Born Too Soon? The Educational Costs of Early Elective Deliveries

Libertad González
(Universitat Pompeu Fabra & Barcelona School of Economics)

Parijat Maitra
(Stockholm University)

Abstract: A growing body of research shows that even mild shocks in early life can have long-lasting effects on human capital development, underscoring the importance of early-life interventions. In parallel, advances in obstetric practices have made it increasingly possible to control the timing of childbirth. Cesarean sections and labor induction are now widespread, and in many cases, they are performed electively, often for non-medical reasons. While the rise in medically scheduled deliveries has sparked concerns about short-term health risks, little is known about the long-term consequences of early elective deliveries (EED), especially in low-risk pregnancies. We study the effects of early elective birth timing on children's health and educational outcomes, focusing particularly on cognitive development as proxied by grades in primary school. We exploit a natural experiment in Spain: the abrupt cancellation of a generous birth benefit at the end of 2010. The policy change prompted a spike in elective deliveries in the last week of December 2010. We show that children born due to this elective timing, who experienced slightly shorter gestation and lower birth weights within the normal range, subsequently faced elevated respiratory hospitalizations in their first year of life and, crucially, significantly worse school performance at age seven. Our findings provide causal evidence on the risks of early elective deliveries for medium-term child outcomes, and trace a clear path linking neonatal health to human capital formation.

1. Introduction

The profound and enduring impact of early-life conditions on human capital development is a well-established fact in economics (Almond et al. 2018). This body of literature highlights the critical importance of early interventions (González & Trommlerová 2022 etc.), including the in-utero period. Simultaneously, modern obstetric practices, including the widespread use of Cesarean sections and labor induction, have granted medical professionals unprecedented control over the timing of childbirth. These procedures are increasingly performed electively (Osterman and Martin 2014), often for non-medical considerations (Halla et al. 2020). While the short-term health implications of such medically scheduled deliveries have received attention (Borra et al. 2019, Card, Fenizia, and Silver 2018, Costa-Ramón et al, 2018), the long-term consequences, particularly for healthy, low-risk pregnancies, remain largely unexplored.

This paper addresses this gap by investigating the causal effects of early elective birth timing on children's health and, more importantly, their long-term educational attainment, as measured by grades in primary school. Our empirical strategy leverages a unique natural experiment in Spain: the sudden repeal of a substantial child benefit at the close of 2010. This policy change created a sharp, exogenous incentive for parents to induce labor or schedule C-sections in late December 2010, ensuring eligibility for the expiring benefit. This led to a discernible spike in births during this period, providing a quasi-random assignment to slightly earlier delivery for a subset of infants.

Our key findings are threefold. First, we confirm that the policy change indeed led to a significant increase in births in the last week of December 2010, which were associated with slightly shorter gestational periods and lower birth weights, albeit still within the normal range. Second, we demonstrate that these electively delivered infants experienced an elevated incidence of respiratory hospitalizations and diagnoses in their first year of life, suggesting immediate, albeit non-severe, health consequences. Third and most critically, we show that these children, by age seven, exhibited significantly worse performance on test scores in primary school. These educational deficits persist even after accounting for factors such as age at testing, suggesting a direct negative impact on cognitive development rather than merely being "young for grade."

Our study builds upon and contributes to several important bodies of literature. First, we contribute to the growing literature on the long-term effects of early-life health shocks on human capital. A significant body of work has documented the detrimental impact of adverse prenatal and perinatal conditions, such as exposure to pollution (Currie et al. 2021), natural disasters (Maclean et al. 2016), or nutritional deficiencies (Almond and Mazumder 2013), on later-life outcomes. More specifically, research has shown that lower birth weight and shorter gestation are associated with poorer health, educational, and labor market outcomes (e.g., Almond et al. 2005; Black, Devereux, and Salvanes 2007; Royer 2009; Bharadwaj et al. 2013; Figlio et al. 2014). Our contribution here is to provide causal evidence on the effects of *mild* gestational age reductions induced by elective deliveries, focusing on outcomes within the "normal" birth range, which is often less studied compared to severe prematurity or low birthweight. Our results align with a more recent strand of literature that points to the developmental sensitivity to even mild shocks in early life (Almond et al. 2018).

Second, our paper speaks to the literature on the rise and consequences of medical interventions in childbirth. The increasing rates of Cesarean sections and labor induction globally have raised concerns about potential short-term health risks for mothers and infants (Buckles & Guldi 2017; Teitler et al. 2019; Halla et al. 2020; Jacobson et al. 2021). Our focus, however, is on the *long-term* effects on children's development, especially when these procedures are undertaken for non-medical reasons. This differentiates our work from studies primarily concerned with the immediate health outcomes of C-sections (e.g., Costa-Ramón et al. 2018, Card, Fenizia, and Silver 2018), and extends the inquiry into the long-term developmental trajectory.

Third, our study utilizes a natural experiment arising from a policy change, similar to other studies that have exploited policy discontinuities or changes to identify causal effects in early life (e.g., Løken et al. 2012; Black et al. 2007). The abrupt cancellation of the Spanish birth benefit provides a compelling quasi-experimental setting to isolate the causal impact of elective birth timing, circumventing endogeneity issues inherent in observational studies of birth timing decisions. This approach allows us to make strong causal claims about the consequences of slightly earlier elective births.

Fourth, while the effects of prematurity and low birthweight are well-documented, less is known about the long-term implications of being born at the earlier end of the "full-term" or "late preterm" range. Our findings demonstrate that even a small reduction in gestational age within this seemingly "normal" range can have significant and persistent negative consequences for cognitive development and educational outcomes. Our findings have important policy implications, highlighting the potential unseen costs of non-medically indicated elective deliveries, and potentially informing guidelines for obstetric practices.

Finally, our study contributes to a deeper understanding of the behavioral and intertemporal responses triggered by seemingly non-distortionary, lump-sum cash transfers in the context of early-life policy interventions. While programs like Spain's "baby bonus" were effective in achieving their proximate objective (stimulating fertility), the abrupt cancellation of the policy at the end of 2010 induced a sharp, anticipatory shift in delivery timing. This finding reveals that parents treated the expiring benefit as akin to a transfer conditioned on the date of birth, and responded accordingly. Importantly, such behavioral responses emerged even though the transfer was, in theory, lump-sum and should not have distorted decisions under standard economic assumptions. The bunching we observe around the cut off suggests that in practice, such transfers can alter decision-making when parents are operating under uncertainty about future outcomes and when the perceived short-run benefits of early delivery outweigh concerns about less salient long-term risks.

More broadly, our results highlight the potential for early-life policies (especially those tied to rigid eligibility thresholds) to influence parental behavior in ways that carry unintended consequences for child development. Because long-term cognitive and health deficits are not immediately observable at birth, parents may underweight these risks when making time-sensitive delivery decisions. As a result, even within the normal gestational window, minor reductions in fetal maturity induced by policy incentives can translate into persistent deficits in cognitive outcomes, as we document. These findings call for a more cautious and forward-looking approach to the design and termination of early-childhood cash transfer programs.

The remainder of the paper is organized as follows. Section 2 describes the institutional context and our empirical strategy. Section 3 presents the data. Section 4 discusses our main results on health and educational outcomes. Section 5 explores potential mechanisms, and section 6 concludes.

2. Institutional Background and Empirical Strategy

2.1 The Spanish Birth Benefit and its Cancellation

In July 2007, the Spanish government introduced a universal child benefit (or "baby bonus") providing a one-time payment of €2,500 for every child born. This was a substantial amount, equivalent to approximately two months of average earnings, and was highly salient to prospective parents. The benefit was abruptly canceled in May 2010, with the cancellation effective from January 1, 2011 (less than eight months later). This created a strong incentive for expecting parents whose babies were due around the end of 2010/beginning of 2011 to accelerate the birth to ensure eligibility for the benefit. The sudden nature of the cancellation, with no prior announcement of a phase-out, maximized the incentive for elective deliveries.

2.2 Empirical Strategy: A Difference-in-Differences Approach

We follow a difference-in-differences strategy, comparing the timing of births and the health and educational outcomes of children born in the vicinity of December 31, 2010 with those of children born in the same dates of the surrounding years. The specification varies slightly for the three sets of outcomes.

Birth timing

A key aspect of our identification strategy is to demonstrate that the policy indeed induced a shift in birth timing. We examine the density of births around the cutoff date, expecting a jump (bunching) just before the cutoff and a drop (a "hole") just after, if elective deliveries occurred. Our empirical approach relies on comparing the number of births in late December of 2010 relative to early January of 2011, using the surrounding years as controls (two years before and two years after). We estimate the following regression:

$$\boldsymbol{Births_{jt} = \alpha + \beta Dec2010_{jt} + \delta_{dw} + \varphi_{dy} + \mu_h + \lambda_t + \varepsilon_{jt}} \qquad \textbf{(1)}$$

Where *Births* is the number (or the log number) of births taking place on day j of year t. Our explanatory variable of interest is a dummy for those born in 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, whereas the year dummies control for any aggregate factors (including, but not limited to, the business cycle which is quite possibly correlated with birth rates over time). Our full specification 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. If the benefit cancellation affected the timing of births, we expect β to be positive. We estimate equation (1) for five different samples. First, we limit the sample to only the seven days before and after the turn of the year, as we expect most of the action to take place in the days immediately surrounding the cut off date, i.e., 31 Dec. 2010. We then extend the window to ten, fourteen, and twenty days, and the full months before and after the cut off date. The full sample thus includes all births in the months of December and January, for the December–January pairs from 2008–09 to 2012–13.

Health outcomes

Comparing health outcomes of children born in (late) December of 2010 to those of children born in December of the surrounding years would confound the effects of the change in birth timing with any differential characteristics of the subsample of children who were born early because of the benefit cancellation. To solve this selection issue, our birth timing analysis above identifies the "manipulation period", i.e. the precise dates in December 2010 and January 2011 where we observe birth shifting. We then compare the health outcomes of all children born during the manipulation period (i.e. combining December- and January-born kids), to children born on the same dates in the surrounding years. By including the full manipulation period, we rule out composition effects due to the non-random subsample of families who reacted by anticipating birth. To control for aggregate time effects, we also include children born in October-November and February-March of the treated and control years, thus leading to our difference-in-differences specification. Our approach allows us to estimate "intent-to-treat" effects (since we are unable to determine which specific children were "switched" as a result of the policy). We formalize this empirical strategy in the following specification:

$$\boldsymbol{Y_{ijt} = \alpha + \beta_1 Dec\text{-}Jan_{jt} + \beta_2 (Dec2010\text{-}Jan2011)_{jt} + \lambda_t + \varepsilon_{ijt}} \qquad \textbf{(2)}$$

Where *Y* denotes the health outcome of child *i*, born on day *j* of year *t*. The variable *λ* represents year fixed effects, defined as cohorts of children born between October of a given year and March of the following year, from 2008–09 through 2012–13. These binary indicators account for cohort-level variation. The variables *Dec–Jan* and *Dec2010–Jan2011* identify births occurring in December and January across all years (2008–13), and births during the specific reform window, i.e., December 2010 to January 2011 respectively. The latter serves as our primary explanatory variable. The coefficient of interest, β_2, thus provides a difference-in-differences estimate by comparing outcomes for children born in December–January during the reform period (2010–11) with those born in the same months in surrounding years, using births in October–November and February–March as control groups. We estimate this model using five sample windows. First, we restrict the analysis to births occurring within seven days before and after December 31 (and, correspondingly, October 31 and February 28/29 for the control periods). We then expand the window incrementally to ten, fourteen and twenty days, and finally to include the entire months before and after the cut off. The full sample thus covers all births occurring between October and March, from 2008–09 to 2012–13.

School performance

The children who were born in December due to the benefit cancellation would have started school a year early, since December 31 coincides with the strict threshold for school cohorts in Spain. As a result, children born in our "manipulation period" include a higher share of children who are "young for their grade", which would bias our estimates if we followed the same difference-in-differences strategy as for the health outcomes. We thus propose a more flexible specification for the school outcomes, where we include children born throughout the year, and we control directly for "age at test", i.e., we compare school grades for children in the manipulation window to those of children born on the same dates of the surrounding years (Calsamiglia & Loviglio 2020). This controls for age at test but not for selection effects. However, while the selection effect would be present for both children born in early January of 2011 and those born in December of 2010, the causal effect of birth shifting would only appear for the December 2010 group, which allows us to disentangle them. We use the following specification:

$$Y_{imt} = \alpha m + \beta_1 Dec2010_{it} + \beta_2 Jan2011_{it} + \gamma X_{it} + \lambda_t + \varepsilon_{it} \quad (3)$$

where Y is the second-grade test score of child i born in month m of year t. The variables *Dec2010* and *Jan2011* are treatment indicators for children born in December 2010 and January 2011, respectively. In alternative specifications, we replace these with a series of dummies for 10-day intervals around the cutoff (e.g., December 1–10, December 11–20, etc.), allowing us to capture more granular effects of birth timing. All regressions include year-of-birth fixed effects (λ) and a rich set of child-level controls X, including gender, municipality population size, and parental background. To account for age at test, we estimate the model under four timing specifications: (i) a linear trend in month of birth; (ii) month-of-birth fixed effects; (iii) a linear trend in week of birth; and (iv) week-of-birth fixed effects. This framework allows us to test for both broad and narrow timing effects of being born around the cut off, while flexibly controlling for age-at-test and time-of-year trends in educational performance.

3. Data

Our analysis draws on several rich administrative datasets from Spain.

Birth Records: We obtain comprehensive birth records for Spain, including exact date of birth, gestational age, birth weight, and other demographic information, for years 2008 to 2013. This allows us to precisely identify the cohort affected by the policy change and analyze birth timing and health at birth.

Hospital Discharge Records: We link birth records for the relevant cohorts to hospital discharge records in years 2015 to 2020, when the treated cohort is 5 to 10 years old, enabling us to identify hospitalizations for respiratory and other disorders during childhood.

Primary healthcare record: We obtained medical histories from the public primary healthcare system BIFAP (Base de Datos para la Investigación Farmacoepidemiológica en Atención Primaria) in several Spanish regions, for the relevant cohorts of children (born in 2008-2013). These data allow us to observe doctor visits, diagnoses, referrals, and prescriptions at ages 0 to 10.

Educational Records: We obtained administrative data on primary school grades for children born in years 2008 to 2013 in one large Spanish region (Catalonia). We have scores in all subjects in second grade (age 7) for a random 50% sample of children born in the relevant years. We use grades in the different subjects (language, math, science, etc) as measures of cognitive development, while grades in physical education may relate more to physical development, and grades in "civic values" may also capture behavioral aspects. We also have information on school type (public vs. private).

