The Effect of an Income Shock at Birth on Child Health: Evidence from a Child Benefit in Spain

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Very preliminary!

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Abstract: Family income has been shown to be strongly associated with child health and development, in different countries and time periods. However, the extent to which this relationship is causal has been hard to establish. I take advantage of the unexpected introduction of a generous child benefit in Spain in 2007 to analyze the effect of a (transitory) shock to household income right after birth on child health outcomes. I follow a regression discontinuity approach, comparing children born in a close neighborhood of the threshold date for benefit eligibility (which was unknown in advance), from birth to age 5. I use administrative data from birth and death certificates, as well as hospital records. My contribution relies on a credible and clean identification strategy, combined with high-quality administrative data, for a type of subsidy that is common in many countries (a "maternity allowance"). I find no significant effect of the subsidy on neonatal, infant or child mortality. However, children whose mothers were eligible for the transfer had significantly higher hospitalization rates in the months and years following benefit receipt. I study potential channels that can explain this result, and conclude that the child benefit most likely did not improve the health of eligible children, but it did increase their health care utilization. Eligible mothers took longer to go back to work after birth, which may have improved detection and treatment of child health problems.

JEL Codes:
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1. Introduction

I study how the introduction of a generous "maternity bonus" in Spain affected child health, from birth to age 5. I exploit the unexpected introduction of the benefit to perform a regression discontinuity analysis, where I compare child health over time in a narrow neighborhood of the eligibility threshold, which was based on date of birth. I use administrative data from birth and death certificates and hospital records, which I supplement with a variety of survey data. I find that children whose mothers received the benefit were not significantly different from control ones in terms of health at birth (or shortly thereafter), but they soon suffered higher overnight hospitalization rates. I explore several potential channels, and conclude tentatively that child health was unaffected by benefit receipt (on average), but health care utilization was higher in treated families, possibly as a result of lower maternal labor force participation.

There is mounting evidence that early interventions and shocks (in utero and shortly after birth), even mild ones, can have important long-term effects on health, human capital development, and adult earnings (Almond et al. 2017). Not so much is known about the "middle years", i.e. the effects of early shocks on child outcomes, and how those translate into adult ones, as well as the relevant mechanisms.

Among the most common public policies aimed at improving child well-being are transfers to families with children. All OECD countries have some form of subsidies that target families with young children, spending on average 2.4% of GDP on family benefits (OECD Family Database). The goal of these subsidies is typically to prevent fertility rates from falling (further) below the replacement rate, and to ensure a minimum standard of living for all children, thus supporting child health and well-being. The effectiveness of these types of policies on both dimensions has been hard to demonstrate.

A few recent papers have examined the effects of (conditional) cash transfers early in life on health outcomes at birth and in the long-term. Hoynes, Miller, and Simon (2015) examine the effects of the U.S. Earned Income Tax Credit Program (EITC), finding reductions in the incidence of low birth weight among mothers who benefited from a benefit expansion while pregnant. Almond, Hoynes, and Schazenbach (2011) find that the roll-out of a near-cash benefit in the US (Food Stamps) increased birth weight, especially among African Americans. Regarding long-term health effects, Hoynes, Schazenbach, and Almond (2016) find that the rollout of Food Stamps reduced the adult incidence of metabolic syndrome (i.e., obesity, high blood pressure, diabetes, etc.). The available evidence thus shows significant effects of cash transfers on outcomes at birth and in the long-term. Surprisingly, we know little regarding intermediate outcomes, and thus whether the long-term effects on health are driven or mediated by childhood health.

A handful of recent papers have analyzed the effects of public transfers during childhood on child health and development. Milligan and Stabile (2009, 2011) study the impact of public transfers in Canada, while Gaitz and Schurer (2017) study the effects of the introduction of a child benefit in Australia. These papers use survey data (with small sample sizes), follow a difference-in-differences approach, and rely on parental-reported measures of child health. Instead, I exploit high-quality administrative data, which allows me to follow a regression discontinuity design, thus arguably providing unbiased and precise estimates of the effects of interest. Moreover, I focus on a cash transfer paid immediately after birth, tracing out its effects over time, versus previous studies that look at contemporaneous effects.

There is also a broader literature, spanning several disciplines, that has studied the effect of family income on child health and development. Yeung et al. (2002) are often cited in their discussion of the mechanisms via which income can affect child outcomes: the "resources" channel, the "family process" channel (parental stress), and the time allocation channel (parental labor supply).

Beyond documenting associations, as in Case et al. (2002), some recent papers in economics have attempted to identify causal effects, by exploiting some exogenous source of variation in family income. Kuehnle (2014) constructs an instrument based on local labor market conditions, while Coti and Simon (2015) exploit stock market fluctuations. Cesarini et al. (2015) use lottery winners in Sweden. They find no evidence that winning the lottery has positive health effects on children, although they do find an increase in health care utilization, measured with hospitalization rates.[1]

I contribute to this literature with a well-identified study on the effects of a one-time large transfer (a child benefit or mother's allowance) at the time of birth on child health (ages 0 to 5). Compared with Cesarini et al. (2015), I exploit a more policy-relevant income shock (the receipt of public transfers), versus lottery winning, arguably a very different type of shock to unearned income.

I argue that the specific way in which a universal child benefit was introduced in Spain in 2007 makes it possible to use it as a "natural experiment" to study the impact of a lump-sum, generous "maternity bonus" on child outcomes. In a national speech on July 3, 2007, the Spanish president announced (unexpectedly) the introduction of a new unconditional family benefit, which would pay €2,500 to all mothers, immediately after giving birth. The subsidy would be paid for children born from July 1, 2007 on.

This setup suggests that one can use the discontinuity generated by the birth date cutoff for benefit eligibility to evaluate the effect of the subsidy (an unexpected,

[1] There is also a literature on the effects of income shocks on adult health, see for example Adda et al. (2014), as well as studies on the link between macroeconomic fluctuations and (adult and infant) health, see Ruhm (2013) and Dejehia & Lleras-Muney (2004).

transitory income shock) on children and their families. I show that children born right before and right after July 1, 2007 come from families who are identical (on average) along a range of observable characteristics. However, only those with children born in July received the new allowance, so that benefit eligibility can be seen as quasi-randomly assigned, based on whether the delivery fell on one side or the other of the threshold (which was not known at the time). Thus, families having the child on July 1 (and onwards) can be seen as the "treated group", while those who delivered in late June are the "control group". Following the children in both groups of families over time allows me to study any impact of the benefit on child outcomes.

