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Baby bonus in Switzerland: Effects on fertility, newborn health, and birth-scheduling

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Abstract

This paper studies the effect of birth allowances (so-called baby bonus) on fertility, newborn health, and birth-scheduling in Switzerland. Switzerland provides an optimal quasi-experiment: 11 out of 26 cantons introduced a baby bonus during the last 50 years at different points in time. To identify the effect of changes in the baby bonus, we employ an event study with control groups using several administrative data sets on births, stillbirths, and infant deaths in Switzerland from 1969 to 2017. While there is no evidence for birth-scheduling, we find, however, a sizable but only temporary increase in the fertility rate of 5.5% and a permanent but diminishing increase in the birth weight of 2.8%. The latter effect is particularly strong at the lower end of the birth weight distribution. Furthermore, we document substantial heterogeneity by citizenship of mothers.

KEYWORDS
birth allowances, birth-scheduling, fertility

JEL CLASSIFICATION
H31, J13

1 | INTRODUCTION

Children are expensive. Therefore, many governments introduced policies to ease financial pressure on families. Among them are birth allowances that incorporate a lump-sum transfer at the event of birth. Birth allowances—also called baby bonuses—are designed for the vulnerable transition from being a couple without a child to becoming parents. Providing financial support in this critical period can affect parental behavior in the short- and medium- to long-run.

In the medium- to long-run, birth payments could incentivize couples to become parents and consequently boost fertility. This is a crucial topic for countries with an aging population and with fertility rates below the replacement level of 2.1 children per woman. Due to the improved financial situation and the decline in financial stress, birth payments may also improve newborn health.

In the short-run, expecting parents might (re-)schedule births when a new baby bonus policy is introduced and thereby affect newborn health. Specifically, financial incentives may motivate parents to shift a birth forward or backward around the expected date of delivery. This can have severe long-run consequences for the unborn child because advancing or postponing a birth affects newborn health by giving the fetus less or more time to grow within the maternal womb (Borra et al., 2019; Brunner and Kuhn, 2014; Gans and Leigh, 2009; Neugart and Ohlsson, 2013; Tamm, 2013).

In this paper, we study the effect of introducing, increasing, or abolishing birth payments on fertility, newborn health, and birth-scheduling. For the empirical analysis, we draw on several administrative data sets from 1969 to 2017. We build outcome variables based on the Swiss birth register, the universe of stillbirths, and the statistics on infant deaths. Combining these outcome variables with cantonal information on birth allowances allows us to study the causal impact of birth payments in a unique quasi-experimental setting. Nevertheless, authorities leave cantons a certain degree of freedom for birth allowances: Cantons are free to implement birth payments and free to set the amount. Based on this, we implement an event study difference-in-difference estimation.

Our results show that introducing a baby bonus affects fertility and newborn health. The fertility rate increases by around 5.5% at the mean in the first year of the post-treatment period, but fades out quickly. Newborn weight increases by around 2.8% at the mean, and the effect diminishes over time. To study heterogeneous effects across the socio-economic spectrum, we approximate socio-economic status of the mother by her country of origin. We find that the fertility effect is driven by mothers with citizenship from low- and middle-income countries (LMICs). Birth weight, however, significantly increases for mothers from a high-income country.

In contrast to previous studies, we do not find evidence for birth-scheduling around the policy changes. We argue that this results from several features in the Swiss setting. First, changes are in absolute terms smaller than in other countries with birth allowances. Second, the daily birth number per canton is likely too small to detect significant results.

This paper contributes to the literature on impacts of cash transfers on fertility behavior and newborn health. Furthermore, it adds to the literature on policy announcement effects on birth-scheduling and newborn health.

Studies, such as Gans and Leigh (2009), Tamm (2013), Neugart and Ohlsson (2013), Brunner and Kuhn (2014), or Borra et al. (2019), have found evidence of birth scheduling and fertility adjustments.[1] Due to the quasi-experimental setting in the Swiss context, we are the first to introduce a plausible control group: Cantons that do not change their birth allowance system at a given point in time and cantons that never introduced such a policy. Previous studies almost exclusively analyzed national policy changes instead of cantonal policy changes. Thus, they had to rely on regularities in the data before or after the policy change to predict an alternative outcome in the absence of the policy change to which the actual number of births per specific day in the year could be compared to. There is another issue when only using data from before the policy change to predict the alternative outcome: observations at the boundary of the sample period have a strong impact on the estimated time trend in case of a nonlinear trend. Furthermore, we can analyze introductions, increases, and abolition of the baby bonus within one country. This setting allows us to study asymmetries as the parental choice of delaying or scheduling a birth early is different.

More generally, there is a large and growing strand of literature analyzing the impact of cash transfers on fertility (Cohen et al., 2013; González, 2013; González and Trommlerová, 2021; Kearney, 2004; Laroque and Salanié, 2014; Milligan, 2005). Several of these studies find an impact on fertility when parents face financial support. Most closely related to our study is Milligan's (2005) analysis of a policy reform in birth allowances in Quebec. This policy led to transfers up to CAD 8000 (roughly CHF 6000)[2] for the third child. The author finds a strong effect on fertility. While the absolute amount is significantly higher in the Canadian study (depending on the canton multiplied with a factor from 3 to 6), the Swiss transfers are already being paid for the first child. Therefore, the Swiss case allows to study fertility effects at both the intensive and extensive margin. Second, while many works exclusively focus on fertility and labor market outcomes, we extend the analysis to newborn health outcomes giving a broader picture of cash transfers. Third, with the panel structure of the data and the long history of Swiss family allowances, we can study the impact of baby bonuses over time and show a fading out of the effect with every year after the implementation.

Finally, based on the staggered implementation across cantons and time, we can base our estimates on several policy changes which increases the external validity of our results. Due to the concerns raised recently by Goodman-Bacon (2021), we do not choose a two-way fixed effects (2WFEs) difference-in-difference model. Goodman-Bacon (2021) shows that the treatment estimate of 2WFE regression can be severely biased if effects change over time. Furthermore, it is difficult to assess the parallel trends assumption with this model. While several authors, among them De Chaisemartin and d'Haultfoeuille (2020), Callaway and Sant'Anna (2020), and Athey and Imbens (2018) propose solutions to the described problem, we chose to use an event study design with control group that incorporates dummies for every year relative to the introduction of the treatment. We will refer to this strategy by *event study DiD*. This setting allows us to study effects over time instead of a single coefficient.

We organize the remainder of the paper as follows. Section 2 describes the institutional background. Section 3 describes the data. Section 4 introduces the empirical strategy. We present various results, sensitivity analyses, and a discussion of the results on fertility and newborn health in Section 5 and on birth-scheduling in Section 6. Section 7 concludes.

2 | THE SWISS BABY BONUS

Switzerland has a decentralized federal political system with three interdependent governmental levels (federal, cantonal, and municipal). Family allowances are regulated on the federal level. However, each canton has the authority to adjust the local payments individually. Family allowances are financed via contributions to the family compensation office and not via taxes. Therefore, they are detached from other regulatory decisions or tax incentives designed for families. Depending on the canton, expecting parents have to collect their family allowances directly from their employer or the family compensation office.

