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The Impact of Scheduling Birth Early on Infant Health*

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Abstract: We take advantage of a unique 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 universal "baby bonus" would stop being paid to babies born after December31st, 2010. Using administrative data from birth certificates and hospital records, we find that about 2,000 families shifted their date of birth from January 2011 to December 2010 (outof 9,000 weekly births). The affected babies, born about one week early on average, weighed about 200 grams less at birth, and suffered a sizeable increase in hospitalization rates in the first two months of life, mostly for respiratory disease.

JEL: H31, I12, J13

Keywords: Policy evaluation, child benefit, baby bonus, infant health, fertility, birth-weight.

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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, 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 recent decades, from 15% in 1990 to 26% in 2009 (OECD 2011). In the United States, a recent article in the *New York Times* reported that more than half of all births are "hastened either by drugs or surgery, double the share in 1990".[2]In the UK, about 25% of all births were induced in 2013-14, and 26%were delivered via c-section (Royal College of Obstetrics & Gynecologists 2016). Many of these inductions and c-sections are not medically indicated (Engle and Kominiarek 2008, Mally et al. 2010).

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) and is estimated to weigh more than 2,500 grams.Many 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).[3]There are two concerns with these early elective deliveries. First, 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 thenatural variation in the length of a pregnancy, which can vary by as much as five

[1] A birth can be scheduled via 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] "Heavier Babies do Better in School", The Upshot, *New York Times* 2014/10/12.

[3] 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).

weeks (Jukic et al. 2013; only about 70% of women deliver within 10 days of their due date, Mongelli et al. 1996).

Moreover, the association between gestational age and a range of infant health complications has recently been shown in the medical literature to persist across those thresholds (Madar et al. 1999, Clark et al. 2009, Lindstrom et al., 2009; MacKay et al., 2010; Boyle et al., 2012), respiratory problems in particular.In fact, in the mid-2000's the American Congress of Obstetricians and Gynecologists launched a campaign to eliminate early elective deliveries before the 39th week of pregnancy, absent a clear medical reason (Buckles and Guldi 2016). In economics, a recent literature has documented strong associations between health at birth (proxied by birth-weight) and a range of long-term outcomes,[4] associations not limited to children born below 2,500 grams.

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 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 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.

[4] Including infant and adult health and mortality, test scores, educational attainment, employment, and earnings (Currie & Hyson 1999, Behrman & Rosenzweig 2004, Almond et al. 2005, Black et al. 2007, Oreopoulos et al. 2008, Royer 2009, Johnson & Schoeni 2011,Figlio et al. 2014, among others). 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.

We use detailed, high-quality administrative data from birth and death certificates and hospital records, for the universe of children born in Spain from 2000 to 2012. The simplicity of our policy change, the magnitude of the benefit, and the rich data allow us to estimate timing effects credibly and precisely. We first show that there was a significant spike ("bunching") in the number of births in late December 2010, with a corresponding "hole" in early January 2011. 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 about one week early as a result.

We show that the effect of the benefit cancellation on birth timing was more pronounced among college-educated, 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 (although we cannot rule out completely that c-sections have a stronger direct effect on health for shorter gestation babies).

We are then able to evaluate the short- and medium-term 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 also 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. 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 (Panel A) 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. 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. This is confirmed by our analysis of weeks of gestation at birth.

We are then able to follow up the newborns for the first 33 months (almost three years) after birth. We find that the affected babies experienced a sizeable increase in hospitalization rates, with almost 500 “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 are able to rule out several confounding factors that may have been responsible for the reported increase in hospitalizations, such as air quality or the flu season. We also explore the specific aspects of the policy change that could be driving the results, and conclude that the observed health effects cannot be attributed to hospital congestion, benefit receipt, or maternal stress. The most likely channel seems to be shorter gestational age at birth (lower fetal maturation), driven by a shifting of (elective) scheduled births.

Our paper contributes to a previous literature showing that the timing of birth can react to economic incentives (Dickert-Colin and Chandra 1999,Schulkind and Shapiro 2014, and LaLumia et al. 2015 for the US, Gans and Leigh 2009 for Australia, Tamm 2011 and Neugart and Ohlsson 2012 for Germany, Brunner and Kuhn 2014 for Austria).Several of these studies (see Table 1) also assess the impact of changes in birth timing on health at birth (mainly birth-weight).

To our knowledge, ours 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 and childhood. 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).

The uniqueness of our policy shock and the quality of our data also allow us to contribute to the previous literature in terms of the identification of the effect of economic incentives on the timing of birth, and the importance of announcement effects. We have a sharp and well-publicized reform that cut benefits by a large amount at a pre-specified date. Our policy is universal, and we are able to observe eligibility and the level of the benefit precisely, since it is only a function of date of birth and independent of family income. In contrast, some of the previous papers (such as those for the US, see Table 1) use cross-sectional and time variation in tax benefits, with varying amounts and no clean "control group", and they have to approximate tax savings based on (few) observed household characteristics. In addition, the fact that we have detailed data with precise date of birth (unlike Schulkind and Shapiro 2014 or Brunner and Kuhn 2014, who only observe the month) allows us to focus on the days right around the cutoff, where the effect is most pronounced.

In addition, our policy shock provided individuals with an incentive to bring the birth forward without affecting the timing of conceptions or fertility decisions (at the threshold), given that the benefit was announced only seven months in advance. Thus we can be reasonably certain that our estimated health effects are a result of the timing of birth, as opposed to conception. The birth timing effects found in previous studies, such as Brunner and Kuhn (2014) and Schulkind and Shapiro (2014), are likely to be a combination of birth-scheduling and the timing of pregnancy, thus making the results on newborn health difficult to interpret. We share this advantage with Gans and Leigh (2009), but in their case the reform generated an incentive to postpone birth, rather than bring it forward. In terms of external validity, bringing birth forward is more relevant, given that the increasingly common practice of elective delivery can only shift birth forward, before the woman goes into labor spontaneously.

The remainder of the paper is organized as follows. Section 2 describes the institutional setting and the policy change that generates our natural experiment. Our data sources are presented in section 3. Section 4 describes our results regarding the effect of the benefit cancellation on the timing of births, while section 5 presents the health effects. Section 6 concludes.

2. Institutional framework: The benefit cancellation

In 2007, the Spanish government introduced a new, universal child benefit, which would pay €2,500 to all mothers right after giving birth, in a one-time payment. The new "baby bonus"was to be paid with the only requirement that the mother was a legal resident of Spain for at least two years prior to the birth of the child. The announcement was unexpected and the benefit was introduced retrospectively, thus generating no incentives for parents to manipulate the timing of birth (indeed, González 2013 finds no jump in the monthly number of births around the 2007 threshold). 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. The benefit could be received shortly after birth (as early as three weeks later) if requested explicitly; otherwise it was deducted from the household's tax liability when filing for income taxes corresponding to the year of birth (the following year).

Three years later, the benefit was eliminated without warning in the first round of budget cuts as a result of the economic slowdown. There is no reason to believe that the benefit cancellation was anticipated. It was announced by the President in a hearing before Congress at his own initiative on May 12, 2010, together with six other budget cuts. The government had argued against budget cuts as recently as May 5, 2010. Even the opposition leader considered cuts improvised, and the media spoke of perplexity in

Congress. According to the national press, the measure was taken after a week of pressures from EU members and large drops in share values at the Madrid stock exchange. The measure was approved on May 20, 2010 and ratified by the Parliament on May 27, 2010.

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 orafterwards would affect babies born in January 2011 differently from those born in December 2010.

The elimination of the child benefit could have had a range of short and longer-term effects. In particular, it may have discouraged fertility. However, any reduction in the number of conceptions would have led to fewer births starting 9 months after the announcement at the earliest (February 2011).[5] For ongoing pregnancies, however, the pre-announced cancellation created an incentive for those with a due date close to the threshold to advancetheir delivery date in order to qualify for the €2,500.[6]No other tax benefits generated incentives to give birth in December rather than January, in 2010-11 or the surrounding years.[7] The "natural experiment" generated by the benefit

[5] Abortions could have reacted immediately to the policy announcement, leading to lower fertility as early as January 2011 (or even late December 2010). 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. We also test explicitly for fertility effectsin section 4.

[6] There was 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*". They 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*."

[7]There are two main tax benefits associated with children in Spain. First, a child tax allowance that increases with birth order, amounting to about €600 for the average family (which did not change over the time period we analyze). Second, a tax credit for working mothers introduced in January 2003, amounting to €1200 per year during the first 3 years of age of the child. Since it is prorated depending on the month of birth of the child, it generates negligible incentives to shift the date of birth (Azmat and Gonzalez 2010). We show that our results do not change when

cancellation thusoffers 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 run National Health Service, which is highly regarded with respect to facilities and personnel. Hospital choice among public institutions is permitted in several regions, 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). Mothers with private insurance (many public servants who may opt for private healthcare, as well as some affluent 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,as well as births with obstetric intervention (including induction of labor, epidural, forceps, and episiotomy; Redondo et al. 2013, Escuriet et al. 2014). It's been suggested that the high percentage of cesarean sections without medical indication in private hospitals could be due to their "greater receptivity to women's demands" (Redondo et al. 2013).

The standard recommendation is for new mothers to be discharged from the hospital 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 and death certificates from the Spanish National Statistical Institute. These population-level data provide detailed information on the universe of births and deaths taking place annually in Spain, as recorded in the official national registry. Parents are required to register the birth in a Civil Registry office between 24 hours and 8 days after the delivery takes place, by presenting the original birth certificate provided by the health centre (seeCasado, 2008, p. 56).The birth certificate is filled out by the hospital (not the

our period of analysis is 2007- 2012, a period during which tax benefits associated with children were unchanged.

parents) at the time of birth, and contains the date and time of birth, as well as the doctor's signature.

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 stillbirths), and mortalityduring 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 for years 2000 to 2012, purchased from the Spanish National Statistical Institute.

The death-certificate microdata provide information on date of death, age and sex. We also supplement the publicly available files with the exact date of birth. We compute mortality rates at the date of birth level as the number of fatalities in a given age range, divided by the total number of children born on each specific date (from the birth-certificate data).

Our second data source is the Hospital Morbidity Survey for 2000-2014, which provides essentially an annual census of all overnight hospitalizations in Spain.[8] This 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 variables such as province and sex. It does not provide direct information on procedures, drugs administered, or costs. Diagnoses are provided at the 3-digit level, grouped in 17 "chapters", following the International Classification of Diseases (ICD-9-CM).

We construct the date of birth for each individual in the hospital data using the information on age and date of release, and select all hospitalizations for the cohorts of babies born on the relevant dates. We focus on child hospitalizations during the first 33 months of life, given that the youngest cohort in our sample (January 2012 births) has not turned 3 yet in the most recent year of hospital data available (2014). We only include hospitalizations with an associated medical diagnosis, i.e. we exclude hospital stays for exploration, observation, or testing purposes (8% of all hospitalizations), as

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

well as the birth hospitalization of healthy newborns (not provided). We compute hospitalization rates (by age and diagnosis) for children born on a given date as the number of hospital stays (from the hospital data), divided by the total number of children born on that date (from the birth-certificate data).

We also analyze maternal hospitalizations at childbirth, which serve as a check for the daily number of births as reported in the birth-certificate data (since more than 98% of all registered births in Spain take place in a hospital during the relevant period). Maternal hospitalizations are recorded in Chapter 11 ("Complications of pregnancy, childbirth, and the puerperium"). We select hospitalizations related to “normal delivery”[9] (ICD-9-CM code 650), as well as those related to “other indications for care in pregnancy, labor and delivery” (codes 651-59) and “complications occurring mainly in the course of labor and delivery” (codes 660-69), which we group as "Birth with complications".[10]

We focus our analysis on births taking place in December and January of 2000-01 to 2011-12, with October and November as our “control” months in the health analysis. Table A1reports summary statistics for our full sample. The total number of newborns in the sample is 1,712,552. Panel A shows the health outcomes at birth, from the birth-certificate data.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 6% of babies are born prematurely (under 37 weeks of gestation). Regarding mortality, 3 in 1,000 pregnancies end in stillbirth, while less than 1 in 1,000 babies does not survive the first 24 hours after delivery.

Panel B shows descriptive statisticsfor mortality rates by 2 and by 12 months of age. There are about 2.5 deaths per 1,000 births during the first two months of life, and about 3.2 during the whole of the first year of life.

[9] Delivery requiring minimal or no assistance, with or without episiotomy, without fetal manipulation (e.g., rotation version) or instrumentation (forceps) of a spontaneous, cephalic, vaginal, full-term, single, live-born infant.This would include some inductions, but not c-sections.

[10] We cannot separate birth hospitalizations precisely from other hospital stays surrounding birth, since we only observe the main diagnosis associated with each hospitalization. We can be sure that a birth took place when the main diagnosis is “normal delivery with no complications”, butwe cannot separately identify births with complications from other pregnancy-related complications shortly before or after birth.