I first show that treated and control children were the same at the time of birth (on average) in terms of a range of measures of newborn health. Then, I construct measures of health at later ages, up to age 5 (so far). I construct child mortality rates (an extreme measure of health) using death-certificate data, which include exact date of birth. I find no significant effect of the subsidy on neonatal, infant or child mortality. Hospital records allow me to construct hospitalization rates by age and detailed diagnosis. I find that children whose mothers were eligible for the transfer had significantly higher hospitalization rates in the months and years following benefit receipt. I then study potential channels that can explain this result. I am able to rule out several possible mechanisms, and hypothesize that benefit recipients may have increased their use of hospital services, without an actual worsening of the children's health, possibly due to increased maternal time with the child leading to higher detection and testing, and more intensive treatment. I do not find evidence that the child benefit improved the health of eligible children, but it did increase their health care utilization.

The remainder of the paper is organized as follows. Section 2 describes the identification strategy. Section 3 introduces the data sources and presents some descriptive statistics. I describe the main results in section 4, while section 5 explores several alternative mechanisms. Section 6 concludes.

2. Identification strategy

The benefit introduction lends itself to a sharp regression discontinuity design. The treatment variable is an indicator for children born on or after July 1, 2007, while the running variable is the date of birth. Thus, I compare the health outcomes of children born a few days before and after July 1, 2007. Those born after that date received the baby bonus, which may have affected their health status, although probably with some delay. The benefit introduction (announced on July 3) was unexpected, and the first checks were sent in November 2007, i.e. 5 months after the announcement. The specification is the following:

(1) $$H_{id} = \alpha + \beta \cdot post + \gamma_1 d + \gamma_2 (d \cdot post) + \Pi \mathrm{X}_{\mathrm{im}}^{'} + \varepsilon_{im}.$$

H is a measure of the health status of child i who was born on day d. Day of birth d (the "running" variable) is normalized to zero at the threshold (July 1, 2007). The main parameter of interest, β, captures any potential discontinuity or "jump" in H at the cutoff. The identifying assumption is that no other factor affected families with children born on or after July 1, 2007 discontinuously. I do allow for a smooth trend (a polynomial) in date of birth, which is allowed to change at the threshold.

In practice, I estimate all regressions aggregated at the date of birth level, so that the unit of observation is the date of birth (one observation is one day). I use five different samples, which include 1, 2, 3, 4 or 8 weeks before and after the cutoff date (July 1), so the number of observations in the regressions is always between 14 and 112 (days).

The main outcome variable is the count of overnight hospital stays (or its log) when the children were younger than 5 years of age. We then disaggregate hospital stays by age of the child (0-1 month, 2-23 months, and 2-5 years), and by main diagnosis associated with the stay. We also study mortality as an additional, extreme measure of health.

3. Data and descriptive statistics

I use three main sources of data. The main measures of health status are derived from the Hospital Morbidity Survey (2006-2014). This data set provides individual-level information on the universe of overnight hospital stays in Spain, with information on the age of the patient, the main diagnosis associated with the hospitalization, and some demographic characteristics (sex, province of residence). Diagnoses are provided at the 3-digit level, grouped in 17 "chapters", following the International Classification of Diseases (ICD-9-CM). For the main analysis, I select information on all hospital stays for children born within the 16 weeks surrounding the benefit threshold date, when they were ages 0 to 5.

In order to test for the validity of the RDD approach, I also use birth-certificate data for 2007. This data source provides individual-level data on all registered births in Spain, with rich demographic information as well as measures of health at birth, such as weeks of gestation and birth-weight. I also use death-certificate data (2006-2014) to analyze effects on child mortality.

Descriptive statistics for the main sample (the 8-week window around the threshold) are provided in Table 1. During the period of analysis (May-August 2007), there were about 1,300 births per day in Spain (first panel). By age 5, there were about 800 overnight hospital stays per day of birth (second panel), a hospitalization rate of 61% (note that we cannot separate the intensive from the extensive margin, since we do not have individual identifiers in the hospital data).

Out of the 17 groups of diagnoses or ICD "chapters", I analyze separately the nine most common, and group the remaining ones (less than 6% of all hospital stays) as "Rest of diagnoses". The most common groups of diagnoses in the sample are: perinatal conditions, respiratory disease, and infections. I also look separately at hospitalizations that end with no diagnosis, or that are reported as associated with "other factors" (check-ups, suspected infectious disease, etc). I then split hospital stays by age of the child so that each age group has about the same number of hospitalizations (under 1 month, 2 to 23 months, and 2 to 5 years). Hospital stays are very common in the first month of life, and become less common as children age.

Child mortality rates are very low. I observe about 3.5 deaths per date of birth during the first month, equivalent to close to 2.6 deaths per 1,000 births (3.45/1,328). The infant mortality rate (during the first year) is 3.8 per 1,000, and it reaches 4.6 by age 5.

4. Results

4.1 Validity checks

I perform two sets of validity checks. First, I check for bunching at the threshold. If the benefit introduction was known in advance, families could have reacted by postponing the delivery date, in order to become eligible for the new subsidy (as in Gans & Leigh 2007). In this specific case, birth timing reactions are unlikely since the benefit was not announced in advance. In any case, I run regressions of the form of equation (1), where the dependent variable is the daily number of births (or its log). The results are reported in the first two rows of Table 2. I find no evidence of a discontinuous jump in the number of births around the benefit introduction date, with small and insignificant coefficients in all specifications.

Second, I check for balance in covariates around the cutoff date. The identification strategy relies on the assumption that children born right before and right after the cutoff are similar (on average) in both observable and unobservable dimensions. Thus, we should observe no discrete jumps in health at birth or family characteristics at the threshold, once we control for a polynomial in date of birth.

Table 2 reports the results of estimating equation (1), using as outcome variables a range of measures of health at birth, as well as the circumstances surrounding the delivery, and mother characteristics. We find no significant discontinuities in the fraction of multiple births, birth-weight, or 24-hour mortality. There is weak evidence that children born after July 1 were less likely to be premature (had higher weeks of gestation), although the coefficient is only significant (at 90%) in one out of the five specifications. Eligible children were no more likely to be born via c-section or at home (versus a health facility), although we find weak evidence that their deliveries had more complications. There are no important differences in the age of the mothers on both sides of the cutoff. Overall, out of 60 estimated coefficients, only 2 are significant at a 95% confidence level, which leads me to conclude that the children born on both sides of the threshold date are balanced in terms of covariates at the time of birth, thus supporting the RDD identification assumption.

