The Impact of Scheduling Birth Early on Infant Health*

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Abstract: We take advantage of a natural experiment to provide new, credible evidence on the health consequences of scheduling birth early for non-medical reasons. In May 2010, the Spanish government announced that a 2,500-euro universal "baby bonus" would stop being paid to babies born after December 31st, 2010. Using administrative data from birth certificates and hospital records, we find that more than 2,000 families shifted their date of birth from January 2011 to December 2010 (out of 9,000 weekly births). The affected babies had about 250 grams lower birth-weight, and suffered more than 600 additional hospitalizations during their first few months of life.

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1. Introduction

What is the effect of scheduling birth early for non-medical reasons on infant health? This question is hard to answer. The ideal randomized trial would be difficult to implement, since it would imply the scheduling of a randomly chosen subset of births at different stages of the pregnancy, which may be questionable on ethical grounds. We provide novel causal evidence on this question by taking advantage of quasi-experimental variation, driven by a policy change in Spain that increased the incidence of scheduled births temporarily and exogenously.

An increasing number of births are scheduled early for non-medical reasons in many countries.[1] In OECD countries, cesarean-section rates have increased almost twofold in the last two decades, from 15% in 1990 to 26% in 2009 (OECD 2011).[2] In the United States, a recent article in the *New York Times* reported that about half of all births are "hastened either by drugs or surgery, double the share in 1990",[3] and induction rates have increased from under 10% in the early 1990s to more than 23% in 2012 (Osterman and Martin 2014).[4]

Infant health has been shown to have important long-term consequences (Smith 2009, Fletcher et al. 2010), but little is known about the kinds of early interventions that can successfully affect the health of the newborn. To the extent that scheduling birth early for non-medical reasons has negative health impacts, a policy targeting this practice may have far-reaching effects.

Conventional wisdom in the health profession appears to hold that more time in the womb doesn't help the fetus once it has reached full-term (37 weeks of gestational age)

[1] A birth can be scheduled via either induction or cesarean-section. Labor induction consists of administering the pregnant woman certain hormones (prostaglandin, oxitocin) that trigger childbirth. Both inductions and c-sections can take place for medical reasons or electively. Elective induced labor can lead to an unanticipated c-section (Stock et al. 2012), so that the two procedures are not exclusive.

[2] Some OECD countries have relatively low c-section rates (20% in France in 2011), whereas others show much higher incidence (33% in the US, 35% in Portugal, 38% in Italy) (OECD 2013).

[3] "Heavier Babies do Better in School", The Upshot, *New York Times* 2014/10/12.

[4] In the US, it is estimated that about half of all labor inductions are performed electively (Engle and Kominiarek 2008), as well as up to one in five c-sections (Mally et al. 2010).

and is estimated to weigh more than 2,500 grams.[5] A significant share of births are scheduled after those thresholds, but before the mother has gone into labor spontaneously, many of them for convenience reasons (for the families and/or the doctors).[6] However, this notion has been challenged by recent correlational evidence in the medical literature (Boyle et al. 2012), showing that the association between gestational age and a range of infant health complications persists across those thresholds.[7] Moreover, scheduling a delivery even just a couple of days before the "due date" can imply bringing forward the date of birth by up to several weeks, given the uncertainty associated with the estimation of the due date and the documented natural variation in the length of a pregnancy. Recent evidence suggests that the length of a human pregnancy can vary naturally by as much as five weeks (Jukic et al. 2013), and there is evidence that only about 70% of women deliver within 10 days of their due date, even when estimated by ultrasound (Mongelli et al. 1996).

A related recent literature in economics has also documented strong associations between health at birth (proxied by birth-weight) and a range of short and long-term outcomes.[8] For instance, a recent paper by Figlio et al. (2014) uses data for millions of school children in Florida, showing that higher birth-weight is associated with higher test-scores in elementary school, and that this association is not limited to children born below the 2,500 grams threshold.

Are we inducing too many babies, too soon? We address this question by taking advantage of a "natural experiment" that shifted forward the date of a large number of scheduled births for non-medical reasons, temporarily and "exogenously". The natural

[5] The medical literature has documented that babies born prematurely (before week 37) and/or smaller than 2,500 grams are more likely to suffer health complications (Moster et al. 2008, Crump et al. 2011).

[6] For instance, research has documented an unusually small number of births during weekends and holidays (Mossialos et al. 2005, Lefevre 2014), obstetric conferences (Gans et al. 2007), and inauspicious days (Lo 2003, Lin et al. 2006).

[7] In fact, in 2011 the American Congress of Obstetricians and Gynecologists began pushing to eliminate induced labor before the 39th week of pregnancy, absent a clear medical reason.

[8] Including infant and adult mortality, infant and adult health, test scores, educational attainment, employment, and earnings (Currie and Hyson 1999, Behrman and Rosenzweig 2004, Almond et al. 2005, Black et al. 2007, Oreopoulos et al. 2008, Royer 2009, Johnson and Schoeni 2011, Figlio et al. 2014). This literature can be traced back to the fetal origins hypothesis of Barker (1990), according to which low fetal growth would increase the risk of adult disease.

experiment is generated by the pre-announced cancellation of a generous universal child benefit in Spain. In May 2010, the Spanish government announced that babies born starting January 1, 2011 would not receive the existing 2,500-Euro baby-bonus. For ongoing pregnancies with a due date near the cutoff, the benefit cancellation generated an incentive to schedule the birth in December (versus January).

We can view the decision to schedule birth early in the context of a model where parents derive utility from both consumption and infant health. The benefit cancellation represents an income shock, conditional on scheduling birth before the cutoff date. However, inducing birth early has uncertain consequences for the health of the newborn, given the lack of reliable causal evidence. Our results shed light on the sign and magnitude of this effect, which can help parents and health professionals make informed decisions about the timing of birth in the future.

We use detailed, high-quality administrative data from birth certificates and hospital records, for the universe of children born in Spain from 2000 to 2012. We first show that there was a significant spike in the number of births in late December 2010, with a corresponding trough in early January 2011. The simplicity of our policy change, the magnitude of the benefit, and the rich birth-certificate data allow us to estimate timing effects credibly and precisely. This is illustrated in Figure 1. We show the fraction of December births among all births taking place during the last week of December or the first week of January (panel A), for years 2000-01 to 2011-12. In "normal" years, about 50% of the turn-of-the-year babies are born in December. In the weeks surrounding the benefit cancellation, the fraction shoots up to 56%, a clear outlier. Our regression analysis confirms that about 2,000 births, or almost 6% of all January births, were shifted back to December in order to qualify for the benefit. We also find that the average shifted baby was born between one and two and a half weeks early as a result.

We are then able to evaluate the health effects of early delivery for the affected babies. Our identification strategy relies on comparing the health outcomes of all babies born close to the New Year of 2010-11, to those born on the same dates in the surrounding years (before and after), using October and November as "control months". By including both (late) December and (early) January births in our "treated group", we control for any potential composition (or "selection") effects, e.g. the possibility that only relatively healthy (or unhealthy) babies were shifted. By including October and November births as a control group, we account for other factors that could have affected the health of all babies born in late 2010-early 2011.

We find that babies born close to the benefit cancellation date weighed significantly less, as illustrated descriptively in Figure 2 (panel A). We show the average birth-weight of all babies born in late December or early January (as well as late October-early November), from 2000-01 to 2011-12. There is a clear positive trend over time, but the reform period is again an obvious outlier, with average birth-weight more than 20 grams lower in December-January of 2010-11 than in the preceding or the following year (note that both affected and unaffected babies are included).

Our regression results suggest that affected babies (those delivered early due to the benefit cancellation) were born up to 300 grams (9 percent) smaller on average as a result, to be expected since they were born earlier. However, we do not find a significant increase in the fraction of babies born below 2,500 grams, suggesting that the increase in scheduled births was driven by full-term pregnancies.

We are then able to follow up the newborns for the first 33 months after birth, almost three years. We find that the affected babies experienced a sizeable increase in hospitalization rates, with more than 600 “too many” hospitalizations in the affected cohort, concentrated in the first two months after birth. Our most striking finding shows close to a 50% spike in bronchitis hospitalizations during the second month of life among babies born within one week of December 31, 2010.

We show that the effect of the benefit cancellation on birth timing was more pronounced among college-educated, older, native mothers, as well as among higher-order and multiple births. The spike in December 2010 births was also significantly more pronounced in provinces with a higher proportion of private hospital beds, suggesting that the scheduling was more prevalent among families with access to private health insurance (i.e. of higher socio-economic status). These are roughly the same characteristics that describe families who schedule birth in normal times, which suggests that our results may have external validity beyond the sample of families affected by this specific reform.

We are also able to tease out how many of the switched births were scheduled c-sections versus inductions. Our results suggest that both methods were used. We also find that the spike in December c-sections was driven by a switching of the dates of scheduled c-sections toward December, not by an increase in the overall incidence of c-sections. This suggests that the health effects that we document are driven by time in the womb, and not method of delivery.

Our paper contributes to a previous literature showing that the timing of birth can react to economic incentives (see Table 1). Dickert-Colin & Chandra (1999) found that families with more to gain tax-wise were more likely to give birth in December rather than January in the US. Their results were confirmed with better data by LaLumia et al. (2015), who found "*evidence of a positive, but very small, effect of tax incentives on birth timing*."

Several recent papers have studied the effect on birth timing of benefit reforms that generated incentives either to advance or to postpone the date of birth in other countries. Tamm (2011) and Neugart and Ohlsson (2012) analyze a reform in the parental leave system in Germany, and Brunner & Kuhn (2013) study the cancellation of a baby-bonus in Austria. Gans and Leigh (2009) analyze the increase in the generosity of a child benefit in Australia, and also assess the impact on health at birth. However, their ability to estimate health effects is limited, since they only observe health variables (at birth) the same year of the reform, so that they cannot use the surrounding years as a benchmark. Moreover, their policy reform led to the postponement, rather than advancement, of births.

Recent work by Schulkind and Shapiro (2014) exploits the incentives generated by the US tax system to give birth in December versus January to look at the effects on health at birth. They find some evidence that the resulting spikes in scheduled births in December led to lower birth-weight and Apgar scores. Our approach is superior on several dimensions. First, we have a sharp and well-publicized reform that cut benefits by a large amount at a pre-specified date, and use previous and later years as controls. In contrast, they use cross-sectional and time variation in tax benefits, with varying amounts and no clean "control group". Their average tax savings from a December birth are about 790 US dollars.

Second, we are able to observe benefit eligibility precisely, since it is only a function of date of birth. Schulkind and Shapiro (2014) have to approximate tax savings based on (few) observed household characteristics. Third, we are able to show credibly that the benefit cancellation had sizeable effects on birth timing, while Lalumia et al. (2013) show convincingly that the birth timing effects generated by tax incentives in the US are small, probably because most people are unaware of the tax gains associated with a December versus a January birth. Moreover, the timing effects found for the US are likely to be a combination of birth-scheduling and the timing of pregnancies, while our December spike is credibly driven only by the timing of births, not pregnancies, given the benefit was announced only seven months in advance. Fourth, we have detailed data with precise date of birth (while Schulkind and Shapiro only observe the month), so we are able to focus on the days right around the cutoff. Finally, we observe health outcomes not only at birth (from birth certificates), but also during the first 33 months of life (from hospital records).

To our knowledge, this is the first study to provide credible causal evidence on the effect of scheduling birth for non-medical reasons on, first, health outcomes at birth, and then, subsequent health outcomes during infancy. We are able to link an exogenous increase in the number of births scheduled early to shorter gestational age and lower weight at birth, and then show how the affected cohort of babies was more likely to be hospitalized for conditions, such as respiratory disease, that correlational evidence in the medical literature has shown to be associated with gestational age (Escobar et al. 2006, Mally et al. 2010, Boyle et al. 2012).

We also contribute to the literature on the effect of early influences on human capital formation. Previous work has emphasized that infant health has important long-term consequences. Much less is known, however, regarding what kinds of early interventions would successfully affect health at birth and subsequent outcomes. We identify one such intervention (scheduling birth early for non-medical reasons), which is widely used in practice as well as easy to target via policy, credibly showing how it can affect health outcomes, at birth as well as later on.

2. Institutional framework: 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.[9]

Three years later, on May 10, 2010, the benefit was eliminated in the first round of budget cuts as a result of the economic slowdown. The government announced that the baby bonus would stop being paid for babies born after December 31, 2010. The announcement thus pre-dated the effective cancellation date by almost 7 months. Crucially, none of the other measures announced at that time or afterwards would affect babies born in January 2011 differently from those born in December 2010.

The elimination of the child benefit could have a range of short and longer-term effects. In particular, it may have discouraged fertility. 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).[10] For ongoing pregnancies, however, the pre-announced cancellation created an incentive for those with a due date close to the threshold to advance their delivery date in order to qualify for the 2,500 Euros.[11] The "natural experiment" generated by the benefit cancellation offers an unprecedented opportunity to evaluate the health effects of scheduling birth early for non-medical reasons.

Maternity care in Spain is mainly provided by the publicly funded and publicly run National Health Service, which is highly regarded with respect to facilities and personnel. Hospital choice among public institutions is permitted in several regions,

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

[10] Abortions could have reacted immediately to the policy announcement, leading to lower fertility as early as January 2011 (or even late December 2010). Note that women with a due date in 2010 would have had no incentive to get an abortion as a result of the benefit cancellation. Given the uncertainty associated with due dates, we do not expect that abortions would generate selection in births right around the cutoff. However, in order to minimize potential bias generated by selective abortions, we focus on births taking place very close to December 31, and we also test explicitly for fertility effects.

[11] 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 advance births*". The same newspaper interviewed a mother-to-be due in January 2011, who explained: "*I think it's risky to advance 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: "*Advancing birth is (...) absolutely not advised, since it generates risks for the health of the baby*."

though in practice it is usually based on proximity to the hospital. In the period under analysis, about 25% of deliveries took place in privately funded and run hospitals (Ministry of Health and Social Policy 2009). Private hospitals in Spain can be privately owned and operated, or privately owned, but dependent on the National Health System. Mothers with private insurance (many public servants who may opt for private healthcare as well as some well to do families) tend to give birth in private clinics in the absence of birth complications. Guidelines of patient care and average length of hospital stays are similar to the National Health System, but private hospitals show a much higher prevalence of c-sections (Redondo et al. 2013). The standard recommendation is for new mothers to be discharged from the hospital before 48 hours after births with no complications. In practice, the average birth hospitalization in the Spanish National Health Service is about 2.8 days for vaginal deliveries and 5.6 days for c-sections.

3. Data sources

We have two main sources of data. First, we use micro data from birth certificates from the Spanish National Statistical Institute. These population-level data provide detailed information on the universe of births taking place annually in Spain, as recorded in the official national registry. All families are supposed to register their newborn babies within eight days of birth, and the time and date of birth are set in the documentation provided by the health center that assisted the delivery.

The variables included in the birth-certificate data come from a standardized form that families fill out at the time of registration, and include parental demographic characteristics, method of delivery, weeks of gestation at birth, birth-weight, late fetal death (fetuses with 20 or more weeks of gestation that die in utero, which we also refer to as stillbirth), and neonatal mortality (death during the first 24 hours after birth). There is no information on Apgar scores or congenital disorders. We supplement the publicly available files with the exact date of birth for each newborn,[12] for years 2000 to 2012.

