Alternative specifications

(+/-1 week)	**1**		**2**		**3**		**4**	
Dep. var.: Number of births								
	282	***	285	***	282	***	294	***
	(49,5)		(40,4)		(39,0)		(32,7)	
Number of births moved	987		997,5		987		1029	
Dep. var.: ln(number of births)								
	0,218	***	0,221	***	0,217	***	0,228	***
	(0,040)		(0,033)		(0,031)		(0,026)	
Share of births moved	12%		12%		11%		12%	
N	154		154		154		154	
Year dummies	Y		Y		Y		Y	
Day of week dummies	Y		Y		Y		Y	
Holiday dummy	Y		Y		Y		Y	
Year*day of week	N		Y		N		Y	
Day of year dummies	N		N		Y		Y	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. An observation is a day. The sample includes all births in the last week of December or the first week of January, for December-January pairs from 2000-01 to 2010-11. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). Standard errors are shown in parentheses.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table A2. The effect of benefit cancellation on birth-weight: Quantile regressions

(+/-1 week)	p10		p20		p30		p40		p50		p60		p70		p80		p90	
Dep. var.: Birth weight	-16.86	*	-20.33	***	-13.66	***	-10.18	**	-11.54	**	-18.64	***	-20.78	***	-18.62	***	-18.76	**
	(8.782)		(6.222)		(5.292)		(5.125)		(5.138)		(5.492)		(5.327)		(6.087)		(7.559)	
Dep. var.: Birth weight (in logs)	-0.00623	*	-0.00715	***	-0.00467	***	-0.00328	**	-0.00359	**	-0.00557	***	-0.00604	***	-0.00509	***	-0.00494	**
	(0.00329)		(0.00222)		(0.00180)		(0.00163)		(0.00164)		(0.00165)		(0.00157)		(0.00169)		(0.00197)	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. Column headers refer to the different deciles of the distribution (“p50” is the median). An observation is an individual birth. The sample includes all births in the last week of December or the first week of January, for December-January pairs from 2000-01 to 2010-11. The coefficients correspond to a binary explanatory variable indicating December 2010- January 2011 births (the weeks right around benefit cancellation). Standard errors are shown in parentheses. Control variables include mother and father’s age, mother’s immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table A3. Placebo health regressions

	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Dep. var.: Birth weight	0.343		-5.777	*	-6.726	**	-4.866	**
	(4.853)		(3.377)		(2.737)		(2.378)	
Dep. var.: ln(birth weight)	0.00511		-0.00196		-0.00235	**	-0.00191	**
	(0.00176)		(0.00123)		(0,00100)		(0.00087)	
Dep. var.: BW<2,500	-0.00209		0.00172		0.00178		0.00178	
	(0.00240)		(0.00168)		(0.00119)		(0.00119)	
Dep. var.: BW<2,750	-0.00229		0.00181		0.00255		0.00183	
	(0.00323)		(0.00225)		(0.00183)		(0.00159)	
Dep. var.: BW<3,000	-0.00267		0.00103		0.00161		-0.00020	
	(0.00416)		(0.00289)		(0.00235)		(0.00203)	
Dep. var.: Prematurity	-0.00609	***	-0.00167		-0.00075		-0.00042	
(under 37 weeks)	(0.00235)		(0.00165)		(0.00135)		(0.00117)	
Dep. var.: Mortality	0.00055		0.00066	*	0.00070	**	0.00075	***
(first 24 hours)	(0.00055)		(0.00039)		(0.00032)		(0.00028)	

(*** 99%, ** 95%, * 90%)

Note: Each coefficient comes from a different regression. An observation is an individual birth. The sample includes all births in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2009-10 (the reform period has been excluded). The coefficients correspond to a binary explanatory variable indicating December 2009- January 2010 births. Standard errors are shown in parentheses. Control variables include mother and father’s age, mother’s immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table A4. Heterogeneous effects of the benefit cancellation on neonatal health

