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Born Too Soon? The Educational Costs of Early Elective Deliveries

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Abstract: We examine the impact of early elective birth timing on children's health and educational outcomes, focusing on cognitive development as measured by elementary school grades. We exploit a natural experiment in Spain: the abrupt termination of a generous child benefit at the end of 2010, which led to a sharp increase in elective deliveries during the final week of December. Children born during this spike had slightly shorter gestation periods and lower birth weights (within the normal range), and experienced a higher incidence of respiratory disorders during infancy. We find that the affected cohort of children had significantly lower academic performance at age seven (in second grade), suggesting large persistent effects on cognitive development. Our results provide causal evidence on the medium-term costs of early elective deliveries, and underscore the link between neonatal health and human capital.

Keywords: Education, health, birthweight, family benefits.

JEL codes: I2, I1, J13

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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). A recent body of literature highlights the critical importance of early interventions, 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 some attention (Borra et al. 2019, Card, Fenizia, and Silver 2018, Costa-Ramón et al. 2018), their long-term consequences, particularly for healthy, low-risk pregnancies, remain largely unexplored. Earlier delivery leads (mechanically) to lower birth-weight, and a number of studies have shown strong associations between birth-weight and cognitive development (Figlio et al. 2014) and even long-term earnings (Black et al. 2007), which raises concerns about scheduling deliveries early for non-medical reasons.

We address this gap by investigating the causal effects of early elective birth timing on children's health and, crucially, their cognitive development, as measured by grades in elementary 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 incentive led to a discernible spike in births during this period, providing a quasi-random assignment to slightly earlier delivery for a well-identified 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 led to slightly shorter gestational periods and lower birth weights, albeit still within the normal range. Second, we show that the affected cohort of infants experienced an elevated incidence of respiratory hospitalizations and diagnoses in their first year of life, suggesting immediate, albeit non-severe and short-lived, health consequences.[1] Third and most critically, we show that these children, by age seven, exhibited significantly worse performance on test scores in primary school (by about 0.08 standard deviations). These educational deficits persist 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). These studies typically rely on within-sibling or within-twin variation in birth-weight, where lower weight is interpreted as a proxy for worse fetal health. Our contribution is to take advantage of a setting where lower birth-weight is driven by a purposeful manipulation of the timing of birth (in healthy pregnancies), which allows us to provide credible causal estimates of the effect of early elective delivery on cognitive development, with direct evidence of the driving intervention and mechanism. Our results align with the recent strand of literature

[1] These findings are consistent with those in Borra et al. (2019).

that points to the developmental sensitivity to even mild shocks in early life (Almond et al. 2018).

Second, we speak 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 and Guldi 2017, Teitler et al. 2019, Halla et al. 2020, Jacobson et al. 2021). Our focus 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, we leverage 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 negative association between prematurity and low birthweight and long-term outcomes is 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, we contribute 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" may have been effective in achieving their proximate objective (stimulating fertility) (González 2013, González and Trommlerová 2023), the abrupt cancellation of the policy at the end of 2010 induced a sharp, anticipatory shift in delivery timing. The expiring benefit was akin to a transfer conditioned on the date of birth, and families responded accordingly. The bunching that we observe around the cutoff 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 under-weigh 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. Our findings call for a more cautious and forward-looking approach to the design and termination of early-childhood cash transfer programs, as well as the design of medical guidelines regarding early elective deliveries.

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 to the mother, immediately after the birth of a child. 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 for children born from January 1, 2011 (less than eight months later). The cancelation created a strong incentive for expecting parents with due dates in early January of 2011 to accelerate the birth to ensure eligibility for the benefit. The sudden nature of the cancellation, with no phase-out, maximized the incentive for early 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 our three sets of outcomes (birth timing, health, and school results).

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 (December 31, 2010), expecting a jump (bunching) just before the cutoff and a drop (a “hole”) just after, if elective deliveries occurred as a result of the benefit cancelation.

Our empirical approach relies simply 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:

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

Our sample includes births in December or January of 2008-09 to 2012-13. The dependent variable (*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 children 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 daily 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 (December 25 to January 7), as we expect most of the action to take place in the days immediately surrounding the cutoff date (Dec. 31, 2010). We then extend the window to ten, fourteen, and twenty days, and the full months before and after the cutoff 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, we use our birth timing analysis above to identify 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 full 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 "shifted" as a result of the policy). We formalize this empirical strategy in the following specification:

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

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 fixed effects account for aggregate factors that affect health outcomes for each cohort (for example, a bad flu season in the first winter of a child's life). 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 in 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 months.

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 months). 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

Our measure of cognitive development is based on school grades in elementary education (second grade). We can estimate equation (2) for our school outcomes, but doing so would conflate the effect of our treatment of interest (the spike in early elective deliveries in December 2010) with "age-at-test" effects (Calsamiglia & Loviglio 2020). The reason is that in Spain, the (natural) year of birth determines (sharply) the year when a child starts school, which means that children born in December start school a year earlier than children born the following January. Thus, the children who were born in December of 2010 due to the benefit cancellation started school a year earlier (than they would have otherwise). As a result, children born in our "manipulation period" (December 2010 and January 2011) include a higher share of children who are "young for their grade", which would bias our estimates upwards 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 (2008 to 2013), and we control directly for "age at test", i.e., we compare school grades for children in the manipulation window (December-January of 2010-11) to those of children born on the same dates of the surrounding years. We estimate the following specification:

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

where Y is the (standardized) average grade 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 m; (ii) month-of-birth fixed effects; (iii) a linear trend in week of birth; and (iv) date-of-birth fixed effects (in 10-day intervals). This framework allows us to estimate our effect of interest, while flexibly controlling for age-at-test and time-of-year trends in educational performance.

This new specification controls for age at test, but is potentially subject to selection effects (since the children who were "shifted" from January to December are not necessarily a random subsample). However, while the selection effect would be present for both children born in early January of 2011 and those born in December of 2010 (with opposite signs), the causal effect of birth shifting would only appear for the December 2010 group, which allows us to disentangle them.

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 at birth, 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 (the Hospital Morbidity Survey), when the treated cohort is 5 to 10 years old, enabling us to estimate effects on hospitalizations for respiratory (and other) disorders during childhood.

Primary healthcare records: We obtained medical histories from the public primary healthcare system[2] 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 elementary 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.[3] 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), as well as some demographic information about children and their families.

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[2] "Base de Datos para la Investigación Farmacoepidemiológica en Atención Primaria" (BIFAP).

[3] Grades are coded as fail, pass, good and excellent, which we recode as 1, 2, 3 and 4.

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 November, December, January and February for the 2010–11 period (in blue) alongside the same weeks in the four adjacent years. A sharp increase in births is visible in the weeks leading up to December 31, 2010, relative to the comparison years. This spike is immediately followed by a marked drop in births in the first week of January 2011, suggesting a shift in the timing of deliveries around the policy cutoff date. 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 of 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. These figures visually confirm the manipulation in birth timing around the end of December 2010 and support our empirical strategy focusing on this “manipulation window”.

The spike in births in late December 2010 and the drop in early January 2011 could reflect (at least in part) a behavioral response in abortions in earlier months (specifically, fewer abortions of pregnancies due before January 1 and more for those due after, in anticipation of the benefit cancelation). Figure A1 shows the weekly number of abortions by due date in the benefit cancellation years and the surrounding ones. We find no evidence of a discontinuity in the weekly number of abortions by due date around the cutoff, with 2010-11 following a pattern similar to earlier years. This absence of a shift in abortion timing rules out this channel as a driver of the observed bunching in births in December of 2010, reinforcing our interpretation that the spike reflects changes in delivery timing (rather than pregnancy terminations).

We formalize these descriptive patterns in the regression framework defined in equation (1), with results shown in Table 1. Panel A 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 around the cutoff. Across all specifications, we observe a statistically significant elevated number of births in December 2010, with the magnitude of the (daily) estimates growing as the window narrows. The estimated effect rises from 119 additional daily births (column 1) to 279 (column 5), confirming that the birth shifting is concentrated in the days immediately surrounding the benefit cutoff.

Panel B of Table 1 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 0.09 increase in December 2010 births suggests that approximately 4.7% 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 0.21, implying that around 11.7% of early January births were brought forward into the final week of December.[4]

[4] $e^{0.21}$ minus 1, divided by 2 (to avoid double-counting, since each shifted birth is one fewer in January and one more in December).