Our second data source is the Hospital Morbidity Survey for 2000-2013, which provides essentially an annual census of all overnight hospitalizations in Spain.[13] This

[12] Purchased from the National Statistical Institute.

[13] According to the National Statistical Institute, the data include 96% of hospitals in Spain, and 99% of all overnight hospital stays.

survey contains information, at the level of the individual hospital stay, on date of release, age (in years, months and days), main diagnosis, and length of the hospital stay, as well as some additional geographic and demographic variables. This allows us to construct the date of birth for each individual in the sample, and thus select all hospitalizations for the cohorts of babies born on the relevant dates, at different ages. We focus on hospitalizations during the first 33 months of life.[14]

We focus our analysis on births taking place in December or January of 2000-01 to 2011-12, with October and November as our "control" months in the health analysis. Table 2 reports summary statistics for our main sample. The total number of newborns in the sample is 1,712,552. Panel A shows the health outcomes at birth. Average weight at birth is about 3,200 grams, with less than 1% of the babies below 1,500 and about 8% below 2,500. Average gestational length is 39 weeks, and about 7% of babies are born prematurely. Regarding mortality, 3 in 1,000 pregnancies end in stillbirth, and less than 1 in 1,000 babies does not survive the first 24 hours after delivery.

Panel B shows descriptive statistics for our infant health outcomes beyond birth, based on the hospital data. There were about 44 hospitalizations per 100 babies aged 0 to 33 months.[15] We split hospitalization rates by age, using shorter age ranges at earlier ages. There are about 14 hospital stays for 100 live births during the first week, about 3 at ages between seven and thirty days, and between 3 and 5 in the four following age ranges. Overall, there are almost 33 hospitalizations for each 100 births during the first year of life, while we observe almost 11 hospitalizations for each 100 births at ages 12 to 33 months. These numbers are comparable to those reported by the European Hospital Morbidity Database, according to which hospitalization rates of children younger than 1 (excluding healthy birth hospitalizations) were above 30 per 100 births in many European countries in 2011.[16]

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[14] The youngest cohort in our sample (January 2012 births) has not turned 3 yet in the most recent year of hospital data available (2014).

[15] This includes birth hospitalizations with an associated medical diagnosis.

[16] For example, 40.5% in Austria, 31.2% in Finland, and 39.3% in Ireland (http://data.euro.who.int/hmdb/).

The most common groups of diagnoses[17] in our sample are perinatal conditions[18] and respiratory disease, which account for 34 and 24 percent of all hospital stays. Excluding delivery hospitalizations, respiratory disease is the top category (31% of all stays), and the most common three-digit diagnosis is acute bronchitis (more than 16%).

4. Effect on birth timing

4.1 Empirical strategy

In this section, we show that the benefit cancellation led to a substantial number of families scheduling birth early in December 2010. We interpret these results in the light of a simple model where parents value consumption as well as infant health, but may not be well-informed about the health effects of scheduling birth early (see Appendix).

Our identification strategy relies on comparing the timing of births around December 31, 2010, using the surrounding 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 (late) December 2010, and "too few" in (early) January 2011, relative to the surrounding years.

More specifically, we focus on births taking place in December or January of years 2000-01 to 2011-12 (including ten years before and one after the reform), and estimate the following regression:

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

where B 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 for December 2010. 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. We are thus controlling for fluctuations in the number of births associated with holidays or weekends, while the year dummies control for any aggregate factors, including the business cycle, possibly correlated with birth rates over

[17] At the level of "chapter", of which there are 17 *(International Classification of Diseases, Clinical Modification* (ICD-9-CM)).

[18] "Certain conditions originating in the perinatal period", or "fetal disease" (ICD-9-CM).

time. Our full specification, which closely follows Gans and Leigh's (2009), also includes interactions between year and day of the week.

The coefficient of interest, β, captures any "extra" daily births taking place in December 2010, compared with January 2011, and relative to the surrounding years.[19] If the benefit cancellation affected the timing of births, we expect β to be positive.

We estimate equation (1) on four different samples. First, we limit the sample to 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. The full sample thus includes all births in the last 4 weeks of December or the first 4 weeks of January, for the twelve December-January pairs from 2000-01 to 2011-12.[20] The number of observations is 672 (28 days, times 2 months, times 12 years). There were on average 1,228 births per day, with a minimum of 806 and a maximum of 1,683 (reached on December 21, 2011, ten days before the benefit cancellation).

4.2 Birth timing results

We start by providing some graphical evidence on the impact of the benefit cancellation on the timing of births. Figure 3 (panel A) displays the weekly number of registered births in Spain during the last six weeks and the first four weeks of the year, for 2008-09, 2009-10, 2010-11 (the reform period), and 2011-2012.The number of births in the first three weeks of December 2010 tracks very closely the figures for 2009, while births shoot up in the fourth and particularly the last week of December 2010, dropping dramatically right after the turn of the year. The gap in the number of births between the last week of December and the first week of January, which is about 200 births in "normal" years, increased to more than 2,000 surrounding the benefit cancellation.

Panel B of Figure 3 shows the difference in the weekly number of births between the benefit cancellation year and the average of the three surrounding ones. Again, there are many more births than usual in the last two weeks of December 2010, and too few in the

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

[20] We also re-do all of the analysis using only the five most recent December-January pairs, from 2007-2008 to 2011-2012.

first week of January 2011. These numbers suggest that there was probably some shifting of births from early January 2011 to late December 2010.

The daily number of births in December and January, for the reform year as well as the previous one, can be seen in Figure 4. In 2009-10, the number of 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. It is easy to see that in the reform year, 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, with around 1,000 births.[21]

We now formalize these observations with our regression analysis. Table 3 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. The first column includes only the 7 days before and after the cutoff, thus the number of observations is 14 days times 12 years (N=168). The first row uses daily number of births as the dependent variable. The result suggests that there were 290 "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.[22] 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 shifted from the first week of January to the last week of December 2010.

The second column in Table 3 expands the window to two weeks before and after the cutoff date. 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 1,484. This indicates that quite a

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[21] Faking the date of birth in the birth certificate would be difficult. Families could have convinced hospitals to change the exact time of birth reported, for births close to midnight on the cutoff date. 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.

[22] 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.

number of births were probably shifted by more than one week. Once we include all four weeks before and after (last column), we estimate that about 2,050 births were shifted from January 2011 to December 2010, or about 6% of all January births.[23]

The 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 980 and 1015 (11-12%). We also re-estimate the regression using only three years before and one year after as controls (see table A2), with the point estimates and significance levels essentially unchanged.

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.[24] The results of these specifications are reported in Table 4. It appears that the "extra" December births took place during the last three weeks of the year (especially the very last), while there were significantly "too few" births extending up to the fourth week of January.

We also extend the analysis to 6 and 8 weeks before and after the turn of the year, with similar conclusions (see columns three to six in Table 4). The fact that the reduced number of births extends late into January suggests that there was probably some within-January shifting as a result of the benefit cancellation. Since the first week of the year was particularly empty because of the shifting to late December, births that would have been scheduled for later in January may have been moved forward, thus generating ripple effects in the following weeks.

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[23] This figure is below the 8% effect of the abolition of a 1,000 Euros baby bonus in Austria in 1997 (Brunner and Kunh 2013), but above the 3% figure estimated for a $1,000 increase in the tax benefit in the US by Lalumia et al (2013). In both the Austrian and the US case, however, the policy, or policy change, was known beforehand, allowing for fertility responses as well as birth timing responses. See Table 1 for a comparison of magnitudes across studies.

[24] In these specifications, the sample includes all births from November 27 to February 4 of the twelve years, i.e. 5 weeks before and after the cutoff. Thus, the reference period includes the week of November 27 to December 3, as well as January 29-February 4.

Our results from Table 4 imply that the average birth in our sample (all births in December and January) was shifted by about 0.08 weeks.[25] In other words, gestational length at birth was 0.08 weeks shorter for the cohort of babies born around the benefit cancellation date (within four weeks before or after). These estimates also imply that between 1,975 and 2,120 births were shifted from January to December as a result of the benefit cancellation.[26]

Assuming that the policy change did not lead to any shifting of birth-dates within December or January, these estimates imply, in turn, that the average *affected* baby was born about 2.5 weeks earlier as a result.[27] However, this estimate should be interpreted as an upper bound since, as mentioned, the benefit cancellation may have led to additional shifting within December and/or January. If we assume that as many births were shifted within December or January as between, then the estimated effect on the affected babies would be half the size, about 1.2 weeks (9 days). We view this as a lower bound on the true average effect on "affected" newborns.

In order to get a better understanding of the effects on gestational age at birth, we re-estimate equation (2) for the number of daily births, now split by weeks of gestation at birth. The most common gestational age at birth is 40 weeks (30% of the births in our sample), followed by 39 weeks (23%). There is also a substantial fraction of births before the 37th week (pre-term births, almost 8%). These regression results are presented in Table 5. They show that the vast majority of the "extra" December births were not pre-term (although we also find a significant effect below 37 weeks).[28] The largest increase is found for 37-38 weeks of gestation. Given that in normal times most births take place at 39-40 weeks, this is consistent with our estimated average shifting of births by between one and two and a half weeks.

Overall, we find strong evidence that a significant number of births were shifted from January 2011 to December 2010 (almost 6% of all monthly births) as a result of

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[25] $0.077 = [(5.3\cdot(-4) + 48.3\cdot(-3) + 66.1\cdot(-2) + 162.3\cdot(-1) + (-119.8)\cdot 0 + (-74.0)\cdot 1 + (-66.2)\cdot 2 + (-43.1)\cdot 3)\cdot 7]/72{,}771$, where we multiply the daily effects by 7 since there are 7 days in each week, and where 72,771 is the total number of births in the relevant eight-week sample for 2010-11.

[26] $1{,}975 = (5.32+48.35+66.13+162.30)\cdot 7$.

[27] $2.5 = 0.076/0.03$, where 0.03 is the fraction of "affected" births: 2,120/72,771.

[28] Current maternal and neonatal guidelines in Spain advise against inducing birth before the 37th week unless specific maternal or child health complications are present (Servicio de Medicina Materno-Fetal, 2015).

the benefit cancellation, with important effects on gestational age at birth for the affected newborns.

4.3 *Who was affected?*

In order to identify the types of families that were more likely to react to the benefit cancellation, we estimate birth timing regressions at the individual level allowing for heterogeneous effects, i.e. interacting the reform variable with a range of family characteristics. We take the sample of December-January births for the twelve years of data, and estimate the following specification, adapted from Dickert-Colin and Chandra's (1999) and Schulkind and Shapiro's (2014):

(2) $Dec\_birth_{it} = \alpha + \beta(Dec2010\text{-}Jan2011)_{it} + \gamma(Dec2010\text{-}Jan2011)X_{it} + \varphi X_{it} + \varepsilon_{tt}$

The dependent variable is 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 be about 0.5 in non-reform years, which is in fact the case, as shown in Figure 1. The main explanatory variable, *Dec2010-Jan2011*, takes value 1 for the reform period, December-January of 2010-11. A positive β would indicate that there were too many December (versus January) births in 2010-11, compared with the surrounding years.

Demographic characteristics X_{it} include: mother's age, mother's immigrant status and marital status, dummies for urban and rural areas, dummies for first-borns, female babies, and multiple births, an indicator for babies with no registered father, and a dummy for mothers in high-skill occupations.[29] Since 2007, we can also include educational attainment of the parents. The γ coefficients capture the differential impact of the reform for different demographic groups.

Table 6 reports the results of estimating equation 4 for the four samples progressively widening the window around the cutoff date. Panel A includes the full sample, while panel B includes only years 2007-08 to 2011-12, where we can control for education. The baseline model reports the results of a benchmark regression with the demographic controls but no interactions. We confirm the results from section 4.2 that the benefit cancellation induced a shifting of births from January 2011 to December 2010. The first specification shows that, if we focus on the 14 days closest to the turn of

[29] We also control for province fixed-effects.

the year, the fraction of December births was almost 6 percentage points too high in 2010-11, as illustrated graphically in Figure 1.

The model with interactions shows the results from the regression that interacts all the control variables with the reform dummy. The results for the one-week window show that mothers older than 35 were more likely to react to the benefit cancellation (by 2 percentage points). The shifting appears less common among immigrant mothers (by almost 3 points). First births were 2 percentage points less likely to be re-scheduled, while a large impact is found for multiple births (6 additional points). We only have information on parental education for 2007 onwards. When estimating equation 4 in the shorter sample (panel B) we find that university-educated parents were more likely to react to the policy change.

These results suggest that the scheduling of births in order to receive the benefit was not driven by women with low socio-economic status, but by relatively educated, older, non-immigrant women, with previous children or expecting multiples. Previous work has documented a higher incidence of c-sections, which are often scheduled, among older women, higher-order births, and multiple births (Lalumia et al. 2013, Aron et al. 1998, Patel et al. 2005). Thus, our findings suggest that at least some of the shifting most likely comes from deliveries that would have been scheduled in any case (see next subsection). These results also indicate that the types of women who shifted their delivery date in response to the benefit cancellation are similar to the women who are likely to schedule a birth in normal times, which suggests that our results may have some external validity.

Higher socio-economic status families are more likely to hold private health insurance in Spain (Costa and García 2003), so it is possible that private health centres were more prone to scheduling births at the parents' request, compared with public hospitals. In fact, c-section rates tend to be much higher in private than in public hospitals in Spain (37 versus 22% in 2009).[30]

The birth certificate data do not contain information on the type of health center where each birth takes place. However, we obtained information from an independent

[30] Source: Spanish Ministry of Health. See also Redondo et al. (2013). In a context of public, universal healthcare, lower rates of c-sections in public hospitals in Spain are consistent with the lower incidence of c-sections among the uninsured in the US (Aron et al., 2000).

data source (the National Catalogue of Hospitals, 2000-2012, from the Spanish Health Ministry) on the number of beds in private clinics 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 4, including the interactions between the reform variable and the controls. We control for a new variable measuring the availability of private hospital beds in each province, and an interaction of the reform variable with the availability of private hospitals.[31] We use two alternative measures of the presence of private hospitals in a province: the number of private hospital beds as a fraction of all hospital beds, and the number of private beds per 10,000 population. We cluster standard errors by province. The results are reported in Table 7.

We find that the spike in December 2010 births was significantly more pronounced in provinces with a higher availability of private hospital beds, even after controlling for province fixed-effects and interactions between the reform and individual characteristics. The results in the first row suggest that a province in the 75th percentile of private hospital beds (about 40%) had a spike in December 2010 births about 2.4 percentage points higher than a province in the 25th percentile (12% of private hospital beds). These results are consistent with private hospitals being more willing to adjust the date of birth on parental request.

4.4 Timing versus method of delivery

The delivery date for a pregnant woman can be shifted forward medically either by inducing birth or via a programmed c-section. While the decision of shifting the date of birth will affect the maturation of the fetus at birth, which can in turn affect health, delivery method may have direct effects on infant health.[32] In this section we analyze whether inductions or c-sections were the main mechanism for birth timing manipulation, and whether the shifting of births was driven by an increase in the overall

[31] In Spain private hospitals can be privately owned and operated, or privately owned, but dependent on the National Health System. We run the analysis with the two alternative definitions of private hospitals. The results are similar.