The Swiss political system is characterized by a direct democracy and a decentralized federalism. Each governmental layer is entitled to decide about all political issues in its sphere of influence. Furthermore, each important new constitutional amendment needs the consent of the voting population, which results both in lengthy processes of implementing new policies and in a tremendous variation of different policies. Thus, even if other family policies exist—such as incentives in tax systems or in child care—it is unlikely that they systematically interfere with the family allowances which we study.[3] Specifically, introductions of the baby bonus mostly occurred prior to other social policies supporting child care.

There are three different family allowances: (1) child allowances, which by federal law since 2009 have to be at least CHF 200 per month,[4] (2) education allowances, which by federal law have to be at least CHF 250 per month, and (3) the one-time birth payment with no federal minimum payout. Thus, cantons are free to implement a baby bonus and to define the amount paid. They may also increase the baby bonus or abolish it at any point in time. This gives rise to large variation across cantons.

An important difference between these benefits is that child and education allowances are monthly money transfers, while the baby bonus is a one-time payment. While the different allowances may change at the same point in time, eligibility to collect one type of allowance varies. All mothers living in a specific canton and giving birth after the implementation date of the baby bonus are eligible for the baby bonus. Thus, there is a sharp cutoff from one day to the next. For child and education allowances, every child eligible in a month can enjoy higher payments after a policy change.[5] This is to clarify that in practice the baby bonus and the child allowance paid in the first month after birth never offset each other.

In this paper, we will only focus on birth payments because the unique setup of this benefit allows us to analyze birth-scheduling, newborn health, and fertility effects. The birth payment is a unique payment to a woman who had a living birth (or a stillbirth after at least 23 weeks of gestation). The birth payment is per newborn. For a multiple birth, a mother can collect the baby bonus for each child.

The baby bonus may affect two outcome margins. Already pregnant mothers may want to shift their birth a few days to become or stay eligible for the birth payment. This is what we call the short-run margin. This short-run margin may affect newborn health via birth-scheduling. Importantly, this effect is not diluted by a change in the fertility behavior because all policy changes were announced less than seven months before the implementation.[6] Therefore, mothers were already pregnant at the time of the policy announcement. Thus, mothers can only schedule the birth in a short period around the expected date of birth.

Birth-scheduling results from financial incentives of introductions, increases, and abolition of birth payments. On the one hand, births may be delayed beyond the date of implementation when a baby bonus is introduced or substantially increased. On the other hand, births may be brought forward when a baby bonus is abolished. It is more difficult to delay a birth than to schedule early, due to the natural end of every pregnancy. There are several ways to delay labor (Coomarasamy et al., 2003; Lima et al., 2018; Shapiro et al., 2013): One is to avoid stress or to take medication to delay labor by up to 48 hours. Another one is to postpone an already planned Cesarean section. Through a delay, a newborn is expected to have a higher weight, since the unborn had more time to grow in the mother's womb.

In the case of an abolition of the policy, mothers may want to speed up the pregnancy. Mothers can schedule a birth early via a Cesarean section or induce labor medically. These choices will lead to an earlier birth and a lighter newborn. As a result, mothers must weigh up their financial gain against the potentially harmful effect on their newborns' health.

In the medium- to long-run, mothers might also adjust their fertility behavior. Thus, higher birth allowances can increase fertility. This may be the result of explicitly choosing to have a (n additional) child, having children earlier or choosing not to abort. Furthermore, the payment may also improve newborn health. Either because different types of mothers choose to have a (n additional) child (i.e., a selection/composition effect) or because more money directly impacts newborn health via, for example, better maternal health or a change in maternal behavior (an extensive overview by Almond et al. (2018) documents various of these effects).

The latter channel is expected to be especially strong for parents with low-socio-economic status based on the findings of the literature on the fetal origins hypothesis. Financially distressed parents may benefit from this extra payment and negative pregnancy outcomes might be prevented and positive birth outcomes promoted. US welfare programs targeted toward low-socio-economic groups such as the Earned Income Tax Credit (EITC) studied by Hoynes et al. (2015) and the Food Stamp Program (FSP) studied by Almond et al. (2011) show substantial beneficial impacts on newborn health. However, the impact of additional income above a certain threshold is much less studied and not documented so far. Furthermore, it is also less clear whether a benefit paid in the future might affect health outcomes today. One argument could work via a reduction in maternal stress based on the knowledge of receiving a transfer in the very near future. Several studies have shown that stress due to various reasons affects newborn health (Aizer, 2011; Black et al., 2016; Camacho, 2008; Currie and Rossin-Slater, 2013; Lee, 2014; Quintana-Domeque and Ródenas-Serrano, 2017).

3 | DATA

We base the empirical analysis on several data sources. The data on all outcome measures such as newborn health, birth-scheduling, and fertility is coming from the Swiss Vital Statistics and Annual Population Statistics provided by the Federal Statistical Office. Information on the amount and the date of implementation of all birth allowances per canton is recorded by the Federal Social Insurance Office (FSIO).

3.1 | Data sources

3.1.1 | Swiss birth register

The birth register covers all births from 1969 to 2017. It contains information on date of birth, sex, and beginning from 1979 birth outcomes, such as weight and length at birth. Based on the information about birth weight, we create a dummy for low birth weight defined as less than 2500 grams to understand which part of the distribution is mostly affected. The latter outcome measures are linked to later life outcomes (Almond et al., 2018). Furthermore, the birth register provides information on birth order and birth interval in months to a preceding birth.

Using the information on the gender of the child, we generate the sex ratio. There are several arguments for how socio-economic and maternal health conditions during pregnancy can affect the sex ratio as summarized by Scalone and Rettaroli (2015). Improving socio-economic conditions can, based on a biological and evolutionary argument, favor boys in the maternal womb. This is because the male fetus is frailer. Thus, we follow the lines of Sanders and Stoecker (2015) and use the sex ratio in live births as a proxy for the miscarriage rate.

To calculate the crude birth rate per 1000 people and the total fertility rate per woman, we merge the data on a canton-year level with the Annual Population Statistics.[7] The Annual Population Statistics is available from 1971 with detailed information on age-specific population starting in 1981. Thus, the crude birth rate can be reported from 1971 onward, while the total fertility rate is only available after 1981.

The birth register also contains information about the mother, such as her age, marital status, citizenship, municipality, and canton of residence. Maternal age, though, serves as an additional outcome measure (also in combination with the birth interval between two consecutive births) to study mechanisms that explain overall fertility and child health.

3.1.2 | Stillbirths and deaths

For the determination of more severe health outcomes, we rely on information provided in the statistics of stillbirths and deaths. As in the birth register, these data sets provide information on the date, municipality, and canton of occurrence. Based on these two measures, we calculate the stillbirth and infant (<1 year) death rate per 1000 births.

3.1.3 | Birth allowances

We have collected the full history of birth allowances per canton from several publications. From 1969 to 1992 the data were published in *Zeitschrift für die Ausgleichskassen* (Bundesamt für Sozialversicherungen, 1992). The publication *AHI-Praxis* covers the period from 1993 to 2004 (Bundesamt für Sozialversicherungen, 2004). Starting from 2005, the data are published online on the website of the FSIO (Bundesamt für Sozialversicherungen, 2020). These publications record information on the date of implementation and the amount of the allowance per canton. Additionally to the date of implementation also the date of announcement is recorded. All health policy reforms were announced not more than seven months prior to their implementation. This guarantees that around the implementation date, the only adjustable margin is birth-scheduling and no fertility adjustment, as mothers were already pregnant by that time. In the medium to long run, however, fertility can be affected.