Appendix Tables A1 and A2 provide robustness checks using subsamples and alternative control years. The results for Catalonia (A1.1) and the BIFAP regions[5] (A1.2) show similar patterns, with significant increases in December births concentrated around the cutoff. For Catalonia (±10-day window), we find that 10.4% of early January births were shifted forward into the last 10 days of December. Table A1.3 shows that the birth timing effects are present regardless of the gender of the child, both in the whole of Spain and in Catalonia. 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, our findings confirm a clear shift in birth timing around the December 2010 cutoff, consistent with parents adjusting delivery dates to retain eligibility for the cancelled benefit.

We next explore the characteristics of the families who shifted birth timing as a result of the benefit cancelation. To do so, we estimate equation (3), where we use parental characteristics as the dependent variable. The coefficients of interest capture any differences in average parental characteristics for children born in the manipulation window (December-January of 2010-11), relative to children born on the same dates in the surrounding years. The results are shown in Table A3. We find that parents who had a child in late December 2010 (where the manipulated births concentrate) are no more or less likely to be married, and they do not differ in paternal age (Panel A). We do find that mothers in the relevant window are slightly older (columns 3 and 4), and both parents are more likely to be native-born (cols. 7-10). The results in Panel B suggest that mothers who reacted to the benefit cancelation were more educated than the average. Finally,

[5] 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 our primary health care database.

Panel C (cols. 11-12) shows that birth shifting was more common in large cities (province capitals).

The evidence in Table A3 indicates (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. 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.

4.2 Effects on health at birth

We next examine whether the observed shift in birth timing around the benefit cutoff had any impact on newborn health, using data from birth certificates. Figure 2 presents descriptive evidence on birthweight. We plot 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, 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. This pattern is consistent with a higher fraction of babies than usual being delivered early (and as a result smaller) in the 2010-11 period.

Table 2 presents the estimated impact of the benefit cancellation on newborn health outcomes (equation 2), focusing on birthweight and gestational age for the full Spanish sample. Columns (1) through (5) show results for progressively narrower windows around the policy cutoff (ranging from full-month comparisons to ±7-day windows). In Panel A, across all specifications, we show that children born around the December 31, 2010 cutoff exhibit significantly lower birthweights. The effect ranges from 6 grams in the full-month comparison (column 1) to 21 grams in the ±7-day window (column 5). The average treatment effect appears small in absolute terms as this estimate reflects an average across all births in the window. Given that only about 11.7% of births in the ±7-day window were actually affected by birth shifting (see Table 1), the implied effect for the treated group is large (equivalent to a 180-gram drop, or 5.6%, 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 –0.06 weeks in the ±7-day window. Again, given the small fraction of births affected, the implied reduction in gestational age for shifted births is substantially larger (more than half a week).

We find no significant increase (Panel C) in the fraction of babies born with birthweight less than 2,500 grams (the threshold typically used as an indicator for low birthweight), 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 (and of similar magnitude) for both boys and girls. In the ±7-day window, birthweight falls by 22 grams for girls and 20 grams for boys (both statistically significant at the 1% level).

Appendix Table A4.1 presents the results for Catalonia (Panels A, B, and C), and the other autonomous communities covered by the primary healthcare 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 about 280 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.

We also explore how the birth-weight effects vary by family characteristics. Table A4.2 shows that the effect is similar for parents with higher and lower educational attainment, while it is stronger for native parents and those who live in urban areas (consistent this the evidence in the previous section that these types of families were more likely to react).

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 0.0066 in the narrowest window, implying a small increase in complications in the "manipulation period" (around 4%). Panel D (firstborn indicator) and Panel E (cesarean births) show no significant changes, suggesting that the composition of births (in terms of parity and surgical intervention rates) remained stable. The unchanged incidence of c-section births is important for our mechanisms (for the health and cognitive development effects). Panel F shows a consistent and statistically significant (small) decline in full-term pregnancies (37+ weeks), with effects growing stronger in narrower windows. In the ±7-day window, the estimate reaches –0.007 (0.76% of the mean) confirming that among the "treated" births, about 7% were induced or scheduled slightly before term.

4.3 Effects on educational outcomes

We now turn to our main results: the effects on cognitive development, as captured in elementary school grades. Figure 3 (Panel A) shows average grades across all subjects by 10-day birth window for the 2010-11 cohort compared to the surrounding years.[6] January-born children perform significantly better than December children, reflecting their being "older for their grade". Children born in late December 2010 (those most likely to have had their births shifted forward) score particularly low, and significantly lower than their counterparts in other years. In contrast, children born in early January of 2011 perform similarly to controls, suggesting no significant composition effects.

Panel B of Figure 3 plots the difference in average test scores (2010-11 vs. control years). The largest and only significant 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 A2 shows average grades by month of birth for our full sample (children born in 2008-2013). 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 our regression models, to isolate the causal effect of early elective delivery from mechanical age effects.

Table 3 presents the results of estimating equation (3) for elementary school grades (standardized). Columns (1) and (2) include a linear trend in month of birth, while

[6] Grades are discrete and take values 1, 2, 3 or 4 for each subject, with higher values representing better results.

columns (3) to (8) use increasingly flexible specifications: month of birth fixed effects, date of birth trends, and date of birth (in 10-day bins) fixed effects. In all specifications, the December 2010 coefficient is negative, with estimates ranging from -0.028 to -0.032, 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 and only significant negative effect is observed for Dec. 21-31 (-0.084 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.[7] The results from our preferred specification (column 8) are also shown in Panel C of Figure 3. The small and insignificant coefficients found for children born in early January indicate again that selection/composition effects are not significant.

We explore composition effects more directly in Table A6.1, where we report the results of estimating equation (3) for parental characteristics. We find (weak) evidence that children born in the final days of December 2010 have slightly more educated parents who are also more likely to be native Spaniards and live in larger cities, again mirroring the earlier evidence of mild positive selection into treatment. Table A6.2 shows that the affected cohort of children is no more or less likely to attend private school.

While the average estimated impact on grades shown in Table 3 (-0.08 standard deviations or SDs) may seem modest in isolation, given that only about 17% of the births in the Dec. 21-31 window were affected (i.e., moved forward from January into

[7] We also estimate equation (2) for (standardized) school grades. The estimated coefficients for the +/-10-day window are larger (-0.14 without controls and -0.11 with), reflecting the combined effect of earlier birth and the fact that shifted children started school a year earlier (and were thus young for their grade).

December),[8] our estimates suggest that the children whose births were shifted forward to just before the December 31 cutoff scored, on average, 0.5 SDs lower in second grade than children born on the same dates in other years (-0.0842/0.17). This loss of 0.5 SDs corresponds to a developmental delay of approximately 16 months (since the "effect" of being a month older is -0.03, see Figure A2), in terms of cognitive performance at age 7. In other words, these children perform like peers who are over one year younger, simply because they born slightly earlier (and "pushed" into school a year earlier) due to the policy.

We can compare the magnitude of our estimated effects to the findings in previous papers. Figlio et al. (2014) find that 10% lower birth-weight (within twin pairs) is associated with 0.045 SDs lower test scores in grades three to eight.[9] We find that a cohort of children who were born with about 1% lower birthweight (-29.46/3,216, see Table A4.1) had 0.08 SDs lower test scores in second grade. Our estimates are thus an order of magnitude higher.

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 December 21-31, 2010 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 impacts are not driven by size/physical

[8] From Table A1.1 (column 4) about 10.4% of the (early) January births were shifted to December ($e^{0.19}$=1.21, minus 1, divided by two). This corresponds with 17% of the (late) December births (21/121).
[9] Bharadwaj et al. (2018) find similar magnitudes in Chile (about 0.05 SDs).

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 the groups of children affected by the policy-induced early birth (and school entry). We perform a parallel analysis by subject for grades defined as binary outcomes instead of numerical variables. Appendix Table A7.1 shows that the effects are not driven by an increase in the fraction of children who fail a subject, but by a lower fraction obtaining a high pass or the top grade.