[32] Jensen and Wüst (2014), for instance, find evidence of positive health effects of planned c-sections versus inductions for breech babies.

incidence of these procedures, versus a shifting of dates for births that would have taken place via induction or c-section in any case. We thus shed light on the extent to which the negative effects on infant health can be traced to method of delivery versus time in the womb.

Our birth-certificate data do not provide information on whether each birth was induced. We do observe c-sections, but only starting in 2007. We supplement the analysis of c-sections from the birth certificate data with an analysis of birth-related maternal hospitalizations from the Hospital Morbidity Survey.

We turn to the direct analysis of c-sections first. About 22% of all births in our birth-certificate data were cesarean sections. We run the analysis both with the data aggregated at the day level, and at the individual level. For the day-level analysis, we estimate equation (1), using the daily number of c-sections as the dependent variable. The coefficient of interest captures any “extra” c-sections in December 2010 relative to January 2011, using the surrounding years as controls. Note that this estimate of the spike in c-sections in December 2010 includes both procedures that were re-scheduled from January to December due to the benefit, and any scheduled c-sections that would have been spontaneous vaginal births in the absence of the benefit cancellation (i.e. both “switched” and “extra” c-sections).

The results for the day-level model (equation 1) are presented in Panel A of Table 8. We detect significantly “too many” daily c-sections in late December 2010; about 120 per day when we focus on the one-week window. Table 2 (and A2) shows that the total increase in the number of deliveries in the last week of December 2010 was close to 280 per day, so that c-sections would account for almost half of the overall spike in December 2010 births.

We then try to assess whether there were any “extra” c-sections as a result of the benefit cancellation, versus just “switching” of births that would have taken place via c-section even in the absence of the reform. In order to do so, we turn to equation (3), which is estimated on the individual-level sample of births, including October and November as additional, control months.

(3) $C\text{-}section_{it} = \alpha + \beta_1(Dec\text{-}Jan)_{it} + \beta_2(Dec2010\text{-}Jan2011)_{it} + \gamma X_{it} + \lambda_t + \varepsilon_{it}$,

The dependent variable is now an indicator of c-section births, and the explanatory variable of interest is a dummy for December 2010 or January 2011 births, so that any switching of scheduled deliveries from January 2011 to December 2010 is not captured, and only "extra" c-sections would lead to a positive coefficient.

The results of this specification are shown in Panel B of Table 8. We find that the incidence of c-sections was not significantly higher during the reform period (December 2010-January 2011), compared with the surrounding years (and relative to October-November). Only when focus on the one-week window do we find a (marginally) significant, although small, effect (of less than 1 percentage point, for an average of 22%). The benefit cancellation thus seems to have increased the number of babies born via c-section only marginally, if at all. These results suggest that a large fraction of the changes in the timing of births can be attributed to a shifting in the timing of c-sections, rather than an increase in their incidence.[33]

We next use the hospital data to confirm the extent to which birth induction vs. c-sections were used as a tool to shift childbirth. Our sample now includes all overnight hospitalizations related to pregnancy, labor, and delivery, in October, November, December or January of 2000-01 to 2011-12. We can split them between "normal deliveries" (which include inductions that do not end up in c-sections), and "complications during pregnancy, labor and delivery" (which include all c-sections).

Again, we estimate both a day-level specification following equation (1), and an individual-level specification following equation (3). When estimating equation (1), the dependent variable is the number of maternal hospitalizations, aggregated by date of admission. The results are shown in panel A of Table 9.[34] The first row confirms our birth-timing results, showing that there were "too many" pregnancy and birth-related hospitalizations in December 2010 compared with January 2011, and relative to previous years. The magnitudes are in fact very close to (less than one standard deviation away from) our main timing effects in Table 3.[35]

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[33] We cannot test directly for "switching" versus "extra" inductions with our data.

[34] The results for maternal hospitalizations using only the subsample of more recent years (2007-08 to 2011-12) are shown in appendix Table A3.

[35] This supports the argument that it was hard to fake the date of birth on the birth certificates, unless women could also fake their hospitalization dates in the hospital records.

The second and third rows in Panel A of Table 9 show the distribution of the increase in maternal hospitalizations between normal deliveries and other complications. We find a significant increase in both, which suggests that some of the shifted births took place through inductions that did not lead to additional complications.

In Panel B in Table 9 (similar to Panel B of Table 8), we test whether the overall incidence of c-sections (or other birth complications) increased. We estimate equation (3) at the level of the individual maternal hospitalization, and use as a dependent variable an indicator for whether the birth was a c-section (or suffered from any other complications). If our health effects were driven by the birth procedure (rather than time in the womb), we would expect to see an increase in the incidence of c-sections or birth-related complications during this period.

The results show that there was no significant increase in the incidence of c-sections or other birth-related complications in the period surrounding the benefit cancellation. These results are consistent with the benefit cancellation mostly affecting the timing of births that would have been c-sections in any case, rather than an increase in the incidence of this procedure versus natural birth.

All in all, our results in this section suggest that the effect of the benefit cancellation on the timing of births took place at least in part via early scheduling of c-sections in private hospitals. We do not find evidence for an increase in the incidence of c-sections or other birth complications.

5. Effects on newborn and infant health

5.1 Empirical strategy

Once it has been established that the benefit cancellation led to the early scheduling of a substantial number of births, we now move on to estimating the potential health impact on the affected babies. We expect that, since a number of babies were born earlier, they must have been born smaller as a result (lower weight at birth), almost mechanically. Moreover, if less time in the womb is detrimental, we may expect later health problems for the same cohort of babies, particularly for the sort of conditions associated with gestational age in the medical literature, such as respiratory disorders.[36]

In order to pin down the causal effect of shifting births forward for non-medical reasons on infant health outcomes, our identification strategy still relies on comparing births taking place near the benefit cancellation date with the surrounding years. However, comparing the health of babies born in December 2010 versus January 2011 would conflate the causal effect of shifting the birth date with composition effects, due to any potential differential characteristics of the families that switched birth from January to December. In order to overcome this "selection" effect, we compare the health of all babies born around the New Year (including both December and January births), in the reform period versus the surrounding years.

To illustrate the potential for composition effects, 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 months in surrounding years, we would rightly conclude that the reform had no effect.[37]

It could still be that other factors were affecting the health of newborns in the reform period compared with the surrounding years, such as the business cycle or weather shocks. In order to account for aggregate time effects, we include October and November as "control" months. We also want to control for family characteristics that may be correlated with newborn health, so we run the regressions at the individual level and include demographic controls.

The sample thus includes births taking place in October, November, December or January of 2000-01 to 2011-12, and the specification is the same as equation (3):

$$(4)\ Health_{it} = \alpha + \beta_1(Dec\text{-}Jan)_{it} + \beta_2(Dec2010\text{-}Jan2011)_{it} + \gamma X_{it} + \lambda_t + \varepsilon_{it},$$

where *Health* is one of a set of variables measuring the health status of newborn baby i, born in year t. We control for demographic characteristics *X*, and include year dummies

[36] See Escobar et al. (2006), Mally et al. (2010), Boyle et al. (2012).

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

(in fact indicators for each October-November-December-January set), as well as a dummy for December-January births. The main explanatory variable, *Dec2010-Jan2011*, takes value 1 for babies born during the reform period, December-January of 2010-11.

The coefficient of interest, β_2, is thus a difference-in-differences estimate that compares outcomes for December-January babies born in the reform period (2010-11) with those born in December-January of the surrounding years, using October-November births as controls. The main identification assumption is that there was no other factor affecting the health of babies born in December 2010-January 2011 differentially with respect to babies born in October-November 2010, other than seasonal factors, present every year.

Our first indicator of health at birth is weight at birth. We use the continuous variable in grams as well as its natural log, and we also construct several binary indicators (birth-weight under 1,500, 2,500, 3,000 and 3,500 grams). As additional measures of health at birth, we analyze late fetal deaths and neonatal mortality. We then estimate regressions for health outcomes during the first 33 months after birth. The dependent variable in these regressions is an indicator for overnight hospitalization in a given age range. We also run separate regressions for the main diagnoses.

We estimate our health regressions on four different samples. First, we limit the sample to only the seven days before and after December 31 (October 31 for the control months). We then extend the window to two, three, and four weeks before and after. The full sample thus includes all births in the last 4 weeks of October and December or the first 4 weeks of November and January, for the twelve years from 2000-01 to 2011-12.[38]

5.2 Health outcomes at birth

The main results for health at birth are reported in Table 10. We first report the results for birth-weight. Figure 2 shows average birth-weight for all babies born close to the New Year, for the twelve years in our sample, with a linear trend estimated without the reform period (we also show birth-weight for October-November births). It is apparent

[38] We also re-do all of the analysis using only the five most recent October-January sets, from 2007-2008 to 2011-2012.

that average birth-weight was unusually low in December-January 2010-11, the benefit cancellation period.

This observation is confirmed in our regression analysis. The regression results from estimating equation (4) for birth-weight are shown in panel A of Table 10 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 15 grams smaller in the reform period. Although this effect may seem small, it is worth remembering that only 6% of babies in this sample were "affected" by the benefit cancellation (see Table 2). Thus, a 15-gram average effect for all newborns implies that affected babies were on average around 260 grams smaller (about an 8% effect).[39] The estimated magnitude of the effect is very similar if we take the two-week window sample (300 grams, or about 9%).

These estimates assume that the benefit cancellation induced no early scheduling other than from January to December. If some births were scheduled earlier within December or January as a result of the policy change, then our estimates for the "affected" babies would be an upper bound. In fact, if we assume that at most as many births were scheduled "within" December or January as "between", a lower bound for the effect on the treated (newborns switched as a result of the policy change) would be half the magnitude, i.e. between 130 and 150 grams.

We also find significant results when we use the natural log of birth-weight as the dependent variable (second row of Panel A in Table 5). Birth-weight in logs is the variable that other papers typically use when studying the medium- and long-term effects of birth-weight (Black et al. 2007, Figlio et al. 2014). Babies born close to the benefit cancellation date weighted on average 0.5 log-points less, implying that affected babies were on average between 4 and 9% smaller.[40]

We also use as dependent variables indicators for babies born below 1,500, 2,500, 3,000 and 3,500 grams. The results for these thresholds are reported in the last four rows of panel A in Table 10. We do not find an increase in the fraction of babies under 1,500

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[39] According to the results in table 1, 1,014 births were moved forward in the 1-week sample, out of a total of 17,791 births (5.7%). Thus, for those babies who were shifted, the effect was -14.8/0.057 = -263 grams. For the 2-week window, the estimated effect for shifted babies is -12.5/0.0412 = -309 grams.

[40] For treated babies, the effect was between -0.005/0.0570 = -8.6 and -0.005/0.114=-4.3.

or 2,500 grams (the two thresholds typically used as indicators of very low and low birth-weight, respectively). We do find that the reform led to a significant increase in the proportion of babies born below 3,000 and 3,500 grams (for a mean birth-weight of 3,200), and thus a corresponding decrease in the fraction above those thresholds.

Our results thus show that the shifting of birth dates led to a significant reduction in birth-weight for the affected babies, although not at the very bottom of the weight distribution. The results are very similar if we run the analysis using only the five most recent years of data (see table A4).[41] Towards the end of the pregnancy (weeks 37 to 39), a fetus is thought to gain about 200 grams a week (Doublet et al. 1997). In our data, one extra week of gestation is associated with about 150 grams higher weight at birth.[42] Thus, our birth-weight results are consistent with effects on gestational length of 1 to 2 weeks per affected newborn baby.

The second panel of Table 10 estimates the effect of the benefit cancellation on late fetal deaths and neonatal mortality, as extreme measures of health. The coefficients are positive, but not significantly different from zero.

Our regression results suggest that the shifting of birth dates as a result of the benefit cancellation led to significantly smaller babies. It would be tempting to claim that, since the fraction of very small babies did not increase, the early scheduling may have had no real health effects. This is not supported by the previous literature, which finds significant long-term effects of birth-weight on a range of outcomes, not only for babies at the bottom of the distribution (Royer, 2011; Figlio et al., 2014). In any case, we provide additional evidence on health effects using data on post-birth hospitalizations, as a more unequivocal measure of health problems.

5.3 Health effects beyond birth

We have documented that the benefit cancellation led to a significant shifting of births towards December 2010, which in turn led to a cohort of babies born earlier and with significantly lower weight. The medical literature has documented that lower gestational

[41] Compared to the findings in Schulkind and Shapiro (2014), our estimates imply a slightly larger impact. Our results imply that a benefit reduction of \$790, the average tax saving reported by SS in the US, would have led to a 0.12% decrease in birth-weight. The corresponding figure implied by their results is about 0.10%.

[42] This coefficient comes from a regression for birth-weight in our sample, where we control for sex and multiplicity, as well as a linear term in weeks of gestation.

age at birth as well as low birth-weight are associated with a number of medical problems shortly after birth, including a higher incidence of respiratory disease (Escobar et al. 2006, Mally et al. 2010, Boyle et al. 2012).

We study health problems after birth using data from the Spanish Hospital Morbidity Survey (HMS), which records all overnight hospitalizations in Spain annually. We only include hospitalizations with an associated medical diagnosis, i.e. we exclude hospital stays for exploration, observation, or testing purposes, as well as the birth hospitalization of healthy newborns. We merge the HMS data with the birth-certificate micro data by date of birth,[43] which allows us to calculate a hospitalization rate by date of birth as the number of hospital stays (in a given age range and/or with a given diagnosis) divided by the total number of children born on a given date. We thus estimate equation (4), at the date rather than the individual level, using as outcome variables a set of hospitalization rates, by age and main diagnosis. The same child may have been hospitalized multiple times, so the results should be interpreted as number of hospital stays per 100 births, and not the fraction of babies with at least one hospital stay. We record hospitalizations from birth until 33 months of age (1,000 days).[44] The main results are reported in Tables 11 and 12.[45]

Table 11 shows that there was no spike in hospitalizations during the first week after birth in the period surrounding the benefit cancellation date.[46] This suggests that the shifting of birthdates around the New Year of 2011 did not lead to an increase in birth complications. We do find a significantly elevated hospitalization rate for our turn-of-the-year babies between 7 days and one month of age. For ages after one month, the coefficients are not statistically significant at 95% for any of the four samples (varying the window around the cutoff date).

The magnitude of the estimated effects suggests that the cohort of affected children suffered a hospitalization rate between 0.0069 and 0.0084 higher than normal during the

[43] Date of birth is estimated using the information provided on date of release from the hospital, plus age in years, months and days, so that some measurement error is likely.

[44] We cannot look at hospitalizations at older ages yet since the most recent data available at the time of writing are those for 2014.

[45] See Table A5 for the results when using the shorter sample (2007-08 to 2011-12 births).

[46] We reported this finding in Borra, González & Sevilla (2016), where we show hospitalization results for ages 0 to 21 days, without disaggregating by diagnosis.

first month of life. Since the average hospitalization rate in this age range was 0.0349, this represents about a 20% increase. The magnitude is highest in the two and three-week window samples.

Note that broadening the range of birth-dates around the threshold has two competing effects. One the one hand, the broader the window, the lower the fraction of affected children (whose birth-date was shifted). On the other hand, as we move away from the threshold, the additional affected children have potentially been shifted by more. The overall impact on the magnitude of the “intent-to-treat” health effects is unclear. The birth-weight results (table 10) suggest that the declining fraction of affected babies dominates, since the magnitude of the coefficient declines as the window widens. However, this is not the case in the hospital effects. We come back to this discussion when we disaggregate these effects by diagnosis.