4.2 Main results on hospitalizations and mortality

The main health results are reported in Table 3. The first row shows the estimated average treatment effect in the five different samples, for the number of overnight hospital stays by age 5, including all diagnoses. The first specification, where I include only children born within 7 days on each side of the threshold, does not control for any trend in date of birth. The following three specifications (which include children born 2, 3 and 4 weeks around the cutoff) control for a linear trend in date of birth, and the last one (for the 8-week window) includes a second-order polynomial.

The coefficient in the first column shows that the children born in the first week of July of 2007 suffered about 82 additional hospitalizations (per day of birth) by age 5, compared with children born in the last week of June, or about 10% more. This finding is fairly stable across specifications, reaching 87 in the final one. The second row of Table 3 presents the results for hospital stays by age 5 in logs. The results confirm that eligible children had about 10 log-points more hospitalizations. The effect is marginally significant at standard confidence levels, and it allows us to reject that benefit eligibility reduced hospital stays by age 5 by more than 1 log-point.

This result is illustrated in Figure 1, which shows the number of hospital stays by age 5, aggregated by week of birth, for children born in 2007. I also display linear fits on both sides of the threshold birth date (July 1). While children born in the weeks immediately preceding the benefit introduction experienced about 5,500 hospitalizations (per week of birth) by age 5, those born in the first two weeks of July suffered more than 6,000.

Hospital stays by age

I then analyze hospitalization effects by age of the children. I find no significant difference in hospitalizations (in logs) when the children were under one month of age (third row of Table 3), in any of the specifications, and the coefficients are all below 4 log-points. This is not surprising given that the benefit was paid only months after birth (even though families could conceivably have reacted immediately after the announcement, in anticipation of the expected income shock).

A positive effect on hospitalizations is present in the second age group (age 2 months to 2 years), and even more strongly at ages 2 to 5 years, as illustrated in Figure

2. The final specification suggests that eligible children were 12 log-points more likely to be hospitalized at ages 2 to 23 months (19 points at 2-5 years). The results thus suggest that benefit receipt led to an increase in overnight hospital stays among eligible children.

Placebo tests

If the observed positive "jump" in hospital stays is causally driven by the benefit, we should observe no similar discontinuity among children born on the same dates, in the surrounding years. None of the children born in June and July of 2006 were eligible for the benefit, while all of those born in June-July of 2008 were. Table 4 shows the results of estimating equation (1) for the sample of children born close to July 1 of 2006 and 2008, as "placebo" tests. I find that children born right after July 1 were not significantly more likely to be hospitalized by age 5 than children born before, in either the 2006 or the 2008 sample. The coefficients are small and mostly negative in the sample of children born in 2006, while they are positive but imprecisely estimated for 2008. In the first (last) specification, the coefficient for the 2007 sample was 0.099 (0.104), versus 0.031 (0.067) for 2008.

Hospitalizations by main diagnosis

Next, I study the hospitalization effects by diagnosis. Table 5 shows the results separately for the main diagnosis groups. Even though perinatal disorders are the most common diagnosis in the sample, there is no difference in their incidence on both sides of the threshold, again consistent with the benefit being paid months after birth (and families not reacting immediately to the announcement). The three diagnoses with the largest coefficients (in both the first and the last column) are: respiratory disease, "rest of diagnoses", and "other factors". Digestive disease and infections also have sizeable coefficients in some specifications.

According to the results in the last column of Table 5, almost one quarter of the overall increase in hospitalizations can be attributed to respiratory disease, about a fifth to “rest of diagnoses”, and one sixth each to “other factors” and infectious disease.

Child mortality

I also analyze mortality as an extreme health outcome (as well as a potential source of selection in the sample of hospital stays). Table 6 shows the results from the mortality specifications. The outcome variables are now the number of deaths by age one month, one year, and five years (neonatal, infant and child mortality, respectively), by date of birth. The first row, for neonatal mortality (in levels), shows positive but insignificant effects, as does the second one for infant mortality. Mortality by age 5 is slightly higher among children who were eligible for the benefit, and this effect is statistically significant in some specifications.[2]

The results are confirmed in the analysis in logs. I find no significant effect on mortality shortly after birth, but some evidence of significantly higher mortality at older ages. Child mortality is between 30 and 60 log-points higher in the treated group. This result is illustrated in Figure 3.

4.3 Heterogeneity analysis

The results so far strongly suggest that the child benefit did not improve, and may have worsened, average health outcomes for the affected children, as measured by overnight hospital stays and child mortality. It is conceivable that a zero average effect is hiding positive effects for certain sub-groups, such as the lower-income segment of the population. Hospital records do not provide information on household income, but I can use geographic information (province of residence) to stratify the analysis by average

[2] I also run placebo mortality specifications in the samples of children born around July 1 of 2006 and 2008. I find no “effect” for 2006, and negative coefficients. However, for 2008 coefficients are all positive and some even significant, which casts doubt on the main mortality effect estimated for the 2007 sample.

income in the region. I split the 52 Spanish regions into below and above per capita income in 2007, and estimate separate regressions for the two subgroups. The results are presented in Table 7. The coefficients are slightly larger, and more precisely estimated, in the lower-income sample, where we see significant increases of 14-15 log-points in hospital stays among children eligible for the benefit. Thus, we can rule out significant average health improvements as a result of the child benefit in the poorer regions. *(mention no significant differences by gender)*

5. Mechanisms

In this section I spell out the potential mechanisms via which an unexpected shock to household income shortly after birth can affect child health, and I do my best to test for them empirically.

The literature (refs) has suggested that household income can improve child health and development via three main channels: i) Higher ***resources*** can result in more/higher-quality material inputs (nutrition, heating, health care, quality and safety of toys, furniture, household appliances, etc); ii) An income effect could reduce ***parental labor supply*** and thus increase parental time inputs (including possibly breastfeeding), and iii) Higher income may have positive effects on ***parental stress*** and conflict (the "family process" channel), affecting children's emotional and psychological well-being (as well as potentially the incidence of violence and abuse).