We also explore heterogeneity in the effects on cognitive development by child and family characteristics. We expect that more affected groups of children (in terms of birth shifting and newborn health) would experience stronger impacts on school grades. The results in Table A7.2 show that the effect on grades is stronger for girls, children with native parents, and those living in higher income and larger municipalities. These patterns are consistent with the types of "complier" families (see sections 4.1 and 4.2).[10]

4.4 Effects on respiratory disease

Borra et al. (2019) found that the cohort of children affected by the birth shifting had higher rates of respiratory hospitalizations in the first two months of life, and they also documented no persistent effect on hospitalization rates at ages 0 to 3. We revisit the potential health effects of the early elective deliveries of late December of 2010, using new sources of data (from primary healthcare) and a longer time horizon, since persistent health effects are a potential mechanism for the impact of school outcomes.

Table 5 and Figure 4 examine the early and long-term respiratory outcomes for children born around the policy cutoff, using primary care data (and equation 2). While we find no consistent evidence of increased overall respiratory diagnoses due to the birth

[10] In Catalonia, the magnitude of the birth timing effect was 13.4% for girls versus 8% for boys (Table A1.2). The estimated effect on birthweight was -36.4 grams for girls versus -21.3 for boys.

shifting (other than possibly elevated respiratory diagnosis at age 10, see Figure 4), our estimates suggest that, on average, children born during the ±7-day policy window had 0.0113 more bronchiolitis diagnoses before age 1, compared to the control group (given a baseline mean of 0.165 diagnoses per child, this corresponds to a nearly 10% increase). We also see similar results for bronchitis diagnoses at age 0 (~ 8% increase), but the results are only significant for the ±14-day window.[11]

On the other hand, we don't find significant differences (both short- or long-term) in the number of visits to primary care (Table A8.2) or medication prescriptions for respiratory disorders (Table A8.3). Nor do we find (Figure A3) 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 data from 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 in Panel A of Table 6) *fewer* hospital stays per 100 births for all disorders.

When it comes to hospitalizations for respiratory disorders, the children born in the ± 7 days around the cutoff date (column 5 in Panel B of Table 6), had 0.47 *fewer* hospitalizations (significantly) per 100 births. For the bigger time windows around the

[11] The effects on respiratory diagnoses are driven by boys (see Appendix Table A8.1).

threshold (columns 1 to 4 in Panel B) none of the coefficients are statistically significant, and are, in fact, quite close to zero. For the Catalonian sample (Table 7) we find mostly insignificant results: the “treated” children are no more likely to be hospitalized at ages 4 to 10 (overall, or for respiratory disorders).[12]

5. Discussion of 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 cognitive development.

One key mechanism driving these effects 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.[13] 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

[12] We also estimate the effects on health outcomes using equation (3), for consistency with the education results. Our health results seem fairly robust across specifications (equation 3 also detects higher respiratory diagnoses in the treated group in the first year of life).

[13] Note, however, that the effects on early respiratory disease are stronger for boys, while the effects on school outcomes are larger for girls.

systematically occurring at a level that would offset these effects. Any other compensating investments would tend to *improve* treated children's outcomes, 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 school 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. Previous studies (Chan et a. 2016, Gleason et al. 2022, Hadders-Algra et al. 2023, Grotheer et al. 2023, Schmitt et al. 2023, Gale-Grant et al. 2022, Nivins et al. 2023) have shown that even modest reductions in gestational length within the "normal" range can produce measurable differences in brain maturation, suggesting that slightly earlier births, though clinically full-term, carry a subtle degree of neurobiological immaturity that can help explain the educational deficits that we document.

6. Conclusion

We leverage a unique natural experiment in Spain (the abrupt cancellation of a generous child 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, the affected cohort of children exhibited significantly worse grades across all subjects in elementary 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 contributes 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 in full-term pregnancies, our research suggests that even small shifts in birth timing (for non-medical reasons) 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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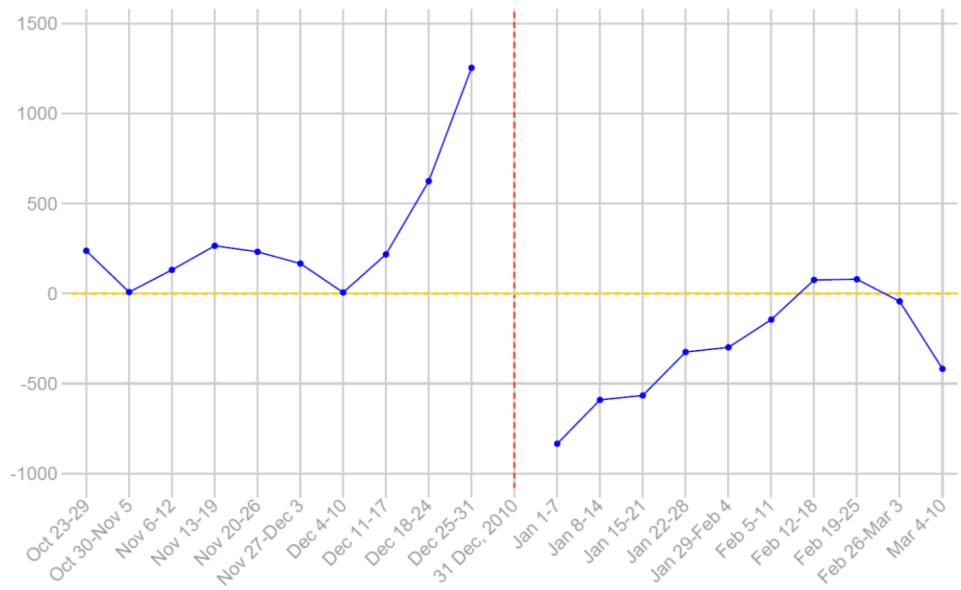
Figure 1. Birth timing

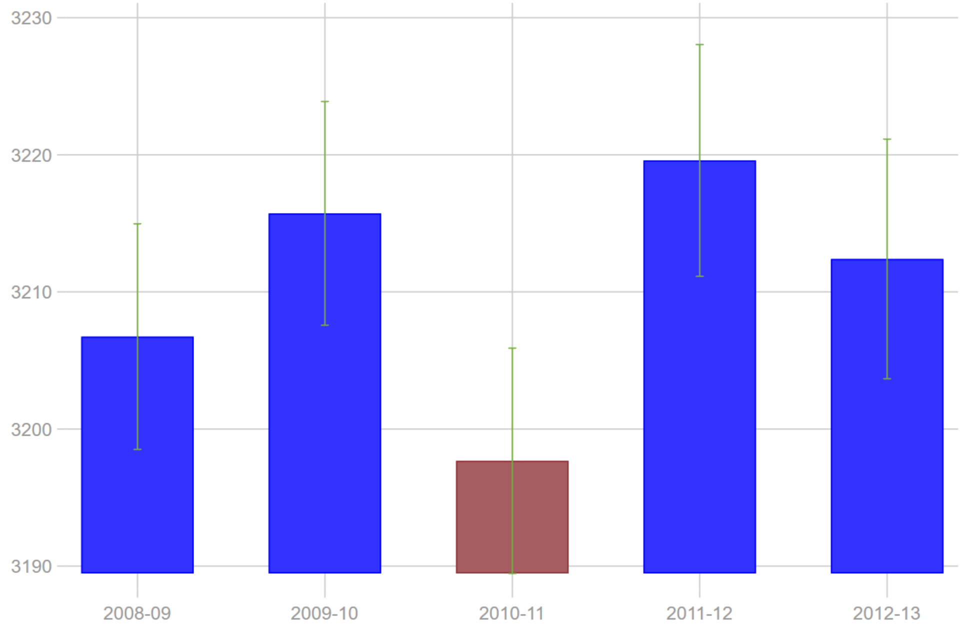
Panel A. Weekly number of births, December and January 2010-11 and the surrounding years

Panel B. Difference in weekly number of births, 2010-11 vs. surrounding years (2008-09, 2009-10 and 2011-12)