4. Results

4.1 Impact on Birth Timing

We begin by presenting descriptive evidence on birth patterns around the benefit cutoff. Panel A of Figure 1 plots the weekly number of births in December and January for the 2010–11 period (in blue) alongside the same weeks in the adjacent years 2009–10 and 2011–12. A sharp increase in births is visible in the week leading up to December 31, 2010, relative to both comparison years. This spike is immediately followed by a marked drop in births in the first two weeks of January 2011, suggesting a shift in the timing of deliveries around the policy cutoff date. More importantly, birth patterns in the surrounding years remain relatively stable across the same period, reinforcing the notion that the 2010–11 pattern reflects a one-off distortion driven by the benefit cancellation. Panel B (Figure 1) plots the difference in weekly births between 2010–11 and the average of the three surrounding years (2009–10, 2011–12, and 2012-13). The figure shows a clear divergence starting in mid-December, with the number of births peaking in the final week of the year (just before the policy cut off) by nearly 1,500 births above trend. This excess is immediately offset by a decline of comparable magnitude in early January, consistent with anticipatory birth shifting. The difference gradually returns to zero by mid-February, indicating that the effect is concentrated around the policy threshold and not driven by longer-run seasonal fluctuations. These figures visually confirm the manipulation in birth timing around the end of December 2010 and support the empirical strategy focusing on this manipulation window.

We formalize these patterns in the regression framework defined in Equation (1), with results presented in Table 1. Panel A (Table 1) reports the average increase in the number of daily births in December 2010, relative to January 2011, across different time windows around the cutoff date. Column (1) uses the full months of December and January, while columns (2) to (5) progressively narrow the comparison window to ±20, ±14, ±10, and ±7 days. Across all specifications, we observe a statistically significant increase in births in December 2010, with the magnitude of the effect growing as the window narrows. The estimated effect rises from 106.3 additional daily births (column 1) to 172.6 (column 5), confirming that the birth shifting is concentrated in the days immediately preceding the benefit cutoff. Panel B reports results using the log of the number of daily births as the dependent variable. The pattern is consistent: the estimated coefficient on the December 2010 dummy remains positive and statistically significant across all columns. In the full-month specification (column 1), the estimated 7.8% increase in December 2010 births suggests that approximately 3.9% of births that would have occurred in January were shifted to December. When restricting the window to the last 7 days of December and the first 7 days of January (column 5), the estimated effect rises to 12.3%, implying that around 6.2% of early January births were brought forward into the final week of December.

Appendix Tables A1.1–A1.2 and A2 provide robustness checks using subsamples and alternative control years. The results for Catalonia (A1.1) and the BIFAP regions, i.e., the autonomous communities of Aragón, Canarias, Cantabria, Castilla-La Mancha, Castilla y León, Comunidad Valenciana, Extremadura, La Rioja, Madrid, Murcia, Navarra, and Principado de Asturias covered by the primary health care database (A1.2) show similar patterns, with significant increases in December births concentrated around the cut off. For Catalonia (±10-day window), we find that 7.4% of early January births were shifted forward into the last 10 days of December. Table A2 replicates the main analysis (in Table 1) using an alternate set of control years (2006–07 to 2012–13), and the results remain robust in both levels and logs. Together, these findings confirm a clear shift in birth timing around the December 2010 cut off, consistent with parents adjusting delivery dates to retain eligibility for the cancelled benefit.

4.2 Effects on Early-Life Health Outcomes

We next examine whether the observed shift in birth timing around the benefit cutoff had any impact on newborn health. Figure 2 presents descriptive evidence on birthweight and gestational age distributions. Panel A plots average birthweight in the one-week window spanning December 31, separately by year. A sharp drop is visible in 2010–11 compared to surrounding years (2008-09, 2009-10, 2011-12, and 2012-13), suggesting that children born around the cut off were slightly lighter at birth. While year-to-year variation is otherwise small and stable, the 2010–11 value stands out as a clear deviation from trend. Panel B shows the distribution of gestational age (in weeks) for births in December 2010 compared to the same month in control years. December 2010 births are more concentrated in gestational weeks 37 to 39, and under-represented in week 40, indicating a shift toward slightly earlier deliveries. This pattern is consistent with medically induced or scheduled births in response to the policy cut off. Together, these patterns suggest that some of the births shifted into late December were delivered earlier than they would have been otherwise, potentially affecting newborn health.

Table 2 presents the estimated impact of the benefit cancellation on newborn health outcomes, focusing on birthweight and gestational age for the full Spanish sample. Columns (1) through (5) show results for progressively narrower windows around the policy cut off (ranging from full-month comparisons to ±7-day windows). In Panel A, across all specifications, children born around the December 31, 2010 cutoff exhibit lower birthweights. The effect ranges from –6.075 grams in the full-month comparison (column 1) to –21.04 grams in the ±7-day window (column 5), which is statistically significant at the 1% level. The average treatment effect appears small in absolute terms as this estimate reflects an average across all births in the window. Given that only 6.2% of births in the ±7-day window were plausibly affected by birth shifting, the implied effect for the treated group is large (equivalent to a nearly 340-gram drop if concentrated solely among shifted births). Consistent with the drop in birthweight, Panel B shows a statistically significant reduction in gestational age (in weeks). The estimated effect increases in magnitude as the window narrows, from –0.03 weeks in the full-month specification to –0.062 weeks in the ±7-day window. Again, given the small fraction of births affected (6.2%), the implied reduction in gestational age for shifted births is substantially larger (around one full week). We also find no significant increase (Panel C) in the fraction of babies born with birth-weight less than 2,500 grams (the threshold typically used as an indicator for low birth-weight), thereby suggesting that the increase in scheduled births were primarily driven by healthy pregnancies. Panels D and E show that the decline in birthweight is present for both boys and girls. The effect is consistently larger for girls across all windows. In the ±7-day window, birthweight falls by 22.3 grams for girls and 20.2 grams for boys (both statistically significant at the 1% level).

Appendix Table A4 presents the results for the autonomous community of Catalonia (Panels A, B, and C), and the other autonomous communities covered by the BIFAP database (Panels D, E, and F). The results for Catalonia confirm the main findings and point to a possibly stronger regional response, particularly in terms of birthweight loss (a decline of nearly 400 grams for the treated children in the ±10-day window) near the cutoff. Similarly, across the BIFAP regions, the evidence aligns closely with the national results, confirming that the effects on gestational age and birthweight are not limited to specific regions.

Appendix Table A5 explores whether the benefit-induced shift in birth timing had broader consequences on delivery conditions and newborn health. The outcome variables span six domains: place of delivery, assisted births, delivery complications, firstborn status, cesarean births, and full-term pregnancies. Panel A confirms that virtually all births occur in health centers regardless of timing, with no meaningful effect detected. Panel B shows no consistent change in the use of medically assisted births (possibly suggesting that the increase in early deliveries was not driven by a rise in instrumental or assisted interventions). Panel C shows a statistically significant increase in delivery complications, particularly in the ±10 and ±7-day windows. The coefficient rises to 0.00659 in the narrowest window, implying a non-trivial increase in complications among the shifted births (around 10%). Panel D (firstborn indicator) and Panel E (cesarean births) show no significant changes, suggesting that the composition of births (e.g., parity or surgical intervention rates) remained stable. Panel F shows a consistent and statistically significant decline in full-term pregnancies (37+ weeks), with effects growing stronger in narrower windows. In the ±7-day window, the estimate reaches –0.007 confirming that among the "treated" births (6.2%), about 11% were induced or scheduled slightly before term.

Table A3 provides evidence of mild positive selection into the December 2010 birth cohort. Families more likely to shift births forward include those who are: more educated (particularly mothers), slightly older mothers, more likely to be Spanish-born, and residing in urban areas (though to a limited extent). These patterns suggest that better-informed or higher-resource families may have been more capable of responding to the policy, though the magnitude of the selection effects is modest overall.

Table 5 and Figure 4 examine the early and long-term respiratory outcomes for children born around the policy cutoff, using BIFAP primary care data. While we find no consistent evidence of increased overall respiratory diagnoses due to the birth shifting (other than possibly elevated respiratory diagnosis at age 10; Figure 4), our estimates suggest that, on average, children born during the ±7-day policy window had 0.0113 more bronchiolitis diagnoses at age 1, compared to the control group (given a baseline mean of ~ 0.1156 diagnoses per child, this corresponds to a nearly 10% increase). We also see similar results for bronchitis diagnoses at age 1 (~ 8% increase), but the results are only significant for the ±14-day window. On the other hand, we don't find significant differences (both short- or long-term) in the number of visits to primary care (Table A8.1) or medication prescriptions for respiratory disorders (Table A8.2). Nor do we find (Figure A2) any systematic or sustained effect on BMI (standardized for age) for the affected cohort (possibly suggesting that the policy-induced shift in gestation did not lead to persistent effects on body mass or growth during early or late childhood).

Next, we examine the effect of benefit cancellation on long-term hospitalizations, both overall and specifically for respiratory disorders, for the whole Spanish sample (reported in Table 6) and for Catalonia (Table 7) using the Hospital Morbidity Surveys of 2015-2020. Given the way the data are structured, the same child may have been hospitalized multiple times. Therefore, the results should be interpreted as the number of hospital stays per 100 births on day j of year t, rather than as the fraction of children with at least one hospital stay. Our estimates suggest that children born in the ±7-day window around 31 Dec, 2010 had 0.9 (column 5; Panel A of Table 6) fewer hospital stays per 100 births for all disorders. When it comes to hospitalizations for respiratory disorders, we see that the children born in the ± 7 days (column 5; Panel B of Table 6) around the cutoff date, had 0.475 (significant at 95 %) fewer hospitalizations per 100 births. For the bigger time windows around the threshold (columns 1 to 4; Panel B) none of the coefficients are significant, and are, in fact, quite close to zero. For the Catalonia subsample (Table 7) we find similar results: the "treated" children are no more likely to be hospitalized at ages 4 to 10 (overall, or for respiratory disorders). Lastly, we look (Tables A9.1 and A9.2) at the health outcomes using the education specification in (3). Our results seem fairly robust across specifications.

4.3 Effects on Educational Outcomes

We now turn to the main result of this study. Figure 3 Panel A shows average second-grade test scores by 10-day birth window for the 2010–11 cohort compared to the surrounding years. Children born in late December 2010 (those most likely to have had their births shifted forward) score significantly lower than their counterparts in previous years. In contrast, children born in early

January 2011 perform similarly to controls, suggesting no deviation in outcomes for those unaffected by the policy. Panel B (Figure 3) plots the difference in average test scores (2010–11 vs. control years). The sharpest negative deviation occurs in the Dec. 21–31 window, (with estimates around –0.06). This negative effect is not mirrored in adjacent January windows, reinforcing the idea that the drop in performance is concentrated among children whose birth timing was altered by the policy and who started school a year earlier as a result. Figure A1 shows average second-grade test scores by month of birth for the full sample. There is a clear declining gradient from January to December, consistent with age-at-test effects: children born earlier in the year are older when tested and perform better on average. This motivates the need to control carefully for month of birth in the regression models to isolate the causal effect of being shifted into an earlier school cohort from mechanical age effects.

Table 3 presents regression estimates of the effect of being born in December 2010 or January 2011 on average second-grade (standardized) test scores. Columns (1) and (2) include a linear trend in month of birth, while columns (3) to (8) use increasingly flexible specifications: month fixed effects, week trends, and week fixed effects. In all specifications, the December 2010 coefficient is negative, with estimates ranging from –0.0276 to –0.0321, and statistically significant in models with linear controls (columns 1 and 5). The January 2011 coefficients are negative but smaller and not statistically significant in any model. When breaking the treatment window into 10-day intervals, the largest negative effect is observed for Dec. 21–31 (up to –0.0910; -0.0842 in our preferred specification with week of birth fixed effects), confirming that the school performance drop is highly concentrated in the group most likely to have shifted into the earlier cohort. While the average impact (-0.08 standard deviations or SDs) may seem modest in isolation, given that only 7.4% of the births in the Dec. 21–31 window were affected (i.e., moved forward from January into December), our estimates suggest that the children whose births were shifted forward to just before the December 31 cutoff (and who therefore started school a year earlier than they otherwise would have) scored, on average, 1.13 SDs lower on second-grade tests than they would have if born in January. This loss of 1.13 SDs in second-grade test scores corresponds to a developmental delay of approximately 37 months (the "effect" of being a month older is -0.03), in terms of cognitive test performance at age 7. In other words, these children perform like peers who are over three years younger, simply because they were pushed into school too early due to the policy.

Table 4 explores whether the observed learning gap is concentrated in particular school subjects. The outcomes are mapped to a 1–4 scale and cover nine subjects, including core academic areas, languages, and physical/behavioral domains. The Dec. 21–31 cohort shows consistently negative and significant coefficients in core/cognitively demanding subjects: Mathematics (-0.0824***), Catalan (–0.0725**), English and Natural Environment (–0.0571* and -0.0646** respectively). On the other hand, for Physical Education, and for Social and Civic Values, we don't see these "grade effects" (possibly suggesting that the observed effects are not driven by size/physical health or behavioral outcomes). In contrast, children born in adjacent 10-day windows (Dec. 11–20 or Jan. 1–10) do not exhibit significant differences, again confirming that the effect is specific to those affected by the policy-induced early school entry. We see (Table A7) these consistent patterns when we look at subject-wise binary outcomes where the outcome variables are indicators (columns 2-4) for receiving the highest grade, receiving one of the two highest grades, and passing the exam (equals 0 for children who failed and 1 for everyone else). Lastly, we also find that the treated children (the Dec. 21–31 cohort) were no more or less likely to attend private schools (Table A6.2).

5. Mechanisms

Our findings suggest that early elective deliveries, even when resulting in births within the "normal" gestational range, have tangible negative consequences for children's development. We consider potential mechanisms through which these effects operate.

One key mechanism appears to be ***worse neonatal health***. As shown in Section 4, the "treated" group experienced shorter gestation and lower birth weight. This was followed by a higher incidence of respiratory problems in the first year of life. While these acute health issues may have resolved, they could signify a period of increased vulnerability and stress on the developing infant, potentially impacting neurodevelopment. Although we do not detect persistent health effects in later childhood, the initial health challenges could have enduring, subtle effects on cognitive pathways.

Another potential mechanism relates to ***parental compensating investments***. One might hypothesize that parents of children born slightly earlier due to elective deliveries might make greater investments to compensate for potential developmental disadvantages. Our preliminary analysis shows no significant effect on private school attendance, suggesting that significant compensating educational investments are not systematically occurring at a level that would offset these effects. Any other compensating investments would tend to *improve* treated children's, so that they would tend to bias our estimates downwards. Along the same lines, note that the parents who shifted birth would have received the €2,500 benefit, which may have benefited children. However, Borra et al. (2025) exploit the introduction of the Spanish baby bonus and find no direct effects of receipt of the child benefit on children's health or education outcomes.