All of these channels would suggest that a positive income shock should improve child health. Since I find that the child benefit, if anything, led to more hospitalizations, these mechanisms cannot rationalize the results. There are several alternative channels that could conceivably lead to an unexpected income shock worsening child health.

First, a recent literature has found significant negative "payday" (and lottery) effects on health (refs), as well as a negative correlation between the unemployment rate and (adult and child) health (refs). Positive income shocks may lead to an increase in unhealthy behaviors (such as driving, drinking or smoking), which may in turn worsen health outcomes.

An income shock may also have increased the demand for additional children, if they are normal goods. Parents eligible for the benefit may have increased subsequent fertility, which may have resulted in lower investments per child, as predicted by Becker's quantity-quality model of fertility.

Finally, parents may have increased their use of medical services (including hospital stays) without an actual worsening of children's health status. For example, they could have used the additional income to purchase private health insurance, and private health centers may have different criteria for admitting children overnight. Alternatively (or additionally), eligible mothers, who stayed home longer after birth, may have been more likely to make use of medical services (given a certain condition or symptom), due to better detection, treatment, or testing.

I test for the presence of these different channels to the extent possible.

Household expenditures

Regarding material inputs, a recent paper (González 2013) found that the Spanish child benefit did not result in significant changes in household expenditure patterns during the year following birth (including no increase in "child-related" expenditures).

I follow the same approach as González (2013) to analyze specific expenditure categories, based on the channels mentioned above. I use the Spanish Household Budget Survey for 2008, and estimate equation (1), using household expenditure (in different categories) as the outcome variable, and month of birth of the child as the running variable (since the exact date of birth is not available). (The results are reported in Appendix Table 1.) I find that families that were eligible for the benefit did not spend more on any behaviors that may have been harmful for the health of the children: alcohol, tobacco or drugs, transportation (or gas specifically), or lotteries, gambling and betting. I also find no discontinuity in health-related expenditures, or on private health insurance specifically. However, the sample size is small, which makes the results fairly imprecise.

Maternal time and breastfeeding

González (2013) also found that treated mothers took longer to return to the labor market after birth, and their children started daycare slightly later. This may have led to longer breastfeeding and lower exposure to infectious disease at early ages, which would predict improved health in the first year. I use the National Health Survey for 2011 to test for some of these potential effects. The results are reported in Appendix Table 2. The evidence suggests that treated children were more likely to be breastfed for at least 6 months. Again, I find no evidence that eligible families were more likely to purchase private health insurance.

Family stress and conflict

In order to test for any potential effects of benefit receipt on family conflict, I analyze the effect of eligibility on parental separation or divorce. I use data from the Labor Force Survey to estimate equation (1), again using month of birth as the running variable. The results are reported in appendix Table A3. I find that benefit receipt is associated with lower parental separation rates in 2008. However, by 2009 eligible families have split to the same extent as non-eligible ones, suggesting the benefit had short-lived positive effects on family stress or conflict.

Subsequent fertility

I use birth-certificate data for the universe of registered births in Span to test for potential effects of the benefit on subsequent fertility. The dependent variable in equation (1) is now a set of indicators for whether a mother who had a child on date t (close to the threshold of July 1, 2007) had another child within the next 2, 4 or 6 years (aggregated at the day level). This exercise would capture the effect of an unexpected income shock on subsequent fertility. The results are presented in appendix table x. I find no significant fertility effects of the benefit introduction.[3] Thus, we can rule out that fertility effects are driving the negative effects on child health.

Childcare

Starting childcare later may have negative health effects? (references) Baket et al. 2008, Bettina Siflinger's paper.

Parental attention- detection, testing and treatment

Is the increase in hospital stays driven by better detection and treatment or disease, or to a higher incidence of health problems? This is hard to answer. Our results in Table 5 do suggest that a non-negligible fraction of the increase in hospital stays can be attributed to visits with no associated diagnosis ("other factors", i.e. stays associated with observation of the patient, testing, or suspected disease, with no actual medical diagnosis reported by the end of the stay). However, we also find a significant increase in hospitalizations for respiratory disease. Since only a small fraction of children with respiratory disease (bronchitis is the most frequent) are hospitalized, it is hard to rule out (or confirm!) that eligible families experienced higher hospitalization rates without an accompanying increase in the incidence or severity of the underlying condition. *(NHS results suggest no worsening of parental-reported child health)*

[3] Note that this exercise isolates the income effect from the price effect of the subsidy. González (2013) found that fertility increased after the benefit introduction, and attributes the effect to the benefit reducing the "price" of children. All children born after June 2007 would be eligible for the benefit. The only difference between June and July 2007 children is that mothers who gave birth in July received the benefit, but both June and July mothers would be eligible for the subsidy if they had an additional child within the following three and a half years (since the benefit was cancelled in December 2010).

6. Conclusions

Summing up: We find that benefit receipt did not lead to significant improvements in child health, in spite of higher breastfeeding and lower parental separation rates (in the short-term). Better parental-reported health, but more hospitalizations. I rule out several possible mechanisms, and hypothesize that benefit recipients may have increased their use of hospital services, without an actual worsening of the children's health, possibly due to increased maternal time with the child leading higher detection and testing, and more intensive treatment.