3.2 | Descriptive statistics

We show descriptive statistics for birth measures, child characteristics, and maternal characteristics for the overall sample in Table 1. Over the entire time period from 1969 to 2017, we observe on average around 81,000 births per year. The crude birth rate per 1000 people in Switzerland is 11.8 and the total fertility rate per woman 1.6. On average, 5.0 fetuses out of 1000 births die in a mother's womb and 7.6 infants out of 1000 births die within the first year. At birth there are slightly more males (0.514) which directly translates into a sex ratio of 0.946 girls per 1 boy. The average Swiss family has a birth interval of slightly over 3 years between children and the average birth weight of a newborn is 3334 grams. 5.7% of children are born with a birth weight of less than 2500 grams. Mothers are on average 29 years old when giving birth, mostly married (91 percent) and 74 percent of them are Swiss.

Figure 1 shows the geographic variation in birth allowances for six different years. Cantons with birth allowances are mostly concentrated in the French speaking part and in the region of Central Switzerland. Figure A.2 shows the time variation in birth allowances for all cantons that introduced the baby bonus at some point in time. Introductory birth payments are relatively similar across cantons. Three cantons (Geneva, Vaud, and Fribourg) have already put baby allowances in place before 1969.[8] Several cantons adjust the amount of baby allowances over time. Two cantons (Solothurn and Schaffhausen) abolish the baby bonus after some years again.

TABLE 1 Descriptive statistics

	Mean	Std. dev.	Min	Max
Overall birth measures				
Total yearly births	80,578	7083	71,375	102,520
Fertility rate	1.621	0.256	1.146	2.914
Crude birth rate (per 1000 people)	11.762	2.170	7.531	19.170
Stillbirth rate (per 1000 births)	5.083	2.841	0	22.422
Infant death rate (per 1000 births)	7.581	4.939	0	36.082
Child characteristics				
Male	0.514	0.016	0.421	0.631
Birth interval (months)	37.550	3.102	27.366	48.301
Birth weight	3333.551	49.468	3216.849	3902.548
Share low birth weight	0.057	0.012	0.016	0.104
Maternal characteristics				
Age of the mother	28.985	1.586	25.952	32.631
Married at birth	0.905	0.070	0.635	0.991
Swiss at birth	0.735	0.119	0.408	0.977
N (canton × years)	1274			

Notes: The full sample covers all births, stillbirths, and infant deaths from 1969 to 2017.

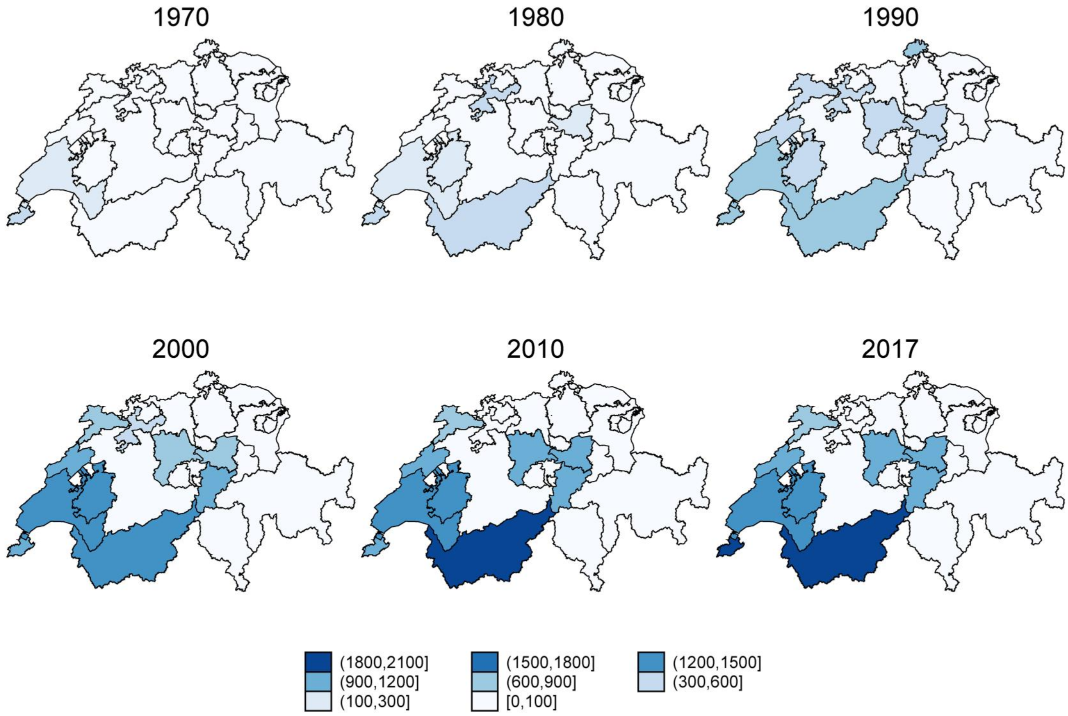

FIGURE 1 Geographic variation of birth allowances. This figure shows the amount of birth allowances provided per child per canton in current year values. The focus is on geographical variation so that birth allowances are drawn for all cantons every 10 years up to the most recent year available [Colour figure can be viewed at wileyonlinelibrary.com]

4 | EMPIRICAL STRATEGY

To answer our research questions, we employ two types of event studies. We aim to identify the causal effect of the baby bonus on health and fertility outcomes in an *event study DiD* where we show how effects evolve over time, and specifically after the treatment. To test for birth-scheduling behavior, we use a *time-series event study* design where we predict the total number of births per day and canton in absence of the treatment and look for manipulation around the cutoff by displaying the residuals from the prediction.

4.1 | Fertility and newborn health

To estimate the causal effect of the baby bonus on health and fertility, we compare cantons with baby bonus to cantons without baby bonus before and after the introduction.[9] In our main specification, we use an event study DiD model. Event studies differ from a standard difference-in-difference design in that the treatment is no longer uniquely characterized by a binary indicator, but a set of dummies indicating the time relative to the introduction. Thereby, we can analyze the evolution over event time. Furthermore, it allows to control for canton fixed effects, year fixed effects, and cantonal trends. Specifically, we estimate the following regression:

$$y_{ct} = \gamma_c + \lambda_t + \sum_{\tau=2}^{m} \delta_{-\tau} D_{c,t-\tau} + \sum_{\tau=0}^{q} \delta_{+\tau} D_{c,t+\tau} + \eta_c \times t + \epsilon_{ct}. \quad (1)$$

The dependent variable, y_{ct} represents the total fertility rate, the crude birth rate, the birth weight, share of low weight births, the stillbirth rate, the infant death rate, and the sex ratio as well as maternal age, and the birth interval in canton c and year t. γ_c are canton fixed effects, λ_t denotes (calender) year fixed effects, and η_c allows for canton specific linear time trends. ϵ_{ct} is an error term.

The variables $D_{c,t-\tau}$ and $D_{c,t+\tau}$ equal 1 in the m pre-treatment periods and in the q post-treatment periods, respectively. We omit category $\tau = -1$, which is the event-year before the introduction. Thus, the set of coefficients δ_τ for $\tau \in [-m, q]$ shows the change in outcomes in cantons with a baby bonus compared to cantons without a baby bonus relative to the event year $\tau - 1$.