Finally, a direct impact on ***cognitive development due to neurodevelopmental immaturity at birth*** is a plausible candidate. Our analysis suggests that the performance deficits are significantly larger than what would be predicted by age at test alone. This indicates that the earlier birth, even within the "normal" range, may lead to subtle but meaningful differences in brain maturation and cognitive readiness, manifesting as lower grades. Our results suggest that even minor reductions in gestational age can have subtle but pervasive effects on brain development and function, which become evident when children are challenged with complex cognitive tasks in a formal educational setting.

6. Conclusion

We leverage a unique natural experiment in Spain (the abrupt cancellation of a generous birth benefit) to provide causal evidence on the long-term educational costs of early elective deliveries. Our findings reveal that the financial incentives to deliver in late December 2010 led to a spike in births characterized by slightly shorter gestational periods and lower birth weights, though still within the clinically "normal" range.

We then show that these electively delivered children experienced an elevated risk of respiratory hospitalizations in their first year of life. More importantly, at age seven, these children exhibited significantly worse grades across all subjects in primary school. These educational deficits are not explained by being "young for grade" at school entry, suggesting a direct negative impact on cognitive development linked to neurodevelopmental immaturity at birth.

Our study makes several important contributions to the literature on early-life human capital formation. We provide causal evidence on the detrimental effects of mild gestational age reductions, and we trace a path from policy-induced elective birth timing to subtle neonatal health vulnerabilities and, subsequently, to observable long-term educational disadvantages.

The implications of our findings are significant for public health and obstetric practice. They underscore the potential unseen costs associated with non-medically indicated early elective deliveries. While often perceived as benign, particularly when resulting in full-term infants, our research suggests that even small shifts in birth timing can have meaningful and lasting consequences for children's cognitive development and educational trajectories. This highlights the importance of careful consideration and adherence to medical indications when timing childbirth. Future research could delve deeper into the specific neurocognitive pathways affected by these subtle gestational age differences and explore potential interventions to mitigate these developmental risks.

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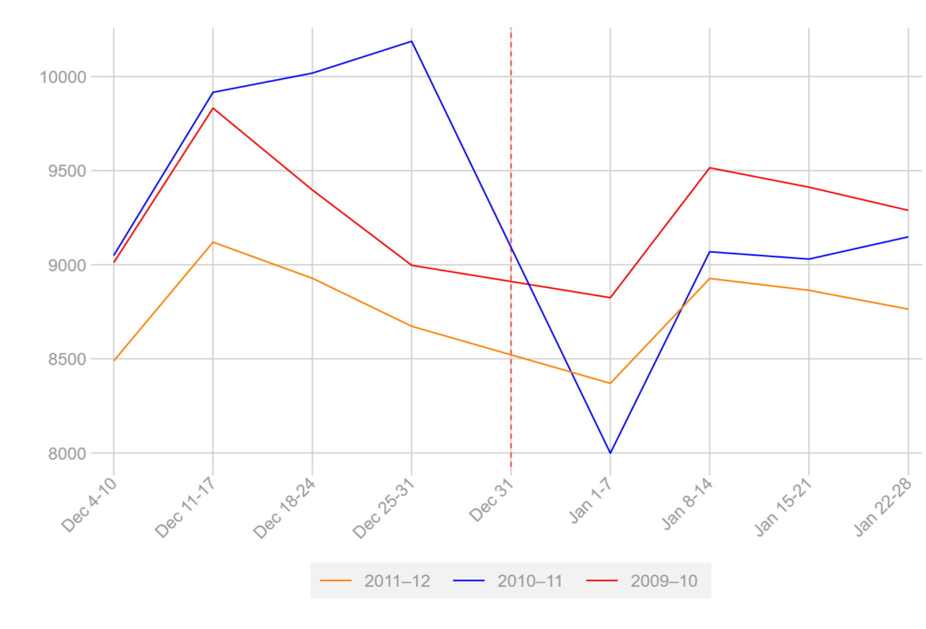
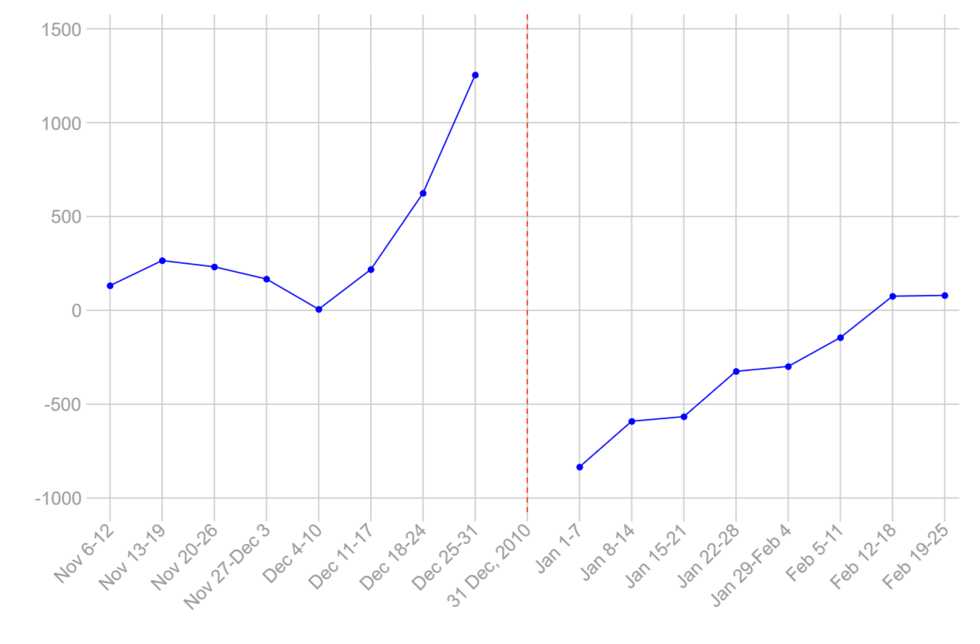
Figure 1. Birth timing

Panel A. Weekly number of births, December and January 2010-11 and the surrounding years



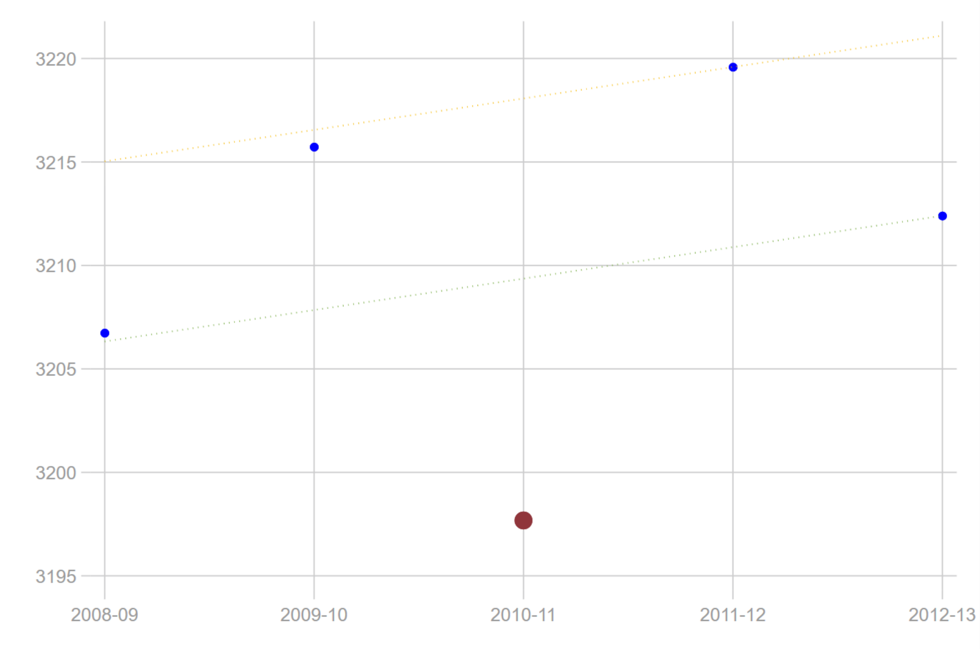
Panel B. Difference in weekly number of births, 2010-11 vs. three surrounding years



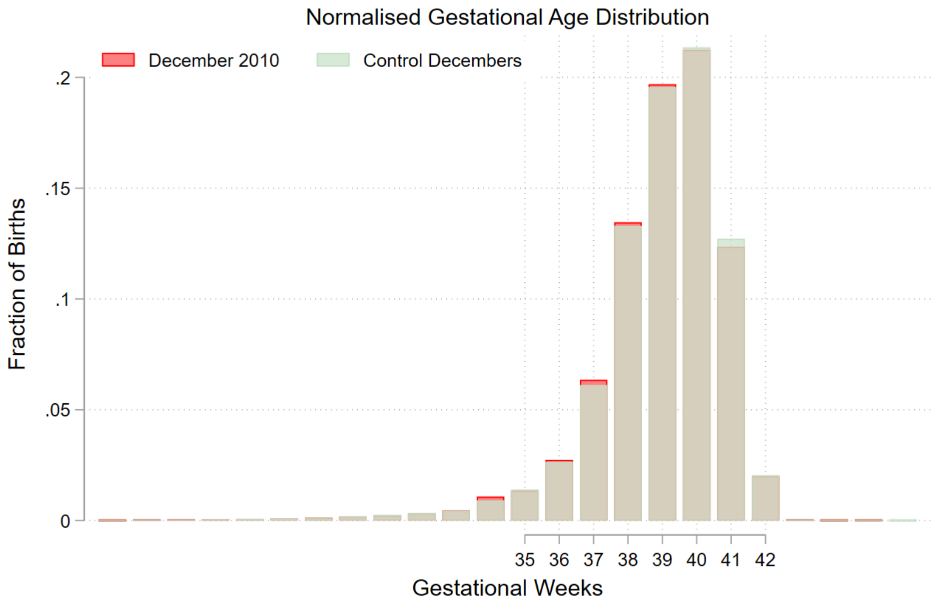
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013

Figure 2. Birthweight and gestation weeks

Panel A. Average birthweight in the one-week window around December 31, 2010-11 vs. the surrounding years



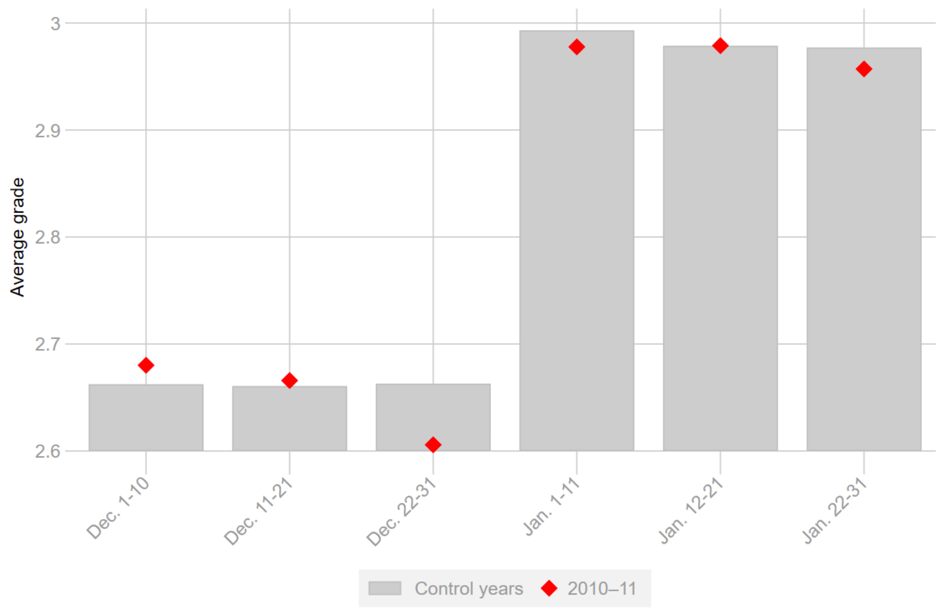
Panel B. Weeks of gestation (births in December, 2010 vs births in the control Decembers)



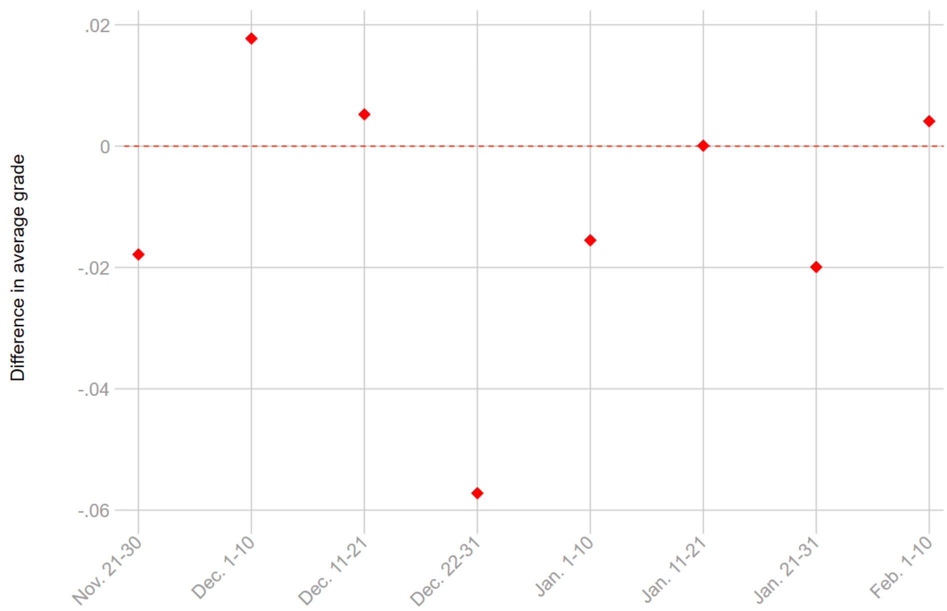
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013

Figure 3. School performance

Panel A. Average second grade test score by date of birth (2010-11 vs the surrounding control years)