The effects reported in Table 11 translate into about 500 additional hospitalizations at ages one to four weeks[47] (for between 2,000 and 4,000 “treated” children). Each overnight hospitalization of an infant (aged less than one year) has an estimated average cost of about 4,900€,[48] which implies that the increase in hospitalizations driven by the benefit cancellation had a direct cost of two and a half million Euros. Note, however, that once we aggregate all age groups (last row of Table 11), we find no significant increase in hospitalization rates for the effected cohort.

We then run parallel specifications where the dependent variable is the hospitalization rate for specific diagnoses.[49] We focus on the most frequent category in each age range, but the top three for ages 7-30 and 31-59 days, in order to capture the main driver(s) of the aggregate effects documented in Table 11. The results are reported in Table 12.

The most frequent category at very early ages is “Certain conditions originating in the perinatal period”, which includes “conditions which have their origin (…) before birth through the first 28 days after birth”. About 34% of all hospital stays in our full

[47] In the 4-week window sample, the estimated effect is 0.0074 (0.0084 in the 3-week window sample). Multiplying by the total number of births during the relevant weeks, the total estimated increase in hospital stays is between 462 (0.0084x54,965) and 539 (0.0074x72,771).

[48] According to the Spanish Registry of Hospital Discharges (Ministry of Health 2014).

[49] The data set follows the International Classification of Diseases (ICD-9-CM), which groups all diagnoses in 17 large categories, disaggregated further at 3- and 4-digit levels.

sample fall in this category. We find no increase in "perinatal" hospitalizations in the first week of life, but we do find some evidence of abnormally high hospitalization rates with this diagnosis at ages 7 to 30 days. In this age range, the main 3-digit "perinatal" diagnosis is "perinatal infection", and the results show a significant increase, although only in the 3- and 4-week window samples.

At all but the first two age ranges, the most common diagnosis associated with hospital stays is respiratory disease ("Diseases of the respiratory system", with 24% of all hospital stays in the full sample and 31% of all non-delivery hospital stays), and the most frequent 3-digit code in this category is bronchitis ("acute bronchitis or bronchiolitis"). We find that the cohort of children born close to the benefit cancellation date suffered abnormally high hospitalization rates for respiratory disease during the first two months of life (ages 7-30 and 31-59 days). This effect is driven by bronchitis.

Regarding magnitudes, we can compare the estimated coefficients with the average incidence of respiratory disease and bronchitis in these age ranges. This reveals that the cohort of affected children suffered a 33% increase in hospitalization rates for respiratory disease (36% in bronchitis), at ages 7 to 59 days (using the coefficient from the 2-week window sample).[50] The estimated effects are of similar magnitude when we use only the more recent years as controls (see Table A6).

The coefficients for respiratory disease and bronchitis at ages one to two months (31-59 days) decline in magnitude as we broaden the window of birth-dates around the threshold. This is consistent with our previous results for birth-weight, and reflects the fact that the fraction of affected infants in the sample declines as we move away from the cutoff date.

Overall, the results from Tables 10-12 suggest that the newborns whose birth-date was affected by the benefit cancellation weighed between 150 and 300 grams less at birth compared with control babies, and suffered a much higher risk of overnight hospitalization during the first two months of life, primarily due to respiratory disease. We do not find essentially any significant effects on hospitalization rates after two months of age.

Interpretation and robustness checks

[50] Coefficients 0.0043+0.0071=0.0114, over average incidence 0.0117+0.0227=0.0344.

Our results in Tables 11-12 show that children born in December 2010-January 2011 suffered more overnight hospitalizations during their first two months of life, compared with infants born in the same dates of the surrounding years, and relative to October-November births. We interpret these results as the effect of the cancellation of the baby bonus in January 2011, which we have shown led many families to shift birth from January to December. In this section, we discuss two potential issues with our interpretation. First, were there any confounding factors that could have driven the reported increase in hospitalizations? And second, what specific aspects of the policy change are driving the results?

Regarding potential confounding factors, we are not aware of other contemporaneous policy changes that would have affected December 2010-January 2011 babies differentially with respect to October-November births during their first 15 months of life. However, we may worry that the effects on respiratory disease could be driven by weather or air quality spikes.[51] We have performed two checks in order to rule out this possibility.

First, we checked that the winter of 2010-11 was not one of particularly high incidence of bronchitis among the population aged 2 and older. To this end, we create a daily database with all bronchitis hospitalizations in November, December, January and February of 2000-01 to 2011-12. Our "affected" cohort of children was born in late December-early January of 2010-11, so that the spike in hospitalizations when they were one week to two months old would show up mostly in January-February 2011 hospital stays. Thus, we run regressions where the outcome variable is the daily number of bronchitis hospitalizations (of individuals in a given age range), and the main explanatory variable is an indicator for January-February 2011. We control for calendar month fixed-effects and turn-of-the-year dummies. The results are reported in Table 13. We again detect an abnormally high bronchitis hospitalization rate in early 2011 among one- to two-month olds. However, there is no spike in bronchitis among older children, or among adults. We also don't find any contemporaneous spike in asthma-related hospitalizations, for any age range.

[51] Poor air quality has been shown to affect children's health negatively (see Neidell 2004, Currie et al. 2009, or Corneus and Spiess 2012).

Second, we re-estimated the regressions in Table 12 excluding Madrid from the sample, given that air quality in Madrid is notoriously bad in the winter months, and it is likely that February experiences severe thermal inversions that result in massive increases in air pollution exposure. This could drive our results if there were important age-based nonlinearities, combined with a pollution spike in February 2011.[52] The results are robust to the exclusion of the province of Madrid from the sample (see panel B of Table 14).

A related concern is the influenza season. Recent evidence suggests that gestational length may be affected by maternal flu (Currie and Schwandt, 2013), and the flu season peaks in January-February. In order to rule out that an unusual flu season is driving our results, we re-estimate our baseline regressions, including as a control the overall incidence of the flu in the month of birth (see panel C of Table 14).[53] The coefficients of interest are barely altered.

A different issue is that there are several channels through which the benefit cancellation could have affected the health of the relevant cohort. As we have shown, many children were born early. Shorter gestational age at birth could have had persistent health effects. However, there are at least two other possibilities. The excess of births in December 2010 could have generated congestion in hospitals, pushing doctors to perform births faster or do things differently, with potentially persistent infant health effects. Moreover, babies born in January 2011 did not receive the 2,500 baby bonus, while October-November 2010 births did, which could be an additional reason why December 2010-January 2011 babies have more health problems compared with October-November 2010 ones.

The evidence suggests that congestion effects were probably not important. First, we have found no increase in birth complications around the benefit cancellation date (panel B of Table 9 and first row of Table 11). Moreover, Figure 4 and Table 4 show that the increase in the December 2010 number of births was quite spread out over the last two or three weeks of the year. The highest number of daily births was reached on December 21, but only with less than 10% more births than the busiest day of December

[52] We thank an anonymous referee for pointing out this possibility.

[53] The flu data come from the database of "Diseases of Compulsory Reporting", as made public by the Spanish National Statistical Institute.

2009. We also find that the excess December births were quite spread out geographically.[54]

Finally, the children born in January 2011 did not receive the 2,500 benefit, which could lead to worse health outcomes compared with October-November 2010 infants. We address this possibility by estimating an additional specification where we include February and March as additional "control" months. Children born in February and March of 2011 did not receive the benefit. The results of this additional specification are reported in panel D of Table 14. The hospitalization results remain, suggesting that benefit receipt is not the main driver of the worse health outcomes of December 2010-January 2011 births.[55] In panel E, we show that the results are also robust to controlling for benefit eligibility (a dummy equal to 1 for births taking place between July 2007 and December 2010).

We have thus shown that the cancellation of the baby bonus led to a large number of births being shifted from January 2011 to December 2010 (via scheduled c-sections and inductions). The evidence is consistent with this shifting of birth-dates having had important health effects on the relevant cohort of babies, as reflected in a higher incidence of hospitalizations during the first two months of life, many of those related to respiratory disorders. We do not think this can be attributed to congestion in hospitals, or to the January births not receiving the monetary benefit. The most likely channel seems to be shorter gestational age at birth (lower fetal maturation), given that we did not find a higher incidence of c-sections (see section 4.4).

The medical literature provides evidence of a correlation between gestational age and respiratory disease (Liu et al. 2014, Goyal et al. 2011). In terms of medical pathways, there is evidence that early, scheduled birth ("birth in the absence of labor") may deprive the fetus of certain hormonal changes that take place during the last few weeks of pregnancy and during the onset of spontaneous labor, which affect pulmonary maturation and may contribute to pulmonary dysfunction after birth (Mally et al. 2010, Jain and Eaton 2006, Goldenber and Nelson 1975).

[54] We find that the timing effect was present across the 50 Spanish provinces. Results are available upon request.

[55] We also estimate "placebo" regressions where February and March are labeled as "treated" months, and find no effect of the benefit cancellation on hospitalizations, thus confirming that benefit receipt had no effect in this dimension. Results are available upon request.

The magnitude of our results is also roughly consistent with the correlations reported in the medical literature. For example, Dietz et al. (2011), using hospital data for the US, find that the hospitalization rate within the first two weeks after birth (excluding delivery hospitalizations) was more than 70% higher for children born at 38 weeks, compared with those born at 39-40 weeks. Our results suggest that the children affected by the benefit cancellation suffered a hospitalization rate between 40 and 90% higher than the control group during the first four weeks after birth.[56]

6. Discussion and conclusions

We take advantage of the cancellation of a child benefit in Spain in December 2010 to analyze the effect of scheduling birth early on the health of newborns. We exploit individual-level birth certificate and hospital 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. We also find that the shifting of birth-dates took place at least in part via the early scheduling of c-sections in private hospitals. More importantly, early delivery had significant health consequences for the affected babies. Babies who were born early as a result of the reform weighed between 130 and 300 grams less at birth on average. They were also about 20 percentage points more likely to be hospitalized during their first two months of life.

Our results provide new, credible empirical evidence showing that scheduling birth early for non-medical reasons can have important (short-term) health consequences for babies. We interpret our results as showing that scheduling birth one to two weeks early (for mostly full-term pregnancies) leads to less mature, smaller newborns that are hospitalized more often in their first months of life. Long-term evaluation of these effects is not possible yet, but we can use the findings in the existing literature to place our results into perspective.

[56] Table 2 shows that, in the +/- 4 week window sample, the average hospitalization rate was 0.1421+0.0349=0.177 during the first 30 days after birth. Table 11 shows that the effect of the benefit cancellation was to increase the hospitalization rate by -0.0007+0.0069=0.0043. Given there were 72.771 births in the "treated" 8-week period, this translates into 313 extra hospitalizations (72.771*0.0043). But only between 2,000 and 4,000 children were actually shifted because of the policy change (table 3), so the percentage increase in the hospitalization rate for the treated is between 313/2,053=0.15 and 313/4,106=0.076, which, over the average of 0.177, equals 0.152/0.177=86% or 0.076/0.177=43%. The magnitude is higher if we focus only on the age range 7 to 30 days.

Fligio et al. (2013), using register data from the state of Florida, find that a 10 percent increase in birth weight is associated with about 0.05 of a standard deviation increase in test scores in grades three to eight. They also find that the magnitude of the effect remains at different points in the birth-weight distribution (including over 2,500 grams). Their estimates imply that an 8 percent drop in birth-weight, such as the one that we find, would translate into 0.04 of a standard deviation decrease in test scores, or roughly about one position less in a class of 40 children. Similarly, using Royer's (2011) estimates from California birth records, a decrease in birth weight of 260 grams would imply a drop in educational attainment of the order of 0.10 years.[57] Using Black et al.'s (2007) results for Norway, an 8 percent decrease in birth weight would be estimated to lower the probability of high school completion by about 0.8 percentage points, and full-time earnings by about 1 percent. In addition, Bhalotra and Venkataramani (2015) use data for the US to show that high exposure to respiratory disease during infancy had long term effects on educational attainment, disability, employment, and income.

These estimates are unlikely to translate directly to Spain, since they are derived from data for the US and Norway.[58] We present them only as suggestive of the order of magnitude of the potential long-term effects of scheduling birth early. It is also worth noting that our Spanish December 2010 babies received a 2,500-Euro benefit, which may have had positive compensating effects on their health and development. However, we are not interested in the long-term effects of the Spanish benefit cancellation per se. Combined with recent results in the literature, our findings suggest that tinkering with the timing of birth for convenience (or economic) reasons may have negative long-term effects for babies.

Our findings also 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

[57] Royer's effects, like ours, are driven by babies weighing more than 2,500 grams at birth.

[58] In addition, both Royer (2009) and Black et al. (2007) derive their conclusions from twin births.

devise a not-so-steep cancellation mechanism, so that, for instance, the benefit amount could have declined more slowly over time.

The results also highlight the fact that parents may be willing to trade-off income and health, at least to some extent. In this context, accurate information about the health consequences of scheduling birth early can help inform the decisions of families as well as health professionals.

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Appendix. A simple theoretical framework

We frame our empirical analysis in terms of a simple model of a utility-maximizing household in the tradition of Becker (1965) and Grossman (1972). The model focuses on the tradeoffs faced by a household when deciding whether to schedule birth early (for non-medical reasons), and the resulting impact on the health of the child. We assume that households derive utility from a composite consumption good (*c*) and infant health (*h*):

$$(1)\ U(c,h) = u(c) + v(h),$$

where *u* and *v* are both strictly increasing and concave functions of *c* and *h*, respectively. The household is expecting a child with due date in January 2011, and maximizes utility with respect to the binary decision of whether to schedule birth in December or not, denoted by *s*, where *s* takes value 1 if the birth is scheduled in December, and 0 otherwise.[59] If $s=0$, the birth takes place in January (either spontaneously, or as a result of it having been scheduled for medical reasons).[60]

The household is subject to a budget constraint, and an infant health production function. The budget constraint is $pc=y+bs$, where *p* is the price of the composite consumption good *c* (which is normalized to 1), and *y* is household labor income. We assume that the household supplies 1 unit of labor inelastically, and that leisure does not enter the utility function. The household receives the child benefit *b* only if birth is scheduled in December ($s=1$).

The infant health production function is $h = h(s)$. We denote by h_1 the health outcome of the child under $s=1$ (i.e. if the delivery date is shifted to December), and h_0 as the health outcome under $s=0$ (no shift). We hypothesize (but do not impose) that

[59] We assume that doctors and other health professionals play no explicit role in the decision of scheduling birth early.

[60] The assumption that the scheduling decision *s* is binary obscures the fact that, in practice, there are three steps involved: scheduling the birth or not, and, if so, induction versus c-section, and when. The procedure may have direct effects on infant health, while the timing decision will affect the maturation of the fetus at birth, which can also affect health. We try to disentangle these two effects (procedure vs. timing) in the empirical analysis.

$h_0>h_1$ (i.e. h_1-$h_0<0$).[61] Households may have imperfect information about the infant health production function (the values of h_0 and h_1).[62]

The household will choose to schedule birth early ($s=1$) if and only if: $U(y+b, h_1) > U(y, h_0)$, i.e. $u(y+b) + v(h_1) > u(y) + v(h_0)$, or $u(y+b) - u(y) > - [v(h_1) - v(h_0)]$. The first term in the last inequality is positive (if $b>0$) since u is strictly increasing in income. The second term is 0 if there are no health effects of scheduling birth ($h_1=h_0$), and positive if there are negative health effects (since v is strictly increasing in h). If parents have imperfect information about $h(s)$, they will use their "best guess" when making their decision, perhaps assisted by a medical professional.