We weight the estimates in the canton-year cell differently, depending on the outcome. The fertility rate is weighted by the number of fertile women, the crude birth rate is weighted by population size, and health measures are weighted by the number of births. Robust standard errors are clustered at the cantonal level, that is the level at which the treatment takes place (Abadie et al., 2017). Because of the relatively small number of clusters,[10] we additionally report wild-cluster bootstrapped standard errors in a robustness analysis.

To ensure that we have the same amount of pre-treatment years for all baby bonus cantons, we choose the pre-treatment period to be 5 years. The pre-treatment periods yield important insights to ensure that our identification strategy is valid.

We rely upon several assumptions to identify a causal effect. The key assumption is the *parallel trends assumption*. This assumption states that in the absence of treatment, treated units would have experienced the same trends in average outcomes as the control units. One concern might be, that assignment to the baby bonus could depend on (potentially) unobserved factors, such as culture or religion. However, as long as such confounders are constant across time, which is typically the case with culture and religion, they do not affect the results since they are captured by the canton-fixed effects. Furthermore, one could expect cantons to introduce a baby bonus when facing declining fertility. This would lead to estimating lower bounds. However, declining fertility prior to treatment can be assessed in this setting. Specifically, the event study design allows us to directly asses the plausibility of the parallel trends assumptions by showing that the pre-treatment coefficients do not significantly differ from 0. This holds true for all outcome variables but especially so for fertility.

Furthermore, the identification requires *no anticipatory response*. That is, individuals should not foresee that they will be treated so that they change their behavior before the treatment takes place. Since the announcement of the policy takes place at most 7 months prior to implementation and is, thus, shorter than a standard gestation period, we can rule out anticipatory effects by design of the policy.

Another assumption to identify the causal effect is the *stable unit treatment value assumption*. This assumption would be violated if other policies were introduced that interfere with the introduction of the baby bonus policy. In a robustness analysis we, thus, control for child allowances—one of the other policies in the family allowances package, which, however, are much more regulated due to minimal payment amounts based on the federal law.

4.2 | Birth-scheduling

To test for birth-scheduling behavior, we use a time-series event study design. First, we collapse our individual level data on the daily cantonal level. Next, we regress the following equation:

$$y_{imyc} = \alpha + \beta_c + \gamma_y + \delta_m + \zeta_i + \epsilon_{imyc}, \tag{2}$$

where y_{imyc} is the total (log) count of births per day i, in month m, in year y, and canton c. With β_c we include canton fixed effects, and with γ_y and δ_m year and month fixed effects, respectively. ζ_i are, depending on the specification, day-of-week or day-of-year fixed effects. Day-of-week fixed effects can be more precisely estimated and root on the idea that daily births vary across the day of the week due to, for example, relatively few planned births via Cesarean sections on the weekend. Day-of-year fixed effects, instead, control for specific dates unrelated to the day of the week such as day-specific holidays or the first day of a month in case parents have a preference or aversion for any of these dates. Based on the fact, that our sample includes control cantons, the different coefficients (γ_y, δ_m, and ζ_i) on time fixed effects can be identified on top of a treatment effect in a given year and canton. Finally, we calculate residuals from a linear prediction and plot these residuals for the 60 days around the policy change. Thereby, we pool over the same event across cantons and time. Robust standard errors are clustered at the cantonal level.

Our identification of the birth-scheduling effect relies again on the assumptions of parallel trends. As other untreated cantons serve as control, these cantons must provide an appropriate counterfactual so that they describe the trend treated cantons would have followed in absence of the treatment. Furthermore, depending on the specification, the day-of-week or day-of-year fixed effects should not vary over the included time frame on top of the included year and month fixed effects. Visual inspection of the residual graphs shows that the residual approach results in a noisy pattern around zero further away from the cutoff date and, therefore, seems to appropriately de-trend the data.

We show the birth-scheduling effect individually for introductions, increases of above CHF 200, and the abolition of the policy. Given the specific event, we would expect a certain pattern accentuating the closer the cutoff. For example, in case of an introduction of a baby bonus we would expect parents to shift the birth after the introduction time. Thus the prediction in absence of the policy change would be too high before the policy's implementation and therefore the residuals below zero. While after the introduction, we would expect to observe a discontinuous jump in the residuals. An increase of the baby bonus would lead to the same pattern, while an abolition should lead to the opposite picture that is a negative jump around the cutoff.

5 | RESULTS: FERTILITY AND NEWBORN HEALTH

We show our main results of Equation (1) in Table 2 and Figure 2. Figure 2 plots the coefficients δ_τ relative to the time of introduction. The omitted category is event time $\tau = -1$, directly before the introduction in event time 0. Negative event times indicate pre-treatment periods.

We see significant changes in the post-treatment period for the fertility rate, age of mother at birth, birth weight, and the share of children being born with low birth weight. Looking at the coefficients of those four outcomes in the pre-treatment period, we see that they are small and in almost all cases not significantly different from 0. This suggests that the parallel trends assumption is plausible and that they pass the Granger causality test, which states that the effect of the treatment cannot occur before the treatment happened. Note that the low birth weight and birth weight coefficients show some significance in event time −5 and −3. However, the coefficients in pre-treatment are small and considerably, as well as significantly, different from the coefficients in the post-treatment periods. Furthermore, if anything the coefficients on low birth weight trended upwards so that the negative effect after the reform can be interpreted as a conservative estimate.

Looking at how the effect of the baby bonus on the fertility rate, age of mother at birth, birth weight, and low birth weight develops over time, reveals some additional insights. Over time, the effect tends to fade out—this is especially true for the fertility rate: After merely 4 years, the coefficient is close to 0 and insignificant. For age of mother at birth, birth weight and low birth weight, this process is slightly slower, and the coefficient tends to stay marginally significant.

Table 2 shows the treatment coefficients that are plotted in Figure 2. Comparing the effects of δ_τ in the introduction period ($\tau = 0$) to the mean of the dependent variable, allows to interpret the effects better. The fertility rate increases by 0.089 which corresponds to an increase at the mean of 5.5%. This fertility effect is therefore large—although only transitory in nature. The effect on age of mother at birth is −0.474 corresponding to a reduction of roughly 6 months in age at birth. While we argue that maternal age at birth can give insights on the effect on fertility (i.e., the earlier a mother gives birth the less likely she is to be affected of negative shocks such as a health shock or a divorce), we discuss this more in detail in the next section on the potential mechanisms.

Furthermore, birth weight increases by 93 grams in the introduction period. This corresponds to an increase of 2.8% evaluated at the mean. The share of children being born with low birth weight reduces by 0.015 in the introduction period or 1.5 percentage points. In relative terms, this effect is quite large and corresponds to a decrease of 28% evaluated at the mean.

Opposite to these clear patterns, we do not find conclusive evidence of the effect on the crude birth rate, infant deaths, stillbirths, and the sex ratio. The crude birth rate shows positive coefficients in the post-treatment period, which would be in line with the findings on the fertility rate. However, the coefficients are not significant. For infant deaths, and stillbirths we cannot detect a significant change at the time of introduction, nor can we confirm that pre-treatment periods are different from post-treatment periods. While the coefficients mostly oscillate around 0, they tend to increase but stay insignificant in the post-event period. The coefficient on the sex ratio is significant in the post-treatment period 0. However, the effect is small and disappears after one period. Given the direction of the effect, this would indicate a small increase in the miscarriage rate. While both the average birth weight and the incidence of low birth weight improve—thus, an overall shift of the birth weight distribution to the right—this stands in contrast to the deteriorating severe health outcomes. The only way to explain such a pattern is through heterogeneous effects by maternal characteristics. However as the severe health measures are far from significant, we are cautious in interpreting this effect any further.