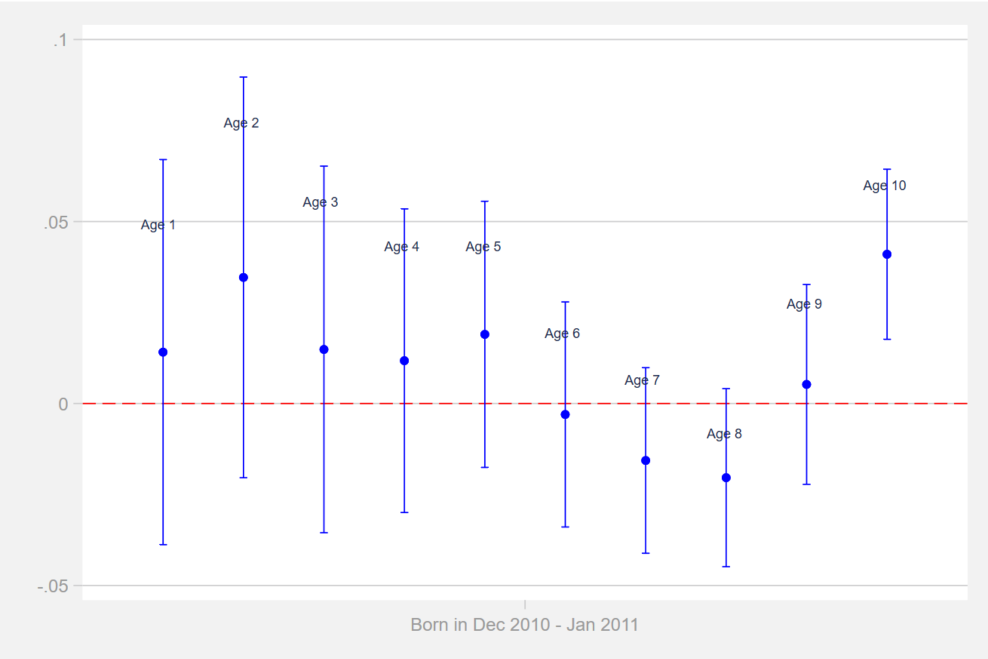


Panel B. Difference in average second grade test score by date of birth (2010-11 vs the surrounding control years)



Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia, Idescat

Figure 4: Respiratory diagnoses by age in years (Primary Healthcare)



Note: Difference-in-difference estimation results for children in the BIFAP database born in the last week of October, December, and February and the first week of November, January, and March, for birth cohorts spanning October 2008–March 2009 to October 2012–March 2013. Each observation corresponds to a child born on day d in year t. We plot estimated coefficients on a December 2010–January 2011 dummy (capturing the weeks surrounding the benefit cancellation date, i.e., 31 December 2010) from equation (2). The outcome is the number of times the child was diagnosed with a respiratory disorder between ages 1 and 10. The figure presents coefficient estimates along with 95% confidence intervals. Robust standard errors are used.
Source: Primary healthcare records for children born in 2008-2013, BIFAP

Table 1: The effect of benefit cancellation on the timing of births (Spain)

Panel A: Number of births

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec, 2010	106.3***	122.5***	143.8***	141.9**	172.6**
	(24.87)	(31.18)	(40.22)	(56.72)	(64.56)
Mean of outcome	1296	1299	1283	1250	1242

Panel B: ln(Number of births)

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec, 2010	0.0782***	0.0878***	0.102***	0.107**	0.123**
	(0.0179)	(0.0222)	(0.0290)	(0.0418)	(0.0471)
Year Dummies	Yes	Yes	Yes	Yes	Yes
Day of week dummies	Yes	Yes	Yes	Yes	Yes
Holiday dummy	Yes	Yes	Yes	Yes	Yes
Day of year dummy	Yes	Yes	Yes	Yes	Yes
N	310	210	140	100	70

Note: Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01. We report coefficients on a December 2010 dummy (the month right before benefit cancellation) from equation (1). An observation is a day. The dependent variables are the daily number of births (Panel A) and the log of daily number of births (Panel B) in Spain, and the sample includes all children born in the last 7 days, last 10 days, last 14 days, last 20 days, or the full month of December, and the first 7 days, first 10 days, first 14 days, first 20 days, or the full month of January(depending on the column), for December-January pairs from 2008-09 to 2012-2013.

Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013

Table 2: The effect of benefit cancellation on birth-weight and weeks of gestation (Spain)

Panel A: Birth weight: Full sample

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec'10-Jan'11 born	-6.075**	-8.833***	-15.12***	-20.24***	-21.04***
	(2.691)	(3.265)	(3.991)	(4.833)	(5.709)
Mean of outcome	3212.01	3211.30	3210.54	3211.10	3210.71
N	1127116	760456	517138	349128	255103

Panel B: Weeks of gestation

Dec'10-Jan'11 born	-0.0331***	-0.0370***	-0.0494***	-0.0595***	-0.0621***
	(0.0107)	(0.0129)	(0.0159)	(0.0193)	(0.0229)
Mean of outcome	38.97	38.97	38.96	38.96	38.96
N	948171	639352	434930	293513	214674

Panel C: Birth weight < 2,500 g

Dec'10-Jan'11 born	0.00135	0.00233	0.00330	0.00472*	0.00461
	(0.00136)	(0.00165)	(0.00203)	(0.00246)	(0.00290)
Mean of outcome	0.08	0.08	0.08	0.08	0.08
N	1127116	760456	517138	349128	255103

Panel D: Birth weight: Boys

Dec'10-Jan'11 born	-5.992	-6.819	-14.62***	-21.15***	-20.16**
	(3.831)	(4.644)	(5.676)	(6.884)	(8.136)
Mean of outcome	3269.52	3268.84	3267.58	3268.40	3268.00
N	579996	391109	266017	179440	130979

Panel E: Birth weight: Girls

Dec'10-Jan'11 born	-6.188*	-11.33**	-15.84***	-19.88***	-22.31***
	(3.725)	(4.525)	(5.535)	(6.690)	(7.891)
Mean of outcome	3151.04	3150.36	3150.11	3150.50	3150.27
N	547120	369347	251121	169688	124124

Note: Difference-in-difference estimation results for the sample of children born in Spain between October and March of the years 2008-09 to 2012-13. The reported coefficients correspond to a dummy for December 2010-January 2011 (the weeks surrounding the benefit cancellation date of 31 December 2010), as estimated in equation (2). Each observation is a child born on day d in year t. The dependent variables are: Panel A: Birth weight in grams (full sample), Panel B: Weeks of gestation, Panel C: Indicator for low birth weight (below 2,500g), Panel D: Birth weight in grams for boys, and Panel E: Birth weight in grams for girls. The sample includes all children born in the last 7, 10, 14, or 20 days or the full months of October, December and February, and the first 7, 10, 14, or 20 days or the full months of January, November and March (depending on the column). Robust standard errors are shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.
Source: Birth certificate micro data, Spanish National Statistical Institute (INE), 2008-2013.

Table 3: Effects on average Second grade test scores (standardised)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
December 2010	-0.0320*		-0.0279		-0.0321*		-0.0276	
	(0.0188)		(0.0203)		(0.0188)		(0.0203)	
January 2011	-0.0237		-0.0192		-0.0221		-0.0184	
	(0.0207)		(0.0221)		(0.0207)		(0.0221)	
Dec. 21-31, 2010		-0.0910***		-0.0869***		-0.0760**		-0.0842**
		(0.0301)		(0.0311)		(0.0302)		(0.0334)
Dec. 11-20, 2010		-0.00129		0.00282		-0.00167		0.0144
		(0.0310)		(0.0319)		(0.0310)		(0.0337)
Dec. 1-10, 2010		-0.00298		0.00112		-0.0188		-0.0145
		(0.0321)		(0.0330)		(0.0321)		(0.0351)
Jan. 1-10, 2011		0.0147		0.0193		0.000374		-0.00936
		(0.0353)		(0.0362)		(0.0353)		(0.0383)
Jan. 11-20, 2011		-0.0214		-0.0168		-0.0203		-0.0106
		(0.0335)		(0.0344)		(0.0335)		(0.0362)
Jan. 21-31, 2011		-0.0602*		-0.0557		-0.0437		-0.0348
		(0.0344)		(0.0353)		(0.0344)		(0.0374)
Controls	Y	Y	Y	Y	Y	Y	Y	Y
Month (linear)	Y	Y	N	N	N	N	N	N
Month fixed effects	N	N	Y	Y	N	N	N	N
Days (linear)	N	N	N	N	Y	Y	N	N
Day fixed effects	N	N	N	N	N	N	Y	Y

Note: The dependent variable is the average second grade test score (standardised grade) of child i, born in month m of year t. Columns 1–2 include a linear trend in month of birth; columns 3–4 include month-of-birth fixed effects; columns 5–6 include a linear trend in week of birth; and columns 7–8 include week-of-birth fixed effects. 'Treated' indicators denote whether a child was born in December 2010 or January 2011, with finer event-time dummies (by 10-day intervals) included in even-numbered columns. All models control for year-of-birth fixed effects, child's gender, municipality population size, and parental background, including educational attainment, country of birth, and indicators for missing values on parental characteristics. Robust standard errors are reported. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia, Idescat

Table 4: Effects on second-grade test scores by subject

	(1) Natural Env.	(2) Social & Cult. Env.	(3) Artistic Educ.	(4) Physical Educ.	(5) English	(6) Catalan	(7) Spanish	(8) Mathematics	(9) Social & Civic Values
Dec. 21-31, 2010	-0.0646**	-0.0531*	-0.0562**	-0.0356	-0.0571*	-0.0725**	-0.0496*	-0.0824***	0.00569
	(0.0270)	(0.0276)	(0.0255)	(0.0259)	(0.0307)	(0.0295)	(0.0295)	(0.0310)	(0.0295)
Dec. 11-20, 2010	0.0138	0.00587	0.00384	-0.0310	0.0223	0.0277	0.0157	0.0277	-0.00918
	(0.0275)	(0.0279)	(0.0248)	(0.0243)	(0.0310)	(0.0303)	(0.0292)	(0.0321)	(0.0285)
Dec. 1-10, 2010	-0.00683	0.00177	-0.0107	-0.00455	-0.00643	-0.0273	-0.0386	0.0373	-0.0440
	(0.0285)	(0.0283)	(0.0258)	(0.0261)	(0.0314)	(0.0313)	(0.0294)	(0.0331)	(0.0306)
Jan. 1-10, 2011	-0.0215	-0.00684	-0.0129	-0.0144	-0.00279	-0.0161	0.00788	0.0161	-0.0145
	(0.0315)	(0.0310)	(0.0278)	(0.0289)	(0.0341)	(0.0334)	(0.0328)	(0.0358)	(0.0328)
Jan. 11-20, 2011	-0.0161	-0.0156	-0.0110	0.0207	0.0396	-0.0337	-0.00940	-0.00263	-0.0544*
	(0.0283)	(0.0284)	(0.0257)	(0.0268)	(0.0321)	(0.0316)	(0.0307)	(0.0326)	(0.0309)
Jan. 21-31, 2011	0.00531	-0.0131	-0.0190	-0.0283	-0.0304	-0.0458	-0.0205	-0.0106	-0.0771**
	(0.0295)	(0.0293)	(0.0275)	(0.0278)	(0.0328)	(0.0333)	(0.0320)	(0.0341)	(0.0313)
Mean of outcome	2.8962	2.8828	2.9122	2.9525	2.7791	2.6183	2.6872	2.7406	2.9489
N	156199	156198	156241	156243	155187	156145	156160	156176	131610

Note: Estimation results for the sample of children born in 2008–2013, and their test scores in the second grade across nine subjects. These outcomes are measured for students enrolled in Catalan primary schools. We report coefficients on dummies for 10-day intervals from December 1, 2010 to January 31, 2011 (the weeks around the benefit cancellation date, i.e., December 31, 2010). An observation is a child born on day d in year t. The dependent variables are subject-specific grades mapped to a 1–4 scale as follows: 9–10 (assessed in 2015–2016) or AE (Assoliment excellent, assessed in 2016–2017 onwards): 4; 7–8 or AN (Assoliment notable): 3; 5–6 or AS (Assoliment suficient): 2; and <5 or NA: 1. We include week of birth fixed effects, and control for the child's sex, parental education (Secondary education or above, Bachelor's degree or higher, and Master's degree or higher), parental nationality (Spanish or non-Spanish), the socio-economic index of the municipality where the child resided in the evaluation year, and the municipality's population size. Robust standard errors are shown in parentheses. Significance levels: * p < .10, ** p < .05, *** p < .01.

Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia, Idescat

Table 5: Diagnosed with Respiratory Disorders (Primary Healthcare)

Panel A: Diagnosed (Respiratory disorders in years 1-10)

	(1) Full months	(2) ±28 days	(3) ±21days	(4) ±14 days	(5) ±7 days
Dec'10-Jan'11 born	-0.0189	-0.0190	0.00487	0.0704	0.102
	(0.0581)	(0.0609)	(0.0701)	(0.0862)	(0.122)
Mean of outcome	6.7796	6.7855	6.7836	6.7687	6.7421

Panel B: Diagnosed (Respiratory disorders in year 1)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.00658	-0.00319	-0.00806	0.0117	0.0141
	(0.0128)	(0.0134)	(0.0154)	(0.0190)	(0.0270)
Mean of outcome	1.2143	1.2148	1.2145	1.2120	1.2128

Panel C: Diagnosed (Bronchitis in years 1-10)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.0133	0.0152	0.0164	0.0321**	0.0348
	(0.0107)	(0.0112)	(0.0131)	(0.0161)	(0.0220)
Mean of outcome	0.5458	0.5467	0.5480	0.5455	0.5452

Panel D: Diagnosed (Bronchitis in year 1)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.00213	0.00372	0.00262	0.00678*	0.00245
	(0.00270)	(0.00283)	(0.00327)	(0.00403)	(0.00559)
Mean of outcome	0.0804	0.0806	0.0805	0.0808	0.0802

Panel E: Diagnosed (Bronchiolitis in years 1-10)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.000214	0.000707	0.00786*	0.00863*	0.0110
	(0.00345)	(0.00359)	(0.00414)	(0.00504)	(0.00725)
Mean of outcome	0.1674	0.1669	0.1663	0.1657	0.1652

Panel F: Diagnosed (Bronchiolitis in year 1)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.00118	0.00103	0.00660**	0.00797**	0.0113**
	(0.00266)	(0.00276)	(0.00320)	(0.00390)	(0.00567)
Mean of outcome	0.1156	0.1153	0.1150	0.1151	0.1156

Difference-in-difference estimation results for the sample of children in the BIFAP database born between October 2008 and March 2013, and diagnosed with respiratory conditions overall (Panel A) and by specific diagnoses and age (Panels B–F). We report coefficients on a December 2010–January 2011 dummy (the weeks around the benefit cancellation date, i.e., 31 Dec 2010) from equation (2). An observation is a child born on day *d* in year *t*. The dependent variables are the number of times the child was diagnosed with: respiratory disorders from age 1–10 years (Panel A), respiratory disorders in year 1 (Panel B), bronchitis from age 1–10 years (Panel C), bronchitis in year 1 (Panel D), bronchiolitis from age 1–10 years (Panel E), and bronchiolitis in year 1 (Panel F). The sample includes all babies in the BIFAP database born in the last 1–4 weeks of October, December, and February and the first 1–4 weeks of November, January, and March (depending on the column), for October–March sextuplets from 2008–09 to 2012–13. Robust standard errors are shown in parentheses. Significance levels are indicated by $p < .1$, ** < .05, *** < .01.
Source: BIFAP database. All diagnoses are included.