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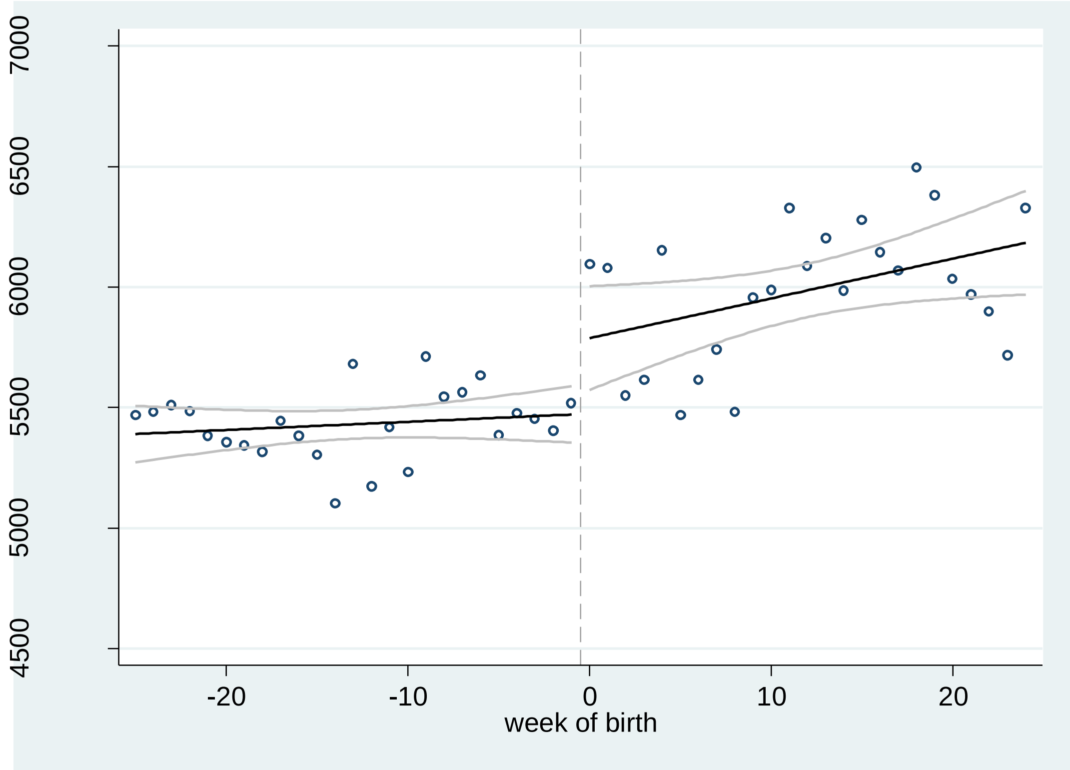
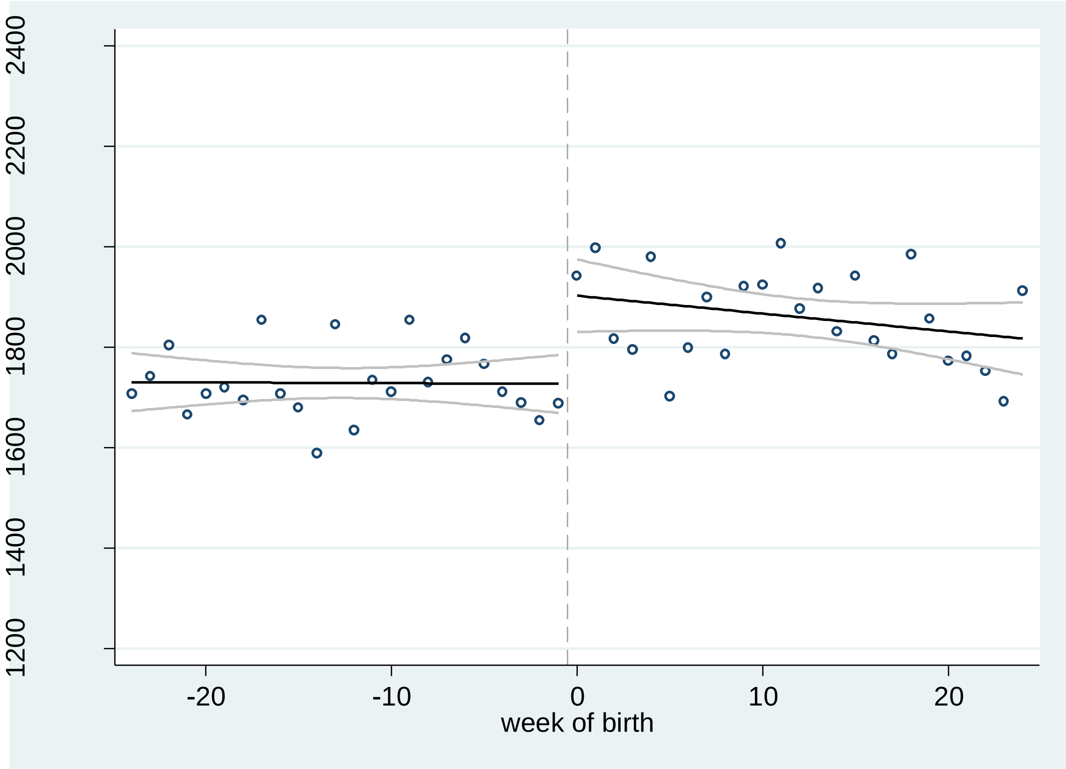
Figure 1. Number of overnight hospital stays by week of birth (ages 0-5)



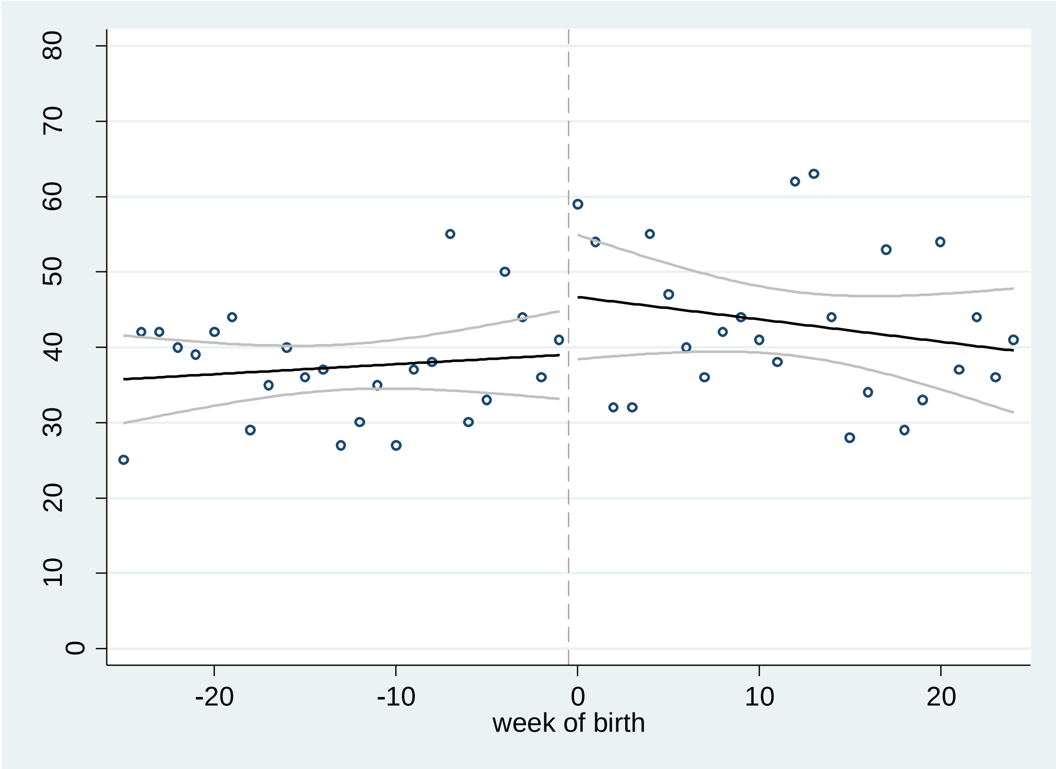
Source: Hospital Morbidity Survey (2006-2014). Week 0 corresponds to children born between July 1 and July 7 of 2007, week -1 refers to children born on June 24-30 of 2007, and so on. The solid lines are linear fits (with 95% confidence intervals).