Panel Cdescribes our infant health outcomes beyond birth, based on the hospital data. There were almost44hospitalizations per 100 births (by age 33 months). We split hospitalization rates by age, using shorter ranges at earlier ages. There are about 14 hospital stays for 100 live births during the first week of life, 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 per 100 births during the first year of life, while we observe almost 11 hospitalizations per 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.[11]

The most common groups of diagnoses in our full sample (at the level of "chapter")are perinatal conditions[12] and respiratory disease[13], which account for 34 and 24 percent of all hospital stays, respectively. We do not report hospitalizations due to perinatal conditions after two months of age sincethey are practically zero. Excluding birth hospitalizations (those for which the age at hospitalization is 0 days), respiratory disease is the top category (31% of all stays), and the most common three-digitdiagnosis isacute bronchitisand bronchiolitis (ICM code 466, more than 16%). The next most frequent diagnoses aredigestive problems and infectious disease.

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 II).

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

[12] Chapter 15: “Certain conditions originating in the perinatal period” (ICD-9-CM codes 760-779),which include “conditions which have their origin (…) before birth through the first 28 days after birth”

[13] Chapter 8: “Diseases of the respiratory system”(ICD-9-CM codes 460-519).

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 orJanuary 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 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.[14]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 fullsample thus includes all births in the last 4 weeks of December and the first 4 weeks of January, for the twelve December-January pairs from 2000-01 to 2011-12. We also re-do all of the analysis using only the five most recent December-January pairs, from 2007-2008 to 2011-2012.

The number of observations in the full sample is 672(28 days, times 2 months, times 12 years). There were on average 1,228 births per day according to the birth-certificate

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

data, 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, for2008-09, 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 drop 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 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 A1. 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.

We now formalize these observations with our regression analysis. Table 2shows 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 panel is estimated using birth-certificate data.The results in the first row suggest 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.[15] 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 2 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 some births may have beenshifted by more than one week. Once we include all four weeks before and after (last column), we estimate that about 2,050births were shifted from January 2011 to December 2010, or about 6% of all January births.

The second panel of Table 2 uses as a dependent variable the daily number of births, estimated from birth-related maternal hospitalizations, in the hospital register data. The estimated effects are close to the results using birth-certificate data in the first panel. The coefficients are larger in the one-week window sample, and somewhat smaller in the four-week window, although the 95% confidence intervals overlap. Birth-related hospitalizations may include some hospital stays of pregnant women close to their delivery date that do not result inbirth. Our results suggest that part of the spike in late December 2010 maternal hospitalizations may have been unsuccessful attempts to deliver early. The results indicate that between 1,175 and 1,740 births were moved from January 2011 to December 2010 as a result of the benefit cancellation.

Faking of the date of birth in the birth certificate seems unlikely to be driving our results. First, 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 (see Figure A1). Additionally, our results in panel B of Table 2 using maternal hospitalization data confirm the results from panel A using birth certificate data, and it would have been harder to fake hospitalization dates in the hospital records.

[15]We calculate the total number of births moved by dividing the baby bonus coefficient by 2, because a birth that is moved from January to December reduces the number of January births by 1, and increases the number of December births by 1, so that it is "counted twice". We then multiply it bythe number of days pre-cutoffin the corresponding window (7 in this case).

The results are not overly sensitive to the set of dummy variables included as controls. Table A2 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%) in Panel A. We also re-estimate the regressions using only three years before and one year after as controls (see Table A3), with the point estimates and significance levels essentially unchanged.

Our estimated timing effect is in the same order of magnitude as those in previous studies of other reforms in different countries. In Table 1, we report the estimated percentage-point shift in the number of births as a result of $1,000 in benefit or tax incentives, across different studies.We find almost a 2 percentage point increase in the probability of a (last week of) December birth with respect to a (first week of) January birth. The analyses of similarly well-publicized reforms in other countries lead to estimated effects of similar magnitude, such as the Tamm (2012) study for Germany (1.8 points) or the ones for Austria (Brunner and Kuhn 2014) and Australia (Gans and Leigh 2009). LaLumia et al. (2015) find a smaller effect for the case of the US (about a 1-point increase), but they acknowledge that the tax benefits of a December versus a January birth are not well known.

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.The sample now is extended to include 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.

The results of these specifications are reported in Table 3 (first two columns). 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 (columns three to six).

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.It is also possible that the low number of births in late January and early February reflect a (negative) fertility effect of the benefit cancellation, since a new conception right after the announcement of the benefit cancellation (on May 12, 2010) would have February 2, 2011 as the estimated due date.We test explicitly for fertility effects in Table A4, and find no evidence of an effect before the fourth week of January 2011.[16]For this reason, when describing our health results we focus on the three-week window around the benefit cancellation date.

Our results from Table 3 imply that the average birth in our sample (all births in December and January) was shifted by almost 0.08 weeks.[17] 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). We calculate that between 1,975 and 2,120 births were shifted from January to December as a result of the benefit cancellation.[18]These estimates imply, in turn, that the average *affected* baby was born about 2.5 weeks earlier as a result.[19]However, this should be interpreted as an upper bound since, as mentioned, the benefit cancellation may have led to additional shifting within January (and/or December), and to fewer births due in early February. If we

[16] We find evidence consistent with a drop in fertility starting the last week of January (a negative significant coefficient in the 4-week window sample).

[17] $0.077 = [(5.3\cdot(-4) + 48.3\cdot(-3) + 66.1\cdot(-2) + 162.3\cdot(-1) + (-119.8)\cdot 0 + (-74.0)\cdot 1 + (-66.2)\cdot 2 + (-43.1)\cdot 3)\cdot 7]/72{,}771$. Each coefficient (in the first column of Table 3) estimates the daily number of "extra" December births or "missing" January births, by week. For instance, the coefficient 5.3 tells us that there were about 5 "too many" daily births in the first week of December 2010. We multiply the daily effects implied by each coefficient by 7 since there are 7 days per week, and by their distance to the threshold (in weeks). This gives us the total number of weeks shifted. We then obtain the average number of weeks shifted by dividing the previous figure by the total number of births in the relevant eight-week sample for 2010-11(72,771).

[18] We calculate the total number of births shifted from January to December by adding up the daily extra births in each of the weeks in December (given by the baby bonus coefficients for each week in Table 3), and multiplying by the number of days per week (7). From the first column of Table 3, $1{,}975 = (5.32+48.35+66.13+162.30)\cdot 7$; and from the third column of Table 3, $2{,}120 = (23.94+2.28+5.33+63.11+ 159.28)\cdot 7$.

[19] From the first column of Table 3, the estimated number of births shifted from January to December is 1,975. We obtain the fraction of affected babies by dividing over the total number of births in the eight relevant weeks (1,975/72,771=0.03). Thus, the number of weeks that each affected child would have been moved is the estimated average number of weeks moved (0.076, from the first column of Table 3) divided by the fraction of affected babies: 0.076/0.03=2.5.

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). The true average effect on "affected" newborns may be even lower if fertility effects were also present.

In order to get a better understanding of the effects on gestational age at birth,were-estimate equation (1) for the number of daily births, now split by weeks of gestation at birth.[20] The most common gestational age at birth is 40 weeks (30% of all births in our sample), followed by 39 weeks (23%). There is also a substantial fraction of births before the 37th week (pre-term births, about 6%, see Table A1). These regression results are presented in Table A5 (Panel A). They show that the vast majority of the "extra" December births were full-term.The largest increase is found for 39-40 weeks of gestation, with a large effect also for weeks 37-38. Medical guidelines in Spain at the time advised against inducing birth before the 37th week unless specific maternal or child health complications were present (Sociedad Española de Ginecología y Obstetricia 2003).[21]

We also estimate a version of equation (1) at the individual birth level, where the outcome variable is reported weeks of gestation at birth.[22] These results are reported in Panel B of Table A5. The estimated effects on average weeks of gestation (between 0.025 and 0.051) are significant, although smaller than those in Table 3 (0.077), suggesting that the average affected birth was shifted by between half and one week.[23] Our gestational age variable is based on the reported date of the mother's last menstrual

[20] There are about 15% missing observations for weeks of gestation in our sample. We have verified that the missing status is not significantly different in the dates close to the benefit cancellation (results available upon request).

[21] These guidelines have been updated and now advise against elective c-sections before week 39 (Sociedad Española de Ginecología y Obstetricia2015).

[22] We estimate the following regression:

$$Weeks_{it} = \alpha + \beta Dec2010_{it} + \varphi X_{it} + \delta_{dw} + \phi_{dy} + \mu_h + \lambda_t + \varepsilon_{it},$$

where the dependent variable is weeks of gestation at birth, and the explanatory variable of interest is a dummy for December 2010 births. We control for demographic characteristics X, province fixed-effects,dummies for day of the week (δ), day of the year (ϕ), holidays (μ), year dummies λ_t (indicators for each December-January set), and interactions between year and day of the week.

[23] We calculate the effect on the births that were actually shifted by dividing the coefficient by the estimated fraction of affected births.Using the coefficient in the last column, -0.025/0.027=0.92 weeks (upper bound). The lower bound would be half this size, 0.46 weeks.

period, so that it is likely subject to some measurement error (Lynch and Zhang 2007, Hall et al. 2013). Moreover, gestational age in weeks will likely miss some shifts in timing of less than one week. In any case, the 95% confidence intervals for our estimated effect on weeks of gestation (in the +/- 4 weeks sample) in Tables 3 and A5 overlap, so that the results using the two different methods are statistically indistinguishable.

Overall, we find strong evidencethat a significant number of births were shifted from January 2011 to December 2010 (almost 6% of all monthly births) as a result of the benefit cancellation, with important effects on gestational age at birth for the affected newborns (about one week).

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 βwould 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.[24]Since 2007, we can also include educational attainment of the parents. We also include a linear time trend. The γ

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

coefficientscapture the differential impact of the reform for different demographic groups.

Table 4 reports the results of estimating equation (2) for the four samples progressively widening the window around the cutoff date. We show the results for the short sample(years 2007-08 to 2011-12), where we can control for education (the results are similar for the full sample). 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 the year, the fraction of December births was more than 5 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 thatfirst births were 3 percentage points less likely to be re-scheduled. The shifting appears less common among immigrant mothers (by more than 2 points). We also find that university-educated parents were more likely to react to the policy change, while a large (if imprecisely estimated) impact is found for multiple births. The interaction with occupation of the mother is not significant. We also try specifications that include an additional interaction with occupation of the father, which is found to be insignificant.

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, 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 andGarcía 2003), and 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).[25]

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 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 (2), 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.[26]We use three alternative measures of the presence of private hospitals in a province: the number of private beds at maternity and child hospitals per 1,000 women aged 15 to 44, the number of private beds per 1,000 population, andthe number of private hospital beds as a fraction of all hospital beds**.** We cluster standard errors by province. The results are reported in Table A6.

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 third rowsuggest 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

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[25] 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).

[26] 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.

The delivery date for a pregnant woman can be shifted forward medically either by inducing birth or via a programmed c-section. While 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.[27] In this section we analyze whether the shifting of births that we observe was driven by an increase in the overall incidence of inductions and c-sections, versus a shifting of dates for births that would have taken place via these procedures in any case. We thus shed light on the extent to which any effects on infant health can be traced to method of delivery versus time in the womb.

Our birth-certificate data do not provide direct information on whether each birth was induced. We do observe c-sections, but only starting in 2007. About 22% of all births in our birth-certificate data were cesarean sections. 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 first 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. 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 resultsare presented inTable 5. We detect significantly "too many" daily c-sections in late December 2010; about 120 per day when we focus on the one-week window (panel A). Tables 2 and A3 show that the total increase in the number of deliveries in the last week of December 2010 was close to 280 per day (panel A), so that c-sections would account for almost half of the overall spike in December 2010 births.

We run parallel regressions using data from maternal hospitalizations. All c-section births are counted as hospitalizations for "complications during pregnancy, labor and delivery". The first row of panel B in Table 5 shows that the number of births with "complications" was significantly higher in December 2010 compared to January 2011. The magnitude of the coefficients (compared with panel B of Table 2) suggests again

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

that a large fraction of the shifted births were c-sections.We also find (last three rows of panel B in Table 5) that maternal hospitalizations in December 2010 were significantly longer.

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 specificationwith birth-certificate data are shown in panel A of Table 6. 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). The benefit cancellation thus does not seem to have increased the number of babies born via c-section.[28]

We also estimate equation (3) with the hospital data, using the sample of all maternal hospitalizations surrounding birth (panel B of Table 6). The dependent variable is an indicator for deliveries that took place via c-section or suffered from any other complications. The results confirm 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 ofc-sections in private hospitals. However, we do not find evidence for an increase in the incidence of c-sections or other birth complications.