Table 6: The effect of benefit cancellation on hospitalisations: overall and respiratory disorders (Spain)

Panel A: Hospitalised: All disorders

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec'10-Jan'11 born	-0.00669***	-0.00784***	-0.00548**	-0.00698**	-0.00915**
	(0.00182)	(0.00222)	(0.00269)	(0.00324)	(0.00381)
Mean of outcome	0.1781	0.1792	0.1786	0.1781	0.1777

Panel B: Hospitalised: Respiratory disorders

Dec'10-Jan'11 born	0.0000223	-0.000463	0.000183	-0.00169	-0.00474**
	(0.00105)	(0.00128)	(0.00155)	(0.00187)	(0.00217)
Mean of outcome	0.0507	0.0513	0.0511	0.0510	0.0506
N	1190013	802900	546104	368720	269507

Note: Difference-in-difference estimation results for the sample of children born in Spain in October 2008-March 2009, October 2009-March 2010, October 2010-March 2011, October 2011-March 2012, and October 2012-March 2013, and hospitalised for all disorders (Panel A) and respiratory disorders (Panel B) in 2015-2020. We report coefficients on a December, 2010-January, 2011 dummy (the weeks around the benefit cancellation date, i.e., 31 Dec, 2010) from equation (2). An observation is a child born on day d in year t. The dependent variable is an indicator variable which takes the value of 1 if the child was hospitalised for all disorders (Panel A) and respiratory disorders (Panel B), and 0 otherwise, and the sample includes all children born in the last 7 days, last 10 days, last 14 days, last 20 days, or the full month of December, and the first 7 days, first 10 days, first 14 days, first 20 days, or the full months of November, January, and March (depending on the column), for October-November-December-January-February-March sextuplets from 2008-09 to 2012-2013. Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.

Source: Hospital Morbidity Survey 2015-2020. All recorded hospitals stays and diagnoses are included

Table 7: The effect of benefit cancellation on hospitalisations: overall and respiratory disorders (Catalonia)

Panel A: Hospitalised: All disorders

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec10Jan11	0.00540	0.00649	0.00779	0.00707	0.00518
	(0.00424)	(0.00518)	(0.00627)	(0.00764)	(0.00901)
Mean of outcome	0.1703	0.1723	0.1726	0.1707	0.1693

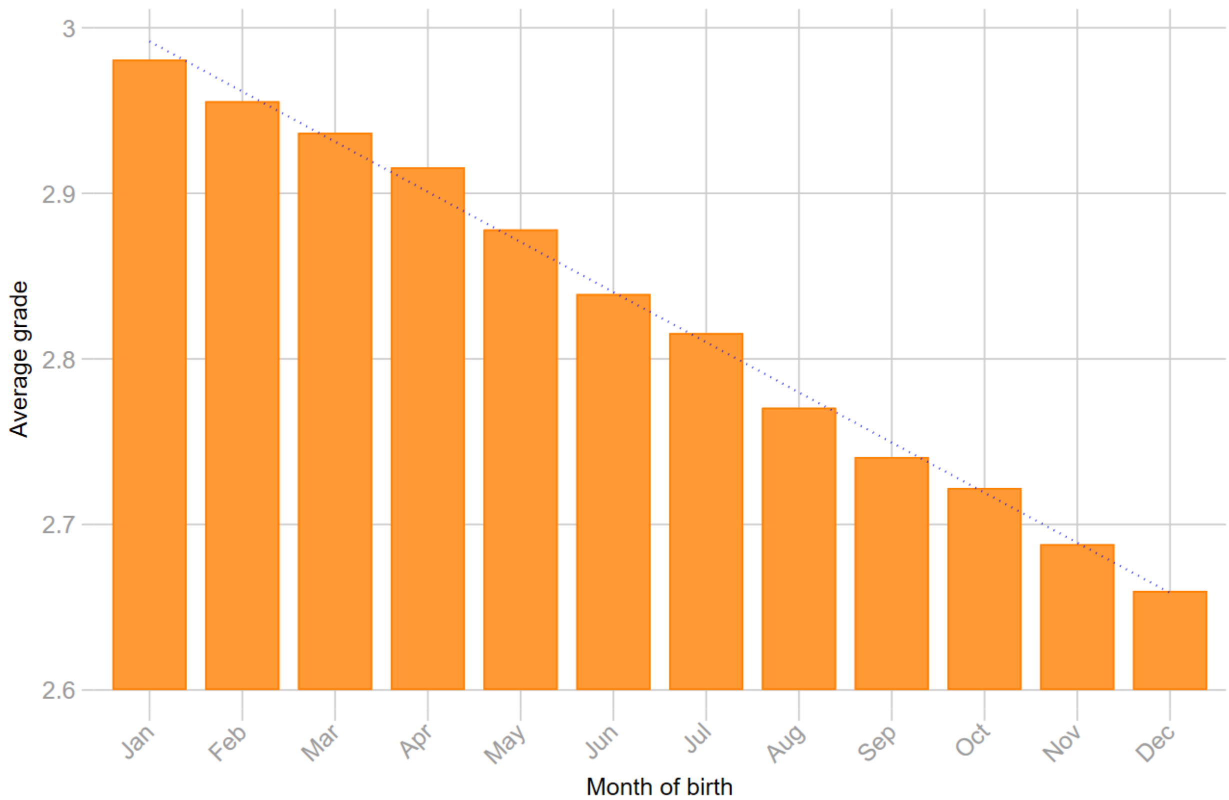
Panel B: Hospitalised: Respiratory disorders

Dec'10-Jan'11 born	0.00508**	0.00659**	0.00704*	0.00523	0.00219
	(0.00243)	(0.00298)	(0.00361)	(0.00443)	(0.00520)
Mean of outcome	0.0492	0.0502	0.0496	0.0494	0.0494
N	206310	138973	94494	63572	46488

Note: Difference-in-difference estimation results for the sample of children born in the autonomous community of Catalonia in October 2008-March 2009, October 2009-March 2010, October 2010-March 2011, October 2011-March 2012, and October 2012-March 2013, and hospitalised for all disorders (Panel A) and respiratory disorders (Panel B) in 2015-2020. We report coefficients on a December, 2010-January, 2011 dummy (the weeks around the benefit cancellation date, i.e., 31 Dec, 2010) from equation (2). An observation is a child born on day d in year t. The dependent variable is an indicator variable which takes the value of 1 if the child was hospitalised for all disorders (Panel A) and respiratory disorders (Panel B), and 0 otherwise, and the sample includes all children born in the last 7 days, last 10 days, last 14 days, last 20 days, or the full month of December, and the first 7 days, first 10 days, first 14 days, first 20 days, or the full months of November, January, and March (depending on the column), for October-November-December-January-February-March sextuplets from 2008-09 to 2012-2013. Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.
Source: Hospital Morbidity Survey 2015-2020. All recorded hospitals stays and diagnoses are included

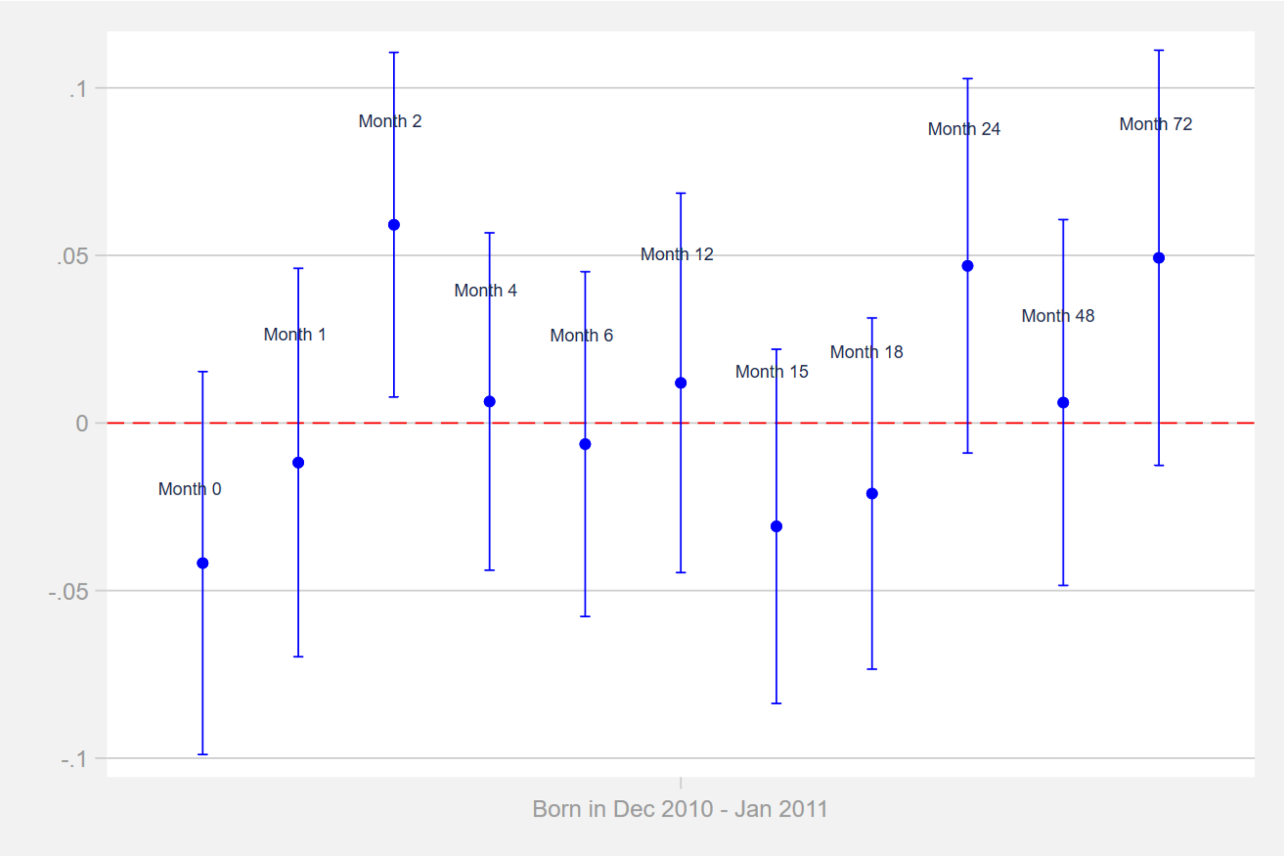
Appendix

Figure A1: Average second-grade test score by month of birth



Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia, Idescat

Figure A2: BMI (standardised) by age in months



Note: Difference-in-difference estimation results for children in the BIFAP database born in the last week of October, December, and February and the first week of November, January, and March, for birth cohorts spanning October 2008–March 2009 to October 2012–March 2013. Each observation corresponds to a child born on day d in year t. We plot estimated coefficients on a December 2010–January 2011 dummy (capturing the weeks surrounding the benefit cancellation date, i.e., 31 December 2010) from equation (2). The outcome is standardized BMI by age in months. The figure presents coefficient estimates along with 95% confidence intervals. Robust standard errors are used.

Source: Primary healthcare records for children born in 2008-2013, BIFAP

Table A1.1: The effect of benefit cancellation on the timing of births (Catalonia)

Panel A: Number of births

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec, 2010	23.73***	28.43***	35.25***	32.78**	36.69**
	(5.591)	(7.164)	(9.850)	(13.12)	(15.33)
Mean of outcome	224	225	221	215	213

Panel B: ln(Number of births)

Dec, 2010	0.106***	0.123***	0.151***	0.148**	0.151**
	(0.0242)	(0.0309)	(0.0428)	(0.0583)	(0.0677)
Year Dummies	Yes	Yes	Yes	Yes	Yes
Day of week dummies	Yes	Yes	Yes	Yes	Yes
Holiday dummy	Yes	Yes	Yes	Yes	Yes
Day of year dummy	Yes	Yes	Yes	Yes	Yes
N	310	210	140	100	70

Note: Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01. We report coefficients on a December 2010 dummy (the month right before benefit cancellation) from equation (1). An observation is a day. The dependent variables are the daily number of births (Panel A) and the log of daily number of births (Panel B) in the autonomous community of Catalonia, and the sample includes all children born in the last 7 days, last 10 days, last 14 days, last 20 days, or the full month of December, and the first 7 days, first 10 days, first 14 days, first 20 days, or the full month of January(depending on the column), for December-January pairs from 2008-09 to 2012-2013.

Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013

Table A1.2: The effect of benefit cancellation on the timing of births (BIFAP CCAAs)

Panel A: Number of births

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec, 2010	44.13***	52.54***	61.19***	65.70**	79.52**
	(14.65)	(17.04)	(21.88)	(29.77)	(33.17)
Mean of outcome	663	665	655	638	636

Panel B: ln(Number of births)

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec, 2010	0.0632***	0.0742***	0.0868***	0.1000**	0.114**
	(0.0207)	(0.0241)	(0.0311)	(0.0438)	(0.0478)
Year Dummies	Yes	Yes	Yes	Yes	Yes
Day of week dummies	Yes	Yes	Yes	Yes	Yes
Holiday dummy	Yes	Yes	Yes	Yes	Yes
Day of year dummy	Yes	Yes	Yes	Yes	Yes
N	310	210	140	100	70

Note: Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01. We report coefficients on a December 2010 dummy (the month right before benefit cancellation) from equation (1). An observation is a day. The dependent variables are the daily number of births (Panel A) and the log of daily number of births (Panel B) in the autonomous communities of Aragón, Canarias, Cantabria, Castilla-La Mancha, Castilla y León, Comunidad Valenciana, Extremadura, La Rioja, Madrid, Murcia, Navarra, and Principado de Asturias, and the sample includes all children born in the last 7 days, last 10 days, last 14 days, last 20 days, or the full month of December, and the first 7 days, first 10 days, first 14 days, first 20 days, or the full month of January(depending on the column), for December-January pairs from 2008-09 to 2012-2013.

Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013

Table A2: The effect of benefit cancellation on the timing of births (Spain; robustness check using alternate control years)

Panel A: Number of births

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec, 2010	97.51***	119.8***	143.8***	152.9***	187.5***
	(23.77)	(29.56)	(37.42)	(51.64)	(56.27)
Mean of outcome	1306	1309	1295	1261	1253

Panel B: ln(Number of births)

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec, 2010	0.0712***	0.0858***	0.102***	0.113***	0.133***
	(0.0167)	(0.0206)	(0.0262)	(0.0369)	(0.0392)
Year Dummies	Yes	Yes	Yes	Yes	Yes
Day of week dummies	Yes	Yes	Yes	Yes	Yes
Holiday dummy	Yes	Yes	Yes	Yes	Yes
Day of year dummy	Yes	Yes	Yes	Yes	Yes
N	434	294	196	140	98

Note: Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01. We report coefficients on a December 2010 dummy (the month right before benefit cancellation) from equation (1). An observation is a day. The dependent variables are the daily number of births (Panel A) and the log of daily number of births (Panel B) in Spain, and the sample includes all children born in the last 7 days, last 10 days, last 14 days, last 20 days, or the full month of December, and the first 7 days, first 10 days, first 14 days, first 20 days, or the full month of January(depending on the column), for December-January pairs from 2006-07 to 2012-2013.
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2006-2013

Table A3: Selection into birth (observable parental and geographic characteristics)

	(1) Mother is married	(2) Mother is married	(3) Mother's age	(4) Mother's age	(5) Father's age	(6) Father's age	(7) Spanish Mother	(8) Spanish Mother	(9) Spanish Father	(10) Spanish Father
Dec., 2010	-0.00376		0.0970***		0.0697**		0.00959***		0.00682***	
	[0.00282]		[0.0315]		[0.0344]		[0.00233]		[0.00225]	
Jan., 2011	0.00333		-0.119***		-0.0988***		-0.00866***		-0.0103***	
	[0.00294]		[0.0327]		[0.0361]		[0.00245]		[0.00237]	
Dec. 21-31, 2010		-0.00316		0.165***		0.0621		0.0108***		0.00723*
		[0.00465]		[0.0521]		[0.0569]		[0.00383]		[0.00371]
Dec. 11-20, 2010		-0.00704		0.0414		0.0926*		0.00979***		0.00911***
		[0.00441]		[0.0494]		[0.0540]		[0.00362]		[0.00348]
Dec. 1-10, 2010		-0.000624		0.0900*		0.0512		0.00809**		0.00375
		[0.00473]		[0.0524]		[0.0575]		[0.00391]		[0.00379]
Jan. 1-10, 2011		-0.00419		-0.247***		-0.0976		-0.0169***		-0.0201***
		[0.00510]		[0.0569]		[0.0629]		[0.00434]		[0.00421]
Jan. 11-20, 2011		0.0115**		0.0128		-0.100*		-0.00312		-0.00520
		[0.00452]		[0.0505]		[0.0554]		[0.00374]		[0.00361]
Jan. 21-31, 2011		0.000741		-0.156***		-0.0982*		-0.00762*		-0.00735*
		[0.00484]		[0.0535]		[0.0594]		[0.00403]		[0.00389]
Week of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1417986	1417986	1416994	1416994	1388951	1388951	1417986	1417986	1417986	1417986

	(1) Mother graduate and above	(2) Mother graduate and above	(3) Father graduate and above	(4) Father graduate and above	(5) Mother Secondary Education and above (Completed Elementary Baccalaureate)	(6) Mother Secondary Education and above (Completed Elementary Baccalaureate)	(7) Mother Secondary Education and above (Completed Higher Baccalaureate)	(8) Mother Secondary Education and above (Completed Higher Baccalaureate)	(9) Father Secondary Education and above (Completed Elementary Baccalaureate)	(10) Father Secondary Education and above (Completed Elementary Baccalaureate)	(11) Father Secondary Education and above (Completed Higher Baccalaureate)	(12) Father Secondary Education and above (Completed Higher Baccalaureate)
Dec., 2010	0.0101***		0.00226		0.00695***		0.0112***		-0.00542**		0.000915	
	[0.00272]		[0.00244]		[0.00219]		[0.00283]		[0.00234]		[0.00288]	
Jan., 2011	0.00183		-0.00408		0.00343		-0.000465		-0.0126***		-0.0129***	
	[0.00284]		[0.00254]		[0.00231]		[0.00296]		[0.00246]		[0.00300]	
Dec. 21-31, 2010		0.0120***		0.00395		0.0117***		0.0148***		-0.00179		0.00151
		[0.00448]		[0.00402]		[0.00360]		[0.00467]		[0.00385]		[0.00474]
Dec. 11-20, 2010		0.00802*		-0.00169		0.000619		0.00877**		-0.0106***		-0.00750*
		[0.00428]		[0.00384]		[0.00344]		[0.00443]		[0.00367]		[0.00451]
Dec. 1-10, 2010		0.0106**		0.00503		0.00929**		0.0104**		-0.00321		0.01000**
		[0.00456]		[0.00408]		[0.00368]		[0.00475]		[0.00393]		[0.00482]
Jan. 1-10, 2011		-0.00585		-0.00822*		-0.00280		-0.0169***		-0.0169***		-0.0282***
		[0.00486]		[0.00432]		[0.00408]		[0.00514]		[0.00433]		[0.00518]
Jan. 11-20, 2011		0.00951**		0.00521		0.00754**		0.0126***		-0.00921**		-0.00140
		[0.00441]		[0.00396]		[0.00352]		[0.00454]		[0.00376]		[0.00463]
Jan. 21-31, 2011		-0.0000741		-0.0111***		0.00434		-0.000619		-0.0125***		-0.0124**
		[0.00469]		[0.00416]		[0.00380]		[0.00488]		[0.00405]		[0.00495]
Week of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986

	(1) Girls	(2) Girls	(3) Municipality pop: 10,001 - 20,000	(4) Municipality pop: 10,001 - 20,000	(5) Municipality pop: 20,001 - 50,000	(6) Municipality pop: 20,001 - 50,000	(7) Municipality pop: 50,001 - 100,000	(8) Municipality pop: 50,001 - 100,000	(9) Municipality pop: More than 100,000	(10) Municipality pop: More than 100,000	(11) Provincial Capital	(12) Provincial Capital
Dec., 2010	0.00132		0.000772		-0.00236		0.000325		-0.000184		0.00515*	
	[0.00292]		[0.00170]		[0.00209]		[0.00187]		[0.00180]		[0.00284]	
Jan., 2011	-0.00288		0.000243		0.00190		-0.000103		0.000741		-0.00197	
	[0.00304]		[0.00180]		[0.00218]		[0.00193]		[0.00189]		[0.00295]	
Dec. 21-31, 2010		-0.00259		0.00176		-0.000699		-0.00291		0.0000690		0.00795*
		[0.00480]		[0.00281]		[0.00344]		[0.00307]		[0.00299]		[0.00468]
Dec. 11-20, 2010		-0.00143		-0.000571		-0.00698**		0.00162		0.00152		0.00869*
		[0.00457]		[0.00264]		[0.00325]		[0.00295]		[0.00282]		[0.00447]
Dec. 1-10, 2010		0.00858*		0.00130		0.00126		0.00220		-0.00242		-0.00186
		[0.00489]		[0.00286]		[0.00353]		[0.00313]		[0.00302]		[0.00475]
Jan. 1-10, 2011		-0.00387		0.000205		0.00302		0.00101		0.00304		-0.00866*
		[0.00525]		[0.00313]		[0.00380]		[0.00335]		[0.00329]		[0.00507]
Jan. 11-20, 2011		-0.00144		-0.00127		-0.000748		-0.00188		0.00239		0.00494
		[0.00469]		[0.00276]		[0.00334]		[0.00295]		[0.00292]		[0.00457]
Jan. 21-31, 2011		-0.00363		0.00203		0.00394		0.000941		-0.00325		-0.00387
		[0.00502]		[0.00299]		[0.00361]		[0.00319]		[0.00307]		[0.00487]
Week of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986	1417986

Note: This table tests for selection into birth based on observable parental and geographic characteristics around the benefit cancellation date of December 31, 2010. The coefficients correspond to dummies for week-of-birth intervals in December 2010 and January 2011, with children born in the same weeks of other years (2008-09 through 2012-13) as controls. The sample includes all children born in Spain between October and March of each year from 2008-09 to 2012-13.Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01. Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013

Table A4: The effect of benefit cancellation on birth-weight and weeks of gestation (Catalonia and BIFAP CCAAs)

Panel A: Birth weight: Full sample (Catalonia)

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
Dec'10-Jan'11 born	-11.45*	-9.755	-13.38	-29.46**	-40.90***
	(6.592)	(7.994)	(9.811)	(11.94)	(14.13)
Mean of outcome	3217.56	3215.88	3215.77	3216.20	3215.20
N	188254	126710	86094	57860	42261

Panel B: Weeks of gestation (Catalonia)

Dec'10-Jan'11 born	-0.0305	-0.0333	-0.0315	-0.0724	-0.105*
	(0.0255)	(0.0309)	(0.0380)	(0.0463)	(0.0556)
Mean of outcome	38.98	38.97	38.96	38.96	38.96
N	163260	109780	74682	50165	36619

Panel C: Birth weight < 2,500 g (Catalonia)

Dec'10-Jan'11 born	-0.00293	-0.00273	0.000700	0.00812	0.00862
	(0.00331)	(0.00401)	(0.00490)	(0.00595)	(0.00702)
Mean of outcome	0.08	0.08	0.08	0.08	0.08
N	188254	126710	86094	57860	42261

Panel D: Birth weight: Full sample (BIFAP CCAAs)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-5.734	-9.166**	-16.45***	-21.24***	-18.31**
	(3.765)	(4.574)	(5.594)	(6.793)	(8.034)
Mean of outcome	3196.98	3196.13	3195.21	3196.18	3195.15
N	575921	388556	264433	178676	130851

Panel E: Weeks of gestation (BIFAP CCAAs)

Dec'10-Jan'11 born	-0.0334**	-0.0391**	-0.0670***	-0.0747***	-0.0624*
	(0.0151)	(0.0184)	(0.0228)	(0.0278)	(0.0330)
Mean of outcome	38.93	38.92	38.92	38.92	38.92
N	479556	323349	220239	148812	109146

Panel F: Birth weight < 2,500 g (BIFAP CCAAs)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.00191	0.00277	0.00472	0.00658*	0.00597
	(0.00193)	(0.00235)	(0.00290)	(0.00352)	(0.00417)
Mean of outcome	0.08	0.08	0.08	0.08	0.09
N	575921	388556	264433	178676	130851

Note: Difference-in-difference estimation results for the sample of children born in Spain between October and March of the years 2008-09 to 2012-13. The reported coefficients correspond to a dummy for December 2010-January 2011 (the weeks surrounding the benefit cancellation date of 31 December 2010), as estimated in equation (2). Each observation is a child born on day d in year t. The dependent variables are: Panel A: Birth weight in grams (Catalonia), Panel B: Weeks of gestation (Catalonia), Panel C: Indicator for low birth weight (below 2,500g) (Catalonia), Panel D: Birth weight in grams (BIFAP CCAAs), Panel E: Weeks of gestation (BIFAP CCAAs), and Panel F: Indicator for low birth weight (below 2,500g) (BIFAP CCAAs). The sample includes all children born in the last 7, 10, 14, or 20 days or the full months of October, December, and February, and the first 7, 10, 14, or 20 days or the full months of January, November, and March (depending on the column). Robust standard errors are shown in parentheses. Significance levels are indicated by * < .1, ** < .05, *** < .01.
Source: Birth certificate micro data, Spanish National Statistical Institute (INE), 2008-2013

Table A5: Effects on other health outcomes (Spain)

Panel A: Delivery at health centres

	(1) Full months	(2) ± 20 days	(3) ± 14 days	(4) ± 10 days	(5) ± 7 days
Dec'10-Jan'11 born	0.000261	0.000681*	0.000628	0.000934*	0.000833
	(0.000300)	(0.000356)	(0.000446)	(0.000542)	(0.000648)
Mean of outcome	1.00	1.00	1.00	1.00	1.00
N	1185205	799663	543936	367253	268427

Panel B: Assisted birth (assisted by medical professionals)

Dec'10-Jan'11 born	-0.000166	-0.0000573	0.00000397	-0.0000871	-0.000168
	(0.000135)	(0.000166)	(0.000209)	(0.000267)	(0.000325)
Mean of outcome	1.00	1.00	1.00	1.00	1.00
N	1185521	799888	544096	367372	268509

Panel C: Delivery complications

Dec'10-Jan'11 born	0.00161	0.00308	0.00458*	0.00753***	0.00659*
	(0.00163)	(0.00198)	(0.00241)	(0.00292)	(0.00343)
Mean of outcome	0.15	0.15	0.15	0.15	0.15
N	1185521	799888	544096	367372	268509

Panel D: Firstborn

Dec'10-Jan'11 born	-0.00273	-0.00209	-0.00360	0.00493	0.000921
	(0.00240)	(0.00291)	(0.00355)	(0.00429)	(0.00507)
Mean of outcome	0.54	0.54	0.54	0.55	0.54
N	1185521	799888	544096	367372	268509

Panel E: Caesarean birth

Dec'10-Jan'11 born	0.000285	0.00243	0.00133	-0.000140	0.00618
	(0.00212)	(0.00258)	(0.00314)	(0.00378)	(0.00446)
Mean of outcome	0.25	0.25	0.25	0.24	0.24
N	1185521	799888	544096	367372	268509

Panel F: Full-term pregnancy (37 weeks or more)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.00244*	-0.00263*	-0.00366*	-0.00636***	-0.00700**
	(0.00128)	(0.00156)	(0.00192)	(0.00233)	(0.00275)
Mean of outcome	0.92	0.92	0.92	0.92	0.92
N	1185521	799888	544096	367372	268509

Note: Difference-in-difference estimation results for the sample of children born in October 2008–March 2009, October 2009–March 2010, October 2010–March 2011, October 2011–March 2012, and October 2012–March 2013. We report coefficients on a December 2010–January 2011 dummy (the weeks around the benefit cancellation date, i.e., 31 December 2010) from equation (2). An observation corresponds to a child born on day d in year t. The dependent variables are as follows: Panels A–F report results for binary outcomes indicating whether the following conditions are met: delivery at a health centre, birth attended by medical professionals, presence of delivery complications, being a firstborn child, Caesarean delivery, and full-term pregnancy (defined as 37 or more weeks of gestation). The sample includes all births occurring in the last 1–4 weeks of October, December, and February, and the first 1–4 weeks of November, January, and March (depending on the specification), for October–November–December–January–February–March sextuplets from 2008–09 to 2012–2013. Robust standard errors are reported in parentheses. Significance levels are denoted as * < .1, ** < .05, *** < .01. Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008–2013.

