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

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Abstract: 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 starting January 1, 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 10,000 births a week nationally). This shifting took place in part via an increase as well as an anticipation of pre-programmed c-sections, seemingly mostly in private clinics. We find that this shifting of birthdates resulted in a significant increase in the number of borderline low birth weight babies, as well as a peak in neonatal mortality. The results suggest that announcement effects are important, and that families and health professionals may face effective trade-offs when deciding on the timing (and method) of birth.

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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 neonatal health. 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 date of birth 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 date of birth 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

[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).

over time, and showed that families with more to gain tax-wise were more likely to give birth in December rather than January.

More recently, several 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 that are 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. Moreover, we have much richer data on family characteristics and health outcomes, which allows us to credibly estimate the health impact of the reform.[2]

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

[2] In particular, they only observed health variables the same year of the reform, so that they could not use previous years as a benchmark.

10 years as a benchmark. We find that more than 2,000 births were anticipated in order to receive the benefit (for a total of 10,000 births a week nationally). This shifting in the timing of birth led to more newborns with borderline low birth-weight, as well as a significant spike in neonatal mortality.

The remainder of the paper is organized as follows. Section 2 provides more details about the child benefit and its cancellation. Section 3 details the empirical strategy and the data sources. Section 4 presents the results, starting with the timing of births, and following with the effects on neonatal health. Section 5 concludes.

2. 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 started being 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.[3]

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.[4]

[3] González (2013) evaluates the effects of the introduction of the benefit on fertility and maternal labor supply.

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

The elimination of the child benefit could have a range of short and longer-term effects. In particular, it may discourage fertility in the medium-term. However, any reduction in fertility as a result of the announcement would lead 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 the date of birth in order to qualify for the 2,500 Euros.

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, 2010 read: "*High-risk baby bonus: The end of the 2,500-euro baby bonus raises controversy about mothers seeking to anticipate births*."[5] 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*."

3. Empirical strategy and data

Our identification strategy is simple: we compare the timing of births and the health of newborns around December 31, 2010, using the previous 10 years as a benchmark. If the cancellation of the benefit had an effect on the timing of births, we expect to observe

[5] Translated by the authors.

“too many” births in December 2010, and “too few” in January, relative to previous years, as well as possibly “too many” low birth-weight or premature babies close to the turn of the year, and even possibly higher neonatal mortality.

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).[6] In the full specification, we also include interactions between year and day of the week.[7] The coefficient of interest, β, captures any “extra” births taking place in December 2010, compared with January 2011, and relative to the previous 10 years. 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, including, first, only the seven days before and after the turn of the year. 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 same analysis on the same four samples at the individual level. This allows us to include individual-level demographic controls, as well as to

[6] In fact, the year dummies are indicators for each December-January pair.

[7] Our specification closely follows the one in Gans & Leigh (2009).

interact the effect of interest with characteristics, in order to identify the types of families most likely to have reacted to the reform. The individual-level specification 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 years. In additional specifications, we interact the reform dummy with all the demographic characteristics.

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 birth dates. The health specifications are equivalent to the one shown in equation 2, except that the dependent variable is one of a range of measures of the health of the newborn. 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. 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. We thus compare the health status of babies born close to December 31, 2010 (the reform period), with that of babies born around the same dates during the previous 10 years, allowing for a long-term (linear) trend.

Data

We use micro data from birth certificates from the Spanish National Statistical Institute. These 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 supplemented 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 end up focusing only on births taking place in December or January.

Descriptive statistics for the main variables of interest are reported in table 1. The 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.

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 3,223 grams, with almost 7% of babies below 2,500. Also about 7% of babies are born prematurely (before the 37th gestational week), and only about 3 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 multiples.

4. Results

4.1 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. Compare that to the last week of December right before the benefit was eliminated, when 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 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 including between one and four weeks before and after the cutoff date for benefit eligibility. We expect most of the shifting to take place in the dates closest to the threshold, but we extend the window 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. 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 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.

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 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), one can see that the daily number of births during December and January fluctuated between 1,100 and 1,500, with a minimum of 999 on December 25 and a max 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

[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.

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 three weeks. This makes it relevant to evaluate any potential health effects as a result of such significant shifting.

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 that progressively widen the window around the cutoff. The average value of the dependent variable for each of the 11 years is displayed in figure 3. Between 2000 and 2010, when looking at births taking place within a week of the turn of the year, between 49 and 51% of those births took place in December, as expected. The reform year is clearly an outlier, with 56% of births taking place in December.