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

5. Effects on newborn and infant health

5.1Empirical 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 bornsmaller 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.The medical literature has documented that lower gestational age at birth as well as low birth-weight are associated with a number of medical problems after birth, including a higher incidence of respiratory disease(Madar et al. 1999, Escobar et al. 2006, Clark et al. 2009, Mally et al. 2010, Boyle et al. 2012).

In order to pin down the causal effect of shifting births forward on infant health outcomes,our identification strategy still relies on comparingbirthstaking 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.

For example, 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.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.

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.[29] We also want to control for family characteristics that

[29] The specification used by Schulkind & Shapiro (2014) is similar in spirit to ours and thus addresses composition concerns.

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 and 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 (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 thatcompares 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. Since we do not have individual identifiers in the hospital data, we estimate equation (4) at the date (*j*) rather than the individual level:

$$(5) Health_{jt} = \alpha + \beta_1(Dec\text{-}Jan)_{jt} + \beta_2(Dec2010\text{-}Jan2011)_{jt} + \lambda_t + \varepsilon_{jt.}$$

The outcome variable is nowone of a set of hospitalization rates for children born on date *j* of year *t*, 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 on date *j*, and not the fraction of babies with at least one hospital stay. We run separate regressions for different age ranges and 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.Thefull sample thus includes all births in the last 4 weeks of October and December and the first 4 weeks of November and January, for the twelve years from 2000-01 to 2011-12.[30]

5.2 Health outcomes at birth

The main results for health at birth are reported in Table 7. 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 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 7 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 15grams 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, a15-gram average effect for all newborns implies that affected babies were on average around 260 grams smaller (about an 8% effect).[31] 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. If we assume that at most as many births

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

[31] According to the results in Table 2, 1,014 births were moved forward in the 1-week sample, out of 17,791 births (5.7%). Thus, for those babies who were shifted, the effect was -14.8/0.057 = -260 grams. For the 2-week window, given that 1,484 births were moved (Table 2) out of 36,414 (4.1%), the estimated effect for shifted babies is -12.5/0.0408 = -306 grams.

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 7). 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.[32]

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 7. We do not find an increase in the fraction of babies under 1,500 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.The results are very similar if we run the analysis using only the five most recent years of data (see Table A7).

Our results are in the same order of magnitude as the existing estimates in the literature. Schulkind and Shapiro (2014) estimate reductions between 2.4 and 6.4 grams in average birth weight as a result of a $1,000 increase in tax benefits. Given their estimate that only 0.7% of births are shifted, those numbers imply decreases in average birth weight between 344 (2.4/0.007) and 910 (6.4/0.007) grams for the affected children. Gans and Leigh (2009) find an increase in 75 grams in the average birth weight when births are delayed in order to qualify for a new baby bonus. Given that about 16% of births were successfully delayed, their estimates imply that affected babies were approximately 460 (75/0.16) grams heavier.

[32] According to the results in Table 2, 1,014 births were shifted forward in the 1-week window, out of 17,791 births (5.7%). If we assume that at most as many births were shifted "within" December or January as "between", then the proportion of affected babies would be double this figure (11.4%). Given that the benefit cancellation lowered average birth weight by 0.5 log-points (column 1 in Panel A of Table 7), then the effect on birth weight for babies whose birth was shifted was between -0.005/0.057 = -8.6 and -0.005/0.114=-4.3 log-points.

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.[33] Thus, our birth-weight results are consistent with effects on gestational length of about one week for affected newborn babies.

Panel B of Table 7 estimates the effect of the benefit cancellation on late fetal deaths and 24-hour mortality, as extreme measures of health at birth. The coefficients are positive, but not significantly different from zero.

Ourregression results suggest that the shifting of birth dates as a result of the benefit cancellation led toa significant reduction in birth-weight for the affected babies, although not at the very bottom of the weight distribution.It would be tempting to claim that, since the fraction of very small babies did not increase, the early scheduling may havehad 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 2009, 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. We next analyze the effect on hospitalization rates from birth until 33 months of age (1,000 days). The main results are reported in Tables 8 and 9.[34]

Panel A in Table 8 shows the results of estimating equation (5) for hospitalization rates at different age ranges. The first row shows that there was no significant spike in hospitalizations during the first week after birth in the period surrounding the benefit cancellation date.[35] This suggests that the shifting of birthdates around the New Year of

[33] 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.

[34] See Tables A8 and A9 for the results when using the shorter sample (2007-08 to 2011-12 births).

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

2011 did not lead to an increase in birth complications (as shown also with maternal hospitalizations in panel B of Table 6). We do find a significantly elevated hospitalization rate for our turn-of-the-year babies when they were between 7 days and one month of age. The magnitude of the estimated effects suggests that the cohort of affected children suffered a hospitalization rate between 0.0060 and 0.0077 higher than normal during the first month of life. Since the average hospitalization rate in this age range was 0.0343 (Table A1), this represents about a 20% increase. The magnitude is highest in the two and three-week window samples.

For ages after one month, the coefficients are not statistically significant at 95% for any of the age ranges or samples. The effect on hospitalizations during the first year of life is positive, if statistically insignificant. The effects at later ages (12 to 33 months) are negative, albeit again insignificant. The overall effect (age 0 to 33 months) is not significantly different from zero. This pattern suggests that the increase in early hospitalizations for the affected cohort may have been followed by an (imprecisely estimated) decrease in the following months, with a zero (or small) overall effect on hospitalization rates by ages 12 and 33 months.

Panel B of Table 8 shows the results for mortality rates by ages 2 and 12 months. We find that the affected cohort of children did not suffer a significant increase in infant mortality rates (we also tried other age ranges, with similarly insignificant results).

Overall, the effects reported in Table 8 translate into more than 400 additional hospitalizations at ages one to four weeks[36] (for between 2,000 and 4,000 "treated" children). Given that each overnight hospitalization of an infant (aged less than one year) has an estimated average cost of about 4,900€ (according to the Spanish Registry of Hospital Discharges, Ministry of Health 2014), our estimates imply that the increase in hospitalizations driven by the benefit cancellation had a direct cost of up to two million Euros.This estimated cost is however an upper bound, which may be attenuated if there were indeed fewer hospitalizations after the first two months, as the negative coefficients for later ages suggest.

[36]In the 3-week window sample, the estimated effect is 0.0077. Multiplying by the total number of births during the relevant six weeks, the total estimated increase in hospital stays is 423 (0.0077x54,965).

We then run parallel specifications where the dependent variable is the hospitalization rate for specific diagnoses.We focus on the most frequent categories (as reported in Table A1), in order to capture the main driver(s) of the effects documented in Table 8, and aggregate the three earlier age groups. The results are reported in Table 9.

The most common group of diagnosesat very early ages is “perinatal conditions” (about 34% of all hospital stays in our full sample).We find no significant increase in “perinatal” hospitalizations in the first two months of life (Panel A of Table 9). The main 3-digit “perinatal” diagnosis is “perinatal infection”, and the results also show small, insignificant coefficients.

The second most common diagnosis associated with hospital stays is respiratory disease (with 24% of all hospital stays in the full sample), and the most frequent 3-digit code in this category is bronchitis (“acute bronchitis or bronchiolitis”). We findthat 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 (Panel A of Table 9). This effect is driven by bronchitis.

The coefficients for respiratory disease and bronchitis at ages zero to two months in Table 9 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.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. Overall, it seems that the former effect dominates, since the magnitude of the coefficient declines as the window widens for our main health outcomes. This is the case for the birth-weight results (Table 7), as well as for our main health results, i.e. the increase in respiratory disease (mainly bronchitis) between birth and 2 months of age (Table 9). The main exception is the estimates for the hospitalization rate at ages 7 to 30 days (Table 8).

Comparing the estimated coefficients with the average incidence of respiratory disease and bronchitis in these age ranges reveals that the cohort of affected children suffered a 25% increase in hospitalization rates for respiratory disease (28% in bronchitis), at ages zero to two months (using the coefficient from the 2-week window sample).[37]The estimated effects are of similar magnitude when we use only the more recent years as controls (see Table A9), and they translate into almost 450 “extra” hospital stays for respiratory disease in this age range.[38]

We find no significant effect on any of the main diagnoses for the older age groups. When we aggregate all hospitalizations by age 33 months (panel D of Table 9), the coefficients for bronchitis are positive, and significant in the larger samples (due to better precision).

Overall, the results from Tables 7-9 suggest that the newborns whose birth-date was affected by the benefit cancellation weighed between 130 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 know from the medical literature that only a small fraction of bronchitis cases (about 1%) require overnight hospitalization (Fitzgerald 2011). This suggests that the total effect of early birth on the incidence of respiratory disease is likely to be much broader than we can capture with hospital records. We do not find essentially any significant effects on hospitalization rates after two months of age.

5.4 Mechanisms and robustness checks

We have shown (Tables 8-9) 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 led many families to shift birth from January to December. In this section, we discuss two potential issues with our interpretation. First,

[37]The relevant coefficient for age 0-2 months is 0.0088 (Table 9). The average incidence of respiratory disease in this age range is 0.0355 (Table A1). Thus, the estimated increase as a fraction of average incidence is 0.0088/0.0355=0.25.

[38]The relevant coefficient in the 3-week window sample is 0.0079 (Table 9). Multiplying by the total number of births during the relevant 6 weeks, the estimated increase in respiratory hospitalizations is 434 (0.0079x54,965).

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?We also try to reconcile our results with previous findings in the medical literature.

Robustness checks

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 three years of life.However, we may worry that the effects on respiratory disease could be driven by weather or air quality spikes, since poor air quality has been shown to affect children's health negatively (Neidell 2004, Currie et al. 2009, Coneus and Spiess 2012). We perform two checks in order to rule out this possibility.

First, we check 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 10. We again detect an abnormally high number of bronchitis hospitalizations in early 2011 among one-week to two-month olds (about 12.5, or 34 log-points,"too many" hospital stays per day). However, there is no spike in bronchitis among older children, or among adults. We also fail to find any contemporaneous spike in asthma-related hospitalizations, for any age range.[39]

Second, we re-estimated the regressions in Table 9 excluding Madrid from the sample (panel A of Table A10), given that air quality in Madrid is notoriously bad in the

[39] We find a reduction in asthma hospitalizations in logs for 1-week to 2-month-old babies. However, the magnitude of this "effect" is very small, as seen in the specification in levels (the incidence of asthma among newborns is very low).

winter months, and it is likely that February experiences severe thermal inversions that result in large increases in air pollution exposure. This could drive our results if there wereimportant age-based nonlinearities, combined with a pollution spike in February 2011. The results are robust to the exclusion of the province of Madrid from the sample.

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, as made public by the Spanish National Statistical Institute (see panel B of Table A10).The coefficients of interest are barely altered.

Mechanisms

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, and shorter gestational age could have had persistent health effects. However, there are at least three 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. Finally, the announcement of the benefit cancellation could have generated elevated stress levels among pregnant women with due dates near the threshold, potentially leading to early birth and negative effects on infant health.

First, the available evidence suggests that congestion effects were probably not important. We didn't find anyincrease in birth complications around the benefit cancellation date, for either mothers (panel B of Table 6) or children (first row of panel A in table 8). Moreover, Figure A1 (and Table 3) 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 under 10% more births than the busiest day of December 2009. We also find that the excess December births were quite spread out geographically.[40]

Second, 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 (Hoynes et al. 2015). We address this possibility in two ways. We estimate an additional specification where we include February and March as additional "control" months, given that children born in February and March of 2011 also did not receive the benefit. We also run specifications where we directly control for benefit eligibility(a dummy equal to 1 for births taking place between July 2007 and December 2010).[41] The results of these additional specifications are reported in panels C and D of Table A10.[42]The hospitalization results remain, suggesting that benefit receipt is not the main driver of the worse health outcomes of December 2010-January 2011 births.[43]

Finally, regarding maternal stress, recent studies looking at the effect of stress levels during pregnancy on birth (and later) outcomes seem to suggest that the effect is small. Aizer et al. (2016) use a siblings fixed-effects approach, and find no significant effect of maternal stress (measured as cortisol levels during pregnancy) on birth weight or gestation length. Other recent studies have analyzed the effects of maternal stress driven by extreme events, such as wars, terrorist attacks, or natural disasters. Their findings suggest small negative effects on birth weight and gestation, especially during the first trimester of the pregnancy (Camacho 2008, Eskenazi et al. 2007, Torche 2011, Mansour and Rees 2012, Foureaux and Manacorda 2016). The average decline in birth weight associated with high levels of stress is relatively low, ranging from the 30-grams decline

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[40] We find that the timing effect was present across most Spanish provinces. Table A11 shows that, even in the provinces where the effect was most pronounced, it translated into less than 1 extra daily birth per hospital in the last week of December 2010.