Note that, in the absence of the child benefit ($b=0$), a family would only schedule birth in December for medical reasons (if they believe that h_1-$h_0>0$). If (parents believe that) h_1-$h_0<0$, then the benefit cancellation creates a simple trade-off: the household will schedule birth early if and only if the increase in u from receiving b is greater than the potential decrease in v from scheduling early. The household will always schedule early if scheduling is thought to be harmless ($h_1=h_0$).

From this perspective, our empirical analysis can be seen as providing us with an estimate for the average value of h_1–h_0, for the subset of families that chose to schedule birth early as a result of the benefit cancellation.

Note that in this basic version of the model, if scheduling birth early has no health benefits for the infant (h_1-$h_0\leq 0$), then receiving the benefit is the only reason for a family to choose $s=1$. We could easily extend the household utility function in order to incorporate the possibility that families may derive direct utility from scheduling birth early, either for "convenience" reasons, or even for reasons related to the health of the mother.[63] In this extended setup, scheduling birth early increases utility via

[61] We assume that receiving the benefit has no direct effect on infant health (via income). This is because our empirical analysis focuses on health outcomes at birth or shortly thereafter, while benefit receipt takes place with some delay (several months after birth).

[62] In fact, h_1–h_0 is exactly what we are trying to learn about in our empirical analysis.

[63] For example, the utility function could be modeled as: $U(c,h,s)=u(c)+v(h(s))+\gamma s$, where γ would capture the direct utility gains from scheduling birth early. This setup leads to three possible cases: the household may not want to schedule birth (even in the presence of the benefit, $b>0$), it may want to schedule even in the absence of the benefit ($b=0$, for "convenience" reasons γ), or it may not schedule for convenience reasons only, but be "pushed" to schedule by the cancellation of the benefit. This third case would define households

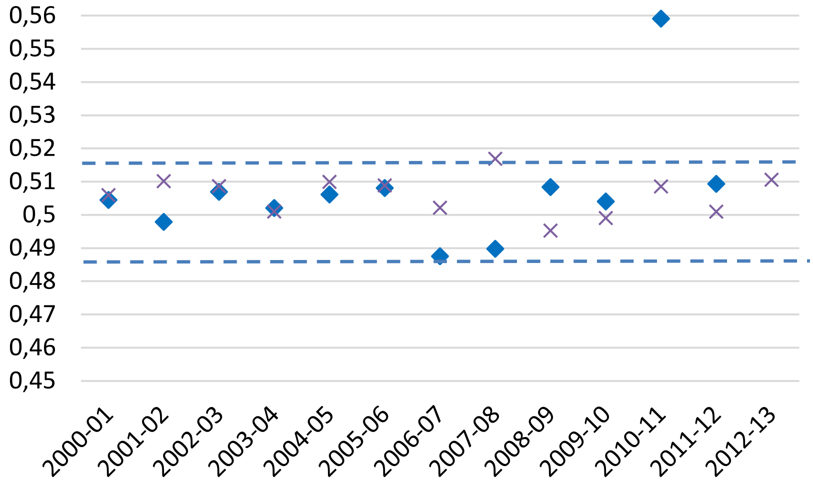
consumption, but also via this additional “convenience” channel. However, the relevant trade-off generated by the benefit cancellation, as well as the interpretation of our empirical results, remains unchanged. The empirical analysis would be providing us with an estimate for the average value of h_1–h_0, for the subset of families “affected” by the benefit cancellation.

This simple model generates implications for the kinds of households that are expected to react to the benefit cancellation. A household will be more likely to schedule birth early ($s=1$) as a result of the policy change if: i) it places a high value on consumption (so that b leads to a large increase in utility via u); and/or ii) it does not place a high value on infant health (via v); and/or iii) its labor income level y is low, so that the marginal utility of income is high; and/or iv) its (expected) health cost of scheduling birth early (h_1–h_0) is small (or negative).

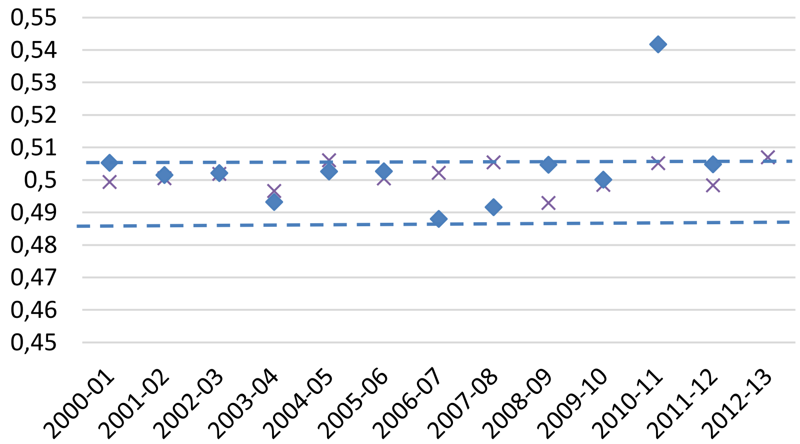
“affected” by the benefit cancellation. The condition for $s=1$ is now: $U(y+b, h_1, 1) > U(y, h_0, 0)$, i.e. $u(y+b) + v(h_1) + \gamma > u(y) + v(h_1)$, or $\gamma + [u(y+b) - u(y)] > - [v(h_1) – v(h_0)]$.

Figure 1. Fraction of births in December, out of all births in Spain close to December 31, in years 2000-01 to 2011-12

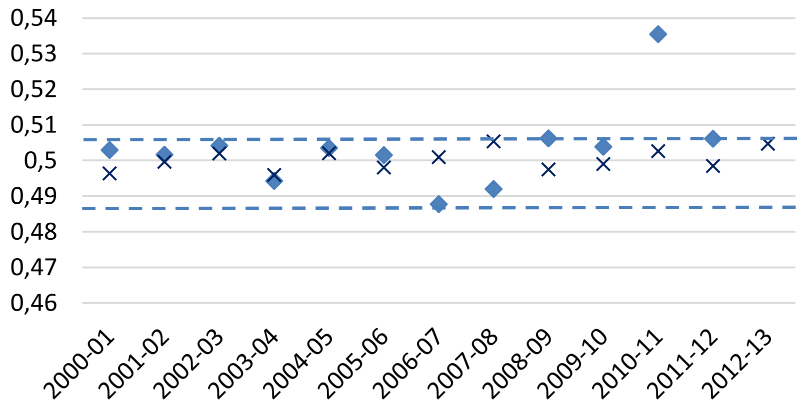
A. One-week window



B. Two-week window



C. Three-week window

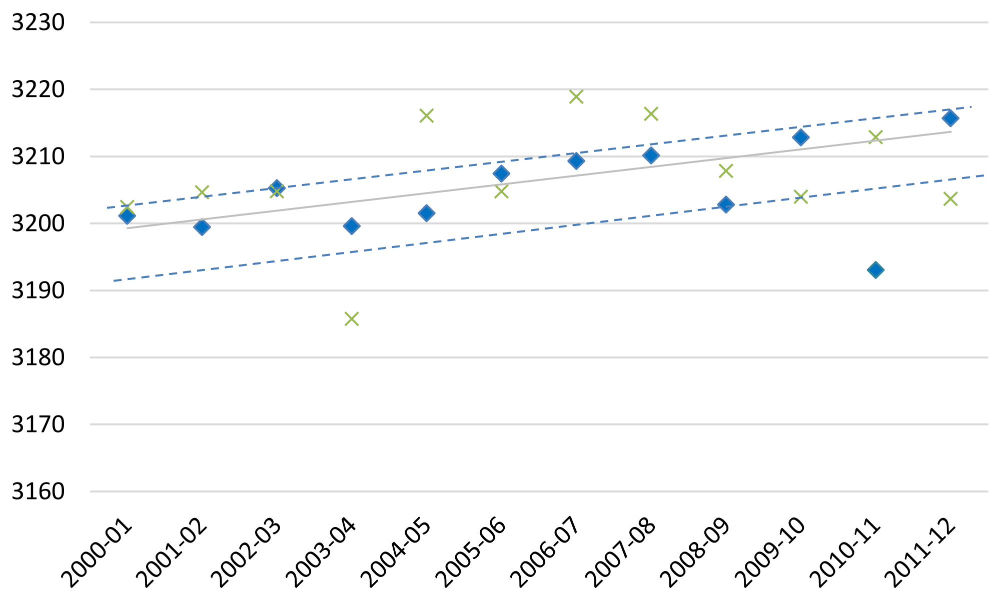


Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012. The crosses show the fraction of October births, out of all births close to October 31. Panel A includes all births between December (October) 25 and January (November) 7; panel B includes births from Dec. (Oct.) 18 to Jan.

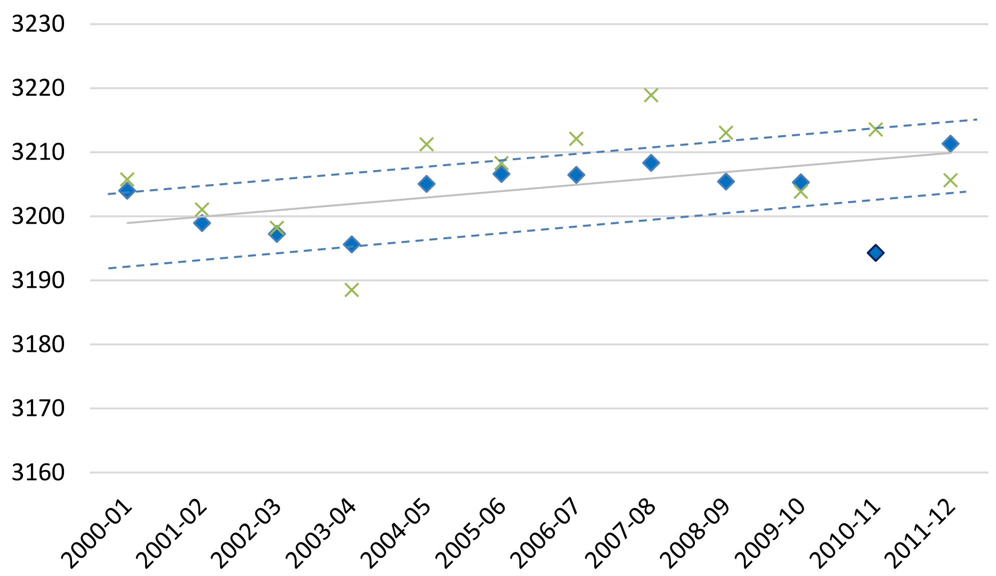
(Nov.) 14, and panel C, from Dec. (Oct.) 11 to Jan. (Nov.) 21. The dotted lines are horizontal, highlighting the range of variation in the non-reform years.

Figure 2. Average birth-weight in grams of all babies born in Spain close to December 31 in years 2000-01 to 2011-12

A. One-week window



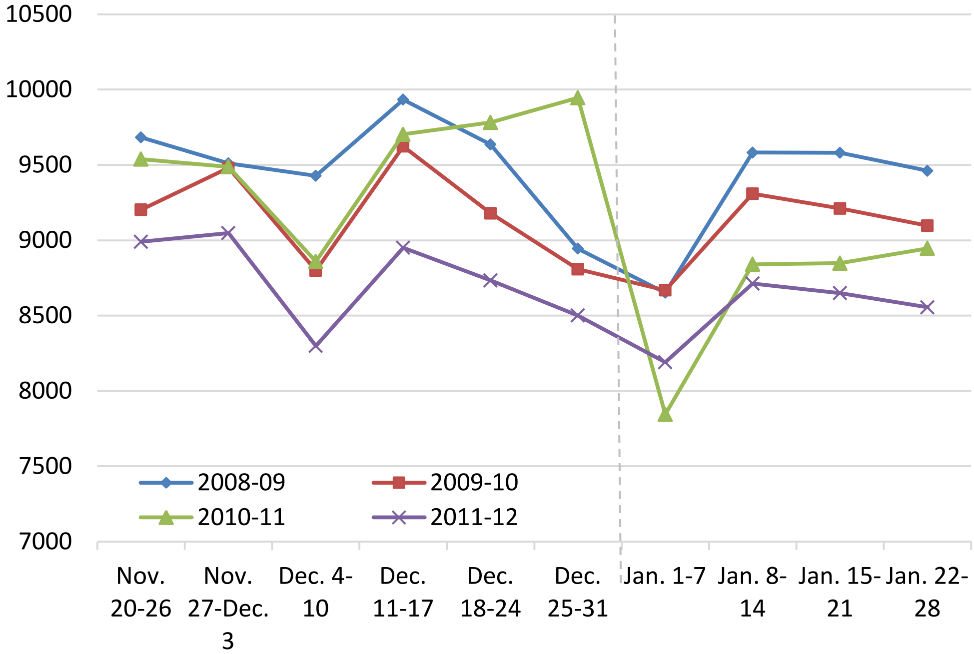
B. Two-week window



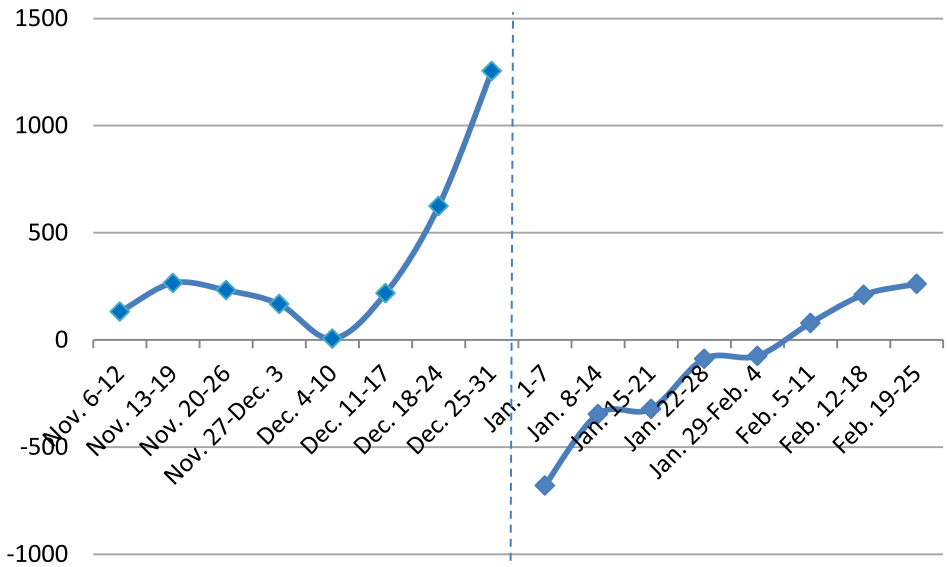
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012. The crosses show average birth-weight for births close to October 31. Panel A includes all births between December (October) 25 and January (November) 7; panel B includes births from Dec. (Oct.) 18 to Jan. (Nov.) 14.The solid line is a linear trend estimated using December and January for all years except 2010-11. The dotted lines are parallel to the linear trend, highlighting the range of variation around the trend in December-January of the non-reform years.