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[9] In these specifications, the sample include all births in December and January of the eleven December-January pairs, i.e. 31 days before and after the cutoff.

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. The results confirm the graphical evidence: in the reform year, there were about 6 percentage points more births in December than January, relative to the previous 10 years. The magnitude of the effect goes down in magnitude 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 interacts 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 significantly more likely to react to the benefit cancellation, as well as native women, those with previous children, and the mothers of twins.

4.2 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.

We estimate regressions of the form of equation 2, except that 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. The coefficient of interest would capture any deviations from trend in the average health of newborns around the turn of the year in the reform period.[10]

We first report the results for birth-weight. Table 5 shows the results 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 17.5 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, a 17-gram average effect for all newborns implies that affected babies were on average almost 150 grams smaller (about a 5% effect). As expected, the effect becomes smaller as we widen the window, turning insignificant in the last column (28 days before and after).

The medical literature seems to agree that babies born below 2,500 grams face significant health risks. The economics literature also finds that being born below this threshold has important long-term effects on both health and economic outcomes (Behrman & Rosenzweig 2004, Black et al. 2007, Johnson and Shoeni 2011). Thus, we next 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-

[10] Note that we compare the health of babies born in December-January of 2010-11 with respect to the previous 10 years, NOT the health of December versus January babies, since doing so would conflate the impact of the reform with composition effects (the differential characteristics of families who switched birth from January to December).

birthweight babies in the reform period, but the results are not statistically different from zero at standard confidence levels.[11] Thus, it appears as if the effect on weight did not take place at this margin.

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 median birth-weight of 3,223). 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 not statistically significant for the first decile. Thus, we conclude that the shifting of birth dates did lead to a reduction in birth-weight for the affected babies, although 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. We create a prematurity indicator, taking value 1 for babies born before week 37 of the pregnancy.[12] 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.

So far, it seems as if the anticipation of births induced by the benefit cancellation had not led to any severe health consequences for newborn babies. As an extreme measure of health, we also evaluate the effect of the reform on neonatal mortality,

[11] In fact, for the one-week window sample, the effect is significant at 11%.

[12] Babies are considered full-term at 37 gestational weeks. Prematurity is associated with negative health outcomes.

defined as the newborn not surviving the 24 hours after delivery. This is the case for 3 to 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 58 newborn deaths.

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 column).

Overall, the results suggest that the anticipation of birth dates as a result of the benefit cancellation resulted in significantly smaller babies (although no increase in the fraction of babies below 2,500 grams), and a significant spike in neonatal mortality.

4.3 Channels

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

The date of birth for a pregnant woman can be anticipated medically either by inducing birth or via an elective c-section. 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. An elective caesarean section can take place for medical reasons or on maternal request, which has become more frequent in recent years, as the procedure has become safer. Either of these methods could have been used by women to bring forward their date of birth.[13] Both are considered risky if performed for non-medical reasons.[14]

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, so that the analysis of c-sections should be treated with caution.

We estimate equation 2 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 appendix table A4. We find that the incidence of c-

[13] Note that an induced birth can lead to an unanticipated c-section, so these two procedures are not exclusive.

[14] "*Inducing labor before 39 weeks increases the risk of complications and premature death, from factors including underdeveloped lungs, infection due to underdeveloped immune system, problems feeding due to underdeveloped brain, and jaundice from underdeveloped liver*." Wikipedia, retrieved 2013-07-23. "*Critics of elective caesarean section, maintain that decision metrics are ambiguous, and that trial of labor would often be successful without open abdominal surgery. The cost to the patient and the baby for unnecessary surgery may be substantial.*" Wikipedia, retrieved 2013-07-23.

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 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 many of those expecting multiples) 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 A4. They suggest that in fact there was some significant 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 a increase as well as a shifting in c-sections. However, c-sections cannot account for all of the effect, since we observe the same pattern 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.[15]

Finally, it is common knowledge in Spain that private clinics are more open to scheduling childbirth in advance compared to public hospitals. 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 births take place. However, we obtained information (from an independent data source)[16] on the number of private clinic beds across the 52 Spanish provinces and over

[15] 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. They refused to provide us with an indicator for observations with imputed information on method of birth.

[16] The National Catalogue of Hospitals, 2000-2011, from the Spanish Health Ministry.