[41] Benefit eligibility is not found to be significantly associated with health at birth or hospitalization rates (results not shown). Hoynes et al. (2015) found a positive effect of cash benefits during pregnancy on newborn health, but the Spanish benefit was paid weeks or months after birth (compared to during pregnancy), so that any effects on health at birth or shortly after would have had to take place via families adjusting their behavior during pregnancy in anticipation of future benefit receipt.

[42] See also Table A12 for birth-weight regressions that control for benefit eligibility.

[43] 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.

in areas with at least one landmine explosion in each trimester of pregnancy (Camacho 2008) to the 50-grams decline associated with exposure to an earthquake during the first trimester (Torche 2011). Any increase in stress levels generated by the cancellation of the benefit in Spain would most likely not qualify as in the same order of magnitude as a war or natural disaster, and is more likely to fall within the "normal" ranges of stress analysed by Aizer et al (2016). Thus, we attribute between zero and a very small fraction of our birth timing and health effects to maternal stress during pregnancy.

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 related to respiratory disorders. We do not think this can be attributed to congestion in hospitals, to the January births not receiving the monetary benefit, or to increased maternal stress. The most likely channel seems to be shorter gestational age at birth (lower fetal maturation) driven by a shifting of (elective) scheduled births, given that we did not find a higher incidence of c-sections (see section 4.4).

Reconciling our results with the medical literature

The medical literatureprovides evidence of a correlation between gestational age (and/or low birth-weight) and respiratory disease (Liu et al. 2014, Goyal et al. 2011), as well as specifically acute bronchitis and bronchiolitis, both during infancy (Koehoorn et al 2008, Boyce et al 2000) and early childhood (Odibo et 2006).Negative associations are even reported in medical studies where shorter gestation is the result of elective inductions and c-sections (Madar et al. 1999, Clark et al 2009).

In terms of medical pathways, lung volume is known to undergo rapid changes during the last trimester of gestation (Kugelman et al. 2013), and 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, even for late preterm or early term babies (Goldenberg and Nelson 1975, Jain and Eaton 2006, Mally et al. 2010).

The magnitude of our results is roughly consistent with some of the correlations reported in the medical literature. For example, Dietz et al. (2011), using hospital data for the US, find that the hospitalization rate in 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. Boyle et al. (2012), using data from the British Millennium Cohort Study, find that the proportion of babies needing three or more hospital admissions during the first 9 months was 90% higher among babies born at 37-38 weeks than among those born at 39-41 weeks.Our results suggest that the children affected by the benefit cancellation suffered a hospitalization rate between 50 and 90% higherthan the control group during the first four weeks after birth.[44]

Some medical studies have found correlations between gestational age and hospitalizations for a range of diagnoses, including gastrointestinal disease, and associations with respiratory disease that persist for months or even years (for example, Boyle et al. 2012). However, these studies are all correlational, so they should not be interpreted causally. If children born smaller/earlier have other underlying health problems, those studies would be over-estimating the effects of gestational age on health. Our results (short-term effects on respiratory disease only) suggest that some of these previous findings can in fact be attributed to unobserved heterogeneity/omitted variable bias.

6. Discussion and conclusions

We take advantage of the cancellation of a child benefit in Spain in December 2010to analyze the effect of scheduling birth early on the health of newborns. We exploit individual-level birth- and death-certificates 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

[44] Table A1 shows that the average hospitalization rate was 0.1440+0.0343=0.178 during the first 30 days after birth. Table 8 shows that the effect of the benefit cancellation was to increase the hospitalization rate by -0.0020+0.0067=0.0047(in the +/- 4 week window sample). Given there were 72,771 births in the “treated” 8-week period, this translates into 342 extra hospitalizations (72,771*0.0047). But only between 2,000 and 4,000 children were actually shifted because of the policy change (Table 2), so the percentage increase in the hospitalization rate for the treated is between 342/2,053=0.17 and 342/4,106=0.08, which, over the average of 0.178, equals 0.17/0.178=93% or 0.08/0.178=47%.

hospitals. Early delivery had significant health consequences for the affected babies. Children who were born early as a result of the reform weighedbetween 130 and 300 grams less at birth, on average. They were also about 33% more likely to be hospitalized for respiratory disease at ages 7 to 59 days.

Our results provide new, credible empirical evidence showing that scheduling birthearly for non-medical reasons can have important (short-term)health consequences for babies. We interpret our results as showing that scheduling birth about one week 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. For example, regarding longer-term health effects, the medical literature suggests thatchildren hospitalized for bronchiolitis during infancy are at higher risk of developing recurrent wheezing or asthma during childhood (Henderson et al 2005). As for other relevant long-term outcomes, Figlio et al. (2014), using register data from the state of Florida, find that a 4 to 9 percent drop in birth-weight, such as the one that we find, would translate into 0.02 to 0.045 of a standard deviation decrease in test scores.[45] Bhalotra and Venkataramani (2015) show that an 18% (one standard deviation) decline in exposure to respiratory disease during infancy (in the US) results in an increase of 0.1 years of schooling, a 0.4 percentage-point increase in the employment rate, and a 1.5% increase in family income.

These estimates are unlikely to translate directly to Spain. 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, in this study 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.

[45] Royer's (2009) estimates from California birth records imply that a decrease in birth weight of 260 grams leads to a drop in educational attainment of about 0.10 years. Black et al.'s (2007) results for Norway imply that a 9% decrease in birth weight lowers the probability of high school completion by about 0.9 percentage points, and full-time earnings by about 1 percent.

Our results have several policy implications. Firstly, despite the existing evidence on the long-run consequences of poor infant health, little is known 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. Policies that discourage the elective scheduling of birth for non-medical reasons may thus lead to significant positive effects on infant health.

Secondly, our findings also suggest that announcement effects are important. The government announced the benefit cancellation seven months in advance, with a single cutoff date. It would perhaps have been advisable to devise a not-so-steep cancellation mechanism, so that, for instance, the benefit amount could have declined more slowly over time.

Finally, our 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 for non-medical reasons can help inform the decisions of families as well as health professionals.

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Appendix I. Magnitude of the effect of economic incentives on the timing of births

We compute the percentage point change in the birth probability associated with a US$1,000 change in 2010 dollar terms. To that end, we translate the benefit amount to dollars in the corresponding benefit year using data on Purchasing Power Parities (OECD 2016), and inflate that amount to 2010 US$ using the Consumer Price Index (BLS, 2016).

Dickert-Conlin, S. and Chandra, A. (1999)
They estimate that increasing the child tax-benefit by $500 raises the probability of having a child in the last week of December by 26.9 percent (page 161). Therefore, for US$1000 of 2010 ($689.5 in the year of their analysis 1996), the corresponding increase amounts to 37.09 percent. Given that there are around 52% of children born in the last week of December (page 170), a 37.09% increase in the probability of having a child in December suggests an increase of approximately 19.2 percentage points (37.09*0.52) in the probability of giving birth in December as opposed to January.

LaLumia, S., Sallee, J.M and Turner, N. (2015)
They estimate that an additional $1,000 of tax savings is associated with a 1 percentage point increase in the probability that a birth occurs in the last week of December (page 258). Their estimates are inflation-adjusted to the year 2009 and remain roughly the same when adjusted to 2010 US$.

Schulkind, Lisa and Shapiro T.M. (2014)
They estimate that a $1000 increase in tax benefits results in between a 0.37 and a 0.54 percentage point increase in the probability of a December birth, depending on the specification (page 144). Their estimates are given for 2000 US$ values, which correspond to US$1266.3 in 2010. Therefore, for a 2010 US$1000 increase their findings imply between 0.29 and 0.43 percentage points increase in the probability of a December birth as opposed to a January birth.

Gans and Leigh (2009)
They estimate that 16% of births were shifted as a response to the implementation of the AUS$ 3,000 benefit in 2004 (Table 1, page 251). Given that AUS$ 3,000 in 2004 correspond to US$2604 in 2010, an increase in 2010 US$1000 would imply 6.2% of births being shifted. Assuming that in the absence of the policy 50 per cent of births happen in July (as opposed to June), a 6.2 percent shift in births corresponds to 3.1 percentage points (0.50*6.2). This policy replaced an existing income-dependent benefit. The authors acknowledge that for some households, the difference between the old payment and the new payment may have been less than $3000 and therefore their estimate overstates the impact of a AUS$3000 financial incentive on birth timing.

Tamm, M. (2012)
The author reports that an average increase of €1730 in 2007 (€6730 with the new policy minus €5000 with the old scheme) leads to an 8 percent increase in the probability of a January birth (Table 2, pages 8 and 9).Given that €1730 in 2007 correspond to 2010 US$2192, the equivalent increase in the probability of a January birth for a 2010 US$1000 increase is 3.65 percent. Assuming that in the absence of policy 50 per cent of births happen in January (as opposed to December), the

corresponding point increase in the probability of a January birth is 1.82 percentage points (0.50*3.65).

Neugart, M. and Ohlsson, H. (2013)
They report that the average increase of €4956 in 2006/7 leads to a 5 percentage point increase in January births. The sample includes working mothers only (Table 4, pages 101 and 102). Given that 2006 €4956 are equivalent to 2010 US$6537, a 2010 $1000 increase would lead to a 0.8 percentage point increase in the probability of a January birth as opposed to a February birth.

Brunner, Beatrice and Kuhn, Andreas (2014)
They report about 8% extra births in December 1996 (the month before the benefit cancellation, see page 373). The mean benefit introduced by this policy was about 1996 €1000, which is equivalent to 2010 US$1478. Therefore, for a 2010 US$1000 their findings imply a 5.41 percent increase. Assuming that in the absence of policy 50 per cent of births happen in December (as opposed to January), the corresponding point increase in the probability of a December birth is 2.7 percentage points (0.50*5.41).

This paper
In Table 2 we report an increase in the number of December births between 6 and 12%, depending on the length of the window (+/- 1 week or +/- 4 weeks) for the €2500 benefit in 2010. This benefit is equivalent to 2010 US$3333. Therefore, for a 2010 US$1000 our findings imply an increase in births ranging from 3.6 to 1.8 percent. Giving that 50 per cent of births happen in December (as opposed to January) in our sample, the corresponding point increase in the probability of a December birth is between 1.8 (.50*3.6) and 0.9 (.50*1.8) percentage points.

Appendix II.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.[46] If $s=0$, with a high probability p the birth takes place in January (either spontaneously, or as a result of it having been scheduled for medical reasons).[47]

The household is subject to a budget constraint, and an infant health production function. The budget constraint is (in expected value) $c=y+(b-\pi)s+b(1-p)(1-s)$, where the price of the composite consumption good c 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 if the birth is scheduled in December ($s=1$) or if it happens early for natural reasons (with probability $1-p$). Scheduling birth has a cost π (e.g. the cost of convincing the doctor to schedule for non-medical reasons).

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 $h_0>h_1$ (i.e. $h_1-h_0<0$).[48] Households may have imperfect information about the infant health production function (the values of h_0 and h_1).[49]

The household will choose to schedule birth early ($s=1$) if and only if: $U(y+b-\pi, h_1) > U(y+b(1-p), h_0)$, i.e. $u(y+b-\pi) + v(h_1) > u(y+b(1-p)) + v(h_0)$, or $u(y+b-\pi) - u(y+b(1-p)) > - [v(h_1) - v(h_0)]$. The first term in the last inequality is positive (if $b-\pi>b(1-p)$, or $b>\pi/p$) 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

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

[47] 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.

[48] We assume that receiving the benefit has no direct effect on infant health (via income).

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

$h(s)$, 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) $h_1-h_0<0$, thenthe benefit cancellation creates a simple trade-off:the household will schedule birth early if and only if the increase in u from receiving$b$ with probability 1(net of π) is greater than the potential decrease in $v$from scheduling early. If scheduling is thought to be harmless ($h_1=h_0$), the household will schedule early if the benefit is large enough($b>\pi/p$).

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.

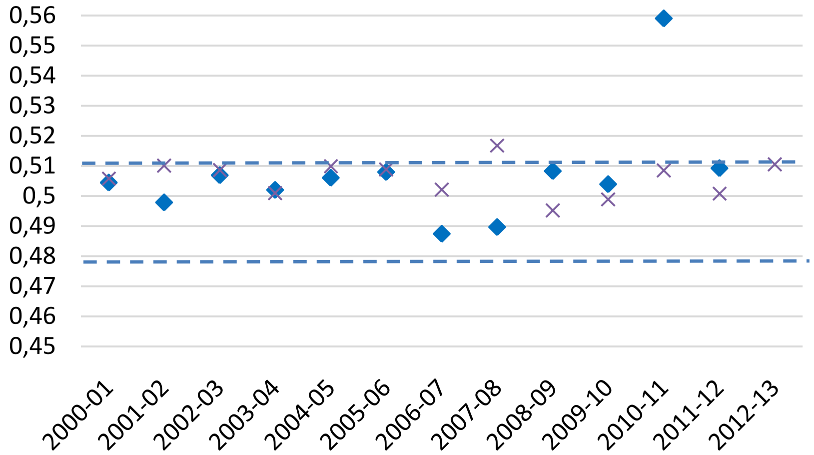
Note that in this basic version of the model, if scheduling birth early has no health benefits for the infant ($h_1-h_0\leq0$), then receiving the benefit is the only reason for a family to choose $s=1$. We could easily extend the household utility functionin order to incorporate the possibility that families may derive direct utility from scheduling birth early, either for "convenience" reasons, or even for reasons related to the health of the mother.[50] In this extended setup, scheduling birth early increases utility via 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); and/or v) the cost of scheduling birth early (π) is low, and/or vi) the probability that the birth takes place in December naturally ($1-p$) is low.