Other explanations for those small or non-significant changes at the time of introduction are multifold. On the one hand, the crude birth rate might be a too noisy measure. Population dynamics, such as immigration or changes in the age pyramid, make this rate an unreliable measure. Especially in Switzerland, with large immigration flows and an aging population, these influencing factors should not be neglected. On the other hand, the small but non-significant effects in the infant deaths, stillbirths, and the sex ratio can also be explained by the fact, that those are very severe and negative infant health measures, that are unlikely to be affected by a cash transfer in a developed economy.

We conduct several robustness checks.[11] Table A.2 reports significance results with wild-cluster bootstrapped standard errors in order to account for the relatively small amount of clusters. Table A.3 shows the event study estimates without cantons that abolish the baby bonus after some time. Those are two cantons that abolished the baby bonus near the end of our data period. The overall pattern is unchanged to our baseline estimation. In Table A.4, we control for child allowances. Child allowances could potentially interfere with the baby bonus. However, as Figure A.1, depicting cantonal changes over time, shows, increases are mostly small and are, thus, unlikely to change fertility or newborn health. Table A.4 confirms this as the results are quantitatively and qualitatively similar to our baseline estimation.

Taken together, the results suggest that the introduction of the baby bonus had a sizable, but transitory effect on fertility. While we show with our robustness analysis that this dissipating effect is not driven by cantons that later abolish the baby bonus, we argue that this might be the result of a behavioral feature, the so called reference dependence (Kahneman and Tversky, 1979). As the birth payment becomes normal (i.e., the new reference point) to parents it might not affect fertility any longer. Furthermore we find a significant, but declining effect on birth weight. This declining effect seems especially driven by the lower end of the birth weight distribution as depicted with the effect on the share of newborns being born with low birth weight. Further, we find no evidence that more severe outcomes, such as infant deaths, stillbirths, or miscarriages as approximated by the sex ratio are affected.

We can compare these effects with other studies. In case of the effect on fertility, we consult the most closely related study of Milligan (2005). Based on the differential design of the birth allowances for Quebec, however, one must be careful when directly confronting the fertility effects. Overall, Milligan (2005) documents a fertility effect ranging from 5% to 10%. As the large change in payments, though, almost exclusively occurs for third born children, the effect of fertility on third children increases to up to 25%. Multiplying our effect on fertility of 5.5% with the difference in the size of the baby bonus ranging from 3 to 6 depending on the canton, we find a similar effect overall equaling 16.5% to 33% and thus matching the 25% of Milligan (2005).

Comparing the effect on newborn health with other studies is more difficult, because of the very specific nature of the programs. Hoynes et al. (2015), for example, document a 2% to 3% reduction in the occurrence of low birth weight because of the EITC, while Almond et al. (2011) find a 0.5% increase in birth weight and a 10% decline in low birth weight due to the FSP. While both the EITC and the FSP are large transfer programs of roughly 2.7‰ of GDP and 2.2‰ of GDP in 2004, respectively, the Swiss baby bonus equaling 0.07‰ of GDP in 2017 is of much smaller size. As the effect on birth weight and especially low birth weight is declining over time, it is probably much more appropriate to compare the effect sizes in the last event year where birth weight significantly increases by 0.6% at the mean and the incidence of low birth weight declines by 7.5% at the mean, though this estimate is not statistically significant. These estimates are thus almost the same as in Almond et al. (2011). However, in Switzerland this result was achieved with much lower costs.

5.1 | Mechanisms and further analyses

Switzerland is among the countries with the highest share of immigrants in Europe. Over 27% of the births in our sample from 1969 to 2017 are given by mothers without a Swiss citizenship. Thus, we can exploit the citizenship of the mother as a proxy for socio-economic status. To do so, we use the World Bank database which categorizes countries into four income levels: High-income, upper middle-income, lower middle-income, and low-income (The World Bank, 2020). To facilitate the analysis and increase group sample size, we generate two groups: High-income countries, consisting only of World Bank's *high-income* countries including Switzerland and LMICs, which comprises the other three categories. Roughly 90 percent of births are given by mothers from high-income countries and approximately 10 percent by mothers of LMICs. Most prevalent across the LMICs are Serbia, Turkey, North Macedonia, Sri Lanka, Bosnia and Herzegovina, and Brazil. Those countries account for 65 percent of births in this group. Certainly, the country of

TABLE 2 Event study DiD estimates

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
Event time estimate: δ_τ									
−5	−0.026	−0.350	−1.184	−0.085	47.483**	−0.007**	0.037	0.140	−0.034
	(0.044)	(0.238)	(1.632)	(0.053)	(14.490)	(0.002)	(1.118)	(0.901)	(0.018)
−4	−0.018	−0.254	0.094	0.002	7.167	−0.004	0.971	−1.071	−0.018
	(0.021)	(0.154)	(0.369)	(0.057)	(13.578)	(0.003)	(0.870)	(0.651)	(0.015)
−3	0.026	0.064	−0.371	−0.023	15.172	−0.006*	−0.949	0.021	−0.026
	(0.049)	(0.298)	(0.192)	(0.038)	(16.289)	(0.003)	(1.182)	(0.929)	(0.015)
−2	0.020	0.079	0.015	−0.022	14.659	−0.001	−0.073	0.031	−0.017
	(0.021)	(0.159)	(0.257)	(0.063)	(26.821)	(0.002)	(0.812)	(1.122)	(0.018)
0	0.089	0.189	−0.475	−0.474***	93.340***	−0.015**	1.151	0.331	0.049*
	(0.046)	(0.293)	(0.848)	(0.110)	(23.412)	(0.004)	(1.320)	(0.821)	(0.018)
1	0.110*	0.423	−0.332	−0.415**	74.973***	−0.011**	2.099	1.027	0.027
	(0.040)	(0.260)	(0.916)	(0.112)	(19.035)	(0.003)	(1.084)	(1.152)	(0.016)
2	0.089	0.344	−0.048	−0.343**	83.869**	−0.013**	1.180	1.708	−0.006
	(0.045)	(0.284)	(0.581)	(0.106)	(22.822)	(0.004)	(0.986)	(0.888)	(0.016)
3	0.109*	0.541	−0.207	−0.317**	79.970**	−0.020***	1.558	0.628	0.025
	(0.049)	(0.317)	(0.757)	(0.099)	(21.578)	(0.003)	(1.012)	(0.785)	(0.022)
4	0.036	0.061	−0.479	−0.307**	65.582**	−0.011*	0.739	−0.232	−0.020
	(0.044)	(0.287)	(0.461)	(0.098)	(19.020)	(0.004)	(0.820)	(0.750)	(0.012)
5	0.068	0.324	0.024	−0.233*	49.411***	−0.009***	1.549	1.112	−0.006
	(0.038)	(0.248)	(0.810)	(0.092)	(10.923)	(0.002)	(0.814)	(0.564)	(0.010)
6	0.038	0.151	−0.421	−0.217*	33.970**	−0.005*	1.780	0.222	0.014
	(0.044)	(0.294)	(0.490)	(0.081)	(11.793)	(0.002)	(0.957)	(0.660)	(0.011)
7	−0.013	−0.174	−0.848	−0.153	24.522	−0.002	−0.031	−0.159	0.014
	(0.033)	(0.220)	(0.706)	(0.080)	(12.638)	(0.002)	(0.688)	(0.593)	(0.016)
8	−0.019	−0.175	−0.428	−0.038	19.108*	−0.004	1.536*	−0.242	0.001