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 clustered by province). The results are reported in table A5. We find that the increase in December 2010 relative to January 2011 births was significantly more pronounced in provinces with more private hospitals, even after controlling for province fixed-effects and interactions between the reform and individual characteristics. These results are consistent with private hospitals being more willing to adjust the date of birth on parental request.

5. 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 births had significant health consequences for the babies. Newborns who were born earlier as a result of the reform were more likely to be borderline low birth-weight (less than 2,750 grams at birth), and there was a significant spike in neonatal mortality.

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 or 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?[17] Did doctors have any economic incentive to do so? These are all interesting questions that we cannot answer with our data.

Finally, our results provide additional evidence that tinkering with the timing of birth for non-medical reasons can have important health consequences for babies.

[17] Faking the date of birth in the birth certificate is 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.

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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	3223	522	4	6560
BW<2,500	0,0693	0,254	0	1
BW<2,750	0,143	0,35	0	1
BW<3,000	0,284	0,451	0	1
Premature (<37w.)	0,072	0,259	0	1
Mortality (24h.)	0,00342	0,05838	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	
Reform	0,0619	***	0,0486	***	0,0403	***	0,0338	***
	(0,0046)		(0,0032)		(0,0026)		(0,0023)	
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. "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	-17,47	***	-12,03	***	-5,723	**	-3,486
	(4,822)		(3,349)		(2,714)		(2,355)
Dep. var.: ln(birth weight)	-0,00613	***	-0,00424	***	-0,00205	**	-0,00129
	(0,00176)		(0,00122)		(0,00098)		(0,00084)
Dep. var.: BW<2,500	0,00374		0,00164		0,00076		0,00018
	(0,00234)		(0,00163)		(0,00132)		(0,00115)
Dep. var.: BW<2,750	0,00726	**	0,00331		0,0025		0,0014
	(0,00321)		(0,00223)		(0,00181)		(0,00157)
Dep. var.: BW<3,000	0,00999	**	0,00606	**	0,00429	*	0,00197
	(0,00415)		(0,00289)		(0,00234)		(0,00203)
N	172323		356061		540690		719024

(*** 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 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

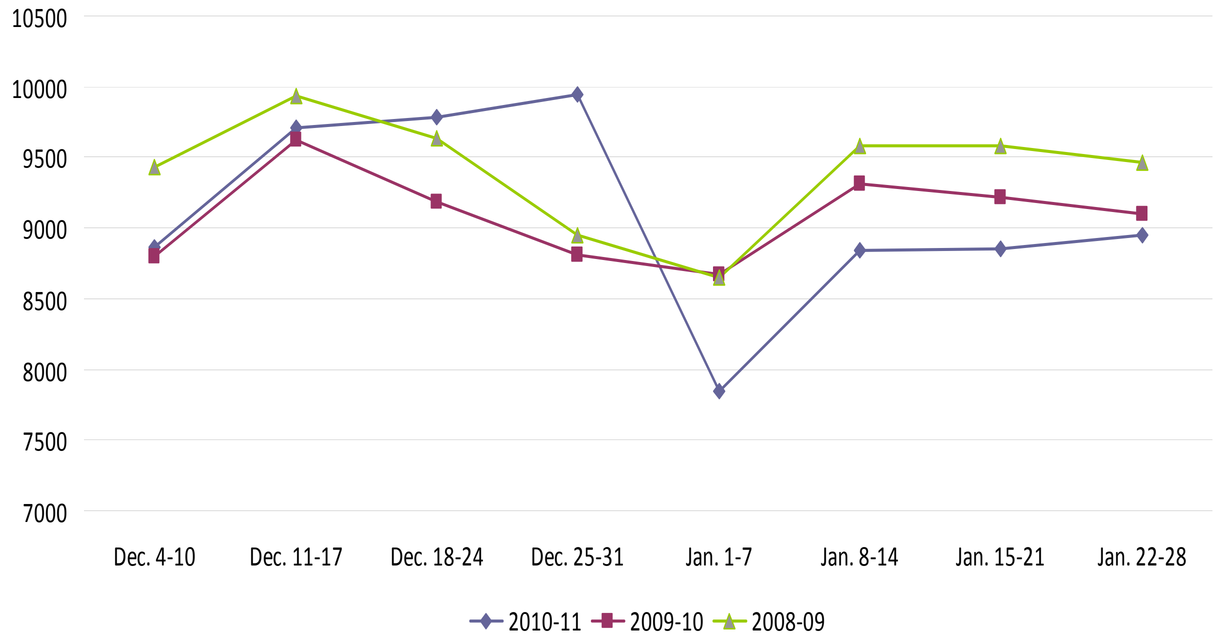
	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Dep. var.: Prematurity	0,00172		-0,00066		-0,0019		-0,00124	
(under 37 weeks)	(0,00228)		(0,00159)		(0,00128)		(0,00111)	
Dep. var.: Mortality	0,000938	*	0,000999	***	0,000875	***	0,000791	***
(first 24 hours)	(0,00054)		(0,00038)		(0,00030)		(0,00027)	