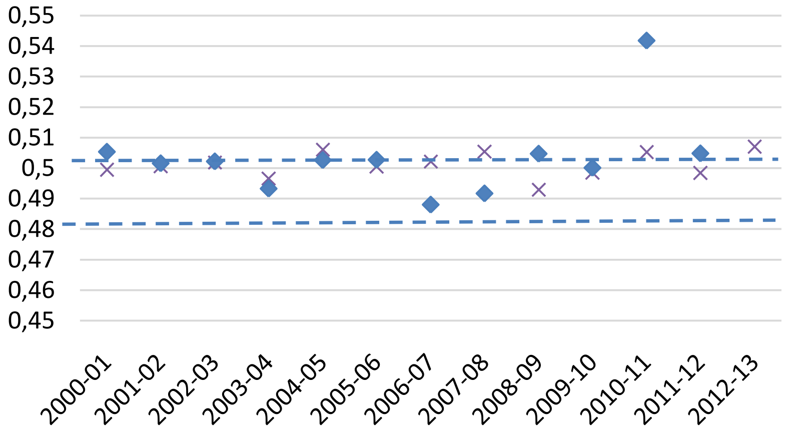
[50] For example, the utility function could be modeled as:$U(c,h,s)=u(c)+v(h(s))+\gamma s$, where γwould capture the direct utility gains from scheduling birth early. This setup leads to three possible cases: the household may not want to schedule birth (even in the presence of the benefit, $b>0$), 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 "affected" by the benefit cancellation. Theconditionfor$s=1$isnow: $U(y+b-\pi, h_1, 1) > U(y, h_0, 0)$, i.e. $u(y+b-\pi) + v(h_1) + \gamma > u(y) + v(h_1)$, or$\gamma + [u(y+b-\pi) - u(y)] > - [v(h_1) – v(h_0)]$.

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

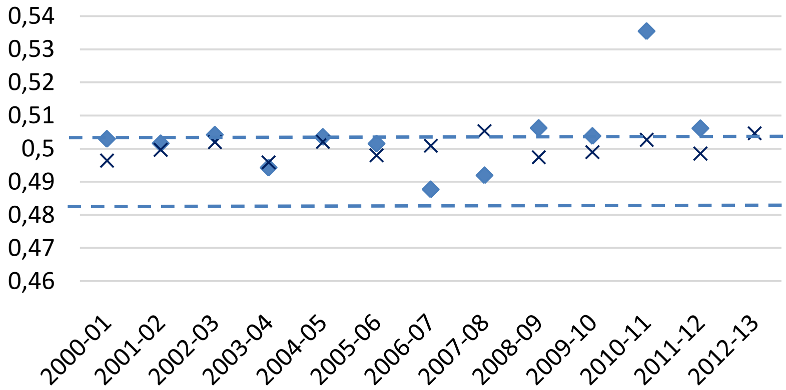
A. One-week window



B. Two-week window



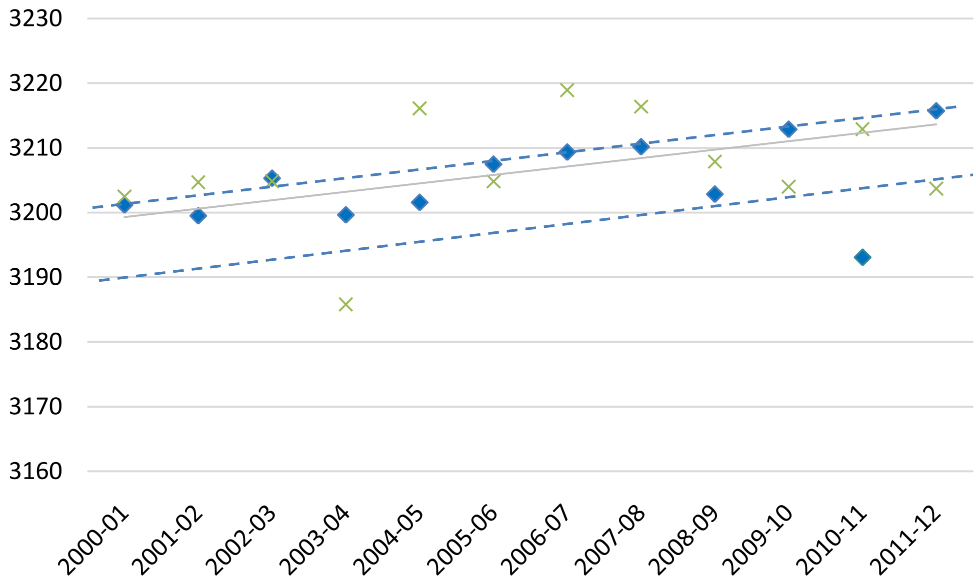
C. Three-week window



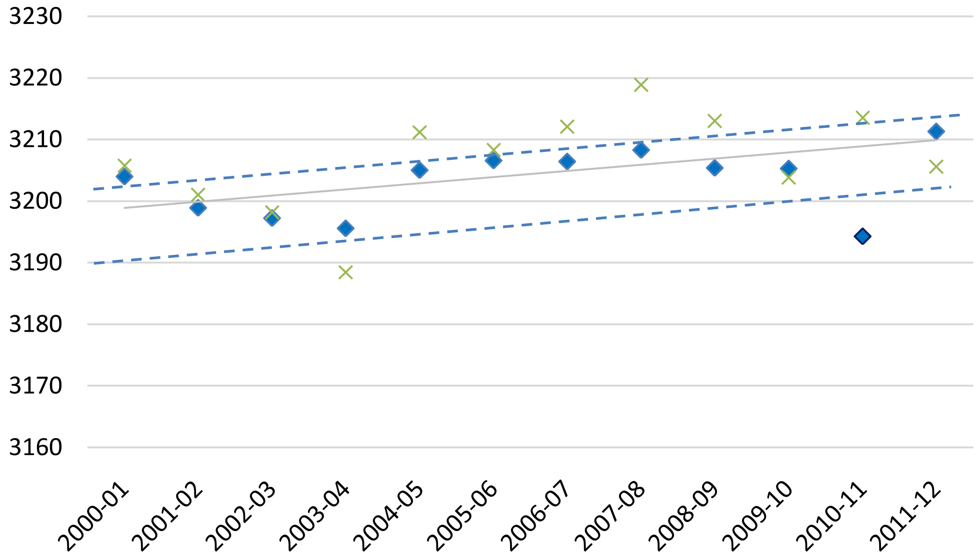
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012. The diamonds (crosses) show the fraction of December (October) births, out of all births close to December (October) 31. Panel A includes all births between December (October) 25 and January (November) 7; panel B 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(October 31) in years 2000-01 to 2011-12

A. One-week window



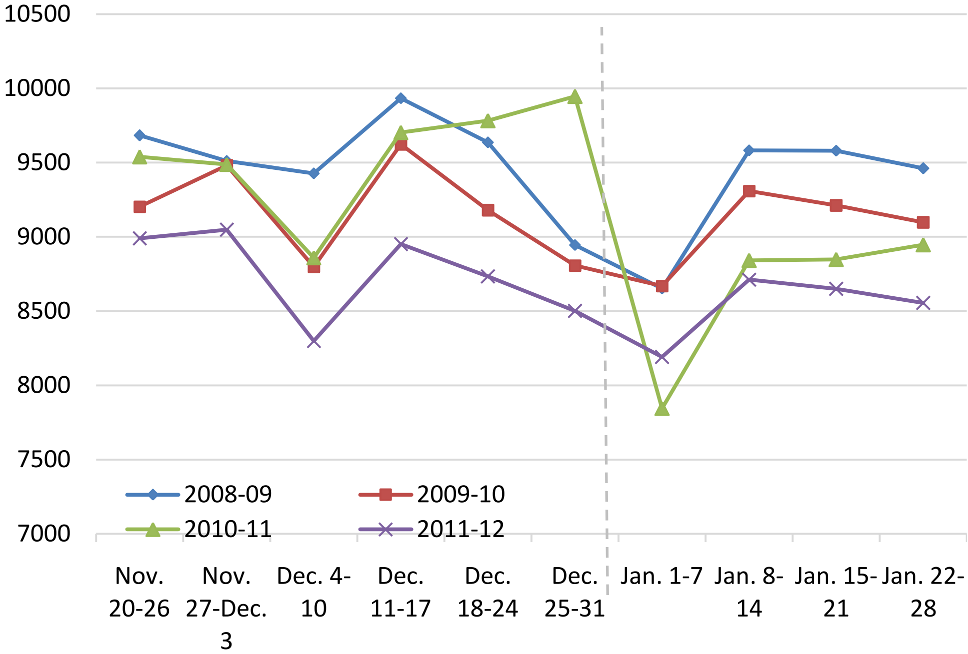
B. Two-week window



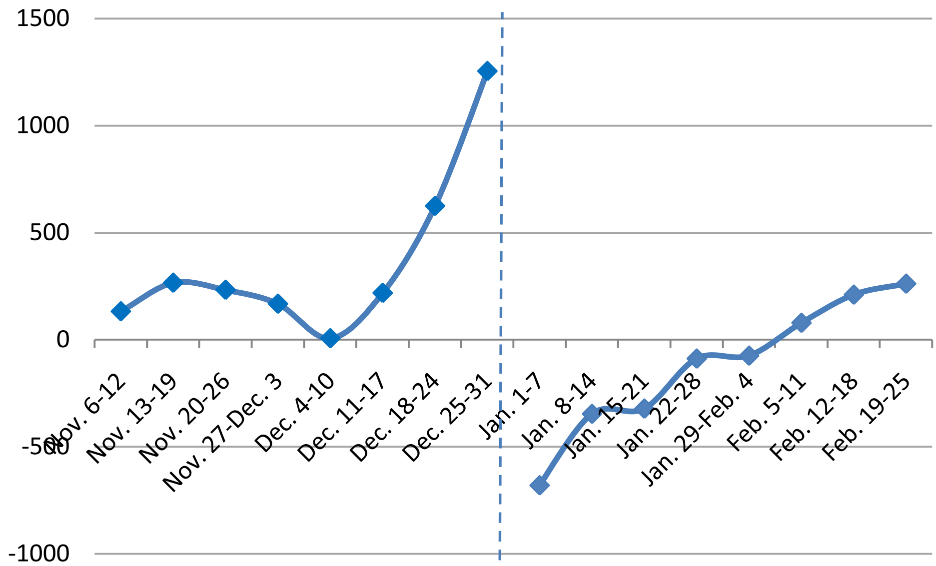
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012. The diamonds (crosses) show average birth-weight for births close to December 31 (October 31).Panel A includes all births between December (October) 25 and January (November) 7; panel B includes births from Dec. (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



Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2011.

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	Healthoutcomes at birth	Healthoutcomesbeyondbirth
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
Policies aimed at reducing early elective deliveries in the US *Incentive:* Postpone	Buckles, K. and M. Guldi (2016)	US Vital Statistics	N.A. (no monetary incentive)	N.A.	Birth-weight, precipitous labor, birth injury, assisted ventilation	None
Introduction of a baby bonus. *Date:* July 1, 2004. *Country:* Australia. *Amount:* $3000. Replacedanincome-dependentbenefit. *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.	Yes (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). Workingmothersonly.	0.8 p. point increase in the probability of a 1st week of Jan. vs. last week of Dec. birth.	Yes (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 (2014)	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.	Thispaper	Daily birth data from birth certificates (2000-2012) and Hospital	1.8 p. point increase in the probability of a last week of Dec. vs. 1st week of	Yes (announced in May 2010)	Birth weight, weeks of gestation, neonatal mortality, delivery	Hospitalizations 0-33 months

Amount: €2.500.*Incentive:* Bring forward	Morbidity Survey (2000-2014).	Jan. birth	method, birth complications

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 shown in Appendix I.