Figure 3. Weekly number of births in Spain, December and January 2008-09 to 2011-12

A. Levels

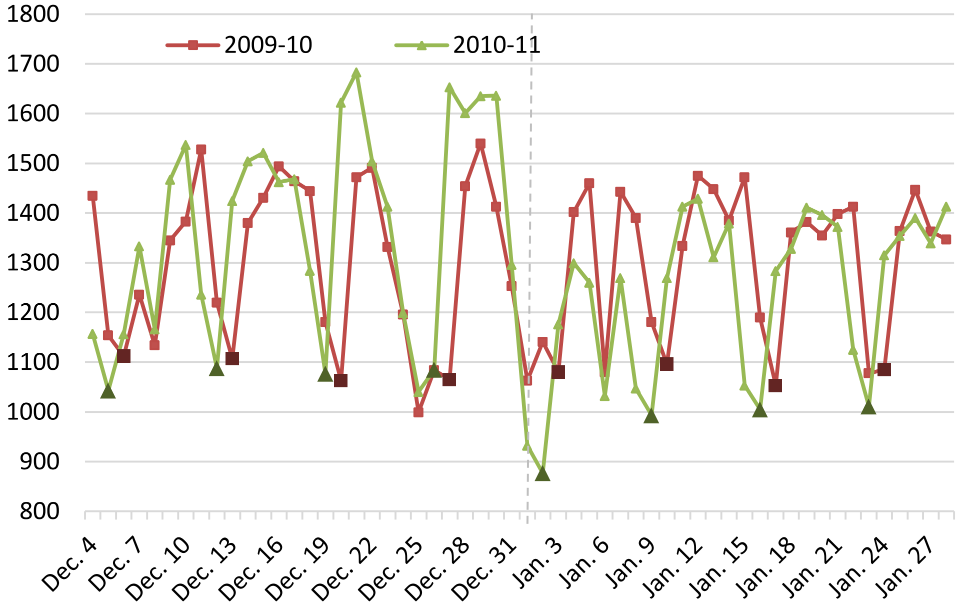


B. Difference between 2010-11 and three surrounding years



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

Figure 4. Daily number of births in Spain, December and January 2009-10 and 2010-11



Note: Sundays are highlighted. Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2011.

Table 1. Previous studies on financial incentives, birth timing and infant health

Policy	Authors and year	Data sources	Births moved per 2010 US$1000	Timing of pregnancy controlled for	Health outcomes at birth	Health outcomes beyond birth
Lower tax liability for December births. *Country:* US. *Amount:* Average tax savings from a December birth about US$790 US. *Incentive*: Bring forward.	Dickert-Conlin, S. and Chandra, A. (1999).	Daily birth data from the US NLSY (1979-1992)	19 p. points in the probability of a last week of Dec. vs. a 1st week of Jan. birth.	No	None	None
	LaLumia, S., Sallee, J.M & Turner, N. (2015)	Social Security administration data plus tax filers data	1 p. point increase in the probability of a last week of Dec. vs. 1st week of Jan. birth.	No	None	None
	Schulkind, Lisa and Shapiro T.M. (2014)	Monthly birth records from the US Vital Statistics (1990 to 2000)	0.3 to 0.4 p. point increase in the probability of a Dec. vs. a Jan. birth.	No	Birth-weight, weeks of gestation, assisted ventilation, Apgar scores, delivery method.	None
Introduction of a baby bonus. *Date:* July 1, 2004. *Country:* Australia. *Amount:* $3000. Replaced an income-dependent benefit. *Incentive:* Postpone.	Gans, Joshua S. & Leigh, Andrew (2009)	Daily birth data from Australian birth records (1975-2004)	3.1 p. point increase in the probability of a first week of July vs. last week of June birth.	Yes (announcement a few weeks in advance)	Birth-weight, delivery method, infant mortality	None
Reform of parental leave system and benefits. *Date:* January 1, 2007. *Country:* Germany. *Amount:* Btw €3,600 less and €25,200 more (earnings-dependent), paid for up to 14 months. *Incentive:* Postpone.	Tamm, M. (2012)	Daily birth data from German Birth records (2000-2007)	1.8 p. point increase in the probability of a 1st week of Jan. vs. last week of Dec. birth.	No (announcement in September 2006)	Birth weight, length at birth	None
	Neugart, M. and Ohlsson, H. (2013)	Daily birth data from German Birth records (2004-2007). Working mothers only.	0.8 p. point increase in the probability of a 1st week of Jan. vs. last week of Dec. birth.	No (announcement in September 2006)	None	None
Abolition of a child benefit. *Date:* January 1, 1997. *Country:* Austria. *Amount:* Max €1,090. *Incentive:* Bring forward.	Brunner, Beatrice & Kuhn, Andreas (2013)	Monthly birth data from Austrian Birth Statistics (1990-2006)	2.7 p. point increase in the probability of a Dec. vs. a Jan. birth	No (announced 10 months in advance)	Birth weight, length at birth, delivery method	None
Abolition of a child benefit. *Date:* January 1, 2011. *Country:* Spain. *Amount:* €2.500. *Incentive:* Bring forward	This paper	Daily birth data from birth certificates (2000-2012) and Hospital Morbidity Survey (2000-2014).	1.8 p. point increase in the probability of a last week of Dec. vs. 1st week of Jan. birth	Yes (announced in May 2010)	Birth weight, weeks of gestation, neonatal mortality, delivery method, birth complications	Hospitalizations 0-33 months

Notes: Papers are ordered chronologically within each country/policy. We compute the percentage point effects on the timing of births associated with a US$1,000 change in 2010 dollar terms. To that end, we translate each benefit amount to dollars in the corresponding benefit year using data on Purchasing Power Parities (OECD 2016), and inflate that amount to 2010 US$ using the Consumer Price Index (BLS, 2016). Details of these calculations are available on request.

Table 2. Descriptive statistics (births in October-January, 2000-01 to 2011-12)

	Average	Stdev.	Min	Max
Panel A. Health Outcomes at Birth				
Birth weight	3,206	(540.3)	500	6500
BW<1,500	0.0095	(0.097)	0	1
BW<2,500	0.0805	(0.272)	0	1
BW<3,000	0.2954	(0.456)	0	1
BW<3,500	0.7089	(0.454)	0	1
Premature	0.0710	(0.257)	0	1
Mother's age	30,88	(5.334)	12	55
Father's age	32,96	(7.284)	0	83
No reported father	0.0176	(0.131)	0	1
Married	0.7039	(0.457)	0	1
Immigrant mother	0.1633	(0.370)	0	1
First birth	0.5545	(0.497)	0	1
Twins	0.0201	(0.140)	0	1
Girl	0.4853	(0.500)	0	1
Gestation weeks	39.04	(1.933)	20	46
Gestation weeks <37	0.0734	(0.261)	0	1
Gestation weeks =37-38	0.2211	(0.415)	0	1
Gestation weeks =39-40	0.5357	(0.499)	0	1
Gestation weeks >40	0.2956	(0.456)	0	1
Late fetal deaths (per 1,000 births)	0.0032	(0.057)	0	1
Mortality within 24 hours (per 1,000 births)	0.0007	(0.026)	0	1
Panel B. Hospital stays over number of births				
Total, age 0-33 months	0.4369	(0.065)	0.2316	1.3861
Age <7 days	0.1421	(0.021)	0.0267	0.2454
Age 7-30 days	0.0349	(0.011)	0.0087	0.0862
Age 31-59 days	0.0386	(0.013)	0.0015	0.1139
Age 60-89 days	0.0265	(0.010)	0.0008	0.0708
Age 90-179 days	0.0390	(0.012)	0.0112	0.1287
Age 6-12 months	0.0477	(0.015)	0.0123	0.2196
Age 12-33 months	0.1082	(0.031)	0.0468	0.6151
Respiratory, age 7-30 days	0.0117	(0.007)	0	0.0366
Bronchitis, age 7-30 days	0.0094	(0.006)	0	0.0366
Respiratory, age 31-59 days	0.0227	(0.011)	0	0.0729
Bronchitis, age 31-59 days	0.0183	(0.010)	0	0.0662

Sources: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012 (Panel A) and Hospital Morbidity Survey, 2000-2013 (Panel B). Note: The sample includes all births in the last 4 weeks of October and December and the first 4 weeks of November and January, for years 2000-01 to 2011-12. The unit of observation is the birth for the gestational age outcomes, the individual baby (including multiple births) for birth-weight and mortality, and the day (birth-date) for the hospitalization variables. The total number of observations (individual babies) is

1,712,552 (but there are 5% missing observations for birth-weight, and about 15% for gestation weeks), and 1,344 days (birth-dates).

Table 3. The effect of the benefit cancellation on the timing of births

	+/-1 week	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
Dep. var.: Number of births				
	289.90***	212.23***	179.60***	146.67***
	(43.522)	(31.079)	(23.221)	(21.267)
Number of births moved	1014	1484	1886	2053
Dep. var.: log(number of births)				
	0.224***	0.162***	0.138***	0.113***
	(0.028)	(0.022)	(0.017)	(0.015)
Share of births moved	12%	9%	7%	6%
N	168	336	504	672
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 2011-12. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). Robust standard errors are shown in parentheses. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Table 4. Week-by-week effects of benefit cancellation on the timing of births

Dep. var.	N. of births	ln(n. births)	N. of births	ln(n. births)	N. of births	ln(n. births)
Nov. 13-19					21.08	0.0158
					(18.92)	(0.0141)
Nov. 20-26					31.69*	0.0240*
					(16.41)	(0.0124)
Nov. 27-Dec. 3			23.94	0.0178	23.99	0.0181
			(17.75)	(0.0130)	(15.25)	(0.0112)
Dec. 4-10	5.32	0.0069	2.28	0.0049	2.29	0.0052
	(34.90)	(0.0247)	(34.75)	(0.0248)	(33.01)	(0.0235)
Dec. 11-17	48.35***	0.0357***	45.33***	0.0337***	45.38***	0.0340***
	(14.99)	(0.0107)	(15.12)	(0.0108)	(12.58)	(0.0090)
Dec. 18-24	66.13*	0.0465**	63.11*	0.0445**	63.16*	0.0448*
	(34.93)	(0.0218)	(34.93)	(0.0218)	(34.33)	(0.0212)
Dec. 25-31	162.30***	0.1187***	159.28***	0.1167***	159.33***	0.1170***
	(33.19)	(0.0238)	(33.42)	(0.0238)	(32.60)	(0.0232)
Jan. 1-7	-119.78***	-0.0990***	-122.85***	-0.1010***	-122.94***	-0.1009***
	(31.29)	(0.0230)	(30.64)	(0.0227)	(29.66)	(0.0222)
Jan. 8-14	-73.96***	-0.0593***	-76.98***	-0.0613***	-76.93***	-0.0610***
	(20.71)	(0.0162)	(20.43)	(0.0162)	(17.80)	(0.0143)
Jan. 15-21	-66.17***	-0.0533***	-69.19***	-0.0552***	-69.14***	-0.0550***
	(19.88)	(0.0159)	(20.11)	(0.0162)	(17.06)	(0.0139)
Jan. 22-28	-43.11***	-0.0334***	-46.14***	-0.0354***	-46.09***	-0.0351***
	(16.05)	(0.0114)	(15.73)	(0.0114)	(13.40)	(0.0095)
Jan. 29-Feb. 4			-29.05*	-0.0207*	-28.99**	-0.0204*
			(16.75)	(0.0124)	(14.14)	(0.0104)
Feb. 4-11					-31.33**	-0.0230***
					(12.35)	(0.0086)
Feb. 12-18					-3.40	-0.0028
					(11.24)	(0.0096)
N. of weeks moved per birth	-0.0766***		-0.0782***		-0.0837***	
	(0.0161)		(0.0145)		(0.0133)	
Sample	Nov.27- Feb.4		Nov.20- Feb.11		Nov.6- Feb.25	
N	744		1008		1344	
Year dummies	Y		Y		Y	
Day of week d.	Y		Y		Y	
Holiday d.	Y		Y		Y	
Year*day of w.	Y		Y		Y	
Day of year d.	Y		Y		Y	

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

Note: Each column corresponds to a different regression. An observation is a day. The sample includes all births in the sample weeks, from 2000-01 to 2011-12. 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). Robust standard errors are shown in parentheses. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Table 5. The effect of the benefit cancellation on the number of births by gestational length

	+/-1 week	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
Dep. var.: N. of births under 37 weeks	0.1981**	0.1162**	0.1051**	0.0791**
(in logs)	(0.0752)	(0.0477)	(0.0410)	(0.0357)
Dep. var.: N. of births 37-38 weeks	0.2937***	0.2135***	0.1582***	0.1293***
(in logs)	(0.0562)	(0.0437)	(0.0336)	(0.0292)
Dep. var.: N. of births 39-40 weeks	0.2305***	0.1706***	0.1501***	0.1245***
(in logs)	(0.0283)	(0.0239)	(0.0184)	(0.0168)
Dep. var.: N. of births over 40 weeks	-0.0040	0.0477	0.0400	0.0297
(in logs)	(0.0745)	(0.0739)	(0.0628)	(0.0539)
N	168	336	504	672
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 (by gestational length) 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 2011-12. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). Robust standard errors are shown in parentheses. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Table 6. The effect of the benefit cancellation on the timing of births, individual-level analysis

Panel A. 2000-2012 sample	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Baseline model				
Reform	0.0582***	0.0448***	0.0369***	0.0306***
	(0.004)	(0.003)	(0.002)	(0.002)
Model with interactions				
Reform	0.0705***	0.0533***	0.0460***	0.0342***
	(0.012)	(0.008)	(0.007)	(0.006)
Reform*	0.0167	0.0132	0.0041	0.0048
Mom under 25	(0.013)	(0.009)	(0.008)	(0.007)
Reform*	0.0229**	0.0167**	0.0102*	0.0125***
Mom over 35	(0.010)	(0.007)	(0.005)	(0.005)
Reform*	-0.0265***	-0.0170**	-0.0124**	-0.0080
Immigrant mom	(0.010)	(0.007)	(0.006)	(0.005)
Reform*	-0.0223***	-0.0117**	-0.0089*	-0.0036
First birth	(0.008)	(0.006)	(0.005)	(0.004)
Reform*	0.0659**	0.0277	0.0291*	0.0205
Twins	(0.027)	(0.018)	(0.015)	(0.013)
Reform*	0.0005	-0.0048	-0.0040	-0.0050
Married mother	(0.009)	(0.006)	(0.005)	(0.004)
Reform*	0.0177	0.0207	0.0265	0.0168
No registered dad	(0.030)	(0.020)	(0.016)	(0.014)
Reform*	-0.0014	-0.0065	-0.0089*	-0.0095**
High-skill occup. mother	(0.009)	(0.007)	(0.005)	(0.005)
Reform*	-0.0008	0.0040	0.0036	0.0043
Capital	(0.009)	(0.007)	(0.005)	(0.005)
Reform*	-0.0159	-0.0055	-0.0072	-0.0077
Rural	(0.010)	(0.007)	(0.006)	(0.005)
Reform*	0.0025	-0.0013	-0.0011	0.0034
Girl	(0.008)	(0.005)	(0.004)	(0.004)
N	198,318	409,408	621,056	825,449