(*** 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 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.

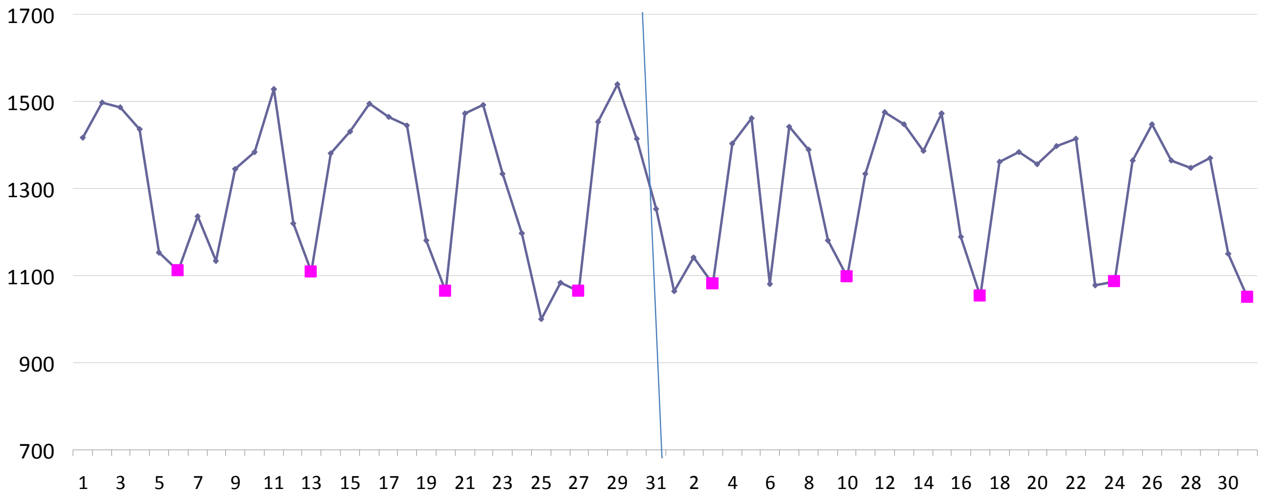
Figure 1. Number of births by week, Spain, December-January of 2008-09 to 2010-11



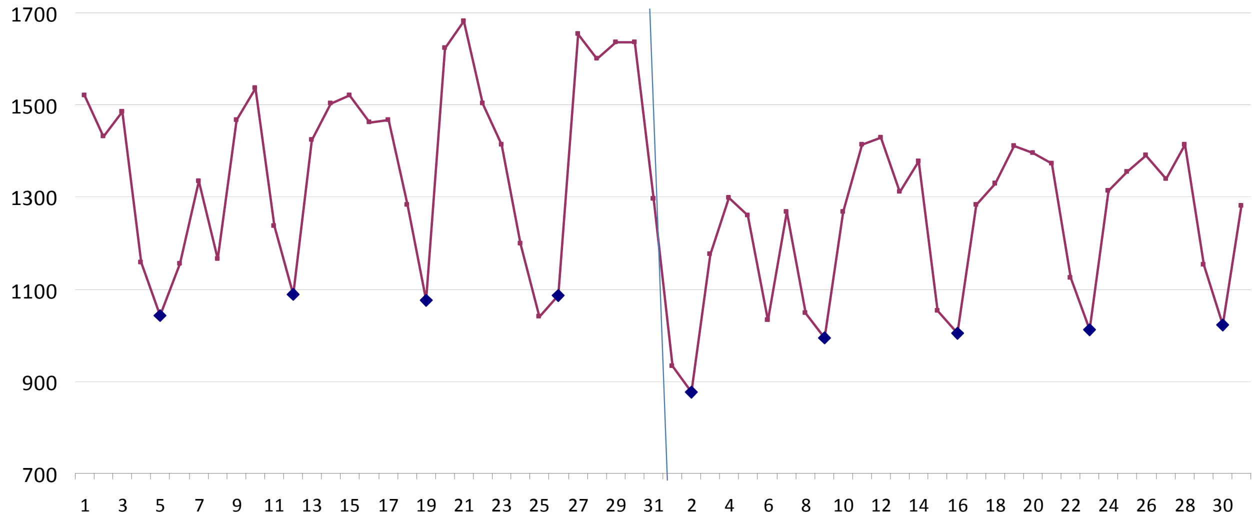
Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