Table A6.1: Selection effects (regression for covariates)

	(1) Parent 1 Master's and above	(2) Parent 1 Master's and above	(3) Parent 2 Master's and above	(4) Parent 2 Master's and above	(5) Parent 1 Graduate and above	(6) Parent 1 Graduate and above	(7) Parent 2 Graduate and above	(8) Parent 2 Graduate and above	(9) Parent 1 Secondary Education and above	(10) Parent 1 Secondary Education and above	(11) Parent 2 Secondary Education and above	(12) Parent 2 Secondary Education and above
Dec., 2010	0.00857**		0.00286		0.0118		0.00437		0.00434		0.00793	
	[0.00381]		[0.00368]		[0.00899]		[0.00874]		[0.00903]		[0.00855]	
Jan., 2011	0.00468		-0.00443		-0.0224**		-0.0201**		-0.0185*		-0.0196**	
	[0.00395]		[0.00374]		[0.00931]		[0.00906]		[0.00954]		[0.00896]	
Dec. 21-31, 2010		0.0193***		0.00678		0.00684		0.0145		-0.00412		0.00712
		[0.00702]		[0.00654]		[0.0153]		[0.0149]		[0.0156]		[0.0146]
Dec. 11-20, 2010		-0.00479		-0.00459		-0.00120		-0.0172		-0.000126		0.00227
		[0.00543]		[0.00545]		[0.0146]		[0.0143]		[0.0146]		[0.0139]
Dec. 1-10, 2010		0.0125*		0.00713		0.0311**		0.0179		0.0177		0.0149
		[0.00686]		[0.00665]		[0.0154]		[0.0149]		[0.0154]		[0.0146]
Jan. 1-10, 2011		0.00835		0.00684		-0.0242		-0.0227		-0.0320*		-0.0196
		[0.00715]		[0.00696]		[0.0162]		[0.0157]		[0.0170]		[0.0158]
Jan. 11-20, 2011		0.00784		-0.0111*		-0.0240		-0.00677		-0.0117		-0.0112
		[0.00665]		[0.00577]		[0.0151]		[0.0147]		[0.0153]		[0.0143]
Jan. 21-31, 2011		-0.00223		-0.00741		-0.0188		-0.0327**		-0.0135		-0.0289*
		[0.00619]		[0.00622]		[0.0158]		[0.0154]		[0.0161]		[0.0153]
Week of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	234031	234031	234031	234031	234031	234031	234031	234031	234031	234031	234031	234031

	(1) Parent 1 Spanish	(2) Parent 1 Spanish	(3) Parent 2 Spanish	(4) Parent 2 Spanish	(5) Girls	(6) Girls	(7) Municipality pop: 10,001 - 20,000	(8) Municipality pop: 10,001 - 20,000	(9) Municipality pop: 20,001 - 50,000	(10) Municipality pop: 20,001 - 50,000	(11) Municipality pop: 50,001 - 100,000	(12) Municipality pop: 50,001 - 100,000	(13) Municipality pop: More than 100,000	(14) Municipality pop: More than 100,000
Dec., 2010	0.00994		0.0124		0.00101		-0.00487		0.00105		0.00168		0.0116	
	[0.00759]		[0.00821]		[0.00970]		[0.00623]		[0.00750]		[0.00625]		[0.00947]	
Jan., 2011	-0.0182**		-0.0136		-0.0240**		0.00902		0.00686		-0.00234		-0.0112	
	[0.00817]		[0.00869]		[0.0102]		[0.00658]		[0.00800]		[0.00648]		[0.00989]	
Dec. 21-31, 2010		0.00965		0.0169		0.0221		0.000282		0.00289		-0.00533		0.0140
		[0.0130]		[0.0140]		[0.0166]		[0.0108]		[0.0129]		[0.0104]		[0.0162]
Dec. 11-20, 2010		0.0167		-0.00207		-0.0360**		-0.000978		-0.00909		0.0156		0.00824
		[0.0123]		[0.0136]		[0.0158]		[0.0102]		[0.0120]		[0.0105]		[0.0155]
Dec. 1-10, 2010		0.00291		0.0240*		0.0206		-0.0143		0.0104		-0.00658		0.0130
		[0.0130]		[0.0138]		[0.0165]		[0.0104]		[0.0130]		[0.0105]		[0.0161]
Jan. 1-10, 2011		-0.0208		-0.0212		-0.0367**		0.00586		0.0240*		-0.00745		-0.0153
		[0.0146]		[0.0157]		[0.0178]		[0.0115]		[0.0144]		[0.0112]		[0.0174]
Jan. 11-20, 2011		-0.0218*		-0.00744		-0.0317*		0.00740		0.00775		-0.00934		-0.00215
		[0.0131]		[0.0137]		[0.0163]		[0.0105]		[0.0130]		[0.0102]		[0.0159]
Jan. 21-31, 2011		-0.0118		-0.0133		-0.00386		0.0138		-0.0100		0.0102		-0.0175
		[0.0138]		[0.0148]		[0.0173]		[0.0113]		[0.0131]		[0.0114]		[0.0168]
Week of birth	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N	232140	232140	209111	209111	234031	234031	234031	234031	234031	234031	234031	234031	234031	234031

Note: This table presents regression results for selection into treatment around the benefit cancellation date (December 31, 2010). Each outcome corresponds to a pre-determined characteristic measured at baseline. Columns are grouped in pairs: the first of each pair shows estimates using linear treatment dummies for December 2010 and January 2011; the second includes 10-day interval event-study dummies. Outcomes include indicators for parental education, parental nationality (Spanish), child gender, and population size of the municipality of residence. All regressions include fixed effects for week and year of birth. Coefficients capture whether children born shortly before or after the cut-off differ systematically in baseline characteristics, which would suggest potential selection. Robust standard errors are in brackets. Significance levels: * $p < .10$, ** $p < .05$, *** $p < .01$. Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia, Idescat

Table A6.2: Attending private school

	(1)	(2)	(3)	(4)
December 2010	0.0260***	0.0196**		
	(0.00915)	(0.00863)		
January 2011	-0.0335***	-0.0248***		
	(0.00948)	(0.00886)		
Dec. 21-31, 2010			0.0193	0.0119
			(0.0154)	(0.0145)
Dec. 11-20, 2010			0.0352**	0.0334**
			(0.0151)	(0.0143)
Dec. 1-10, 2010			0.0226	0.0121
			(0.0156)	(0.0147)
Jan. 1-10, 2011			-0.0304*	-0.0214
			(0.0167)	(0.0159)
Jan. 11-20, 2011			-0.0224	-0.0169
			(0.0154)	(0.0142)
Jan. 21-31, 2011			-0.0487***	-0.0369**
			(0.0158)	(0.0148)
Controls	N	Y	N	Y
Week fixed effects	Y	Y	Y	Y
Observations	234031	233901	234031	233901

The dependent variable is a dummy equal to 1 if the child attends a private school, and 0 otherwise. Columns 1 and 3 report estimates without controls; columns 2 and 4 include the full set of controls. 'Treated' indicators denote whether the child was born in December 2010 or January 2011. Event-time dummies for 10-day intervals around the cut-off (Dec. 1, 2010 to Jan. 31, 2011) are included in columns 3–4. All models include fixed effects for week and year of birth. The full set of controls (in columns 2 and 4) includes child gender, parental education and nationality, missing indicators for background variables, and municipality population size. Robust standard errors are reported. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia, Idescat

Table A7: Effects on second grade test scores (binary grades by subjects)

Natural environment

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0646**	-0.0356***	-0.0307*	0.00168
	(0.0270)	(0.0133)	(0.0183)	(0.00670)
Dec. 11-20, 2010	0.0138	0.00540	0.0179	-0.00949
	(0.0275)	(0.0141)	(0.0176)	(0.00709)
Dec. 1-10, 2010	-0.00683	0.00405	-0.0124	0.00157
	(0.0285)	(0.0147)	(0.0186)	(0.00634)
Jan. 1-10, 2011	-0.0215	0.00227	-0.0178	-0.00596
	(0.0315)	(0.0191)	(0.0176)	(0.00634)
Jan. 11-20, 2011	-0.0161	-0.00572	-0.0103	-0.0000520
	(0.0283)	(0.0175)	(0.0163)	(0.00456)
Jan. 21-31, 2011	0.00531	0.00645	-0.00988	0.00874**
	(0.0295)	(0.0180)	(0.0172)	(0.00441)
Mean of outcome	2.8962	0.2397	0.6795	0.9770
N	156199	156199	156199	156199

Social and cultural environment

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0531*	-0.0186	-0.0340*	-0.000528
	(0.0276)	(0.0137)	(0.0183)	(0.00702)
Dec. 11-20, 2010	0.00587	0.000968	0.00955	-0.00465
	(0.0279)	(0.0140)	(0.0181)	(0.00679)
Dec. 1-10, 2010	0.00177	0.00936	-0.00686	-0.000723
	(0.0283)	(0.0145)	(0.0184)	(0.00670)
Jan. 1-10, 2011	-0.00684	0.00111	-0.00338	-0.00456
	(0.0310)	(0.0191)	(0.0175)	(0.00593)
Jan. 11-20, 2011	-0.0156	-0.00987	-0.00741	0.00165
	(0.0284)	(0.0175)	(0.0165)	(0.00441)
Jan. 21-31, 2011	-0.0131	-0.00116	-0.0173	0.00534
	(0.0293)	(0.0178)	(0.0173)	(0.00432)
Mean of outcome	2.8828	0.2334	0.6722	0.9772
N	156198	156198	156198	156198

Artistic education

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0562**	-0.0176	-0.0364**	-0.00215
	(0.0255)	(0.0136)	(0.0180)	(0.00387)
Dec. 11-20, 2010	0.00384	-0.0108	0.0147	-0.000000190
	(0.0248)	(0.0133)	(0.0175)	(0.00363)
Dec. 1-10, 2010	-0.0107	0.00618	-0.0177	0.000825
	(0.0258)	(0.0145)	(0.0177)	(0.00332)
Jan. 1-10, 2011	-0.0129	-0.00671	-0.00752	0.00132
	(0.0278)	(0.0178)	(0.0171)	(0.00339)
Jan. 11-20, 2011	-0.0110	-0.0109	-0.00349	0.00345
	(0.0257)	(0.0165)	(0.0159)	(0.00227)
Jan. 21-31, 2011	-0.0190	0.00454	-0.0235	0.00000135
	(0.0275)	(0.0175)	(0.0165)	(0.00336)
Mean of outcome	2.9122	0.2065	0.7129	0.9928
N	156241	156241	156241	156241

Physical education

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0356	-0.00777	-0.0296	0.00175
	(0.0259)	(0.0139)	(0.0181)	(0.00324)
Dec. 11-20, 2010	-0.0310	-0.0283**	-0.00305	0.000354
	(0.0243)	(0.0132)	(0.0173)	(0.00310)
Dec. 1-10, 2010	-0.00455	-0.00704	-0.00125	0.00374
	(0.0261)	(0.0145)	(0.0178)	(0.00313)
Jan. 1-10, 2011	-0.0144	-0.0109	-0.00559	0.00215
	(0.0289)	(0.0187)	(0.0169)	(0.00306)
Jan. 11-20, 2011	0.0207	0.0312*	-0.0135	0.00299*
	(0.0268)	(0.0177)	(0.0156)	(0.00180)
Jan. 21-31, 2011	-0.0283	-0.00269	-0.0247	-0.000918
	(0.0278)	(0.0177)	(0.0165)	(0.00301)
Mean of outcome	2.9525	0.2089	0.7503	0.9933
N	156243	156243	156243	156243

English as first foreign language

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0571*	-0.0185	-0.0368*	-0.00177
	(0.0307)	(0.0137)	(0.0189)	(0.0102)
Dec. 11-20, 2010	0.0223	0.00721	0.0108	0.00430
	(0.0310)	(0.0145)	(0.0186)	(0.00959)
Dec. 1-10, 2010	-0.00643	-0.0139	-0.00516	0.0126
	(0.0314)	(0.0147)	(0.0192)	(0.00946)
Jan. 1-10, 2011	-0.00279	-0.00493	-0.00510	0.00724
	(0.0341)	(0.0190)	(0.0193)	(0.00763)
Jan. 11-20, 2011	0.0396	0.0148	0.0214	0.00344
	(0.0321)	(0.0180)	(0.0177)	(0.00707)
Jan. 21-31, 2011	-0.0304	0.00151	-0.0373**	0.00534
	(0.0328)	(0.0179)	(0.0188)	(0.00710)
Mean of outcome	2.7791	0.2314	0.5964	0.9513
N	155187	155187	155187	155187

Catalan language and literature

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0725**	-0.0121	-0.0445**	-0.0159
	(0.0295)	(0.0114)	(0.0182)	(0.0125)
Dec. 11-20, 2010	0.0277	0.0197	0.0106	-0.00250
	(0.0303)	(0.0125)	(0.0181)	(0.0119)
Dec. 1-10, 2010	-0.0273	-0.00238	-0.0194	-0.00551
	(0.0313)	(0.0127)	(0.0190)	(0.0120)
Jan. 1-10, 2011	-0.0161	-0.0184	-0.00280	0.00508
	(0.0334)	(0.0170)	(0.0197)	(0.00983)
Jan. 11-20, 2011	-0.0337	-0.0206	-0.00590	-0.00713
	(0.0316)	(0.0159)	(0.0183)	(0.00934)
Jan. 21-31, 2011	-0.0458	-0.0156	-0.0275	-0.00264
	(0.0333)	(0.0163)	(0.0192)	(0.00988)
Mean of outcome	2.6183	0.1678	0.5289	0.9215
N	156145	156145	156145	156145

Spanish language and literature

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0496*	-0.000497	-0.0309	-0.0181*
	(0.0295)	(0.0120)	(0.0188)	(0.0108)
Dec. 11-20, 2010	0.0157	0.0101	0.00551	0.000128
	(0.0292)	(0.0125)	(0.0184)	(0.00996)
Dec. 1-10, 2010	-0.0386	-0.0112	-0.0203	-0.00708
	(0.0294)	(0.0124)	(0.0192)	(0.00997)
Jan. 1-10, 2011	0.00788	-0.00370	0.0115	0.0000488
	(0.0328)	(0.0178)	(0.0192)	(0.00828)
Jan. 11-20, 2011	-0.00940	0.00641	-0.0137	-0.00214
	(0.0307)	(0.0165)	(0.0182)	(0.00718)
Jan. 21-31, 2011	-0.0205	0.00490	-0.0277	0.00227
	(0.0320)	(0.0170)	(0.0190)	(0.00741)
Mean of outcome	2.6872	0.1677	0.5697	0.9498
N	156160	156160	156160	156160

Mathematics

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	-0.0824***	-0.0358***	-0.0265	-0.0200
	(0.0310)	(0.0124)	(0.0188)	(0.0123)
Dec. 11-20, 2010	0.0277	0.0159	0.0189	-0.00713
	(0.0321)	(0.0138)	(0.0186)	(0.0116)
Dec. 1-10, 2010	0.0373	0.0225	0.0127	0.00212
	(0.0331)	(0.0146)	(0.0192)	(0.0115)
Jan. 1-10, 2011	0.0161	0.0216	0.00750	-0.0129
	(0.0358)	(0.0195)	(0.0188)	(0.00961)
Jan. 11-20, 2011	-0.00263	0.00841	0.000645	-0.0117
	(0.0326)	(0.0177)	(0.0176)	(0.00839)
Jan. 21-31, 2011	-0.0106	-0.00246	-0.00333	-0.00477
	(0.0341)	(0.0182)	(0.0186)	(0.00863)
Mean of outcome	2.7406	0.2186	0.5888	0.9332
N	156176	156176	156176	156176