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

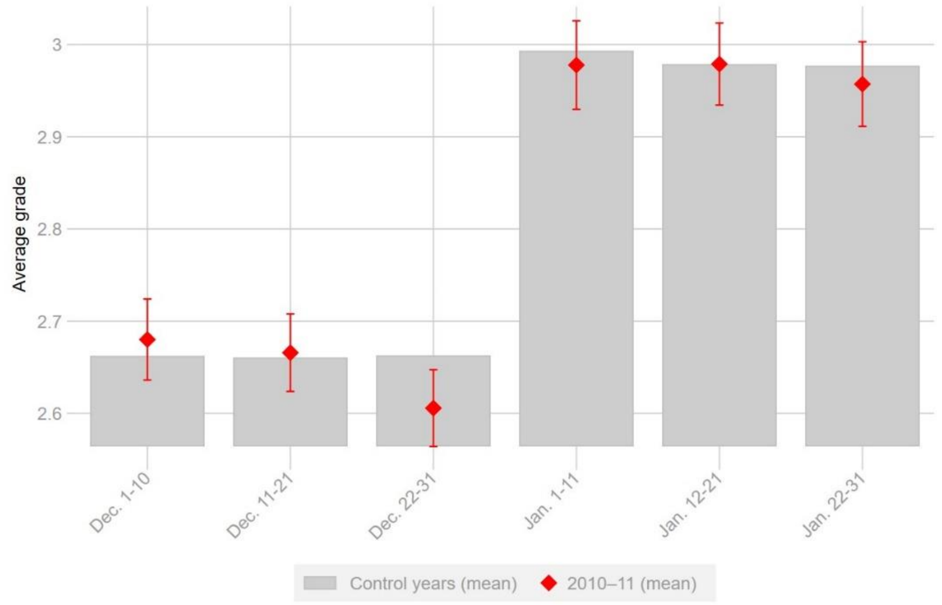
Figure 2. Average birthweight in the one-week window around December 31, 2010-11 vs. the surrounding years



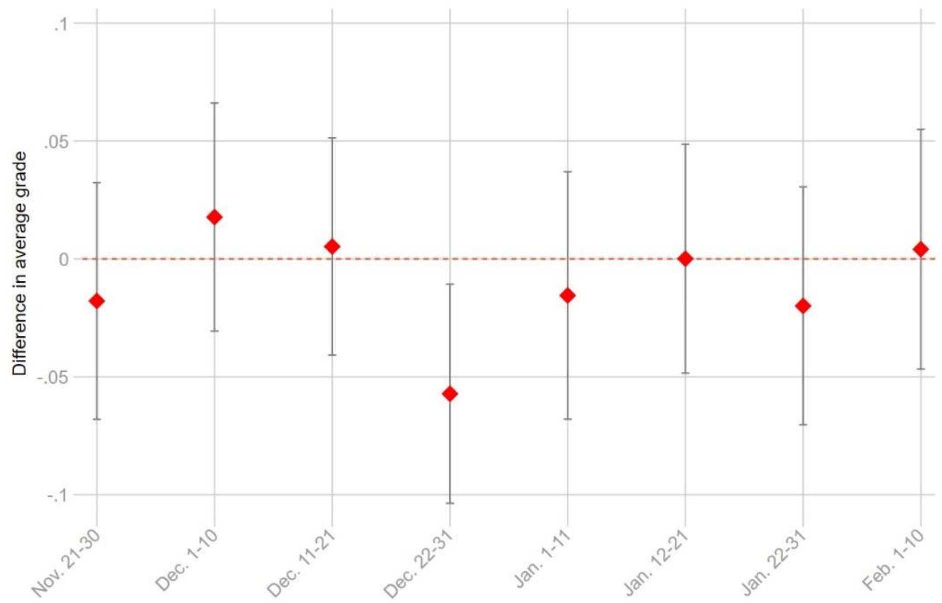
Source: Birth-certificate micro data, Spanish National Statistical Institute, 2008-2013

Figure 3. School performance

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

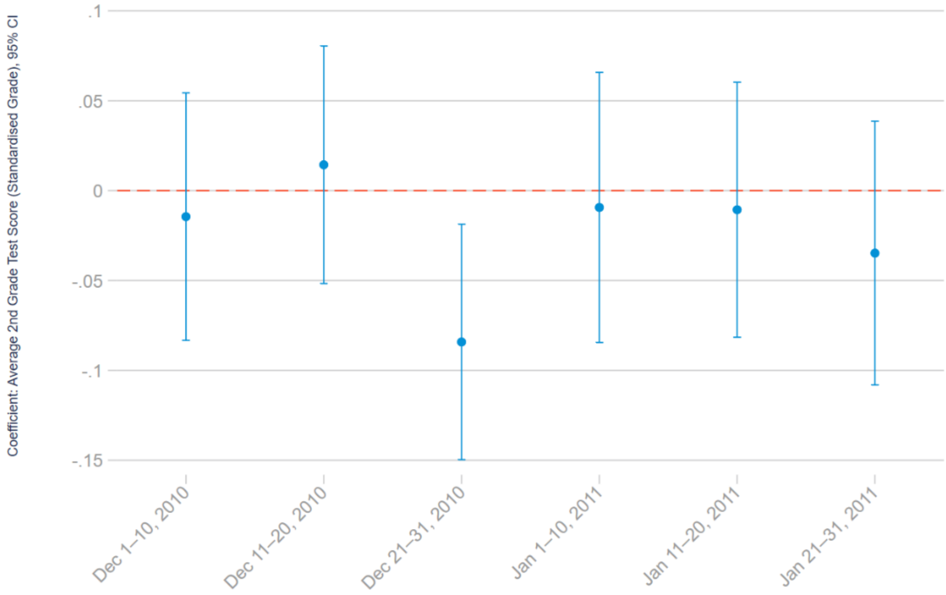


Panel B. Difference in average second grade test score by date of birth (2010-11 vs the surrounding control years)



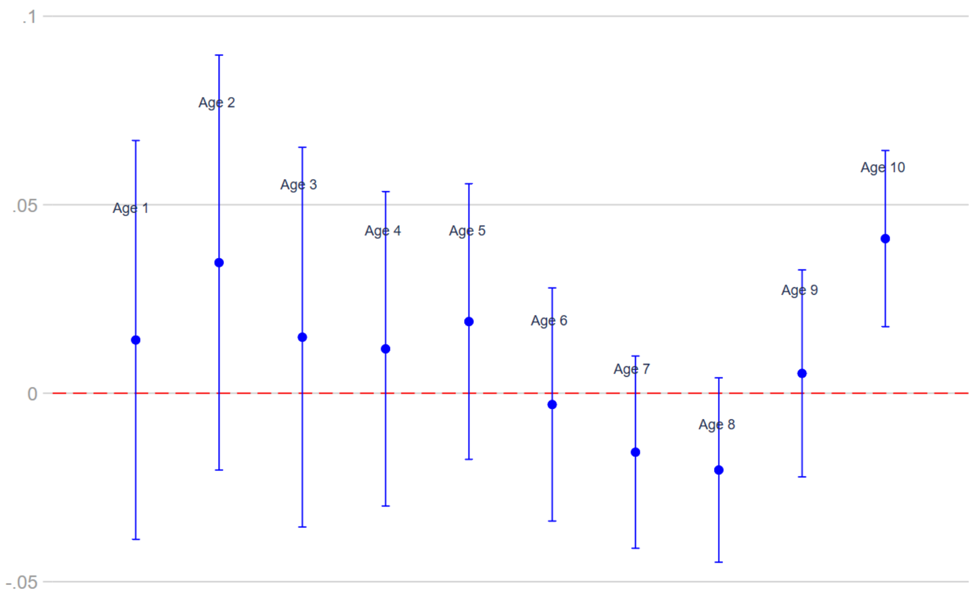
Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia (grades coded 1 to 4, averaged across all subjects), Idescat.

Panel C. Coefficient estimates for the effect of early elective delivery on school grades



Note: Coefficients and standard errors correspond to the specification reported in Table 3 (column 8). The dependent variable is the average second grade test score (standardized grade) of child i, born in month m of year t. The specification includes week-of-birth fixed effects. The reported coefficients refer to indicators for whether a child was born in each of the 10-day intervals. We 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. Source: Primary school test scores (2nd grade) for children born in 2008-2013 in Catalonia (grades coded 1 to 4, averaged across all subjects), 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.