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

(2009-10)



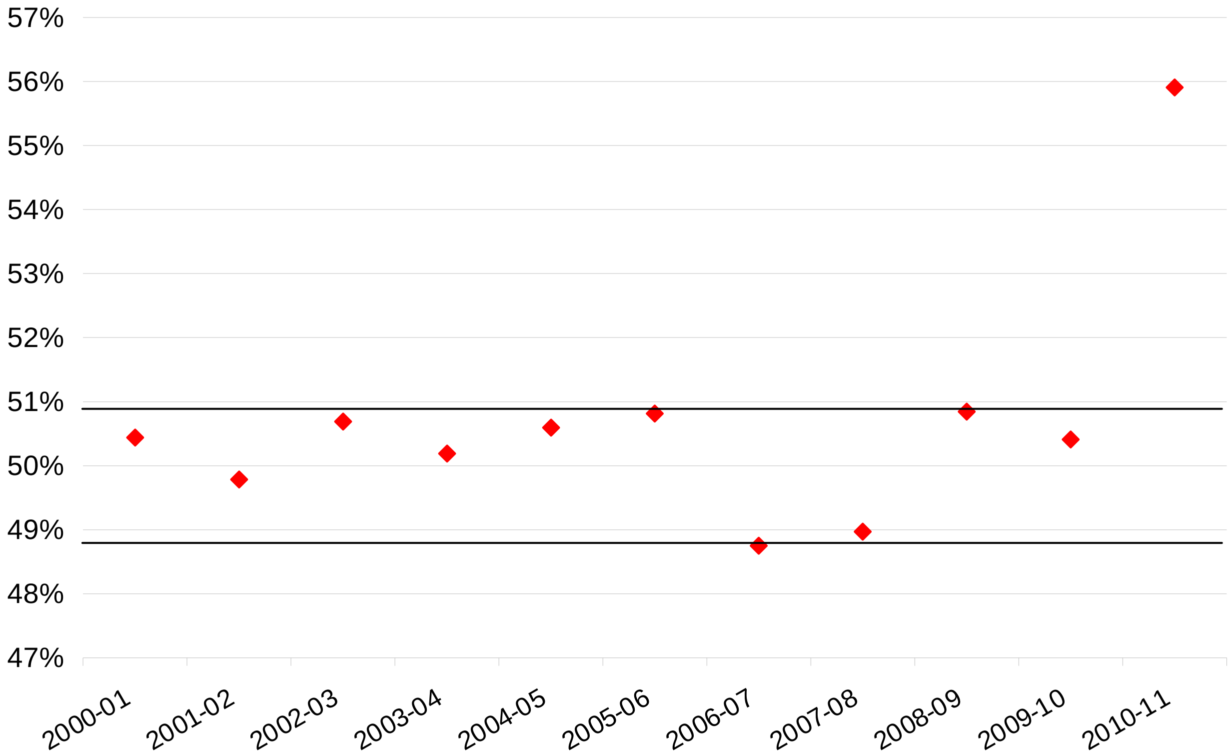
(2010-11)



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	-13,37		-20,01	***	-12,98	**	-9,61	*	-12,8	**	-18,05	***	-20,18	***	-17,51	***	-17,33	**
	(8,801)		(6,239)		(5,361)		(5,083)		(5,284)		(5,616)		(5,279)		(6,177)		(7,740)	
Dep. var.: ln(birth weight)	-0,00478		-0,0068	***	-0,00431	**	-0,00316	**	-0,00387	**	-0,00552	***	-0,00589	***	-0,00474	***	-0,00448	**
	(0,00327)		(0,00220)		(0,00179)		(0,00161)		(0,00165)		(0,00162)		(0,00159)		(0,00173)		(0,00201)	