Figure 2. Number of overnight hospital stays by week of birth (ages 2-5)



Source: Hospital Morbidity Survey (2006-2014). Week 0 corresponds to children born between July 1 and July 7 of 2007, week -1 refers to children born on June 24-30 of 2007, and so on. The solid lines are linear fits (with 95% confidence intervals).

Figure 3. Effect of the benefit on child mortality (age 0-5)



Source: Death registers (2006-2014). Week 0 corresponds to children born between July 1 and July 7 of 2007, week -1 refers to children born on June 24-30 of 2007, and so on. The solid lines are linear fits (with 95% confidence intervals).

Table 1. Descriptive statistics

	Mean	**St.dev.**	**Min**	**Max**
Birth-certificate data				
Number of births	1328	145	1,003	1,538
Multiple birth	0.0197	0.1388	0	1
Weeks of gestation	39.1	1.9	20	45
Prematurity	0.0752	0.2637	0	1
Birth-Weight	3,227	527	500	6500
Low birth-weight	0.0707	0.2563	0	1
Very low birth-weight	0.0081	0.0899	0	1
Mortality 24h.	0.0006	0.0251	0	1
Complications	0.1219	0.3272	0	1
C-section	0.2589	0.4380	0	1
Home birth	0.0042	0.0649	0	1
Mother>35	0.1870	0.3899	0	1
Mother>39	0.0383	0.1919	0	1
Hospital data (levels)				
All stays, age 0-5 years	806	101	573	1105
Respiratory disease (VIII)	150	25	93	237
Perinatal (XV)	190	28	119	241
Digestive (IX)	57	11	31	90
Not well-defined (XVI)	58	11	36	90
Infections (I)	116	17	77	172
Congenital anom. (XIV)	49	11	27	85
Injuries (XVII)	33	7	20	67
Urinary (X)	32	8	16	53
Nervous (VI)	31	8	14	53
Rest of diagnoses	46	13	21	90
No diagnosis	8	4	1	23
Other factors	44	10	23	81
All stays, age 0-1 month	295	34	185	359
All stays, age 2-23 months	264	41	174	423
All stays, age 2-5 years	257	38	169	382
All stays, poor provinces	292	40	205	393
All stays, rich provinces	514	69	368	713
Death-certificate data				
First month mortality	3.45	2.05	0	8
First year mortality	5.02	2.63	0	11
Five-year mortality	6.09	3.01	0	16

Note: Sample includes all children born within 56 days before/after June 30, 2007. An observation is a day (N=112).

Table 2. Balance in covariates (birth outcomes)

	+/- 7 days		+/- 14 days		+/- 21 days		+/- 28 days		+/- 56 days	
Number of births	32.71		-26.52		-3.39		5.79		17.39	
(per day)	(80.31)		(118.44)		(93.00)		(79.38)		(83.82)	
Log n. of births	0.0250		-0.0241		-0.0066		0.0013		0.0107	
(per day)	(0.0633)		(0.0923)		(0.0730)		(0.0623)		(0.0639)	
Multiple birth	0.00128		-0.00068		0.00184		0.00208		0.00169	
	(0.0021)		(0.0030)		(0.0024)		(0.0021)		(0.0022)	
Weeks of gestation	0.0218		-0.00959		0.0128		0.0299		0.0519	*
	(0.0259)		(0.0372)		(0.0302)		(0.0260)		(0.0276)	
Prematurity	-0.0020		-0.00144		-0.00278		-0.0055		-0.0074	*
	(0.0037)		(0.0053)		(0.0043)		(0.0037)		(0.0039)	
Birth-Weight	0.00206		0.00249		0.00414		0.00134		0.00396	
	(0.0027)		(0.0038)		(0.0031)		(0.0027)		(0.0028)	
Low birth-weight	-0.00327		-0.00385		-0.00516		-0.00331		-0.00588	
	(0.0036)		(0.0051)		(0.0042)		(0.0036)		(0.0038)	
Very low birth-weight	-0.00038		0.000462		0.000834		0.000828		4.86E-05	
	(0.0012)		(0.0017)		(0.0014)		(0.0012)		(0.0013)	
Mortality 24h.	-0.00034		-0.00046		-0.00039		-0.00034		-0.00050	
	(0.0003)		(0.0005)		(0.0004)		(0.0003)		(0.0004)	
Complications	0.0090	*	0.0156	**	0.00772		0.00558		0.00552	
	(0.0048)		(0.0069)		(0.0056)		(0.0048)		(0.0051)	
C-section	0.00613		0.00368		0.00774		0.000215		0.00336	
	(0.0065)		(0.0093)		(0.0076)		(0.0065)		(0.0070)	
Home birth	0.000879		0.00157		0.000996		0.000967		0.00124	
	(0.0010)		(0.0014)		(0.0011)		(0.0010)		(0.0010)	
Mother>35	-0.00555		-0.0167	**	-0.0115	*	-0.00677		-0.00679	
	(0.0058)		(0.0082)		(0.0067)		(0.0058)		(0.0061)	
Mother>39	0.00469	*	0.0035		0.00353		0.00454		0.0028	
	(0.0028)		(0.0038)		(0.0032)		(0.0028)		(0.0029)	
N. of days	14		21		28		56		112	
Linear trend	N		Y		Y		Y		Y	
Quadratic trend	N		N		N		N		Y	

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

Note: Each coefficient comes from a different regression. The data source is birth certificates for 2007. The sample includes all children born wihtin x days before/after June 30, 2007 (x given in column header). An observation is a day (of birth). The dependent variable is stated in the row headers. The coefficients reported are for an indicator of births on or after July 1, 2007.