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	119.1***	164.6***	216.8***	257.7***	278.6***
	(24.60)	(28.85)	(32.69)	(37.13)	(48.65)
Number of births moved	1,846	1,646	1,517	1,285	975
Mean of outcome	1,296	1,299	1,283	1,250	1,242

Panel B: ln(Number of births)

	(1) Full months	(2) ± 20 days	(3) ± 14 days	(4) ± 10 days	(5) ± 7 days
Dec. 2010	0.0902***	0.125***	0.165***	0.198***	0.210***
	(0.0182)	(0.0213)	(0.0237)	(0.0268)	(0.0347)
Share of births moved	4.7%	6.6%	9.0%	11.0%	11.7%
N	310	210	140	100	70
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
Year*Day of week	Yes	Yes	Yes	Yes	Yes

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	3,212.01	3,211.30	3,210.54	3,211.10	3,210.71
N	1,127,116	760,456	517,138	349,128	255,103

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
	948,171	639,352	434,930	293,513	214,674

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	1,127,116	760,456	517,138	349,128	255,103

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	3,269.52	3,268.84	3,267.58	3,268.40	3,268.00
N	579,996	391,109	266,017	179,440	130,979

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	3,151.04	3,150.36	3,150.11	3,150.50	3,150.27
N	547,120	369,347	251,121	169,688	124,124

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. 1-10, 2010		-0.00298		0.00112		-0.0188		-0.0145
		(0.0321)		(0.0330)		(0.0321)		(0.0351)
Dec. 11-20, 2010		-0.00129		0.00282		-0.00167		0.0144
		(0.0310)		(0.0319)		(0.0310)		(0.0337)
Dec. 21-31, 2010		**-0.0910*****		**-0.0869*****		**-0.0760****		**-0.0842****
		(0.0301)		**(0.0311)**		**(0.0302)**		**(0.0334)**
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
N	156264	156264	156264	156264	156264	156264	156264	156264
Mean of outcome	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006	0.0006

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. 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)
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. 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)**
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

Note: Difference-in-difference estimation results for the sample of children 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.

Table 6. The effect of benefit cancellation on hospitalisations (in 2015-2020): 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

	(1) Full months	(2) ±20 days	(3) ±14 days	(4) ±10 days	(5) ±7 days
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	1,190,013	802,900	546,104	368,720	269,507

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 (in 2015-2020): 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
Dec. 10-Jan. 11	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	206,310	138,973	94,494	63,572	46,488

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.

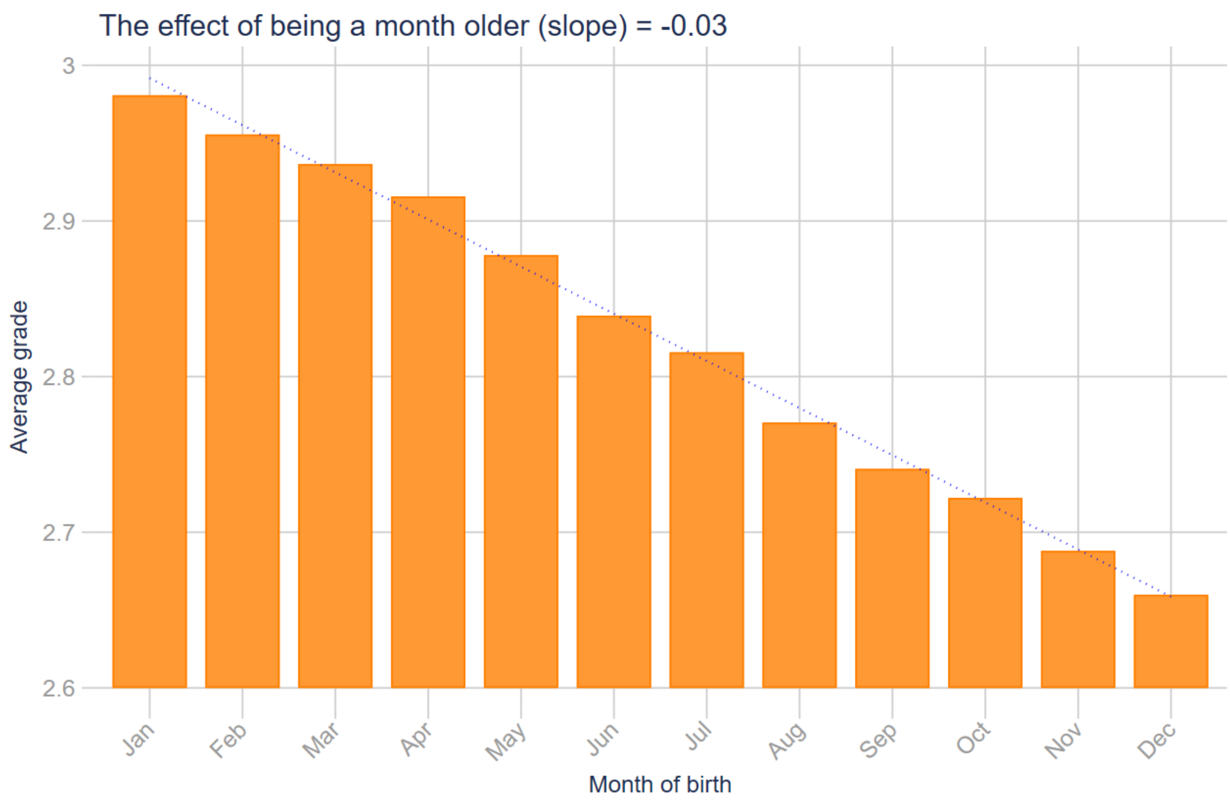
Source: Hospital Morbidity Survey 2015-2020. All recorded hospitals stays and diagnoses are included.

Appendix

Figure A1. Weekly number of abortions by due date relative to Jan 1

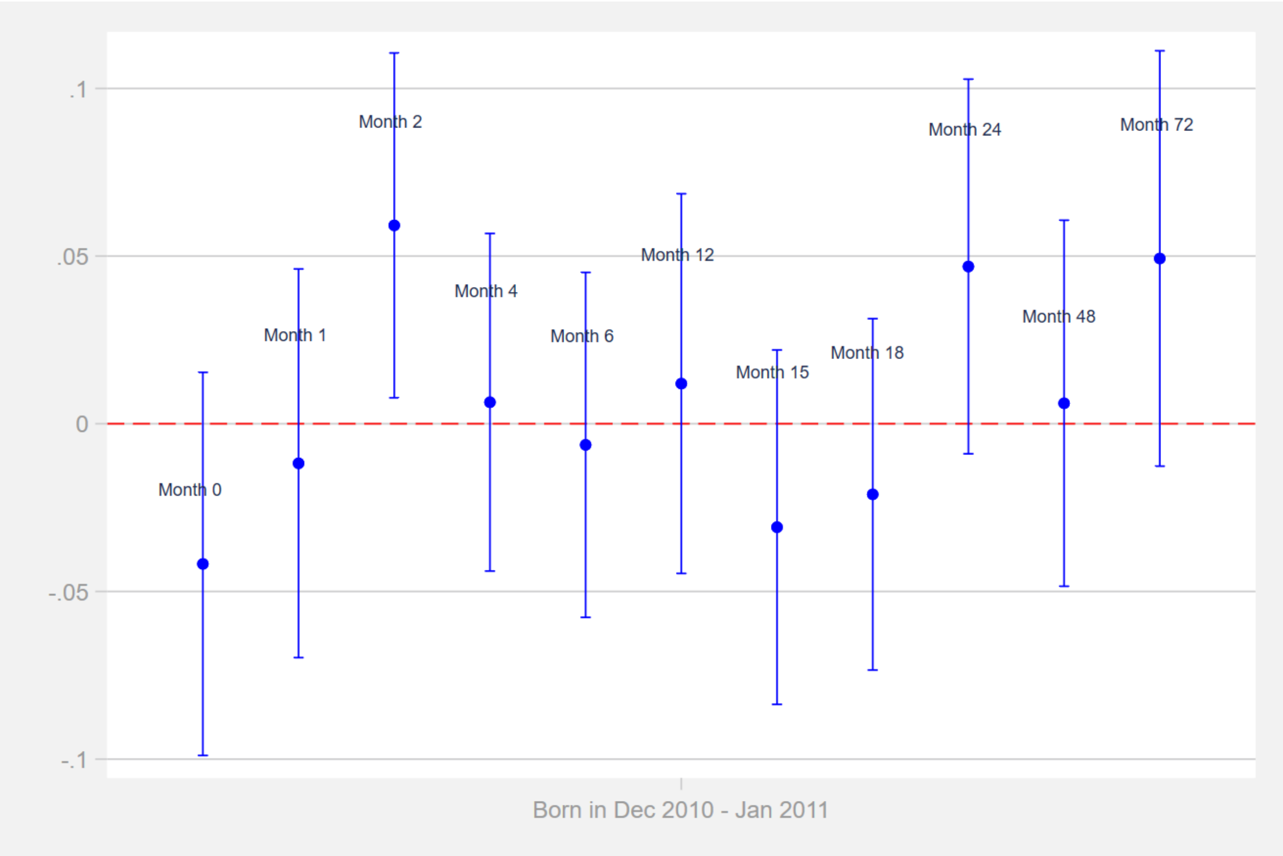
Source: Authors' calculations based on data from the 2008-2010 reports of voluntary pregnancy interruptions from the Spanish Ministry of Health