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

	+/-1 week	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
Panel A. Birth-certificate data				
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.: ln(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%
Panel B. Hospital data				
Dep. var.: Number of maternal	335.81***	205.61***	158.71***	124.40***
hospitalizations	(47.399)	(37.677)	(28.669)	(24.071)
Number of births moved	1175	1439	1666	1742
Dep. var.: ln (number of maternal	0.383***	0.230***	0.177***	0.137***
hosp.)	(0.065)	(0.046)	(0.034)	(0.028)
Share of births moved	21%	12%	9%	7%
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%)

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

Note: We report coefficients ona December 2010dummy (the month right before benefit cancellation)from equation (1). An observation is a day. In Panel A, the dependent variable is the daily (log) number of births, and the sample includes all births in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2011-12. In Panel B, the dependent variable is the daily (log) number of birth-related maternal hospitalizations (CIE 9-MC 650-669), and the sample includes all birth-related maternal hospitalizations in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2011-12.Robust standard errors are shown in parentheses.

Table 3.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%)

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Note: We report coefficients on a set of four dummies for the last 4 weeks of December 2010, as well as four dummies for the initial 4 weeks of January 2011 (the period right around benefit cancellation). An observation is a day. The dependent variable is the (log) daily number of births. The reference weeks are Nov. 27-Dec. 3 and Jan. 29-Feb. 4 in the first two columns, Nov. 20-26 and Feb. 4-11 in the third and fourth columns, and Nov.6-12 and Feb. 19-25 in the last two columns.The sample includes all births in the sample weeks, from 2000-01 to 2011-12. Robust standard errors are shown in parentheses.

Table 4.The effect of the benefit cancellation on the timing of births, individual-level analysis

	+/-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* First birth	-0.0292***	-0.0201***	-0.0182***	-0.0135***
	(0.009)	(0.006)	(0.005)	(0.004)
Reform* Immigrant mom	-0.0229**	-0.0191**	-0.0141**	-0.0092*
	(0.011)	(0.008)	(0.006)	(0.005)
Reform* Any parent university educated	0.0210**	0.0050	0.0030	0.0076
	(0.010)	(0.007)	(0.006)	(0.005)
Reform* Twins	0.0524*	0.0187	0.0244	0.0137
	(0.028)	(0.020)	(0.016)	(0.014)
Reform* Mom under 25	0.0233*	0.0172*	0.0052	0.0057
	(0.014)	(0.010)	(0.008)	(0.007)
Reform* Mom over 35	0.0157	0.0120*	0.0050	0.0081
	(0.010)	(0.007)	(0.006)	(0.005)
Reform* Married mother	-0.0014	-0.0081	-0.0089*	-0.0095**
	(0.009)	(0.006)	(0.005)	(0.005)
Reform* No registered dad	0.0178	0.0125	0.0191	0.0119
	(0.031)	(0.022)	(0.017)	(0.015)
Reform* High-skill mother	-0.0045	-0.0025	-0.0037	-0.0071
	(0.011)	(0.008)	(0.006)	(0.006)
Reform* Capital	-0.0018	0.0042	0.0048	0.0054
	(0.010)	(0.007)	(0.006)	(0.005)
Reform* Rural	-0.0113	-0.0052	-0.0056	-0.0073
	(0.011)	(0.008)	(0.006)	(0.005)
Reform* Girl	0.0024	-0.0015	-0.0007	0.0031
	(0.008)	(0.006)	(0.005)	(0.004)
N	87,677	180,451	273,625	363,396

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

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012.

Note: Each column in the two sub-panels reports coefficients from equation (2). An observation is an individual birth. The dependent variable is a binary indicator that takes value one if the birth happens in December. "Reform" is a binary explanatory variable taking value one if the birth occurs in December 2010-January 2011 (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, four sets of dummies for parental occupation and education, and dummies for first-borns, female babies, and multiple births, as well as province fixed-effects.The sample includes all births in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2007-08 to 2011-12.Robust standard errors are shown in parentheses

Table 5. The effect of benefit cancellation on the timing of c-sections and birth complications, and the duration of maternal hospitalizations

Panel A. Birth-certificate data	**+/-1 week**	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
Dep. var.: N. of births	119.48***	81.26***	61.97***	46.91***
by caesarean section	(37.737)	(18.321)	(14.196)	(12.722)
N	70	140	210	280
Panel B. Hospital data	**+/-1 week**	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
Dep. var.: N. of c-sections	278.59***	170.15***	130.06***	98.66***
and other birthcomplications	(39.17)	(31.52)	(24.23)	(20.59)
Dep. var.: Av. duration of maternal	0.267***	0.142**	0.058	-0.112
hospitalizations (Mean 3.4)	(0.094)	(0.057)	(0.046)	(0.074)
Dep. var.: Av. duration of hospitalizations	-0.012	-0.036	-0.015	-0.017
for normal deliveries (Mean 2.6)	(0.066)	(0.036)	(0.029)	(0.023)
Dep. var.: Av. duration of hospitalizations	0.366***	0.198**	0.072	-0.182*
for birth complications (Mean 3.7)	(0.122)	(0.078)	(0.064)	(0.109)
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%)

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012and Hospital Morbidity Survey micro data, Spanish National Statistical Institute, 2000-2012.

Note: We report coefficients ona December 2010 dummy (the month right before benefit cancellation) fromequation (1). An observation is a day. In Panel A, the dependent variable is the daily number of births delivered by c-section and the sample includes births delivered by c-section in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2007-08 to 2010-12. In Panel B, the dependent variable is the number of maternal hospitalizations due to birth complications (CIE 9-MC 651-669), its average duration in days, and the average duration of all birth-related maternal hospitalizations (CIE 9-MC 650-669) and hospitalizations due to normal deliveries (CIE 9-MC 650), in days. The sample includes birth-related maternal hospitalizations in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2011-12. Robust standard errors are shown in parentheses in both panels.

Table 6. The effect of benefit cancellation on the incidence of c-sections and birth complications

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Panel A. Birth-certificate data				
Dep.var.: Indicator for c-section birth	0.0085	0.0025	0.0035	0.0018
	(0.0175)	(0.0113)	(0.0089)	(0.0077)
N	180,020	365,983	550,976	735,142
Demographic controls	Y	Y	Y	Y
Panel B. Hospital data				
Dep. var.: Indicator for c-sections and	0.0072	0.0045	0.0022	0.0033
other complications (Mean 0.779)	(0.0050)	(0.0038)	(0.0031)	(0.0026)
N	298,380	606,186	914,962	1,223,068
Demographic controls (maternal age)	Y	Y	Y	Y

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

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012 and Hospital Morbidity Survey micro data, Spanish National Statistical Institute, 2000-2012.

Note:We report coefficients on a December 2010-January 2011 dummy (the weeks right around benefit cancellation) fromequation (3). In Panel A, an observation is an individual birth; the dependent variable is a dummy for C-section births; and 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. The sample includes all births 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. In Panel B, an observation is a birth-related maternal hospitalization; the dependent variable is a dummy for birth-related complications (CIE 9-MC 651-669); and control variables include: maternal age, a binary indicator for all December-January births, and year fixed effects. The sample includes allbirth-related maternal hospitalizations (CIE 9-MC 650-669) 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), from 2000-01 to 2011-12. Robust standarderrors, clustered at the date level, are shown in parentheses in both panels.

Table 7. The effect of benefit cancellation on birth-weight and mortality

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
	Panel A. Birth weight			
Dep. var.: Birth weight	-14.7551***	-12.5092***	-5.5886*	-3.6502
	(4.9610)	(3.8661)	(3.3656)	(2.8978)
Dep. var.: Birth weight	-0.0049***	-0.0045***	-0.0020*	-0.0013
(in logs)	(0.0018)	(0.0014)	(0.0012)	(0.0011)
Dep. var.: BW<1,500	0.0004	0.0011	0.0004	0.0003
	(0.0013)	(0.0009)	(0.0007)	(0.0006)
Dep. var.: BW<2,500	0.0013	0.0014	0.0003	0.0005
	(0.0031)	(0.0020)	(0.0016)	(0.0014)
Dep. var.: BW<2,750	0.0053	0.0035	0.0023	0.0021
	(0.0035)	(0.0027)	(0.0023)	(0.0020)
Dep. var.: BW<3,000	0.0092*	0.0064*	0.0035	0.0030
	(0.0048)	(0.0037)	(0.0030)	(0.0025)
Dep. var.: BW<3,500	0.0095**	0.0071**	0.0021	0.0021
	(0.0047)	(0.0034)	(0.0029)	(0.0025)
N	397,505	809,882	1,220,263	1,627,681
	Panel B. Mortality			
Dep. var.: Late fetal death	0.3748	0.4772	0.2393	0.2920
(per 1,000 births)	(0.7716)	(0.5039)	(0.4151)	(0.3515)
Dep. var.: Neonatal mortality	0.0094	0.2097	0.2071	0.0288
(24 hours) (per 1,000 births)	(0.3486)	(0.2184)	(0.1701)	(0.1543)
N	418,539	852,606	1,283,972	1,712,552

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

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.

Note: We report coefficients on a December 2010-January 2011 dummy (the weeks right around benefit cancellation) fromequation (4). An observation is an individual newborn baby. 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. In both panels, the sample includes all babies born 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 quadruplets from 2000-01 to 2011-12. Robust standard errors, clustered at the date level, are shown in parentheses.

Table 8. The effect of benefit cancellation on infant hospitalizations by age

	+/- 1 weeks	+/- 2 weeks	+/- 3 weeks	+/- 4 weeks
Panel A. Hospitalization rates (per child)				
Age <7 days	0.0027	0.0015	0.0013	-0.0020
	(0.0093)	(0.0055)	(0.0043)	(0.0036)
Age 7-30 days (1 week to 1 month)	0.0060*	0.0073***	0.0077***	0.0067***
	(0.0034)	(0.0022)	(0.0018)	(0.0017)
Age 31-59 days (1-2 months)	0.0061	0.0055*	0.0021	0.0010
	(0.0054)	(0.0033)	(0.0026)	(0.0023)
Age 60-89 days (2-3 months)	-0.0019	-0.0020	-0.0026	-0.0011
	(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 0-12 months	0.0035	0.0083	0.0072	0.0040
	(0.0250)	(0.0133)	(0.0095)	(0.0079)
Age 365-1000 days (12-33 months)	-0.0153	-0.0127	-0.0110*	-0.0094*
	(0.0160)	(0.0087)	(0.0065)	(0.0056)
Age 0-33 months	-0.0117	-0.0044	-0.0038	-0.0054
	(0.0344)	(0.0190)	(0.0140)	(0.0118)
N	336	672	1008	1344
Panel B. Mortality rates (per 1,000 children)				
Age 0-2 months	0.0710	0.1669	0.1800	-0.0033
	(0.6686)	(0.4187)	(0.3387)	(0.2945)
Age 0-12 months	0.2976	0.2719	0.3382	0.0428
	(0.7000)	(0.4539)	(0.3724)	(0.3305)
N	140	280	420	560

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

Data sources: Hospital Morbidity Survey micro data (2000-2014), birth-certificate micro data (2000-2013), and death-certificate micro data (2007-2013), Spanish National Statistical Institute.

Note: We report coefficientsonaDecember 2010-January 2011 dummy (the weeks right around benefit cancellation) fromequation (5). An observation is a day (birth-date). The dependent variable is the number of overnight hospitalizations (Panel A) or the number of deaths times 1,000 (Panel B) in a given age range of children born on a given day, divided by the number of children born on that day. Control variables include calendar month dummies, and year fixed effects. 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. Standard errors are shown in parentheses.