(Continues)

TABLE 2 (Continued)

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
	(0.028)	(0.189)	(0.409)	(0.045)	(9.128)	(0.003)	(0.616)	(0.872)	(0.022)
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LinTrends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (canton × years)	636	636	678	862	678	678	862	862	862
R2	0.952	0.937	0.857	0.993	0.719	0.694	0.79	0.672	0.157
Mean dependent	1.63	11.87	37.15	28.86	3342	0.053	7.84	5.13	0.94

Notes: This table shows coefficients for δ_τ from Equation (1) for $\tau \in [-5, 8]$ for each specific outcome variable. The event year represents the year relative to the introduction of the baby bonus. The omitted category is event time $\tau = -1$. Estimates in the canton-year cell are weighted corresponding to the following structure: fertility rate with number of fertile women; crude birth rate with total population; birth interval, age mother, birth weight, share low birth weight, death rate, stillbirth rate, and sex ratio with number of births. Robust standard errors (shown in parentheses) are clustered at the cantonal level and significance levels are indicated by *0.05 **0.01 ***0.001.

Abbreviation: FE, fixed effect.

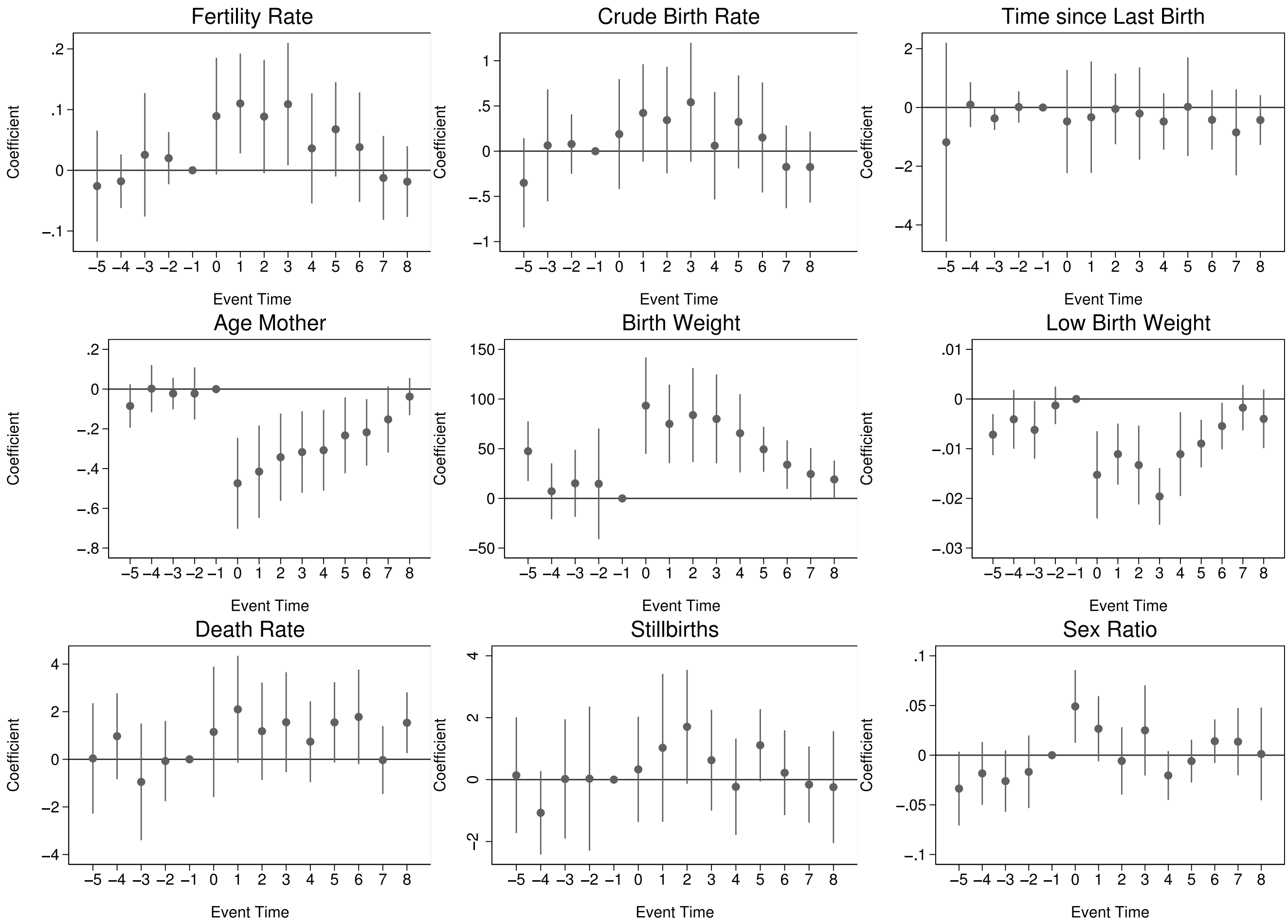

FIGURE 2 Event study DiD results. This figure shows how the effect of the introduction of the baby bonus on the respective outcomes changes over time. It depicts δ_τ from Equation (1) for $\tau \in [-5, 8]$ for each specific outcome variable. The event year represents the year relative to the introduction of the baby bonus. The dots show the point estimates per event time, while the vertical line corresponds to the 95% confidence interval

origin does not perfectly predict socio-economic status: even within nationality, socio-economic status might differ strongly.[12] Nevertheless, we argue that it serves well as a proxy.

Figures 3 and 4 and Table A.5 show the effect on the fertility rate and birth weight for mothers with a country's citizenship either in the high-income or LMIC category. We see a strong discrepancy between those two groups. While the fertility rate for mothers with a high-income country citizenship does not significantly react, we see a strong increase in the fertility rate of mothers with a citizenship from a LMIC. Thus, the average fertility effect that we find is almost solely driven by mothers from LMICs. The finding that mothers with a LMIC background select into giving birth due to the policy, raises the question whether those mothers or their children differ in other characteristics (such as newborn health) from the high-income country mothers. Looking at birth weight, we see that the birth weight for children from mothers from LMICs is higher than from mothers from high-income countries (3364 vs. 3325 grams).[13] This difference might at least partially explain the increase in birth weight of the policy. To see whether this could be the case, we run the event study on birth weight for mothers from LMICs and high-income countries separately.[14] We show the results in Figure 4. The results indicate that the birth weight effect is driven by mothers from high-income countries. Although the effect is almost twice as large in absolute terms for LMIC mothers, it barely reaches significance. Contrary to that, the effect is strongly significant for high-income mothers. While this does not prove that the policy had a direct effect on birth weight, it rules out that LMIC mothers are the sole driver of the birth weight effect.