Figure A2: 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 A3. The effect of the benefit cancelation on BMI (standardized) 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	22.13***	30.70***	44.31***	44.10***	44.86***
	(5.782)	(7.288)	(7.723)	(8.244)	(9.352)
Number of births moved	343	307	310	221	157
Mean of outcome	224	225	221	215	213
Panel B: ln(Number of births)					
Dec. 2010	0.0999***	0.136***	0.188***	0.190***	0.193***
	(0.0250)	(0.0316)	(0.0342)	(0.0386)	(0.0483)
Share of births moved	5.3%	7.3%	10.3%	10.4%	10.6%
N	310	210	140	100	70
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
Year*Day of week	Yes	Yes	Yes	Yes	Yes

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 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	60.81***	86.00***	107.0***	135.6***	148.6***
	(14.45)	(16.80)	(21.10)	(23.10)	(33.85)
Number of births moved	943	860	749	678	520
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	0.0910***	0.130***	0.163***	0.206***	0.216***
	(0.0213)	(0.0247)	(0.0303)	(0.0327)	(0.0477)
Share of births moved	4.8%	7%	9%	11.4%	12%
N	310	210	140	100	70
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
Year*Day of week	Yes	Yes	Yes	Yes	Yes

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 A1.3: The effect of benefit cancellation on the timing of births (by gender): Spain and Catalonia

Panel A: Number of births (Spain)

	(Boys) ± 10 days	(Boys) ± 7 days	(Girls) ± 10 days	(Girls) ± 7 days
Dec, 2010	124.9***	142.1***	132.7***	136.6***
	(22.87)	(31.26)	(20.13)	(26.83)
Number of births moved	625	497	664	478
Mean of outcome	644	639	606	602

Panel B: Log of number of births (Spain)

	(Boys) ± 10 days	(Boys) ± 7 days	(Girls) ± 10 days	(Girls) ± 7 days
Dec, 2010	0.181***	0.204***	0.216***	0.216***
	(0.0305)	(0.0416)	(0.0329)	(0.0438)
Share of births moved	10%	11.3%	12%	12%
N	100	70	100	70

Panel C: Number of births (Catalonia)

	(Boys) ± 10 days	(Boys) ± 7 days	(Girls) ± 10 days	(Girls) ± 7 days
Dec, 2010	17.55***	22.57***	26.55***	22.29**
	(4.083)	(4.902)	(5.847)	(7.854)
Number of births moved	88	79	133	78
Mean of outcome	112	110	103	103

Panel D: Log of number of births (Catalonia)

	(Boys) ± 10 days	(Boys) ± 7 days	(Girls) ± 10 days	(Girls) ± 7 days
Dec, 2010	0.147***	0.186***	0.240***	0.202**
	(0.0368)	(0.0465)	(0.0561)	(0.0776)
Share of births moved	8%	10.2%	13.5%	11%
N	100	70	100	70
Year Dummies	Yes	Yes	Yes	Yes
Day of week dummies	Yes	Yes	Yes	Yes
Holiday dummy	Yes	Yes	Yes	Yes
Day of year dummy	Yes	Yes	Yes	Yes
Year*Day of week	Yes	Yes	Yes	Yes

Note: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each cell reports the coefficient on a December 2010 indicator (the month prior to the benefit cancellation) from equation (1). The unit of observation is a day. Panels A-B use all of Spain; Panels C-D use Catalonia. Columns split boys and girls. The dependent variable is the daily number of births (Panels A and C) or its log (Panels B and D). The sample includes, for each December-January pair from 2008-09 through 2012-13, only the last 10 (or 7) days of December and the first 10 (or 7) days of January, as indicated by the column header. All specifications include year fixed effects, day-of-week fixed effects, a public-holiday indicator, day-of-year fixed effects, and year×day-of-week interactions. "Number of births moved" and "Share of births moved" are the implied counts/shares of births shifted to December 2010 based on the estimated coefficients and the mean of the outcome. 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 alternative control years)

Panel A: Number of births

	(1) Full months	(2) ± 20 days	(3) ± 14 days	(4) ± 10 days	(5) ± 7 days
Dec, 2010	146.2***	192.3***	236.7***	291.5***	326.0***
	(23.59)	(27.81)	(34.25)	(40.01)	(50.70)
Number of births moved	2,263	1,923	1,657	1,458	1,141
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	0.111***	0.147***	0.181***	0.225***	0.249***
	(0.0174)	(0.0204)	(0.0245)	(0.0287)	(0.0345)
Share of births moved	6%	8%	10%	12.6%	14%
N	434	294	196	140	98
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
Year*Day of week	Yes	Yes	Yes	Yes	Yes

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)

A. Marital status, age and nationality

	(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. 1-10, 2010		-0.000624		0.0900*		0.0512		0.00809**		0.00375
		[0.00473]		[0.0524]		[0.0575]		[0.00391]		[0.00379]
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. 21-31, 2010		**-0.00316**		**0.165*****		**0.0621**		**0.0108*****		**0.00723***
		[0.00465]		**[0.0521]**		**[0.0569]**		**[0.00383]**		**[0.00371]**
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	1,417,986	1,417,986	1,416,994	1,416,994	1,388,951	1,388,951	1,417,986	1,417,986	1,417,986	1,417,986

B. Education

	(1) Mother graduate and above	(2) Mother graduate and above	(3) Father graduate and above	(4) Father graduate and above	(5) Mother secondary and above (Elementary Baccalaureate)	(6) Mother secondary and above (Elementary Baccalaureate)	(7) Mother secondary and above (Higher Baccalaureate)	(8) Mother secondary and above (Higher Baccalaureate)	(9) Father secondary and above (Elementary Baccalaureate)	(10) Father secondary and above (Elementary Baccalaureate)	(11) Father secondary and above (Higher Baccalaureate)	(12) Father secondary and above (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. 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]
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. 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]**
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

C. Gender and geography

	(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) Province Capital	(12) Province 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. 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]
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. 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]**
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 A4.1: 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	3,217.56	3,215.88	3,215.77	3,216.20	3,215.20
N	188,254	126,710	86,094	57,860	42,261

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	163,260	109,780	74,682	50,165	36,619

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	188,254	126,710	86,094	57,860	42,261

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	575,921	388,556	264,433	178,676	130,851

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	479,556	323,349	220,239	148,812	109,146

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	575,921	388,556	264,433	178,676	130,851

Note: Difference-in-difference estimation results for the sample of children born in Spain between October and March of 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 indicated in the panel titles. 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 A4.2: Health (at birth) outcomes (Heterogeneity)

Panel A: Child’s gender

	(1) Girls	(2) Boys
Weeks of gestation	-0.0610**	-0.0547**
	(0.0269)	(0.0272)
Mean of outcome	39.00	38.92
N	146608	155103
Birthweight below 2,500 g	0.00555	0.00399
	(0.00366)	(0.00330)
Mean of outcome	0.09	0.08
N	169688	179440

Panel B: Parental characteristics

	(1) At least one parent university graduate	(2) Neither parent university graduate
Weight in grams	-19.95***	-20.33***
	(7.502)	(6.307)
Mean of outcome	3210.56	3211.44
N	134020	215108
Weeks of gestation	-0.0417	-0.0681***
	(0.0294)	(0.0252)
Mean of outcome	38.94	38.97
N	119741	181970
Birthweight below 2,500 g	0.00436	0.00498
	(0.00387)	(0.00318)
Mean of outcome	0.08	0.08
N	134020	215108

	(1) Both parents Spanish	(2) At most one parent Spanish
Weight in grams	-22.37***	-14.01
	(5.463)	(10.21)
Mean of outcome	3190.33	3277.31
N	265758	83370
Weeks of gestation	-0.0914***	0.0563
	(0.0216)	(0.0410)
Mean of outcome	38.96	38.97
N	232695	69016
Birthweight below 2,500 g	0.00540*	0.00267
	(0.00286)	(0.00475)
Mean of outcome	0.09	0.07
N	265758	83370

Panel C: Municipality of residence characteristics

	(1) Population ≤ 20,000	(2) Population 20,001-100,000	(3) Population > 100,000/Provincial Capital
Weight in grams	-11.05	-16.62*	-26.58***
	(9.639)	(9.348)	(6.959)
Mean of outcome	3217.29	3226.01	3199.95
N	85774	92323	171031
Weeks of gestation	-0.0480	-0.0430	-0.0695**
	(0.0382)	(0.0365)	(0.0277)
Mean of outcome	39.01	39.01	38.91
N	74061	79835	147815
Birthweight below 2,500 g	-0.00000796	0.00947**	0.00438
	(0.00478)	(0.00475)	(0.00358)
Mean of outcome	0.08	0.08	0.09
N	85774	92323	171031

Note: Difference-in-difference estimates for children born in Spain between October and March of 2008-09 to 2012-13. We report coefficients from equation (2) on an indicator for being born in the 10-day window around the benefit cancellation date (last 10 days of December 2010 or first 10 days of January 2011). An observation is a child born on day d in year t.