Panel B. 2007-2012 sample	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Baseline model				
Reform	0.0534***	0.0407***	0.0325***	0.0262***
	(0.004)	(0.003)	(0.003)	(0.002)
Model with interactions				
Reform	0.0631***	0.0542***	0.0477***	0.0355***
	(0.013)	(0.009)	(0.007)	(0.006)
Reform*	0.0210**	0.0050	0.0030	0.0076
Any parent university educated	(0.010)	(0.007)	(0.006)	(0.005)
Reform*	0.0233*	0.0172*	0.0052	0.0057
Mom under 25	(0.014)	(0.010)	(0.008)	(0.007)
Reform*	0.0157	0.0120*	0.0050	0.0081
Mom over 35	(0.010)	(0.007)	(0.006)	(0.005)
Reform*	-0.0229**	-0.0191**	-0.0141**	-0.0092*
Immigrant mom	(0.011)	(0.008)	(0.006)	(0.005)
Reform*	-0.0292***	-0.0201***	-0.0182***	-0.0135***
First birth	(0.009)	(0.006)	(0.005)	(0.004)
Reform*	0.0524*	0.0187	0.0244	0.0137
Twins	(0.028)	(0.020)	(0.016)	(0.014)
Reform*	-0.0014	-0.0081	-0.0089*	-0.0095**
Married mother	(0.009)	(0.006)	(0.005)	(0.005)
Reform*	0.0178	0.0125	0.0191	0.0119
No registered dad	(0.031)	(0.022)	(0.017)	(0.015)
Reform*	-0.0045	-0.0025	-0.0037	-0.0071
High-skill occup. mother	(0.011)	(0.008)	(0.006)	(0.006)
Reform*	-0.0018	0.0042	0.0048	0.0054
Capital	(0.010)	(0.007)	(0.006)	(0.005)
Reform*	-0.0113	-0.0052	-0.0056	-0.0073
Rural	(0.011)	(0.008)	(0.006)	(0.005)
Reform*	0.0024	-0.0015	-0.0007	0.0031
Girl	(0.008)	(0.006)	(0.005)	(0.004)
N	87,677	180,451	273,625	363,396

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

Note: Each column in each of the four sub-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 2011-12 (2007-08 to 2011-12 in Panel B). 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). Robust 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, and dummies for first-borns, female babies, and multiple births, as well as province fixed-effects (also parents education in Panel B). Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Table 7. 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*Private beds per	0.0212**	0.0232**	0.0227**	0.0224**
1,000 inhabitants in province	(0.0101)	(0.0093)	(0.0087)	(0.0092)
Reform*Private beds over	0.0876**	0.0887**	0.0838**	0.0819**
total hospital beds in province	(0.0414)	(0.0374)	(0.0347)	(0.0344)
N	198,318	409,408	621,056	825,449
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 2011-12. "Reform" is a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Standard errors, clustered by province, 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, and dummies for first-borns, female babies, and multiple births.
Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012, National Catalogue of Hospitals, Spanish Ministry of Health, 2000-2012, and population by province 2000-2012, Spanish National Statistical Institute.

Table 8. The effect of benefit cancellation on the incidence of cesarean sections

	+/-1 week	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
		Panel A. Daily-level analysis		
Dep. var.: Number of births	119.48***	81.26***	61.97***	46.91***
by cesarean section	(37.737)	(18.321)	(14.196)	(12.722)
N	70	140	210	280
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
		Panel B. Individual-level analysis		
Dep.var.: C-section birth	0.0085*	0.0025	0.0035	0.0018
	(0.005)	(0.003)	(0.003)	(0.002)
N	185,480	378,154	570,411	762,012
Demographic controls	Y	Y	Y	Y

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

Note: Each coefficient comes from a different regression. In Panel A, an observation is a day. The sample includes births delivered by caesarean section 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 2007-08 to 2010-12. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). In Panel B, an observation is an individual birth. The sample includes all births in the last 1 to 4 weeks of October and December or the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January sets from 2007-08 to 2011-12. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). 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, a linear time trend, year fixed effects, and province fixed-effects. Robust standard errors are shown in parentheses. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012.

Table 9. The effect of benefit cancellation on maternal hospitalizations

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Panel A. Daily-level analysis				
Dep. var.: Number of maternal hospitalizations (Mean 1003.7)	335.81***	205.61***	158.71***	124.40***
	(47.399)	(37.677)	(28.669)	(24.071)
Dep. var.: Number of normal deliveries (Mean 253.9)	57.22***	35.46***	28.65***	25.74***
	(12.129)	(9.359)	(7.395)	(5.983)
Dep. var.: Number of birth-related complications (Mean 749.8)	278.59***	170.15***	130.06***	98.66***
	(39.175)	(31.525)	(24.227)	(20.592)
Dep. var.: Average duration of maternal hospit. in days (Mean 3.37)	0.2042**	0.1029*	0.0315	-0.1193*
	(0.0949)	(0.0540)	(0.0433)	(0.0673)
N	168	336	504	672
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
Panel B. Individual-level analysis				
Dep. var.: C-sections and other complications (Mean 0.779)	0.0072	0.0045	0.0022	0.0033
	(0.0046)	(0.0032)	(0.0026)	(0.0023)
N	298,380	606,186	914,962	1,223,068
Demographic controls (maternal age)	Y	Y	Y	Y

(*** 99%, ** 95%, * 90%) Note: Each coefficient comes from a different regression. In Panel A, an observation is a day. The sample includes all birth-related maternal hospitalizations (CIE 9-MC 640-669) 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 2011-12. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). For Panel B, an observation is a hospitalization. The sample includes all birth- related maternal hospitalizations in the last 1 to 4 weeks of October and December or the first 1 to 4 weeks of November and January (depending on the column), from 2000-01 to 2011-12. The coefficients shown correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Control variables include: maternal age, a binary indicator for all December-January births, and year fixed effects. Robust standard errors are shown in parentheses.

Data source: Hospital Morbidity Survey micro data, Spanish National Statistical Institute, 2000-2012.

Table 10. The effect of benefit cancellation on birth-weight and mortality

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Panel A. Birth weigth outcomes				
Dep. var.: Birth weight	-14.755***	-12.509***	-5.589*	-3.650
	(5.696)	(3.966)	(3.233)	(2.799)
Dep. var.: Birth weight	-0.0049**	-0.0045***	-0.0020*	-0.0013
(in logs)	(0.002)	(0.001)	(0.001)	(0.001)
Dep. var.: BW<1,500	0.0004	0.0011	0.0004	0.0003
	(0.001)	(0.001)	(0.001)	(0.001)
Dep. var.: BW<2,500	0.0013	0.0014	0.0003	0.0005
	(0.003)	(0.002)	(0.002)	(0.001)
Dep. var.: BW<3,000	0.0092*	0.0064*	0.0035	0.0030
	(0.005)	(0.003)	(0.003)	(0.002)
Dep. var.: BW<3,500	0.0095*	0.0071**	0.0021	0.0021
	(0.005)	(0.003)	(0.003)	(0.002)
N	397,505	809,882	1,220,263	1,627,681
Panel B. Mortality outcomes				
Dep. var.: Late fetal death	0.375	0.477	0.239	0.292
(per 1,000 births)	(0.600)	(0.412)	(0.341)	(0.295)

Dep. var.: Neonatal mortality (first 24 hours) (per 1,000 births)	0.009 (0.314)	0.210 (0.195)	0.207 (0.154)	0.029 (0.134)
N	418,539	852,606	1,283,972	1,712,552

(*** 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 October and December or the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January quadruplets from 2000-01 to 2011-12. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Robust 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, a binary indicator for all December-January births, a linear time trend, year fixed effects, and a set of 50 province fixed-effects. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Table 11. The effect of benefit cancellation on infant hospitalizations by age

Dep. var.: Hospitalization rate	**+/- 1 weeks**		**+/- 2 weeks**		**+/- 3 weeks**		**+/- 4 weeks**	
Age <7 days	-0,0007		-0,0002		0,0005		-0,0031	
	(0,0099)		(0,0060)		(0,0047)		(0,0039)	
Age 7-30 d. (1 week to 1 month)	0,0069	**	0,0081	***	0,0084	***	0,0074	***
	(0,0035)		(0,0023)		(0,0019)		(0,0018)	
Age 31-59 days (1-2 months)	0,0062		0,0055	*	0,0023		0,0012	
	(0,0054)		(0,0033)		(0,0026)		(0,0023)	
Age 60-89 days (2-3 months)	-0,0018		-0,0020		-0,0026		-0,0010	
	(0,0044)		(0,0027)		(0,0020)		(0,0017)	
Age 90-179 days (3-6 months)	-0,0066		-0,0036		-0,0022		-0,0011	
	(0,0046)		(0,0029)		(0,0022)		(0,0019)	
Age 180-364 days (6-12 months)	-0,0028		-0,0004		0,0009		0,0005	
	(0,0082)		(0,0045)		(0,0032)		(0,0025)	
Age 12-33 months (365-1000 d.)	-0,0153		-0,0127		-0,0110	*	-0,0094	*
	(0,0160)		(0,0087)		(0,0065)		(0,0056)	
Age 0-33 months	-0,0141		-0,0053		-0,0036		-0,0056	
	(0,0351)		(0,0194)		(0,0143)		(0,0120)	
N	336		672		1008		1344	

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

Note: Each coefficient comes from a different regression. An observation is a day (birth-date). The sample includes all days in the last 1 to 4 weeks of October and December and the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January sets from 2000-01 to 2011-12. The dependent variable is the number of overnight hospitalizations in a given age range of children born on a given day, divided by the number of children born on that day. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Control variables include calendar month dummies, and year fixed effects. Standard errors are shown in parentheses. Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2000-2013.

Table 12. The effect of benefit cancellation on hospitalizations by diagnosis and age

Dep. var.: Hospitalization rate	+/- 1 weeks		+/- 2 weeks		+/- 3 weeks		+/- 4 weeks	
Age 0-6 days								
Perinatal conditions (chapter 15)	0,0038		0,0033		0,0034		0,0002	
	(0,0087)		(0,0053)		(0,0042)		(0,0035)	
Age 1-4 weeks (7-30 days)								
Perinatal conditions (chapter 15)	0,0029		0,0036	***	0,0030	***	0,0023	**
	(0,0018)		(0,0012)		(0,0011)		(0,0011)	
Perinatal infection	0,0012		0,0009	*	0,0014	***	0,0012	***
	(0,0007)		(0,0005)		(0,0005)		(0,0004)	
Respiratory disorders (chapter 8)	0,0033	*	0,0043	***	0,0051	***	0,0049	***
	(0,0018)		(0,0012)		(0,0011)		(0,0010)	
Bronchitis	0,0023		0,0032	***	0,0044	***	0,0043	***
	(0,0016)		(0,0011)		(0,0010)		(0,0009)	
Ill-defined conditions (chapter 16)	0,0001		0,0002		0,0002		0,0001	
	(0,0009)		(0,0006)		(0,0005)		(0,0005)	
Age 1-2 months (31-59 days)								
Respiratory disorders (chapter 8)	0,0096	**	0,0071	***	0,0046	**	0,0029	
	(0,0038)		(0,0024)		(0,0019)		(0,0017)	
Bronchitis	0,0086	**	0,0067	***	0,0046	***	0,0028	*
	(0,0034)		(0,0021)		(0,0017)		(0,0016)	
Ill-defined conditions (chapter 16)	-0,0012		-0,0011	*	-0,0008	*	-0,0007	
	0,0009		0,0006		0,0005		0,0004	
Infectious diseases (chapter 1)	-0,0002		0,0004		-0,0002		-0,0001	
	(0,0007)		(0,0005)		(0,0004)		(0,0004)	
Age 2-3 months								
Respiratory disorders (chapter 8)	-0,0027		-0,0031	*	-0,003	**	-0,0019	
	(0,0026)		(0,0017)		(0,0014)		(0,0012)	
Bronchitis	-0,0012		-0,0015		-0,0019		-0,0011	
	(0,0023)		(0,0015)		(0,0012)		(0,0010)	
Age 3-6 months								
Respiratory disorders (chapter 8)	-0,0032		-0,0019		-0,001		-0,0006	
	(0,0021)		(0,0015)		(0,0012)		(0,0011)	
Bronchitis	-0,0014		-0,0006		0,0000		0,0001	
	(0,0018)		(0,0012)		(0,0010)		(0,0009)	
Age 6-12 months								
Respiratory disorders (chapter 8)	-0,0005		0,0007		0,0019		0,0014	
	(0,0027)		(0,0016)		(0,0012)		(0,0011)	
Bronchitis	0,0000		0,0010		0,0017	**	0,0014	**
	(0,0014)		(0,0009)		(0,0007)		(0,0006)	
Age 12-33 months								
Respiratory disorders (chapter 8)	-0,0013		-0,0015		-0,0021		-0,0022	
	(0,0044)		(0,0026)		(0,0021)		(0,0019)	
Bronchitis	0,00003		-0,0002		-0,0001		-0,0007	
	(0,0016)		(0,0010)		(0,0008)		(0,0007)	
Age 0-33 months								
Respiratory disorders (chapter 8)	0,0017		0,0030		0,0037		0,003	
	(0,0101)		(0,0062)		(0,0047)		(0,0042)	
Bronchitis	0,0053		0,0066		0,0072	**	0,0056	**
	(0,0066)		(0,0041)		(0,0032)		(0,0028)	
N	336		672		1008		1344	

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

Note: Each coefficient comes from a different regression. An observation is a day (birth-date). The sample includes all days in the last 1 to 4 weeks of October and December and the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January sets from 2000-01 to 2011-12. The dependent variable is the number of overnight hospitalizations in a given age range and with a given diagnosis, of children born on a given day, divided by the number of children born on that day. We show the results for the main group(s) (chapters) of diagnoses, and the main single (three-digit) diagnosis, in each age range. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Control variables include calendar month dummies, and year fixed effects. Standard errors are shown in parentheses. Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2000-2013.

Table 13. Bronchitis and asthma prevalence by age

Dep. var.: Number of hospital stays	Bronchitis, in levels		Bronchitis, in logs		Asthma, in levels	Asthma, in logs	
Age 1 week to 2 months	12,53	***	0,3481	***	-0,0356	-0,273	**
	(2,5851)		(0,0983)		(0,0405)	(0,1356)	
Age 2 to 5 years	-0,7894		0,0101		0,4326	0,0414	
	(0,9281)		(0,0959)		(0,7439)	(0,1029)	
Age 2 to 17	-0,1190		0,0479		0,6653	0,0170	
	(1,0436)		(0,0893)		(1,1614)	(0,0853)	
Age 18 plus	-2,5669		-0,0172		5,0331	0,0914	
	(4,8113)		(0,0636)		(3,6135)	(0,0593)	
Age 65 plus	-5,4923		-0,0595		2,5146	0,0804	
	(4,1321)		(0,0657)		(2,2873)	(0,0670)	

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

Note: Each coefficient comes from a different regression. An observation is a day. The sample includes all days in November, December, January and February from 2000-01 to 2011-12 (N=1,443). The dependent variable is the number of bronchitis (or asthma) hospitalizations (in levels or logs) that start on a given day, where the patient is in the specified age range (at release). The coefficients correspond to a binary explanatory variable indicating January-February 2011 stays. Control variables include calendar month dummies, and turn-of-the-year fixed effects. Standard errors are shown in parentheses. Data sources: Hospital Morbidity Survey micro data, Spanish National Statistical Institute, 2000-2013.