(*** 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	0,476		-4,844		-6,476	**	-4,723	**
	(4,890)		(3,407)		(2,761)		(2,398)	
Dep. var.: ln(birth weight)	0,00579		-0,00154		-0,00223	**	-0,00180	**
	(0,00175)		(0,00122)		(0,00099)		(0,00086)	
Dep. var.: BW<2,500	-0,00125		0,00122		0,00172		0,00181	
	(0,00235)		(0,00165)		(0,00134)		(0,00117)	
Dep. var.: BW<2,750	-0,00251		0,00119		0,00243		0,00186	
	(0,00325)		(0,00227)		(0,00184)		(0,00160)	
Dep. var.: BW<3,000	-0,00312		0,00038		0,00137		-0,00038	
	(0,00423)		(0,00295)		(0,00239)		(0,00207)	
Dep. var.: Prematurity	-0,00601	***	-0,00228		-0,00084		-0,00058	
(under 37 weeks)	(0,00231)		(0,00162)		(0,00132)		(0,00114)	
Dep. var.: Mortality	0,00051		0,00048		0,00047		0,00054	**
(first 24 hours)	(0,00053)		(0,00037)		(0,00030)		(0,00027)	

(*** 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. 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	***	0,054	***	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 A5. The effect of benefit cancellation on birth timing, by availability of private health centers in the province

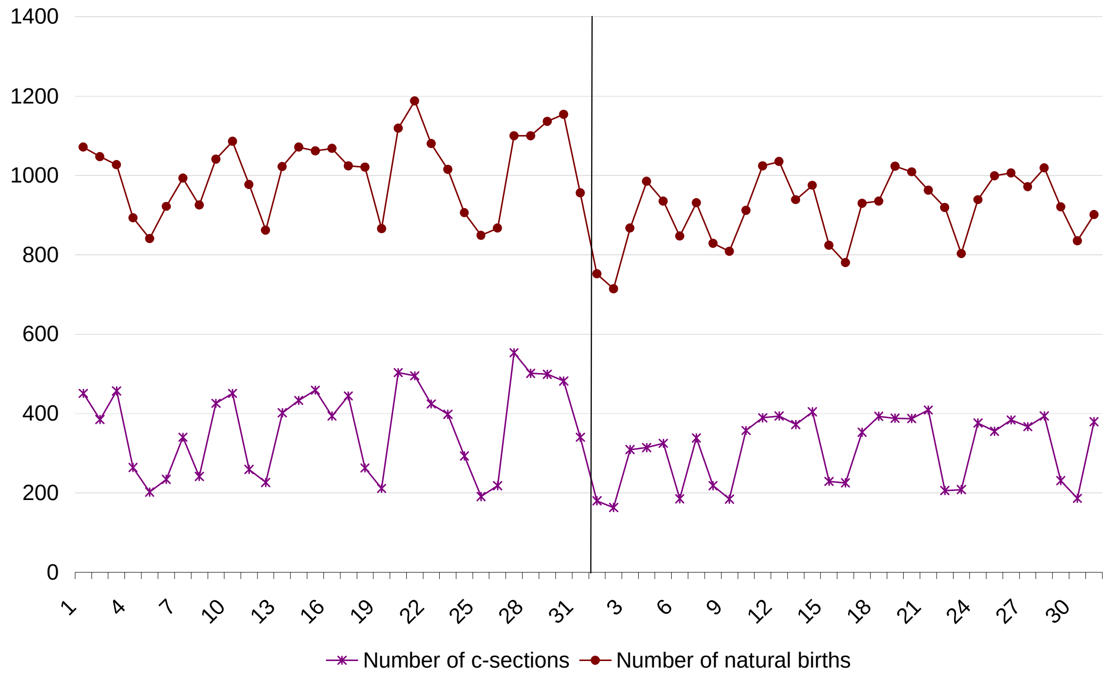
	+/-1 week		+/-2 weeks		+/-3 weeks		+/-4 weeks	
Reform*Fraction private beds in province	0,0918 (0,0423)	**	0,0865 (0,0393)	**	0,0880 (0,0372)	**	0,0861 (0,0369)	**
Province fixed effects?	Y		Y		Y		Y	
All interactions? (between "Reform" and controls)	Y		Y		Y		Y	

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



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