Outcomes are birth weight (grams), weeks of gestation, and an indicator for low birth weight (<2,500 g). Results are shown by subgroup:

Panel A: Child's gender (girls; boys).

Panel B: Parental characteristics: education (at least one parent university graduate; neither parent university graduate) and parental nationality (both parents Spanish; at most one parent Spanish).

Panel C: Municipality of residence size (population ≤ 20,000; 20,001–100,000; ≥100,000 / provincial capital).

The estimation sample includes all children born in the last 10 days of October, December, and February and the first 10 days of November, January, and March, forming October–March sextuplets for 2008-09 to 2012-13. Robust standard errors are in parentheses. Significance levels: * p<.10, ** p<.05, *** p<.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: Diff-in-diffs estimation results for the sample of children born in October 2008-March 2009, Oct. 2009-March 2010, Oct. 2010-March 2011, Oct. 2011-March 2012, and Oct. 2012-March 2013. We report coefficients on a December 2010-January 2011 dummy (the weeks around the benefit cancellation date, 31 December 2010) from equation (2). An observation corresponds to a child born on day d in year t. The dependent variables are indicated in each panel title. The sample includes all births in the last 1-4 weeks of Oct., Dec., and Feb., and the first 1-4 weeks of Nov., Jan., and March (depending on the specification), for Oct.-Nov.-Dec.-Jan.-Feb.-March sextuplets from 2008–09 to 2012–2013. Robust standard errors 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)

A. Parents education

	(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 Seconda ry Educati on 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. 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]
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. 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]**
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	234,031	234,031	234,031	234,031	234,031	234,031	234,031	234,031	234,031	234,031	234,031	234031

B. Other characteristics

	(1) Parent 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. 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]
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. 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]**
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. 1-10, 2010			0.0226	0.0121
			(0.0156)	(0.0147)
Dec. 11-20, 2010			0.0352**	0.0334**
			(0.0151)	(0.0143)
Dec. 21-31, 2010			**0.0193**	**0.0119**
			(0.0154)	**(0.0145)**
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

Note: 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 A7.1: Effects on second grade test scores (binary grades by subjects)

A. Natural environment

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	-0.00683	0.00405	-0.0124	0.00157
	(0.0285)	(0.0147)	(0.0186)	(0.00634)
Dec. 11-20, 2010	0.0138	0.00540	0.0179	-0.00949
	(0.0275)	(0.0141)	(0.0176)	(0.00709)
Dec. 21-31, 2010	**-0.0646****	**-0.0356*****	**-0.0307***	**0.00168**
	(0.0270)	**(0.0133)**	**(0.0183)**	**(0.00670)**
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

B. Social and cultural environment

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	0.00177	0.00936	-0.00686	-0.000723
	(0.0283)	(0.0145)	(0.0184)	(0.00670)
Dec. 11-20, 2010	0.00587	0.000968	0.00955	-0.00465
	(0.0279)	(0.0140)	(0.0181)	(0.00679)
Dec. 21-31, 2010	**-0.0531***	**-0.0186**	**-0.0340***	**-0.000528**
	(0.0276)	**(0.0137)**	**(0.0183)**	**(0.00702)**
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

C. Artistic education

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	-0.0107	0.00618	-0.0177	0.000825
	(0.0258)	(0.0145)	(0.0177)	(0.00332)
Dec. 11-20, 2010	0.00384	-0.0108	0.0147	-0.000000190
	(0.0248)	(0.0133)	(0.0175)	(0.00363)
Dec. 21-31, 2010	**-0.0562****	**-0.0176**	**-0.0364****	**-0.00215**
	(0.0255)	**(0.0136)**	**(0.0180)**	**(0.00387)**
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

D. Physical education

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	-0.00455	-0.00704	-0.00125	0.00374
	(0.0261)	(0.0145)	(0.0178)	(0.00313)
Dec. 11-20, 2010	-0.0310	-0.0283**	-0.00305	0.000354
	(0.0243)	(0.0132)	(0.0173)	(0.00310)
Dec. 21-31, 2010	**-0.0356**	**-0.00777**	**-0.0296**	**0.00175**
	(0.0259)	**(0.0139)**	**(0.0181)**	**(0.00324)**
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

E. English as first foreign language

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	-0.00643	-0.0139	-0.00516	0.0126
	(0.0314)	(0.0147)	(0.0192)	(0.00946)
Dec. 11-20, 2010	0.0223	0.00721	0.0108	0.00430
	(0.0310)	(0.0145)	(0.0186)	(0.00959)
Dec. 21-31, 2010	**-0.0571***	**-0.0185**	**-0.0368***	**-0.00177**
	(0.0307)	**(0.0137)**	**(0.0189)**	**(0.0102)**
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

F. Catalan language and literature

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	-0.0273	-0.00238	-0.0194	-0.00551
	(0.0313)	(0.0127)	(0.0190)	(0.0120)
Dec. 11-20, 2010	0.0277	0.0197	0.0106	-0.00250
	(0.0303)	(0.0125)	(0.0181)	(0.0119)
Dec. 21-31, 2010	**-0.0725****	**-0.0121**	**-0.0445****	**-0.0159**
	(0.0295)	**(0.0114)**	**(0.0182)**	**(0.0125)**

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

G. Spanish language and literature

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	-0.0386	-0.0112	-0.0203	-0.00708
	(0.0294)	(0.0124)	(0.0192)	(0.00997)
Dec. 11-20, 2010	0.0157	0.0101	0.00551	0.000128
	(0.0292)	(0.0125)	(0.0184)	(0.00996)
Dec. 21-31, 2010	**-0.0496***	**-0.000497**	**-0.0309**	**-0.0181***
	(0.0295)	**(0.0120)**	**(0.0188)**	**(0.0108)**
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

H. Mathematics

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	0.0373	0.0225	0.0127	0.00212
	(0.0331)	(0.0146)	(0.0192)	(0.0115)
Dec. 11-20, 2010	0.0277	0.0159	0.0189	-0.00713
	(0.0321)	(0.0138)	(0.0186)	(0.0116)
Dec. 21-31, 2010	**-0.0824*****	**-0.0358*****	**-0.0265**	**-0.0200**
	(0.0310)	**(0.0124)**	**(0.0188)**	**(0.0123)**
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

I. Social and civic values

	(1) Grade	(2) Highest grade	(3) High pass	(4) Pass
Dec. 1-10, 2010	-0.0440	-0.0134	-0.0266	-0.00407
	(0.0306)	(0.0166)	(0.0201)	(0.00508)

Dec. 11-20, 2010	-0.00918	-0.00787	-0.00984	0.00853**
	(0.0285)	(0.0158)	(0.0192)	(0.00358)
Dec. 21-31, 2010	**0.00569**	**-0.000362**	**0.000110**	**0.00594**
	(0.0295)	**(0.0165)**	**(0.0194)**	**(0.00462)**
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 A7.2. Heterogeneity in education effects

Panel A: Child's gender

	(1) Girls	(2) Boys
Dec. 21-31, 2010	-0.126***	-0.0448
	(0.0474)	(0.0471)
Mean of outcome	0.1204	-0.1123
N	75,778	80,486

Panel B: Parental characteristics

	(1) At least one parent university graduate	(2) Neither parent university graduate
Dec. 21-31, 2010	-0.0941*	-0.0833*
	(0.0572)	(0.0426)
Mean of outcome	0.4265	-0.2459
N	57267	98997