Table 14. The effect of benefit cancellation on hospitalizations: Robustness checks

Dep. var.: Hospitalization rate, age 7-59 days	+/- 1 weeks		+/- 2 weeks		+/- 3 weeks		+/- 4 weeks	
Panel A. Baseline								
All stays	0,0131	*	0,0135	***	0,0107	***	0,0086	***
	(0,0072)		(0,0043)		(0,0034)		(0,0031)	
Respiratory disorders (c. 8)	0,0129	***	0,0113	***	0,0098	***	0,0077	***
	(0,0045)		(0,0029)		(0,0023)		(0,0020)	
Bronchitis	0,0109	***	0,0100	***	0,0091	***	0,0071	***
	(0,0041)		(0,0026)		(0,0021)		(0,0018)	
Perinatal conditions (c. 15)	0,0026		0,0037	***	0,0031	***	0,0025	**
	(0,0020)		(0,0013)		(0,0012)		(0,0011)	
Panel B. Dropping Madrid								
All stays	0,0152	*	0,0157	***	0,0126	***	0,0101	***
	(0,0084)		(0,0051)		(0,0040)		(0,0037)	
Respiratory disease	0,0151	***	0,0133	***	0,0115	***	0,0091	***
	(0,0053)		(0,0033)		(0,0027)		(0,0024)	
Bronchitis	0,0127	***	0,0116	***	0,0106	***	0,0084	***
	(0,0048)		(0,0030)		(0,0024)		(0,0022)	
Perinatal conditions	0,0030		0,0042	***	0,0036	***	0,0029	**
	(0,0023)		(0,0015)		(0,0014)		(0,0013)	
Panel C. Controlling for flu								
All stays	0,0130	*	0,0136	***	0,0107	***	0,0085	***
	(0,0072)		(0,0044)		(0,0034)		(0,0031)	
Respiratory disease	0,0125	***	0,0111	***	0,0094	***	0,0073	***
	(0,0045)		(0,0029)		(0,0023)		(0,0021)	
Bronchitis	0,0108	***	0,0099	***	0,0088	***	0,0068	***
	(0,0041)		(0,0026)		(0,0021)		(0,0019)	
Perinatal conditions	0,0025		0,0037	***	0,0032	***	0,0024	**
	(0,0020)		(0,0013)		(0,0012)		(0,0011)	
Panel D. Adding March and February								
All stays	0,0072		0,0100	***	0,0076	***	0,0059	**
	(0,0055)		(0,0034)		(0,0026)		(0,0025)	
Respiratory disease	0,0093	***	0,0096	***	0,0086	***	0,0071	***
	(0,0034)		(0,0022)		(0,0018)		(0,0016)	
Bronchitis	0,0082	***	0,0083	***	0,0078	***	0,0063	***
	(0,0031)		(0,0020)		(0,0016)		(0,0014)	
Perinatal conditions	0,0013		0,0027	**	0,0021	**	0,0010	
	(0,0016)		(0,0011)		(0,0009)		(0,0009)	
Panel E. Controlling for benefit eligibility								
All stays	0,0190	**	0,0174	***	0,0135	***	0,0090	**
	(0,0088)		(0,0053)		(0,0042)		(0,0038)	
Respiratory disease	0,0138	**	0,0120	***	0,0107	***	0,0084	***
	(0,0055)		(0,0035)		(0,0028)		(0,0025)	
Bronchitis	0,0110	**	0,0101	***	0,0094	***	0,0072	***
	(0,0050)		(0,0032)		(0,0025)		(0,0023)	
Perinatal conditions	0,0038		0,0048	***	0,0036	**	0,0020	
	(0,0024)		(0,0016)		(0,0014)		(0,0014)	

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

Note: Each coefficient comes from a different regression. An observation is a day (birth-date). The sample includes all days in the last 1 to 4 weeks of October and December and the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January sets from 2000-01 to 2011-12. The dependent variable is the number of overnight hospitalizations at age 7 to 59 days with a given diagnosis, of children born on a given day, divided by the number of children born on that day. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Control variables include calendar month dummies, and year fixed effects. Standard errors are shown in parentheses. Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2000-2013.

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

(+/-1 week window)	**1**	**2**	**3**	**4**
Dep. var.: Number of births				
	280.08***	282.88***	280.57***	289.90***
	(61.469)	(59.975)	(41.711)	(43.522)
Number of births moved	980	990	982	1015
Dep. var.: ln(number of births)				
	0.216***	0.219***	0.216***	0.224***
	(0.044)	(0.040)	(0.030)	(0.028)
Share of births moved	11%	12%	11%	12%
N	168	168	168	168
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 and the first week of January (depending on the column), for December-January pairs from 2000-01 to 2011-12. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). Robust standard errors are shown in parentheses. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Table A2. The effect of benefit cancellation on the timing of births, 2007-2012 sample

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Dep. var.: Number of births				
	285.56***	206.82***	168.27***	134.44***
	(49.165)	(31.172)	(24.612)	(22.451)
Number of births moved	999	1448	1767	1882
Dep. var.: ln(number of births)				
	0.22***	0.16***	0.13***	0.10***
	(0.034)	(0.022)	(0.018)	(0.016)
Share of births moved	12%	9%	7%	5%
N	70	140	210	280
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 2007-08 to 2011-12. The coefficients shown correspond to a binary explanatory variable indicating December 2010 births (the month right before benefit cancellation). Robust standard errors are shown in parentheses. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012.

Table A3. The effect of benefit cancellation on maternal hospitalizations, 2007-12

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Panel A. Daily-level analysis				
Dep. var.: Number of maternal hospitalizations (Mean 1006.0)	201.91***	157.81***	129.41***	105.78***
	(42.64)	(26.12)	(22.87)	(19.52)
Dep. var.: Number of normal deliveries (Mean 177.4)	29.28*	28.38***	24.93***	21.42***
	(11.81)	(7.42)	(5.92)	(4.90)
Dep. var.: Number of birth-related complications (Mean 828.6)	172.63***	129.43***	104.48***	84.36***
	(33.07)	(22.35)	(19.53)	(16.74)
Dep. var.: Average duration of maternal Hospit. in days (Mean 3.13)	0.0951**	0.0579*	0.0439*	0.0326*
	(0.0325)	(0.0235)	(0.0185)	(0.0154)
N	70	140	210	280
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
Panel B. Individual-level analysis				
Dep. var.: C-sections and other complications (Mean 0.828)	0.0009	0.0035	0.0024	0.0042
	(0.0049)	(0.0034)	(0.0028)	(0.0024)
N	135,682	274,338	412,766	551,073
Demographic controls (maternal age)	Y	Y	Y	Y

(*** 99%, ** 95%, * 90%) Note: Each coefficient comes from a different regression. In panel A, an observation is a day. The sample includes all birth-related maternal hospitalizations (CIE 9-MC 640-669) in the last 1 to 4 weeks of December or the first 1 to 4 weeks of January (depending on the column), for Dec.-Jan. pairs from 2007-08 to 2011-12. The coefficients shown correspond to a binary explanatory variable indicating Dec. 2010 births. For Panel B, an observation is a hospitalization. The sample includes all birth-related maternal hospitalizations in the last 1 to 4 weeks of October and December or the first 1 to 4 weeks of November and January (depending on the col.), from 2007-08 to 2011-12. The coefficients shown correspond to a binary explanatory variable indicating Dec. 2010-Jan. 2011 births. Control variables include: maternal age, a binary indicator for all December-January births, and year fixed effects. Robust standard errors are shown in parentheses. Data source: Hospital Morbidity Survey micro data, Spanish National Statistical Institute, 2007-2012.

Table A4. The effect of benefit cancellation on birth-weight, 2007-2012 sample

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Dep. var.: Birth weight	-14.378**	-10.701**	-3.570	-2.072
	(6.091)	(4.243)	(3.458)	(2.994)
Dep. var.: Birth weight (in logs)	-0.0050**	-0.0039**	-0.0014	-0.0008
	(0.002)	(0.002)	(0.001)	(0.001)
Dep. var.: BW<1,500	0.0005	0.0010	0.0003	0.0001
	(0.001)	(0.001)	(0.001)	(0.001)
Dep. var.: BW<2,500	0.0016	0.0008	0.0002	0.0004
	(0.003)	(0.002)	(0.002)	(0.001)
Dep. var.: BW<3,000	0.0095*	0.0074**	0.0036	0.0029
	(0.005)	(0.004)	(0.003)	(0.003)
Dep. var.: BW<3,500	0.0105*	0.0063	0.0006	0.0006
	(0.0053)	(0.0037)	(0.0030)	(0.0026)
N	175,823	357,968	539,044	719,402

(*** 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 October and December or the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January quadruplets from 2007-08 to 2011-12. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Robust 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, a binary indicator for all December-January births, a linear time trend, year fixed effects, and a set of 50 province fixed-effects. Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012.

Table A5. The effect of benefit cancellation on infant hospitalizations by age, 2007-2012 sample

Dep. var.: Hospitalization rate	+/- 1 weeks		+/- 2 weeks		+/- 3 weeks		+/- 4 weeks	
Age <7 days	-0.0026		-0.0027		-0.0018		-0.0038	
	(0.0048)		(0.0033)		(0.0027)		(0.0023)	
Age 7-30 d. (1 week to 1 month)	0.0037		0.0049	**	0.0053	***	0.0051	***
	(0.0026)		(0.0018)		(0.0015)		(0.0014)	
Age 31-59 days (1-2 months)	0.0089		0.0090	**	0.0066	**	0.0062	**
	(0.0050)		(0.0029)		(0.0022)		(0.0019)	
Age 60-89 days (2-3 months)	0.0002		-0.0004		-0.0011		0.0002	
	(0.0035)		(0.0022)		(0.0017)		(0.0014)	
Age 90-179 days (3-6 months)	-0.0051		-0.0036		-0.0034	*	-0.0029	*
	(0.0031)		(0.0021)		(0.0016)		(0.0014)	
Age 180-364 days (6-12 months)	0.0003		0.0012		0.0018		0.0010	
	(0.0051)		(0.0029)		(0.0022)		(0.0018)	
Age 365-1000 d. (12-33 months)	-0.0077		-0.0102	*	-0.0105	*	-0.0089	*
	(0.0089)		(0.0051)		(0.0042)		(0.0040)	
Age 0-33 months	-0.0023		-0.0019		-0.0030		-0.0031	
	(0.0189)		(0.0111)		(0.0086)		(0.0076)	
N	140		280		420		560	

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

Note: Each coefficient comes from a different regression. An observation is a day (birth-date). The sample includes all days in the last 1 to 4 weeks of October and December and the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January sets from 2007-08 to 2011-12. The dependent variable is the number of overnight hospitalizations in a given age range of children born on a given day, divided by the number of children born on that day. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Control variables include calendar month dummies, and year fixed effects. Standard errors are shown in parentheses. Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2007-2013.

Table A6. The effect of benefit cancellation on hospitalizations by diagnosis and age, 2007-2012 sample

Dep. var.: Hospitalization rate	+/- 1 weeks		+/- 2 weeks		+/- 3 weeks		+/- 4 weeks	
Age 0-6 days								
Perinatal conditions (chapter 15)	-0.0021		-0.0026		-0.0015		-0.0031	
	(0.0046)		(0.0032)		(0.0025)		(0.0022)	
Age 1-4 weeks								
Perinatal conditions (chapter 15)	0.0011		0.0019		0.0018	*	0.0017	*
	(0.0016)		(0.0011)		(0.0009)		(0.0008)	
Perinatal infection	0.0000		-0.0000		0.0006		0.0005	
	(0.0008)		(0.0006)		(0.0005)		(0.0004)	
Respiratory disorders (chapter 8)	0.0029		0.0032	**	0.0037	***	0.0035	***
	(0.0016)		(0.0012)		(0.0010)		(0.0009)	
Bronchitis	0.0021		0.0022	*	0.0030	**	0.0031	***
	(0.0014)		(0.0010)		(0.0009)		(0.0008)	
Ill-defined conditions (chapter 16)	-0.0001		-0.0001		0.0001		0.0001	
	(0.0006)		(0.0004)		(0.0003)		(0.0002)	
Age 1-2 months								
Respiratory disorders (chapter 8)	0.0119	**	0.0100	***	0.0082	***	0.0070	***
	(0.0036)		(0.0021)		(0.0017)		(0.0015)	
Bronchitis	0.0098	***	0.0087	***	0.0072	***	0.0061	***
	(0.0032)		(0.0019)		(0.0015)		(0.0013)	
Ill-defined conditions (chapter 16)	-0.0016	*	-0.0010	*	-0.0008	*	-0.0006	
	(0.0007)		(0.0005)		(0.0004)		(0.0003)	
Infectious diseases (chapter 1)	-0.0003		0.0006		-0.0000		0.0001	
	(0.0006)		(0.0005)		(0.0004)		(0.0003)	
Age 2-3 months								
Respiratory disorders (chapter 8)	-0.0017		-0.0017		-0.0016		-0.0005	
	(0.0020)		(0.0013)		(0.0010)		(0.0009)	
Bronchitis	-0.0006		-0.0004		-0.0008		-0.0001	
	(0.0016)		(0.0011)		(0.0009)		(0.0008)	
Age 3-6 months								
Respiratory disorders (chapter 8)	-0.0024		-0.0019		-0.0018		-0.0019	*
	(0.0016)		(0.0011)		(0.0009)		(0.0009)	
Bronchitis	-0.0016		-0.0012		-0.0014		-0.0015	*
	(0.0013)		(0.0009)		(0.0007)		(0.0007)	
Age 6-12 months								
Respiratory disorders (chapter 8)	-0.0001		0.0005		0.0016		0.0011	
	(0.0022)		(0.0013)		(0.0010)		(0.0008)	
Bronchitis	0.0003		0.0009		0.0017	**	0.0011	*
	(0.0013)		(0.0008)		(0.0006)		(0.0005)	
Age 12-33 months								
Respiratory disorders (chapter 8)	-0.0000		-0.0010		-0.0014		-0.0015	
	(0.0037)		(0.0023)		(0.0018)		(0.0018)	
Bronchitis	0.0010		-0.0001		0.0001		-0.0007	
	(0.0015)		(0.0010)		(0.0008)		(0.0007)	
Age 0-33 months								
Respiratory disorders (chapter 8)	0.0104		0.0091		0.0088	*	0.0077	*
	(0.0084)		(0.0050)		(0.0039)		(0.0035)	
Bronchitis	0.0111		0.0102	**	0.0099	***	0.0079	**
	(0.0058)		(0.0035)		(0.0027)		(0.0024)	
N	140		280		420		560	

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

Note: Each coefficient comes from a different regression. An observation is a day (birth-date). The sample includes all days in the last 1 to 4 weeks of October and December and the first 1 to 4 weeks of November and January (depending on the column), for October-November-December-January sets from 2000-01 to 2011-12. The dependent variable is the number of overnight hospitalizations in a given age range and with a given diagnosis, of children born on a given day, divided by the number of children born on that day. We show the results for the main group(s) (chapters) of diagnoses, and the main single (three-digit) diagnosis, in each age range. The coefficients correspond to a binary explanatory variable indicating December 2010-January 2011 births (the weeks right around benefit cancellation). Control variables include calendar month dummies, and year fixed effects. Standard errors are shown in parentheses. Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2000-2013.