Table 3. The effect of benefit receipt on hospital stays by age

	+/- 7 days		+/- 14 days		+/- 21 days		+/- 28 days		+/- 56 days	
Age 0-5 years	82.29	*	36.37		80.50		82.05	*	86.88	*
(levels)	(41.88)		(60.51)		(53.19)		(44.32)		(48.46)	
Age 0-5 years	0.0992	*	0.0383		0.0905		0.0962	*	0.104	*
(logs)	(0.0515)		(0.0742)		(0.0667)		(0.0549)		(0.060)	
Age 0-1 month	0.038		-0.047		0.015		0.016		0.006	
(logs)	(0.080)		(0.100)		(0.096)		(0.079)		(0.080)	
Age 2-23 months	0.133	*	0.104		0.128	*	0.121	*	0.123	*
(logs)	(0.06)		(0.09)		(0.07)		(0.06)		(0.071)	
Age 2-5 years	0.134	**	0.067		0.134	*	0.157	***	0.193	***
(logs)	(0.05)		(0.08)		(0.07)		(0.06)		(0.068)	
N	14		28		42		56		112	
Linear trend	N		Y		Y		Y		Y	
Quadratic trend	N		N		N		N		Y	

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

Note: Each coefficient comes from a different regression. The data source is the Hospital Morbidity Survey 2007-2012. The sample includes all children born wihtin x days before/after June 30, 2007 (x given in column header). An observation is a day (of birth). The dependent variable is the number of overnight hospital stays in the specified age range. The coefficients reported are for an indicator of births on or after July 1, 2007.

Table 4. Placebos (hospital stays in logs for 2006 and 2008 births)

	+/- 7 days	**+/- 14 days**	**+/- 21 days**	**+/- 28 days**	**+/- 56 days**
Age 0-5,	0.0614	-0.0573	-0.0479	-0.0343	-0.0002
2006 births	(0.053)	(0.059)	(0.063)	(0.057)	(0.060)
Age 0-5,	0.0314	0.1450	0.1170	0.0922	0.0674
2008 births	(0.086)	(0.113)	(0.090)	(0.076)	(0.082)
N	14	28	42	56	112
Linear trend	N	Y	Y	Y	Y
Quadratic trend	N	N	N	N	Y

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

Note: Each coefficient comes from a different regression. The data source is the Hospital Morbidity Survey 2007-2012. The sample includes all children born within x days before/after June 30, 2007 (x given in column header). An observation is a day (of birth). The dependent variable is the number of overnight hospital stays in the specified age range. The coefficients reported are for an indicator of births on or after July 1, 2006 or 2008 (depending on the row).

Table 5. The effect of benefit receipt on hospital stays by main diagnosis

	+/- 7 days		+/- 14 days	+/- 21 days		+/- 28 days		+/- 56 days	
All hospital stays	82.29	*	36.37	80.50		82.05	*	86.88	*
	(41.88)		(60.51)	(53.19)		(44.32)		(48.46)	
Respiratory disease	16.43		7.49	15.39		20.90	**	20.94	**
	(9.40)		(8.45)	(10.17)		(9.14)		(10.11)	
Perinatal	-1.43		-23.11	-2.92		4.63		-1.35	
	(17.33)		(20.93)	(19.23)		(16.34)		(16.65)	
Digestive	11.71	**	10.25	7.57		5.61		2.83	
	(4.21)		(6.82)	(5.26)		(4.78)		(5.13)	
Not well-defined	7.57		7.33	6.44		3.67		6.43	
	(4.54)		(6.58)	(5.27)		(4.73)		(5.32)	
Infections	11.00	*	7.50	7.94		6.17		14.49	*
	(5.59)		(8.63)	(7.04)		(6.48)		(7.49)	
Congenital anoma	2.00		4.64	-0.63		-1.46		0.02	
	(5.39)		(8.20)	(7.04)		(6.01)		(6.15)	
Injuries	3.71		3.44	5.44		3.69		3.21	
	(2.94)		(3.83)	(3.73)		(3.14)		(3.41)	
Urinary	1.57		-2.44	2.24		4.49		5.28	
	(4.48)		(5.99)	(5.32)		(4.69)		(4.94)	
Nervous	3.29		3.66	5.01		3.94		1.50	
	(2.86)		(3.99)	(3.49)		(3.24)		(3.50)	
Rest of diagnoses	12.14		3.13	20.80	**	17.48	**	18.61	**
	(8.01)		(9.19)	(9.39)		(7.40)		(7.79)	
Other factors	14.29	**	14.48	13.22	*	12.93	**	14.90	**
	(6.43)		(9.49)	(7.48)		(5.97)		(6.27)	
N	14		28	42		56		112	
Linear trend	N		Y	Y		Y		Y	
Quadratic trend	N		N	N		N		Y	

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

Note: Each coefficient comes from a different regression. The data source is the Hospital Morbidity Survey 2007-2012. The sample includes all children born within x days before/after June 30, 2007 (x in column header). An observation is a day (of birth). The dependent variable is the number of overnight hospital stays with a given diagnosis in the specified age range. The coefficients reported are for an indicator of births on or after July 1, 2007.

Table 6. The effect of benefit receipt on mortality

	+/- 7 days	+/- 14 days	+/- 21 days	+/- 28 days	+/- 56 days
One month, levels	1.000	1.312	1.657	1.192	0.511
	(1.229)	(1.415)	(1.064)	(0.943)	(1.317)
One year, levels	1.429	1.890	2.201	1.958	1.038
	(1.288)	(1.744)	(1.419)	(1.242)	(1.490)
Five years, levels	2.571	3.491	4.030 **	3.698 ***	2.333
	(1.593)	(2.143)	(1.638)	(1.378)	(1.734)
One month, logs	0.143	0.0536	0.306	0.240	-0.041
	(0.275)	(0.346)	(0.290)	(0.259)	(0.314)
One year, logs	0.427	0.680 *	0.605 *	0.535 *	0.350
	(0.298)	(0.358)	(0.326)	(0.292)	(0.353)
Five years, logs	0.396 *	0.555 *	0.632 **	0.594 **	0.297
	(0.201)	(0.289)	(0.286)	(0.260)	(0.262)
N	14	28	42	56	112
Linear trend	N	Y	Y	Y	Y
Quadratic trend	N	N	N	N	Y

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

Note: Each coefficient comes from a different regression. The data source is death certificates for 2007-2012. The sample includes all children born within x days before/after June 30, 2007 (x in column header). An observation is a day (of birth). The dependent variable is the number of deaths (in levels or logs) in the specified age range. The coefficients reported are for an indicator of births on or after July 1, 2007.