More generally, the seminal paper by Becker and Lewis (1973) highlights the quantity and quality trade off of having children. While a higher income might increase the overall fertility, it could also increase parents' investment in these children and thus their quality keeping the number of children constant. As such, a priori it is not clear which effect will

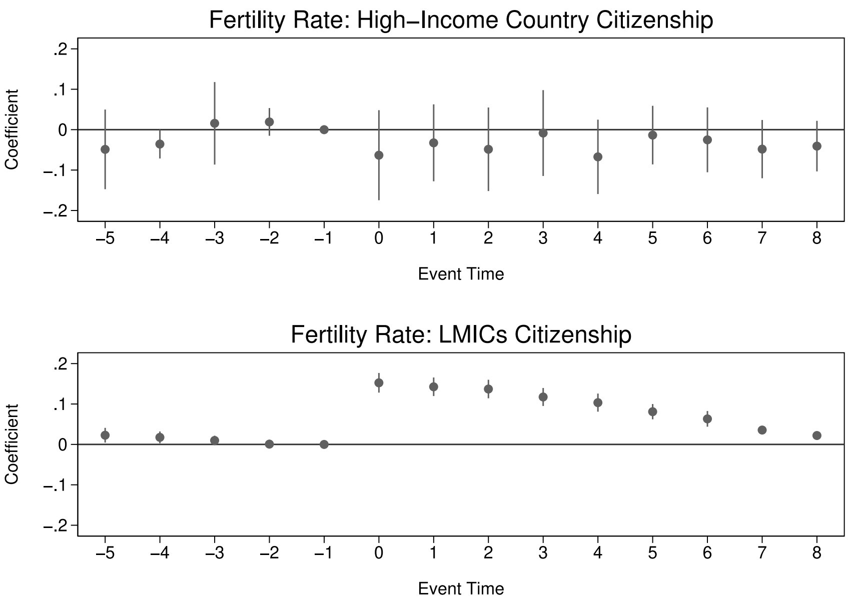

FIGURE 3 Fertility rate and citizenship of mother. This figure shows how the effect of the introduction of the baby bonus on fertility by income country group changes over time. It depicts δ_τ from Equation (1) for $\tau \in [-5, 8]$ for the respective fertility rate. The event year represents the year relative to the introduction of the baby bonus. The dot shows the point estimate per event time, while the vertical line corresponds to the 95% confidence interval. LMICs, low- and middle-income countries

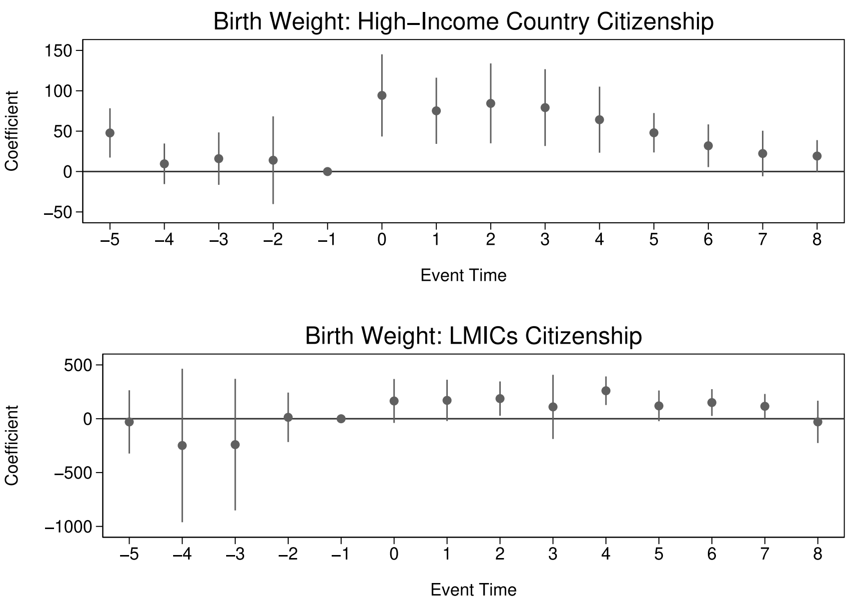

FIGURE 4 Birth weight and citizenship of mother. This figure shows how the effect of the introduction of the baby bonus on birth weight by income country group changes over time. It depicts δ_τ from Equation (1) for $\tau \in [-5, 8]$ for the respective birth weight. The event year represents the year relative to the introduction of the baby bonus. The dot shows the point estimate per event time, while the vertical line corresponds to the 95% confidence interval

dominate. Combining this takeaway with the fact that high-socioeconomic parents have shown to adapt more health promoting behavior in various settings (as extensively summarized by Almond et al. (2018) when it comes to smoking, drinking, healthy diets, doing exercises, going to the doctor, etc.) it is probably not surprising, that in this context the quantity effect (i.e., overall fertility) dominates the quality effect (i.e., birth weight) for LMIC mothers and vice versa for high-income mothers.

We also analyze how the age of the mother and the time between two births change with the introduction of the policy. Subfigure 3 and 4 in Figure 2 show a relatively strong and significant reduction in the age of mothers giving birth and a not statistically significant decline in the birth interval.[15] More precisely, column 4 in Table 2 states that the age of the mother reduces by 0.474 in the introduction period, which corresponds to roughly 6 months. In light of this result, it is at first surprising that we do not find a fertility effect that is long lasting. Mothers who are deciding to have a planned child earlier are less likely to be affected of negative shocks to a partnership or their own health. However, in our setting the decision to have a child earlier does not translate into a long-run increase in fertility. This is very likely the result of the differential impact of fertility on high-income and LMIC mothers. The latter tend to be younger at birth (27.9 vs. 29.2 years) so that opposite to the effect on birth weight, the effect on maternal age seems to be driven by a change in composition of mothers (see also Table A.6).

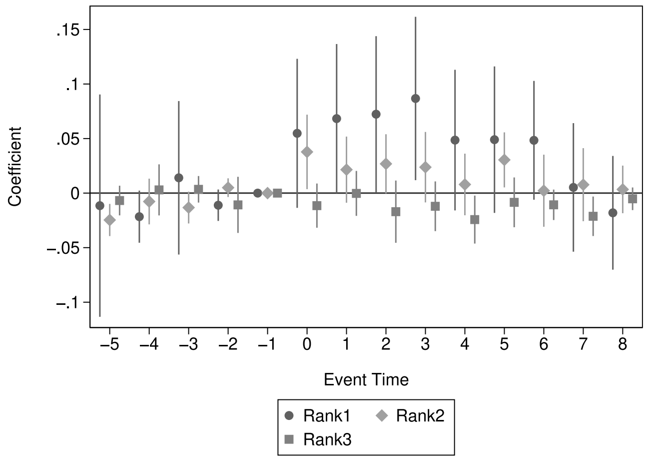

FIGURE 5 Child rank specific fertility effect. This figure shows how the effect of the introduction of the baby bonus on fertility by child rank changes over time. It depicts δ_τ from Equation (1) for $\tau \in [-5, 8]$ for the respective fertility rate. The event year represents the year relative to the introduction of the baby bonus. The dot shows the point estimate per event time, while the vertical line corresponds to the 95% confidence interval

Finally, we also study the intensive and extensive margin of having a first child and having more children in Figure 5 and Table A.7. One might suspect that the intensive margin (i.e., having an additional child) would react more to a financial incentive as the marginal costs for children are decreasing. However, looking at point estimates, we see suggestive evidence that the fertility rate for the first child is slightly higher than for the second or third child. The latter is even totally unaffected by the policy. Though, none of these differences are statistically significant.