Table 9.The effect of benefit cancellation on hospitalizations by diagnosis and age

Dep. var.: Hospitalization rate	+/- 1 weeks	+/- 2 weeks	+/- 3 weeks	+/- 4 weeks
Panel A. Age 0-2 months				
Perinatal conditions	0.0083	0.0074	0.0058	0.0021
	(0.0098)	(0.0057)	(0.0044)	(0.0037)
Perinatal infection	0.0011	0.0010	0.0014	0.0011
	(0.0014)	(0.0011)	(0.0010)	(0.0009)
Respiratory disease	0.0094*	0.0088**	0.0079***	0.0063**
	(0.0053)	(0.0034)	(0.0029)	(0.0027)
Bronchitis	0.0079*	0.0079**	0.0075***	0.0059**
	(0.0048)	(0.0031)	(0.0026)	(0.0025)
Digestive problems	-0.0005	-0.0008	-0.0006	-0.0006
	(0.0012)	(0.0008)	(0.0007)	(0.0006)
Infectious diseases	0.0001	0.0008	-0.0000	-0.0001
	(0.0011)	(0.0007)	(0.0006)	(0.0005)
Panel B. Age 2-12 months				
Respiratory disease	-0.0064	-0.0043	-0.0021	-0.0010
	(0.0054)	(0.0032)	(0.0024)	(0.0021)
Bronchitis	-0.0026	-0.0011	-0.0002	0.0003
	(0.0037)	(0.0023)	(0.0019)	(0.0017)
Digestive problems	-0.0015	-0.0001	-0.0000	-0.0003
	(0.0022)	(0.0014)	(0.0011)	(0.0009)
Infectious diseases	-0.0013	-0.0011	-0.0012	-0.0006
	(0.0019)	(0.0013)	(0.0010)	(0.0008)
Panel C. Age 12-33 months				
Respiratory disease	-0.0013	-0.0015	-0.0021	-0.0022
	(0.0050)	(0.0029)	(0.0021)	(0.0019)
Bronchitis	0.0000	-0.0002	-0.0001	-0.0007
	(0.0017)	(0.0011)	(0.0009)	(0.0007)
Digestive problems	-0.0041	-0.0030*	-0.0024*	-0.0016
	(0.0032)	(0.0018)	(0.0014)	(0.0011)
Infectious diseases	-0.0019	-0.0011	-0.0006	-0.0007
	(0.0020)	(0.0012)	(0.0010)	(0.0008)
Panel D. Age 0-33 months				
Respiratory disease (11.4%)	0.0017	0.0030	0.0037	0.0031
	(0.0119)	(0.0068)	(0.0050)	(0.0044)
Bronchitis (6.5%)	0.0053	0.0066	0.0072**	0.0056*
	(0.0075)	(0.0044)	(0.0034)	(0.0031)
Digestive problems (3.0%)	-0.0060	-0.0038	-0.0030	-0.0025
	(0.0049)	(0.0028)	(0.0021)	(0.0017)
Infectious diseases (2.9%)	-0.0032	-0.0015	-0.0018	-0.0014
	(0.0036)	(0.0022)	(0.0017)	(0.0014)
N	336	672	1008	1344

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

Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2000-2013. Note: We report coefficients on a December 2010-January 2011 dummy (the weeks right around benefit cancellation) from equation (5). An observation is a day (birth-date). 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. Control variables include calendar month dummies, and year fixed effects. 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. Standard errors are shown in parentheses.

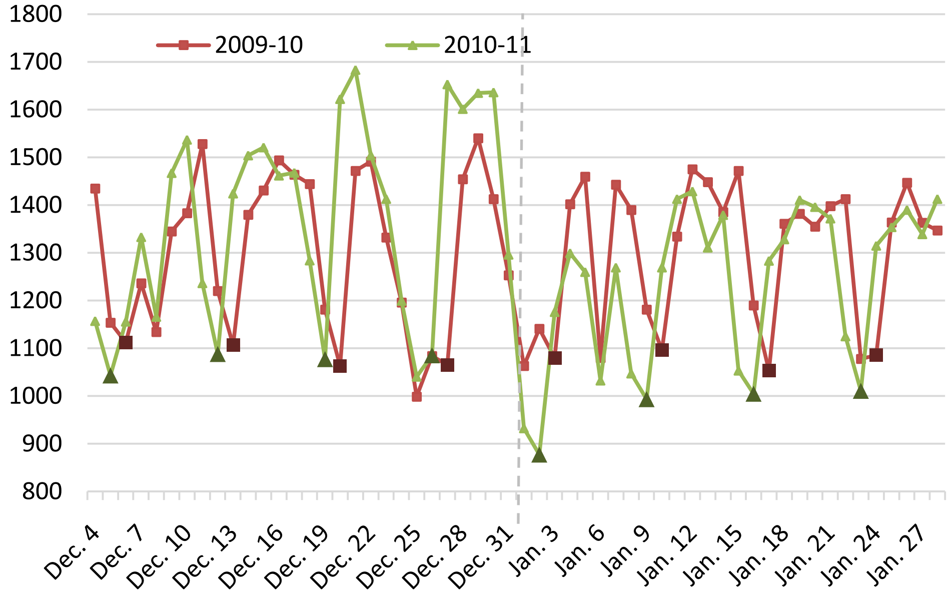
Table 10.The effects of the benefit cancellation on bronchitis and asthma prevalence by age

Dep. var.: Number of hospital stays	Bronchitis, in levels		Bronchitis, in logs		Asthma, in levels	Asthma, in logs	
Age 1 week to 2 months	12.530	***	0.348	***	-0.036	-0.273	**
	(2.585)		(0.098)		(0.041)	(0.136)	
Age 2 to 5 years	-0.789		0.010		0.433	0.041	
	(0.928)		(0.096)		(0.744)	(0.103)	
Age 2 to 17	-0.119		0.048		0.665	0.017	
	(1.044)		(0.089)		(1.161)	(0.085)	
Age 18 plus	-2.567		-0.017		5.033	0.091	
	(4.811)		(0.064)		(3.613)	(0.059)	
Age 65 plus	-5.492		-0.059		2.515	0.080	
	(4.132)		(0.066)		(2.287)	(0.067)	

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

Data sources: Hospital Morbidity Survey micro data, Spanish National Statistical Institute, 2000-2013.
Note: We report coefficients ona January-February 2011 dummy. An observation is a day. 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). Control variables include calendar month dummies, and turn-of-the-year fixed effects.The sample includes all days in November, December, January and February from 2000-01 to 2011-12 (N=1,443). Standard errors are shown in parentheses.

Figure A1. 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, 2009-2011.

Table A1. 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
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	39.1648	(1.586)	33	46
Gestation weeks <37	0.0622	(0.241)	0	1
Gestation weeks =37-38	0.2293	(0.420)	0	1
Gestation weeks =39-40	0.5416	(0.498)	0	1
Gestation weeks =41-42	0.1660	(0.372)	0	1
Gestation weeks >42	0.0009	(0.030)	0	1
Late fetal deaths (per 1,000 births)	3.2793	(57.171)	0	1
Mortality within 24 hours (per 1,000 births)	0.6984	(26.418)	0	1
Panel B. Daily deaths over number of births				
Mortality within first 2 months (per 1,000 births)	2.5066	(1.394)	0	7.6014
Mortality within first 12 months (per 1,000 births)	3.2342	(1.567)	0	8.2645
Panel C. Hospital stays over number of births				
Total hospitalization rates by age				
Total, age 0-33 months	0.4383	(0.065)	0.2316	1.3861
Total, age 0-12 months	0.3301	(0.042)	0.1377	0.7710
Age <7 days	0.1440	(0.019)	0.0296	0.2113
Age 7-30 days	0.0343	(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
2 Hospitalization rates by diagnosis and age				
Perinatal conditions				
All ages, 0-33 months	0.1444	(0.019)	0.0258	0.2228
Age 0-2 months	0.1418	(0.019)	0.0229	0.2109
Perinatal infection				
All ages, 0-33 months	0.0100	(0.005)	0.0000	0.0510
Age 0-2 months	0.0099	(0.005)	0.0000	0.0510
Respiratory disease				
All ages, 0-33 months	0.1138	(0.024)	0.0567	0.2609
Age 0-2 months	0.0355	(0.014)	0.0022	0.0891
Age 2-12 months	0.0435	(0.014)	0.0059	0.1178
Age 12-33 months	0.0348	(0.010)	0.0136	0.0956
Bronchitis				
All ages, 0-33 months	0.0649	(0.018)	0.0171	0.1486
Age 0-2 months	0.0283	(0.013)	0.0011	0.0804
Age 2-12 months	0.0276	(0.012)	0.0011	0.0736
Age 12-33 months	0.0090	(0.004)	0.0008	0.0349
Digestive problems				
All ages, 0-33 months	0.0304	(0.011)	0.0090	0.1641

Age 0-2 months	0.0041	(0.003)	0.0000	0.0388
Age 2-12 months	0.0127	(0.005)	0.0025	0.0470
Age 12-33 months	0.0135	(0.007)	0.0015	0.1133
Infectious diseases				
All ages, 0-33 months	0.0294	(0.008)	0.0082	0.0761
Age 0-2 months	0.0052	(0.003)	0.0000	0.0303
Age 2-12 months	0.0114	(0.005)	0.0010	0.0582
Age 12-33 months	0.0128	(0.005)	0.0023	0.0329

Sources: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012 (Panel A), death-certificate micro data, 2007-2013 (Panel B), and Hospital Morbidity Survey, 2000-2013 (Panel C).

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 (2007-08 for mortality) to 2011-12. The unit of observation is the birth (including multiple births) for gestational age outcomes, the childfor birth-weight and mortality outcomes, and the day (birth-date) for the hospitalization variables outcomes. The total number of observations (individual babies) is 1,712,552(there are 5% missing observations for birth-weight, and about 15% for gestation weeks), and 1,344 days (birth-dates).

Table A2. The effect of benefit cancellation on the timing of births: Alternative specifications

Panel A. Birth-certificate data (+/-1 week)	**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%
Panel B. Hospital data (+/-1 week)	**1**	**2**	**3**	**4**
Dep. var.: Number of maternal hospitalizations	334.98***	339.25***	315.92***	335.81***
	(66.70)	(74.85)	(43.90)	(47.40)
Number of births moved	1172	1187	1106	1175
Dep. var.: ln(number of maternal hosp.)	0.388***	0.394***	0.359***	0.383***
	(0.075)	(0.081)	(0.056)	(0.065)
Share of births moved	21%	22%	20%	21%
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%)

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

Note: We report coefficients ona December 2010 dummy (the month right before benefit cancellation) from equation (1).An observation is a day. In Panel A, the dependent variable is the (log) daily number of births, and the sample includes all births in the last week of December or the first week of January, for December-January pairs from 2000-01 to 2011-12. In Panel B, the dependent variable is the (log) daily number of birth-related maternal hospitalizations (CIE 9-MC 650-669), and the sample includes all birth-related maternal hospitalizations in the last week of December and the first week of January, for December-January pairs from 2000-01 to 2011-12.Robust standard errors are shown in parentheses.

Table A3. The effect of benefit cancellation on the timing of births, 2007-2012 sample

Panel A. Birth-certificate data	**+/-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)	0.219***	0.158***	0.129***	0.103***
	(0.034)	(0.022)	(0.018)	(0.016)
Share of births moved	12%	8%	7%	5%
Panel B. Hospital data	**+/-1 week**	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
Dep. var.: Number of maternal	201.91***	157.81***	129.41***	105.78***
hospitalizations	(42.64)	(26.12)	(22.87)	(19.52)
Number of births moved	707	1105	1359	1481
Dep. var.: ln(number of maternal	0.203***	0.161***	0.133***	0.108***
hospitalizations)	(0.038)	(0.024)	(0.022)	(0.019)
Share of births moved	11%	8%	7%	6%
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%)

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012, and Hospital Morbidity Survey micro data, Spanish National Statistical Institute, 2007-2012.

Note: We report coefficients ona December 2010 dummy (the month right before benefit cancellation)from equation (1).An observation is a day. In Panel A, the dependent variable is the (log) daily number of births, and 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. In Panel B, the dependent variable is the (log) daily number of birth-related maternal hospitalizations (CIE 9-MC 650-669), and the sample includes all birth-related maternal hospitalizations in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2007-08 to 2011-12. Robust standard errors are shown in parentheses.

Table A4. Fertility effects of the benefit cancellation

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Dep. var.: Number of births	-5.39	-21.79	-30.27	-40.7**
	(50.19)	(27.31)	(20.07)	(16.07)
Dep. var.: ln(number of births)	-0.0093	-0.0203	-0.0266*	-0.0331***
	(0.0385)	(0.0205)	(0.0151)	(0.0121)
N	336	672	1008	1344
Yeardummies	Y	Y	Y	Y
Monthdummies	Y	Y	Y	Y
Day of weekdummies	Y	Y	Y	Y
Holidaydummy	Y	Y	Y	Y
Year*day of week	Y	Y	Y	Y
Day of yeardummies	Y	Y	Y	Y

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.
Note: We report coefficients ona December 2010-January 2011 dummy (the weeks right around benefit cancellation)from the following regression: $B_{jt}= \alpha + \beta Dec2010\text{-}Jan2011_{jt} + \delta_{dw} + \phi_{dy}+ \mu_h + \lambda_t + \varepsilon_{jt}$, where B is the (log) number of births taking place on day j of year t and the main explanatory variable is a dummy that takes value 1 for December 2010-January 2011 births. An observation is a day. The sample includes all births 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 quadruplets from 2000-01 to 2011-12. Robust standard errors are shown in parentheses.