Table 7. The effect of benefit receipt on hospital stays, by income level of the province

	+/- 7 days		+/- 14 days	+/- 21 days		+/- 28 days		+/- 56 days	
Provinces below median									
Age 0-5, in levels	48.86	**	35.08	47.74	*	48.13	**	45.36	**
	(20.52)		(28.18)	(25.29)		(21.64)		(22.54)	
Age 0-5, in logs	0.152	**	0.106	0.142	*	0.146	**	0.137	*
	(0.0644)		(0.0885)	(0.0831)		(0.0719)		(0.0740)	
Provinces above median									
Age 0-5, in levels	33.43		1.28	32.76		33.92		41.52	
	(28.02)		(40.16)	(35.20)		(28.54)		(31.98)	
Age 0-5, in logs	0.066		-0.003	0.058		0.065		0.081	
	(0.0557)		(0.0803)	(0.0708)		(0.0568)		(0.063)	
N	14		28	42		56		112	
Linear trend	N		Y	Y		Y		Y	
Quadratic trend	N		N	N		N		Y	

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

Note: Each coefficient comes from a different regression. The data source is the Hospital Morbidity Survey 2007-2012. The sample includes all children born within x days before/after June 30, 2007 (x given in column header). An observation is a day (of birth). The dependent variable is the number of overnight hospital stays (in levels or logs) in the specified age range. The coefficients reported are for an indicator of births on or after July 1, 2007. Observations are split by the average income per capita level of the province (about 21,000€); there are 52 provinces.

Appendix

Table A1. Effect of the child benefit on expenditures (Household Budget Survey)

Dependent variables	**+/- 2 months**	**+/- 2 months**	**+/- 3 months**	**+/- 4 months**	**+/- 6 months**	**+/- 9 months**
Expenditure on gas	-26.73	18.55	79.24	178.3	227.5	-24.77
	(185.60)	(185.70)	(176.80)	(289.20)	(229.20)	(290.50)
Exp. on alcohol, tobacco and drugs	-65.93	-49.35	-13.89	-160.7	-47.66	-256.3
	(111.90)	(116.70)	(98.89)	(164.90)	(134.10)	(169.20)
Expenditure on private health insurance	-21.72	173.3	21.56	-78.09	-65.31	-160.4
	(102.30)	(96.69)	(80.04)	(147.10)	(113.10)	(149.90)
Total health-related expenditures	-96	-81.3	17.5	204.6	-202.1	-134.9
	(238)	(249)	(186)	(426)	(290)	(396)
Number of observations	234	234	315	441	640	941
Linear trend in m	N	N	N	Y	Y	Y
Quadratic trend in m	N	N	N	N	N	Y
Controls	N	Y	Y	Y	Y	Y
Number of months	4	4	6	8	12	18

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

Note: 2008 Household Budget Survey. Running variable is month of birth of the child (0: July 2007).

Table A2. Effect of the child benefit on breastfeeding and private health insurance (National Health Survey)

Dependent variables	**+/- 2 months**	**+/- 3 months**	**+/- 4 months**	**+/- 6 months**	**+/- 9 months**
Breastfeeding 6 months	-0.1287	0.0576	0.1453	0.1832	0.135
	(0.2022)	(0.1559)	(0.1394)	(0.1702)	(0.1390)
N	100	148	188	275	393
Private health insurance	-0.0274	-0.1743	-0.1052	-0.0896	-0.0893
	(0.1364)	(0.1080)	(0.0951)	(0.1178)	(0.0011)
Child in poor, fair or good health	-0.0028	-0.0917	-0.156	-0.1213	-0.2545 **
	(0.1784)	(0.1378)	(0.1196)	(0.1440)	(0.1165)
N	144	222	290	431	666
Linear trend in d	Y	Y	Y	Y	Y
Quadratic trend in d	N	N	N	Y	Y
Number of months	4	6	8	12	18

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

Note: 2011 National Health Survey. Running variable is date of birth of the child (0: July 1, 2007).

Table A3. Effect of the child benefit on parental separation (Labor Force Survey)

Dependent variable	**+/- 2 months**		**+/- 3 months**		**+/- 4 months**		**+/- 6 months**		**+/- 9 months**	
Separated or divorced	-0.0253	***	-0.0205	***	-0.0512	***	-0.0284	***	-0.0262	***
(LFS 2008)	(0.0081)		(0.0062)		(0.0126)		(0.0099)		(0.0076)	
N	2,062		3,026		4,083		5,813		8,691	
Separated or divorced	0.0067		-0.0038		0.0060		0.0073		0.0061	
(LFS 2009)	(0.0097)		(0.0070)		(0.0142)		(0.0095)		(0.0075)	
N	2,127		3,119		4,212		6,194		9,379	
Controls	Y		Y		Y		Y		Y	
Linear trend in m	N		N		Y		Y		Y	
Quadratic trend in m	N		N		N		N		N	
Number of months	4		6		8		12		18	

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

Note: 2008 and 2009 Labor Force Survey. Running variable is month of birth of the child (0: July 2007).

Table A4. Effect of child benefit receipt on subsequent fertility

Sample	+/- 7 days	+/- 14 days	+/- 21 days	+/- 28 days	+/- 56 days
By 2 years	-2.13	-3.64	-0.13	-0.46	4.25
	(13.90)	(10.04)	(7.23)	(6.50)	(4.54)
By 4 years	-13.75	-13.23	-11.00	-3.00	7.98
	(25.88)	(18.78)	(15.57)	(15.20)	(11.29)
By 6 years	0.86	1.78	4.99	13.67	20.61
	(43.41)	(29.44)	(23.00)	(21.84)	(15.84)
N	14	28	42	56	112
Linear trend	N	Y	Y	Y	Y
Quadratic trend	N	N	N	N	N

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

Note: Birth-certificate data 2007-2012. The dependent variable is the number of births by 2, 4 or 6 years after the 2007 child, by date of birth. The running variable is the date of birth of the child, centered around July 1, 2007.