	(1) Both parents Spanish	(2) At most one parent Spanish
Dec. 21-31, 2010	-0.0933**	-0.0535
	(0.0411)	(0.0571)
Mean of outcome	0.1854	-0.3430
N	101611	54653

Panel C: Municipality of residence characteristics

	(1) Low SES	(2) Medium SES	(3) High SES
Dec. 21-31, 2010	-0.0377	-0.0988*	-0.115*
	(0.0585)	(0.0544)	(0.0600)
Mean of outcome	-0.0314	0.0008	0.0382
N	55080	54769	46415

	(1) Population ≤ 20,000	(2) Population 20,001-100,000	(3) Population > 100,000
Dec. 21-31, 2010	-0.0903*	-0.0552	-0.107*
	(0.0542)	(0.0644)	(0.0562)
Mean of outcome	0.1251	-0.0267	-0.1230
N	58912	48706	48646

Note: The table reports split-sample estimates for children born 2008-2013 and their Grade-2 mean test scores (average across nine subjects) in Catalan primary schools. For each subgroup, we estimate the baseline specification separately and (for brevity) report the coefficient on an indicator for being born Dec. 21-31, 2010, the 10-day window around the benefit cancellation date (Dec. 31, 2010). The observational unit is child i born on day d in year t; the dependent variable is that child's standardised average test score. All models include week-of-birth fixed effects and the baseline controls (child gender; parental nationality; municipality socio-economic index; municipality population size), excluding the variable used to define the split in the corresponding panel. Subgroups are: Panel A - Girls vs. Boys; Panel B - At least one parent university graduate vs. Neither parent university graduate, and Both parents Spanish vs. At most one parent Spanish; Panel C - Municipality SES: Low / Medium / High, and Municipality population: ≤ 20,000 / 20,001-100,000 / >100,000. Reported "Mean of outcome" and N refer to each subgroup. Robust standard errors in parentheses. Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8.1. Primary care outcomes by gender

Panel A: Boys

	(1) Diagnosed (Respiratory disorders in years 1-10)	(2) Diagnosed (Respiratory disorders in year 1)	(3) Diagnosed (Bronchitis in years 1-10)	(4) Diagnosed (Bronchitis in year 1)	(5) Diagnosed (Bronchiolitis in years 1-10)	(6) Diagnosed (Bronchiolitis in year 1)
Dec'10-Jan'11 born	0.315^{*}	0.0381	0.0861^{***}	0.00894	0.0144	0.0136
	(0.176)	(0.0393)	(0.0328)	(0.00865)	(0.0111)	(0.00864)
Mean of outcome	6.91	1.27	0.60	0.10	0.19	0.13
N	57848	57848	57848	57848	57848	57848

Panel B: Girls

	(1) Diagnosed (Respiratory disorders in years 1-10)	(2) Diagnosed (Respiratory disorders in year 1)	(3) Diagnosed (Bronchitis in years 1-10)	(4) Diagnosed (Bronchitis in year 1)	(5) Diagnosed (Bronchiolitis in years 1-10)	(6) Diagnosed (Bronchiolitis in year 1)
Dec'10-Jan'11 born	-0.107	-0.00615	-0.0159	-0.00335	0.00872	0.00988
	(0.167)	(0.0368)	(0.0290)	(0.00700)	(0.00917)	(0.00726)
Mean of outcome	6.56	1.15	0.49	0.06	0.14	0.10
N	54988	54988	54988	54988	54988	54988

Difference-in-difference estimation results for children in the BIFAP database born between October 2008 and March 2013. The sample is split by sex (Panel A: boys; Panel B: girls). We report coefficients from equation (2) on an indicator for being born in the last week of October, December, or February and the first week of the adjacent month (November, January, or March), i.e., the weeks surrounding the 31 December 2010 benefit cancellation date. An observation is a child born on day d in year t. The dependent variables are the number of primary-care diagnoses of: (1) respiratory disorders from ages 1-10, (2) respiratory disorders in year 1, (3) bronchitis from ages 1-10, (4) bronchitis in year 1, (5) bronchiolitis from ages 1-10, and (6) bronchiolitis in year 1. The sample includes all babies in the BIFAP database born in those one-week windows (last week of Oct/Dec/Feb and first week of Nov/Jan/Mar) for October-March sextuplets from 2008-09 to 2012-13. Robust standard errors are in parentheses. Significance levels: * p<0.10, ** p<0.05, *** p<0.01. Source: BIFAP database. All diagnoses included.

Table A8.2. Visits to Primary care

Panel A: Visits to Primary Care

	(1) Full months	(2) ±28 days	(3) ±21days	(4) ±14 days	(5) ±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 A8.3. Prescriptions 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 A9.1: Health outcomes (birth weight) using education specification

	(1) Weight in Grams	(2) Birth weight below 2500g
Dec. 1-10, 2010	-5.474	0.00417
	(5.352)	(0.00264)
Dec. 11-20, 2010	2.781	-0.00132
	(4.911)	(0.00240)
Dec. 21-31, 2010	**-14.71*****	**0.00193**
	(5.177)	**(0.00257)**
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	3,216.6578	0.0768
N	2,710,898	2,851,896

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 (Primary care) using education specification

	(1) Diagnosed (Respiratory disorders in years 1-10)	(2) Diagnosed (Respiratory disorders in year 1)	(3) Diagnosed (Bronchitis in years 1-10)	(4) Diagnosed (Bronchitis in year 1)	(5) Diagnosed (Bronchiolitis in years 1-10)	(6) Diagnosed (Bronchiolitis in year 1)
Dec. 4-10, 2010	-0.0621	0.0487	0.0129	0.00438	-0.0174**	-0.0128**
	(0.143)	(0.0321)	(0.0250)	(0.00645)	(0.00824)	(0.00628)
Dec. 11-17, 2010	0.106	-0.00617	0.0361	0.00217	-0.000800	-0.00475
	(0.136)	(0.0300)	(0.0268)	(0.00635)	(0.00846)	(0.00641)
Dec. 18-24, 2010	0.129	0.0108	0.0535*	0.00557	0.00954	0.00315
	(0.137)	(0.0304)	(0.0278)	(0.00667)	(0.00803)	(0.00619)
Dec. 25-31, 2010	**0.0526**	**0.0600****	**0.0118**	**-0.0000649**	**0.0176****	**0.0146****
	(0.138)	**(0.0300)**	**(0.0255)**	**(0.00629)**	**(0.00844)**	**(0.00644)**
Jan. 1-7, 2011	**0.0379**	**0.0236**	**0.0414**	**0.00625**	**0.00221**	**0.00509**
	(0.141)	**(0.0320)**	**(0.0257)**	**(0.00657)**	**(0.00783)**	**(0.00628)**
Jan. 8-14, 2011	0.000870	0.0231	0.0282	0.0191***	-0.00368	-0.00877
	(0.144)	(0.0311)	(0.0288)	(0.00699)	(0.00740)	(0.00575)
Jan. 15-21, 2011	-0.0938	-0.00882	-0.0320	-0.000857	0.00293	0.00636
	(0.136)	(0.0297)	(0.0234)	(0.00634)	(0.00812)	(0.00637)
Jan. 22-28, 2011	0.0693	0.0651**	0.0177	0.0134**	-0.0139*	-0.0104*
	(0.142)	(0.0314)	(0.0257)	(0.00673)	(0.00818)	(0.00607)
Mean of outcome	6.84	1.22	0.55	0.09	0.18	0.13
N	1,201,280	1,201,280	1,201,280	1,201,280	1,201,280	1,201,280

Note: Estimation results for the sample of children born in 2008-2013. We report coefficients on dummies for 7-day intervals from December 4, 2010 to January 28, 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 the number of times the child was diagnosed with: respiratory disorders from age 1–10 years (Col. 1), respiratory disorders in year 1 (Col. 2), bronchitis from age 1–10 years (Col. 3), bronchitis in year 1 (Col. 4), bronchiolitis from age 1–10 years (Col. 5), and bronchiolitis in year 1 (Col. 6). 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: BIFAP database

Table A9.3: Health outcomes (Hospitalization) using education specification

	(1) Hospitalised (any cause)	(2) Respiratory disease	(3) Mental and behavioural disorder	(4) Pneumonia	(5) Bronchitis	(6) Asthma
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)
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. 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)**
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

Note: 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.