Table A5. The effect of the benefit cancellation on gestation length

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Panel A. Daily-level analysis				
Dep. var.: N. of births <37 weeks	16.0409***	11.5745***	10.0771***	8.0175***
	(4.3239)	(3.1953)	(2.8430)	(2.3846)
Dep. var.: N. of births 37-38 weeks	73.7596***	52.8614***	39.4982***	32.5832***
	(16.7676)	(11.6337)	(8.7697)	(7.6339)
Dep. var.: N. of births 39-40 weeks	122.2551***	92.1499***	80.9057***	67.2842***
	(17.3428)	(13.8019)	(10.6833)	(9.6879)
Dep. var.: N. of births 41-42 weeks	25.9215***	17.3746***	12.8240***	11.2284***
	(5.4576)	(6.0936)	(4.6368)	(3.9583)
Dep. var.: N. of births >42 weeks	0.1701	0.4533	0.2321	0.2493
	(0.5482)	(0.3793)	(0.2867)	(0.2424)
N	168	336	504	672
Panel B. Individual-level analysis				
Dep. var: Gestation weeks	-0.0509**	-0.0362**	-0.0300**	-0.0251**
	(0.0196)	(0.0145)	(0.0130)	(0.0117)
N	165228	341161	518185	689240
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%)

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.Note: We report coefficients ona December 2010dummy (the month right before benefit cancellation). In Panel A the regression is equation (1) in the text, an observation is a day, and the dependent variable is the number of daily births for different gestation durations. The sample includes all births (by gestational length) in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2011-12. Robust standard errors are shown in parentheses. In Panel B, the equation is offered in footnote 22, an observation is an individual birth, and the dependent variable is gestation length measured in weeks. Control variables include: an indicator for all December births, mother and father's age, mother's immigrant status and marital status, a dummy for urban areas, two sets of dummies for parental occupation, dummies for first-borns, female babies, and multiple births, and province fixed effects. The sample includes all births in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2011-12. Standard errors, clustered by date, are shown in parentheses.

Table A6. 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 maternity beds per 1,000	0.0824**	0.1146***	0.1264***	0.1165***
females aged 15-44 in province	(0.0397)	(0.0393)	(0.0396)	(0.0362)
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%)

Data sources: 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.

Note:We report coefficients onthe interaction between “Reform”, a binary explanatory variable taking value one if the birth occurs in December 2010-January 2011 (the weeks right around benefit cancellation), and the availability of private health centers in the province from equation (2). An observation is an individual birth. The dependent variable is a binary indicator that takes value one if the birth occurs in December. 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. The sample includes all births in the last 1 to 4 weeks of December and the first 1 to 4 weeks of January (depending on the column), for December-January pairs from 2000-01 to 2011-12.Standard errors, clustered by province, are shown in parentheses.

Table A7. The effect of benefit cancellation on birth-weight, 2007-2012 sample

	+/-1 week	+/-2 weeks	+/-3 weeks	+/-4 weeks
Dep. var.: Birth weight	-14.3777***	-10.7014**	-3.5703	-2.0724
	(5.3420)	(4.1967)	(3.6412)	(3.1183)
Dep. var.: Birth weight (in logs)	-0.0050**	-0.0039**	-0.0014	-0.0008
	(0.0019)	(0.0015)	(0.0013)	(0.0011)
Dep. var.: BW<1,500	0.0005	0.0010	0.0003	0.0001
	(0.0013)	(0.0010)	(0.0008)	(0.0007)
Dep. var.: BW<2,500	0.0016	0.0008	0.0002	0.0004
	(0.0032)	(0.0021)	(0.0017)	(0.0016)
Dep. var.: BW<2,750	0.0062	0.0036	0.0022	0.0020
	(0.0038)	(0.0029)	(0.0025)	(0.0021)
Dep. var.: BW<3,000	0.0095*	0.0074*	0.0036	0.0029
	(0.0051)	(0.0039)	(0.0032)	(0.0027)
Dep. var.: BW<3,500	0.0105**	0.0063*	0.0006	0.0006
	(0.0050)	(0.0037)	(0.0031)	(0.0026)
N	175,823	357,968	539,044	719,402

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

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2007-2012.
Note: We report coefficients ona December 2010-January 2011 dummy (the weeks right around benefit cancellation) from equation (4). 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. The sample includes all babies born 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 quadruplets from 2007-08 to 2011-12. Robust standard errors, clustered at the date level, are shown in parentheses.

Table A8. 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 (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 0-12 months	0.0054	0.0083	0.0074	0.0058
	(0.0148)	(0.0085)	(0.0066)	(0.0057)
Age 365-1000 days (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%)

Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2007-2013.

Note: We report coefficients on aDecember 2010-January 2011 dummy (the weeks right around benefit cancellation) fromequation (5). An observation is a day (birth-date). 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. Control variables include calendar month dummies, and year fixed effects.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. Standard errors are shown in parentheses.

Table A9. 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 0-2 months				
Perinatal conditions	-0.0008	-0.0005	0.0005	-0.0012
	(0.0051)	(0.0034)	(0.0028)	(0.0024)
Perinatal infection	-0.0006	-0.0007	0.0002	0.0002
	(0.0012)	(0.0009)	(0.0007)	(0.0006)
Respiratory disease	0.0147***	0.0132***	0.0119***	0.0105***
	(0.0051)	(0.0035)	(0.0031)	(0.0030)
Bronchitis	0.0119***	0.0111***	0.0103***	0.0092***
	(0.0045)	(0.0031)	(0.0028)	(0.0027)
Digestive problems	-0.0005	-0.0008	-0.0006	-0.0005
	(0.0008)	(0.0005)	(0.0004)	(0.0004)
Infectious disease	0.0002	0.0012*	0.0003	0.0003
	(0.0009)	(0.0006)	(0.0005)	(0.0004)
Age 2-12 months				
Respiratory disease	-0.0042	-0.0032	-0.0017	-0.0013
	(0.0041)	(0.0025)	(0.0020)	(0.0018)
Bronchitis	-0.0018	-0.0008	-0.0005	-0.0005
	(0.0030)	(0.0019)	(0.0016)	(0.0014)
Digestive problems	-0.0010	0.0000	-0.0003	-0.0004
	(0.0015)	(0.0010)	(0.0008)	(0.0007)
Infectious diseases	-0.0011	-0.0014	-0.0014*	-0.0008
	(0.0015)	(0.0010)	(0.0007)	(0.0006)
Age 12-33 months				
Respiratory disease	-0.0000	-0.0010	-0.0014	-0.0015
	(0.0043)	(0.0025)	(0.0019)	(0.0018)
Bronchitis	0.0010	-0.0001	0.0001	-0.0007
	(0.0016)	(0.0010)	(0.0008)	(0.0007)
Digestive problems	-0.0029*	-0.0026**	-0.0024***	-0.0015**
	(0.0016)	(0.0010)	(0.0008)	(0.0007)
Infectious diseases	-0.0013	-0.0007	-0.0006	-0.0008
	(0.0017)	(0.0011)	(0.0008)	(0.0007)
Age 0-33 months				
Respiratory disorders (11.4%)	0.0104	0.0091	0.0088**	0.0077*
	(0.0098)	(0.0057)	(0.0044)	(0.0041)
Bronchitis (6.5%)	0.0111*	0.0102***	0.0099***	0.0079***
	(0.0063)	(0.0039)	(0.0032)	(0.0029)
Digestive problems (3.0%)	-0.0044	-0.0033*	-0.0033**	-0.0024**
	(0.0027)	(0.0017)	(0.0014)	(0.0012)
Infectious diseases (2.9%)	-0.0022	-0.0009	-0.0017	-0.0013
	(0.0030)	(0.0018)	(0.0014)	(0.0011)
N	140	280	420	560

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

Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2007-2013.

Note: We report coefficients on aDecember 2010-January 2011 dummy (the weeks right around benefit cancellation) fromequation (5). An observation is a day (birth-date). 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. Control variables include calendar month dummies, and year fixed effects. 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. Standard errors are shown in parentheses.

Table A10. The effect of benefit cancellation on hospitalizations: Robustness checks

Dep. var.: Hospitalization rate, age 0-2 months	+/- 1 weeks	+/- 2 weeks	+/- 3 weeks	+/- 4 weeks
Panel A. Dropping Madrid residents				
Respiratory disease	0.0110*	0.0103***	0.0093***	0.0074***
	(0.0056)	(0.0036)	(0.0028)	(0.0025)
Bronchitis	0.0092*	0.0092***	0.0088***	0.0069***
	(0.0052)	(0.0032)	(0.0026)	(0.0023)
Panel B. Controlling for flu incidence				
Respiratory disease	0.0092*	0.0086***	0.0076***	0.0060***
	(0.0048)	(0.0031)	(0.0024)	(0.0022)
Bronchitis	0.0080*	0.0079***	0.0073***	0.0057***
	(0.0044)	(0.0028)	(0.0022)	(0.0019)
Panel C. Adding March and February as control months				
Respiratory disease	0.0059	0.0072***	0.0068***	0.0057***
	(0.0036)	(0.0023)	(0.0019)	(0.0017)
Bronchitis	0.0053	0.0062***	0.0062***	0.0051***
	(0.0032)	(0.0021)	(0.0016)	(0.0015)
Panel D. Controlling for benefit eligibility				
Respiratory disease	0.0112*	0.0105***	0.0097***	0.0077***
	(0.0059)	(0.0038)	(0.0030)	(0.0026)
Bronchitis	0.0088	0.0090***	0.0086***	0.0066***
	(0.0054)	(0.0034)	(0.0027)	(0.0024)

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

Data sources: Hospital Morbidity Survey micro data and birth-certificate micro data, Spanish National Statistical Institute, 2000-2013.

Note: We report coefficients on aDecember 2010-January 2011 dummy (the weeks right around benefit cancellation) fromequation (5).An observation is a day (birth-date). The dependent variable is the number of overnight hospitalizations at age 0 to 59 days with a given diagnosis, of children born on a given day, divided by the number of children born on that day. Control variables include calendar month dummies, and year fixed effects. 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. Standard errors are shown in parentheses.

Table A11. The effects of the benefit cancellation on the timing of births by province (+/- 1 week window)

	Additional Daily Births		Additional Births per Hospital	Additional Births per 100 Beds
Panel A. Top 10 percent in the distribution of Additional Births per Hospital				
Almería	4.262	***	0.474	0.286
Castellon	3.097	***	0.619	0.200
Cuenca	0.804		0.804	0.171
Huelva	3.133	**	0.627	0.256
Sevilla	9.666	***	0.403	0.196
Melilla	0.340		0.340	0.198
Panel B. Averages				
Top 10 %	3.550		0.544	0.217
Spain Average	2.793		0.206	0.079

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

Data sources: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012, and National Catalogue of Hospitals, Spanish Ministry of Health, 2000-2012.

Note: In the first column, we report coefficients on December 2010 dummy (the month before benefit cancellation) from equation (1),where an observation is a day, and the dependent variable is the daily number of births in each province. The sample includes all births in the last week of December and the first week of January, for December-January pairs from 2000-01 to 2011-12.The second and third columns divide the estimated province effects in column 1 by the corresponding number of hospitals in the province and the number of hospital beds in the province from the hospitals catalogue, respectively.

Table A12. The effect of benefit cancellation on birth-weight and mortality, controlling for benefit eligibility

	+/-1 week	**+/-2 weeks**	**+/-3 weeks**	**+/-4 weeks**
Panel A. Birth Weight Outcomes				
Dep. var.: Birth weight	-15.06**	-14.19***	-6.51*	-4.65
	(6.5371)	(4.6154)	(3.7856)	(3.3048)
Dep. var.: Birth weight	-0.0036	-0.0040**	-0.0017	-0.0012
(in logs)	(0.0024)	(0.0017)	(0.0014)	(0.0012)
Dep. var.: BW<1,500	-0.0017	-0.0004	-0.0005	-0.0004
	(0.0013)	(0.0009)	(0.0007)	(0.0006)
Dep. var.: BW<2,500	-0.0016	-0.0002	-0.0011	-0.0007
	(0.0033)	(0.0023)	(0.0019)	(0.0017)
Dep. var.: BW<3,000	0.0112**	0.0086**	0.0051	0.0043
	(0.0057)	(0.0040)	(0.0033)	(0.0028)
N	397,505	809,882	1,220,263	1,627,681
Panel B. Mortality Outcomes				
Dep. var.: Late fetal death	-0.0001	-0.0000	-0.0000	-0.0001
(per 1,000 births)	(0.0007)	(0.0005)	(0.0004)	(0.0003)
Dep. var.: Neonatal mortality (24 hours)	-0.0003	0.0001	0.0001	-0.0000
(per 1,000 births)	(0.0003)	(0.0002)	(0.0002)	(0.0002)
N	418,539	852,606	1,283,972	1,712,552

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

Data source: Birth-certificate micro data, Spanish National Statistical Institute, 2000-2012.
Note: We report coefficients on December 2010-January 2011 dummy (the weeks right around benefit cancellation) from equation (4). An observation is an individual newborn baby. Control variables include: an indicator for benefit eligibility (October 2007-December 2010 births), 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. The sample includes all babies born 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 quadruplets from 2000-01 to 2011-12. Robust standard errors, clustered at the date level, are shown in parentheses.