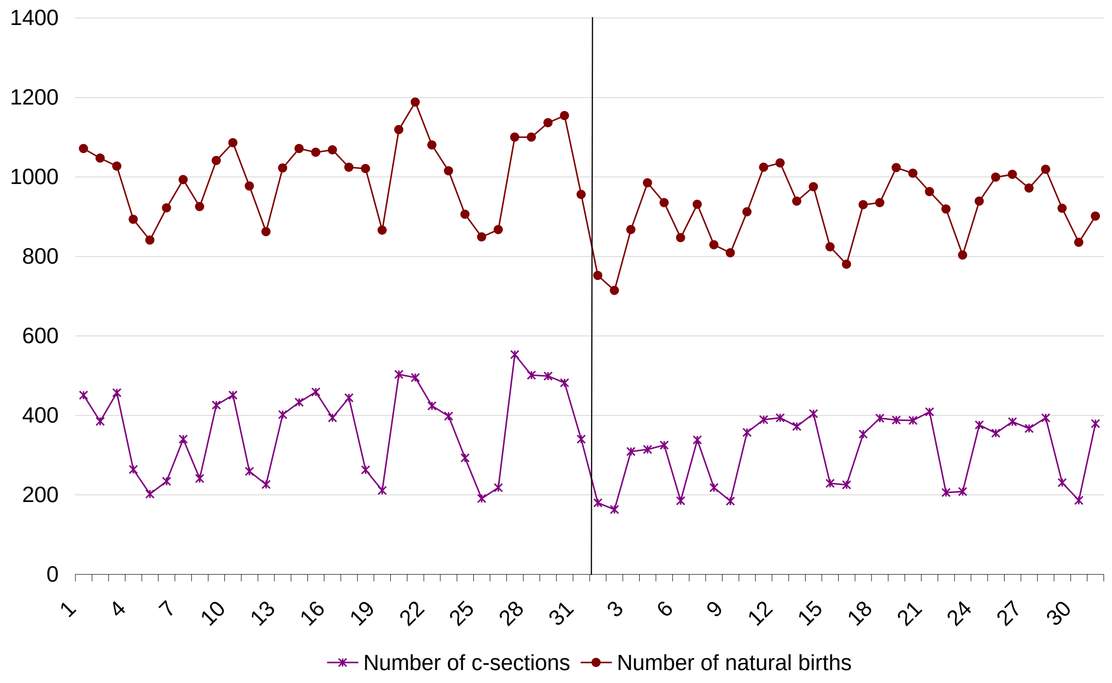
	Birth weight (in logs)		**Prematurity**		**Mortality**	
	+/- 1 week	**+/- 2 weeks**	**+/- 1 week**	**+/- 2 weeks**	**+/- 1 week**	**+/- 2 weeks**
Reform	0,002	0,0032	0,0005	-0,0019	0,004 **	0,0039 ***
	(0,0048)	(0,0033)	(0,0067)	(0,0046)	(0,0017)	(0,0011)
Reform*	-0,0053	-0,004	0,0041	0,0021	-0,0032 **	-0,0018 *
Mom under 25	(0,0055)	(0,0038)	(0,0073)	(0,0051)	(0,0014)	(0,0011)
Reform*	0,0027	0,0043	-0,0072	-0,0056	-0,0007	-0,0012
Mom over 35	(0,0040)	(0,0027)	(0,0054)	(0,0037)	(0,0014)	(0,0009)
Reform* Immigrant	-0,0048	-0,0005	-0,0037	0,0005	0,0024	0,0015
mom	(0,0044)	(0,0030)	(0,0055)	(0,0038)	(0,0014)	(0,0010)
Reform*	-0,0016	-0,0029	-0,0027	-0,0003	-0,0016	-0,002 ***
First birth	(0,0032)	(0,0022)	(0,0044)	(0,0030)	(0,0010)	(0,0007)
Reform*	0,0148	0,0085	0,0475 **	0,0146	0,0023	-0,0006
Twins	(0,0120)	(0,0081)	(0,0191)	(0,0132)	(0,0046)	(0,0029)
Reform*	-0,0053	-0,0058 **	0,007	0,0005	-0,0005	-0,0003
Urban	(0,0037)	(0,0026)	(0,0052)	(0,0036)	(0,0012)	(0,0008)
Reform*	-0,0055	-0,0074 ***	0,0016	0,0038	-0,0021 *	-0,0016 **
Married	(0,0034)	(0,0024)	(0,0046)	(0,0032)	(0,0012)	(0,0008)
Reform*	-0,0163	-0,0169	0,0116	0,0236 *	0,0105	0,006
No reg. father	(0,0163)	(0,0110)	(0,0193)	(0,0138)	(0,0088)	(0,0059)
N	175751	363355	175751	363355	175751	363355
Year trend	Y	Y	Y	Y	Y	Y
Controls	Y	Y	Y	Y	Y	Y

(*** 99%, ** 95%, * 90%)

Note: Each column comes from a different regression. An observation is an individual baby. The sample includes all babies born in the last 1 or 2 weeks of December or the first 1 or 2 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2010-11. The dependent variable is indicated in the column head. "Reform" is a binary explanatory variable indicating December 2010- January 2011 births (the weeks right around benefit cancellation). Standard errors are shown in parentheses. Control variables include mother and father's age, mother's immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and a linear time trend.

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Figure A1. Number of daily births by procedure, Spain, December-January of 2010-11



Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.