Social and civic values

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 21-31, 2010	0.00569	-0.000362	0.000110	0.00594
	(0.0295)	(0.0165)	(0.0194)	(0.00462)
Dec. 11-20, 2010	-0.00918	-0.00787	-0.00984	0.00853**
	(0.0285)	(0.0158)	(0.0192)	(0.00358)
Dec. 1-10, 2010	-0.0440	-0.0134	-0.0266	-0.00407
	(0.0306)	(0.0166)	(0.0201)	(0.00508)
Jan. 1-10, 2011	-0.0145	-0.00189	-0.0101	-0.00260
	(0.0328)	(0.0201)	(0.0194)	(0.00539)
Jan. 11-20, 2011	-0.0544*	-0.0304	-0.0241	0.0000693
	(0.0309)	(0.0187)	(0.0184)	(0.00484)
Jan. 21-31, 2011	-0.0771**	-0.0461**	-0.0356*	0.00452
	(0.0313)	(0.0187)	(0.0194)	(0.00425)
Mean of outcome	2.9489	0.2416	0.7190	0.9883
N	131610	131610	131610	131610

Note: Estimation results for the sample of children born in 2008–2013, and their test scores in the second grade across nine subjects: Natural environment, Social and cultural environment, Artistic education, Physical education, English as a first foreign language, Catalan language and literature, Spanish language and literature, Mathematics, and Social and civic values. These outcomes are measured for students enrolled in Catalan primary schools. We report coefficients on dummies for 10-day intervals from December 1, 2010 to January 31, 2011 (the weeks around the benefit cancellation date, i.e., 31 Dec 2010). An observation is a child born on day d in year t. The dependent variables are: (i) subject-specific grades (Column 1), (ii) a dummy for receiving the highest grade (Column 2), (iii) a dummy for receiving one of the two highest grades (Column 3), and (iv) a pass dummy, which equals 0 for children who failed and 1 for everyone else (Column 4). Grades are mapped to a 1–4 scale as follows: 9–10 (assessed in 2015–2016) or AE (Assoliment excel·lent, assessed in 2016–2017 onwards): 4; 7–8 or AN (Assoliment notable): 3; 5–6 or AS (Assoliment suficient): 2; and <5 or NA: 1. We include week of birth fixed effects, and control for the child's sex, parental education (Secondary education or above, Bachelor's degree or higher, and Master's degree or higher), parental nationality (Spanish or non-Spanish), the socio-economic index of the municipality where the child resided in the evaluation year, and the municipality's population size. Robust standard errors are shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.
Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia, Idescat

Table A8.1: Medicated for respiratory disorders (Primary Healthcare)

Panel A: Medication (Respiratory disorders)

	(1) Full months	(2) ±28 days	(3) ±21days	(4) ±14 days	(5) ±7 days
Dec'10-Jan'11 born	0.00476	0.00709	0.00419	0.00250	-0.0162
	(0.0117)	(0.0123)	(0.0144)	(0.0176)	(0.0228)
Mean of outcome	0.3424	0.3422	0.3437	0.3446	0.3379

Panel B: Medication (Respiratory disorders in year 1)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.000115	0.0000488	0.0000414	0.000103	0.000198
	(0.000387)	(0.000378)	(0.000433)	(0.000551)	(0.000779)
Mean of outcome	0.0017	0.0017	0.0016	0.0018	0.0019

Panel C: Medication (Respiratory disorders in year 2)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.000917	-0.000852	-0.00103	0.000356	0.00141
	(0.000970)	(0.000999)	(0.00105)	(0.00140)	(0.00171)
Mean of outcome	0.0065	0.0066	0.0065	0.0066	0.0063

Panel D: Medication (Respiratory disorders in year 3)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.00201	-0.00118	-0.000655	0.000423	-0.000658
	(0.00128)	(0.00134)	(0.00149)	(0.00188)	(0.00241)
Mean of outcome	0.0120	0.0121	0.0121	0.0122	0.0125

Panel E: Medication (Respiratory disorders in year 4)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.000915	0.00206	0.00240	0.00272	0.00144
	(0.00176)	(0.00184)	(0.00213)	(0.00256)	(0.00355)
Mean of outcome	0.0190	0.0190	0.0189	0.0187	0.0188

Panel F: Medication (Respiratory disorders in year 5)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.000949	0.000895	0.00122	0.00208	0.000820
	(0.00193)	(0.00203)	(0.00237)	(0.00282)	(0.00371)
Mean of outcome	0.0248	0.0248	0.0249	0.0249	0.0244

Panel G: Medication (Respiratory disorders in year 6)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.00390*	0.00419*	0.00299	-0.000787	-0.00187
	(0.00216)	(0.00228)	(0.00271)	(0.00330)	(0.00415)
Mean of outcome	0.0342	0.0342	0.0344	0.0345	0.0334

Panel H: Medication (Respiratory disorders in year 7)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.00137	0.000973	0.000732	-0.00328	-0.00428
	(0.00230)	(0.00242)	(0.00282)	(0.00326)	(0.00453)
Mean of outcome	0.0435	0.0435	0.0438	0.0437	0.0439

Panel I: Medication (Respiratory disorders in year 8)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.00314	0.00345	0.00279	0.00381	-0.00430
	(0.00307)	(0.00326)	(0.00379)	(0.00476)	(0.00601)
Mean of outcome	0.0543	0.0542	0.0545	0.0545	0.0530

Panel J: Medication (Respiratory disorders in year 9)

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.00560*	-0.00490	-0.00474	-0.00433	-0.00452
	(0.00320)	(0.00338)	(0.00392)	(0.00489)	(0.00648)
Mean of outcome	0.0643	0.0642	0.0645	0.0646	0.0624

Panel K: Medication (Respiratory disorders in year 10)

Dec'10-Jan'11 born	0.00289	0.00241	0.000443	0.00141	-0.00440
	(0.00348)	(0.00366)	(0.00426)	(0.00525)	(0.00714)
Mean of outcome	0.0820	0.0817	0.0825	0.0831	0.0815
N	495137	456214	341895	227290	112836

Note: Difference-in-difference estimation results for the sample of children in the BIFAP database born in October 2008-March 2009, October 2009-March 2010, October 2010-March 2011, October 2011-March 2012, and October 2012-March 2013, and medicated for respiratory disorders overall (Panel A) and by age (Panels B - K). We report coefficients on a December, 2010-January, 2011 dummy (the weeks around the benefit cancellation date, i.e., 31 Dec, 2010) from equation (2). An observation is a child born on day d in year t. The dependent variable is the number of times the child was medicated for respiratory disorders from age 1-10 years in total (Panel A) and by years (Panels B - K), and the sample includes all babies in the BIFAP database born in the last 1-4 weeks of October, December, and February and the first 1-4 weeks of November, January and March (depending on the column), for October-November-December-January-February-March sextuplets from 2008-09 to 2012-2013. Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.
Source: BIFAP database.

Table A8.2: Visits to Primary care

Panel A: Visits to Primary Care

	(1)	(2)	(3)	(4)	(5)
	Full months	±28 days	±21days	±14 days	±7 days
Dec'10-Jan'11 born	-0.110	-0.102	-0.0151	0.182	-0.0588
	(0.342)	(0.358)	(0.412)	(0.506)	(0.717)
Mean of outcome	55.4944	55.4958	55.4891	55.3890	55.2650

Panel B: Visits to Primary Care in year 1

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.0713	0.0668	0.0879	0.119	0.0769
	(0.0763)	(0.0798)	(0.0917)	(0.113)	(0.160)
Mean of outcome	11.0271	11.0258	11.0151	10.9998	10.9819

Panel C: Visits to Primary Care in year 2

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.0314	-0.0343	0.0100	0.0492	0.0414
	(0.0598)	(0.0625)	(0.0719)	(0.0885)	(0.126)
Mean of outcome	7.3510	7.3492	7.3441	7.3296	7.3081

Panel D: Visits to Primary Care in year 3

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.0713	0.0668	0.0879	0.119	0.0769
	(0.0763)	(0.0798)	(0.0917)	(0.113)	(0.160)
Mean of outcome	11.0271	11.0258	11.0151	10.9998	10.9819

Panel E: Visits to Primary Care in year 4

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.0597	-0.0468	-0.0376	-0.00382	-0.0106
	(0.0404)	(0.0423)	(0.0488)	(0.0605)	(0.0863)
Mean of outcome	4.1359	4.1359	4.1333	4.1267	4.1166

Panel F: Visits to Primary Care in year 5

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.0586	-0.0438	-0.0172	0.0251	-0.00566
	(0.0361)	(0.0379)	(0.0438)	(0.0538)	(0.0758)
Mean of outcome	3.8080	3.8100	3.8071	3.7989	3.7932

Panel G: Visits to Primary Care in year 6

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	0.0259	0.0304	0.0242	0.0162	0.00162
	(0.0327)	(0.0342)	(0.0393)	(0.0481)	(0.0670)
Mean of outcome	3.2112	3.2130	3.2125	3.2012	3.1876

Panel H: Visits to Primary Care in year 7

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.0549*	-0.0517	-0.0434	-0.0622	-0.116*
	(0.0330)	(0.0345)	(0.0397)	(0.0484)	(0.0684)
Mean of outcome	3.9102	3.9113	3.9125	3.9035	3.8986

Panel I: Visits to Primary Care in year 8

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.0967***	-0.0866***	-0.0932**	-0.0818*	-0.0893
	(0.0311)	(0.0326)	(0.0377)	(0.0464)	(0.0647)
Mean of outcome	3.3659	3.3674	3.3741	3.3646	3.3588

Panel J: Visits to Primary Care in year 9

	(1)	(2)	(3)	(4)	(5)
Dec'10-Jan'11 born	-0.0324	-0.0394	-0.0568	-0.0413	-0.0128
	(0.0332)	(0.0348)	(0.0404)	(0.0493)	(0.0704)
Mean of outcome	3.6173	3.6169	3.6254	3.6181	3.6069

Panel K: Visits to Primary Care in year 10

Dec'10-Jan'11 born	0.0552	0.0367	0.0231	0.0434	-0.0209
	(0.0355)	(0.0371)	(0.0428)	(0.0524)	(0.0737)
Mean of outcome	4.0406	4.0405	4.0499	4.0468	4.0314
N	495137	456214	341895	227290	112836

Note: Difference-in-difference estimation results for the sample of children in the BIFAP database born in October 2008-March 2009, October 2009-March 2010, October 2010-March 2011, October 2011-March 2012, and October 2012-March 2013, and visited primary care in total (Panel A) and by age (Panels B - K). We report coefficients on a December, 2010-January, 2011 dummy (the weeks around the benefit cancellation date, i.e., 31 Dec, 2010) from equation (2). An observation is a child born on day d in year t. The dependent variable is the number of times the child visited primary care from age 1-10 years in total (Panel A) and by years (Panels B - K), and the sample includes all babies in the BIFAP database born in the last 1-4 weeks of October, December, and February and the first 1-4 weeks of November, January and March (depending on the column), for October-November-December-January-February-March sextuplets from 2008-09 to 2012-2013. Robust standard errors shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.
Source: BIFAP database.

Table A9.1: Health outcomes (Birth weight) using education specification

	(1) Weight in Grams	(2) Birth weight below 2500g
Dec. 21-31, 2010	-14.71***	0.00193
	(5.177)	(0.00257)
Dec. 11-20, 2010	2.781	-0.00132
	(4.911)	(0.00240)
Dec. 1-10, 2010	-5.474	0.00417
	(5.352)	(0.00264)
Jan. 1-10, 2011	-18.36***	0.00560**
	(5.829)	(0.00282)
Jan. 11-20, 2011	-1.642	0.00131
	(5.109)	(0.00250)
Jan. 21-31, 2011	1.429	-0.00164
	(5.429)	(0.00267)
Mean of outcome	3216.6578	0.0768
N	2710898	2851896

Note: Estimation results for the sample of children born in 2008 - 2013, and their weight at birth in grams. We report coefficients on dummies for 10-day intervals from December 1, 2010 to January 31, 2011 (the weeks around the benefit cancellation date, i.e., 31 Dec 2010). An observation is a child born on day d in year t. The dependent variables are birth weight in grams for the full sample (column 1), and an indicator for low birth weight defined as less than 2500 grams (column 2). We include week of birth fixed effects. Robust standard errors are shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013.

Table A9.2: Health outcomes (Hospitalisation) using education specification

	(1) Hospitalised (any cause)	(2) Respiratory disease	(3) Mental and behavioural disorder	(4) Pneumonia	(5) Bronchitis	(6) Asthma
Dec. 21-31, 2010	-0.00484	0.00103	-0.000149	0.0000214	-0.00170***	0.0000107
	(0.00351)	(0.00200)	(0.000249)	(0.000779)	(0.000504)	(0.000624)
Dec. 11-20, 2010	-0.0128***	-0.00171	0.000151	-0.00125*	0.000203	0.00111
	(0.00342)	(0.00198)	(0.000239)	(0.000717)	(0.000594)	(0.000675)
Dec. 1-10, 2010	-0.00651*	-0.000615	0.000820**	-0.00175**	-0.000702	-0.00153**
	(0.00364)	(0.00206)	(0.000343)	(0.000753)	(0.000560)	(0.000611)
Jan. 1-10, 2011	-0.00852**	-0.00349	-0.000558***	-0.000601	0.000488	-0.000580
	(0.00390)	(0.00228)	(0.000186)	(0.000840)	(0.000616)	(0.000704)
Jan. 11-20, 2011	-0.00182	0.000360	-0.000155	-0.000313	-0.000454	0.00264***
	(0.00354)	(0.00206)	(0.000230)	(0.000777)	(0.000499)	(0.000755)
Jan. 21-31, 2011	-0.00325	-0.000127	-0.000291	-0.000588	0.00203***	0.000409
	(0.00370)	(0.00216)	(0.000238)	(0.000790)	(0.000621)	(0.000672)
Mean of outcome	0.1816	0.0515	0.0009	0.0077	0.0043	0.0045
N	2862793	2862793	2862793	2862793	2862793	2862793

Estimation results for the sample of children born in 2008-2013. We report coefficients on dummies for 10-day intervals from December 1, 2010 to January 31, 2011 (the weeks around the benefit cancellation date, i.e., 31 December 2010). An observation is a child born on day d in year t. The dependent variables are indicators for whether the child was hospitalised overall or diagnosed with specific conditions: respiratory diseases, mental and behavioural disorders, pneumonia, bronchitis, or asthma. Each column corresponds to a different health outcome. All regressions control for week of birth and birth year fixed effects. Robust standard errors are shown in parentheses. Significance levels are indicated by * <.1, ** <.05, *** <.01.
Source: Hospital Morbidity Survey 2015-2020, Spanish National Statistical Institute.