6 | RESULTS: BIRTH-SCHEDULING

In Figure 6, we report the graphical results of our birth-scheduling analysis.

For none of the three events (introduction, increase, and abolition), there is a clear pattern around the policy change. This holds true for both specifications reported in Panel A (day-of-week fixed effects) and Panel B (day-of-year fixed effects), as well as for both the total count of births per day as shown in Figure 6 and the log of the outcome variable as shown in Figure A.3. If anything, there is a slight decline in births after an increase in the baby bonus of more than CHF 200.

The are several possible reasons for the absence of birth-scheduling. First, daily birth counts per canton are small. This makes it hard to discover a statistically significant effect. While we study cantonal policy changes, previous literature analyzed national programs and thus national birth counts. Second, increases in birth allowances are much smaller as in other countries. Therefore, parents may not be willing to risk their child's health. Third, we also check newspaper articles for media coverage of the baby bonus policy changes. We search for articles about birth allowances on Factiva, one of the most important database for press, company, and business information and the archives of several newspapers.[16] The results of this search are depicted graphically in Figure A.4. There was substantial media coverage on the Swiss baby bonus. Most of these articles describe differences across cantons or recently implemented changes in the payment structure. However, only roughly 25 percent of all relevant articles cover changes in the baby bonus scheme that are going to be implemented in the near future. This is especially true for increases. Thus, it is not surprising that birth-scheduling—a short-term behavioral change—is not taking place.

There is one exception. In 2012, the canton of Geneva doubled the amount from CHF 1000 to CHF 2000. This increase was the result of a cantonal referendum initiated by the political left and led to a lot of discussion and widespread information exchange. Thus, we look in more detail at this specific increase, which is depicted in Figure 7. Both the total number of births and the residuals demonstrate that there is an increase in births happening after the policy change. However, the baseline daily count of births in Geneva is low and therefore the results are not significant.

Summarizing the analysis of birth-scheduling, there seem to be only marginal behavioral effects. This is likely the result of low daily birth counts and small changes in the payment structure. In the specific case of Geneva in 2012, where the baby bonus was doubled to CHF 2000, graphical analysis suggests a postponement of births. But even in this case, we might not have enough power to document a significant effect because of the small amount of daily births.

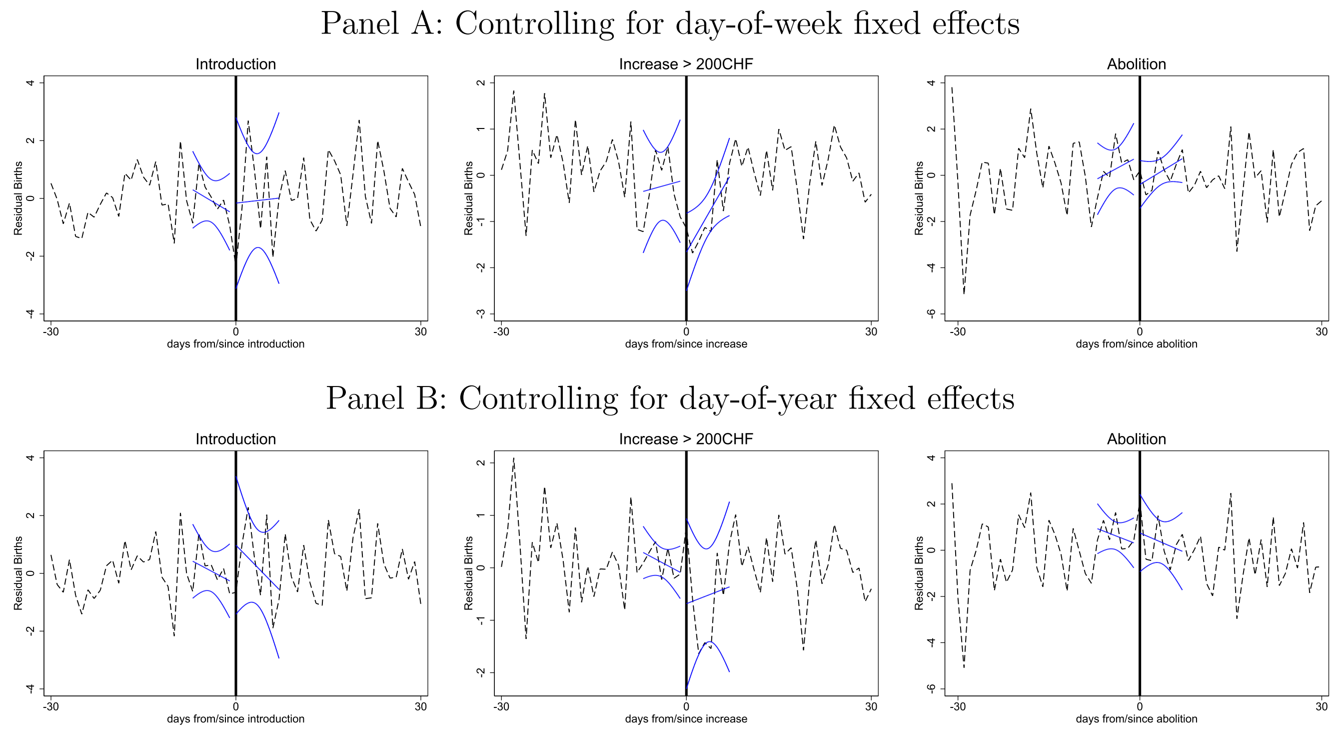

FIGURE 6 Birth-scheduling event study. This figure shows the residuals (in dashed black line) from a linear prediction of estimating Equation (2) of total births per day and a linear fit including a 95% confidence interval (in blue) for the week before and after the policy change. Panel A reports the residual when ζ_i controls for day-of-week fixed effects and Panel B when ζ_i controls for day-of-year fixed effects. The three event studies combine either all introductions, increases above CHF 200, or abolition across cantons and time [Colour figure can be viewed at wileyonlinelibrary.com]

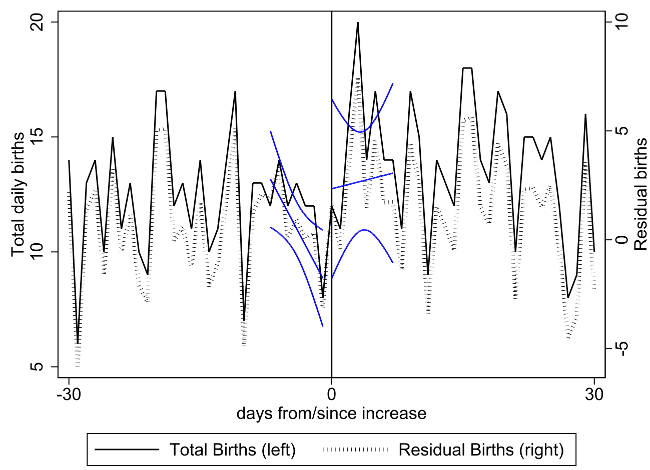

FIGURE 7 Birth-scheduling Geneva: Policy change on January 1, 2012. This figure shows the daily count of total births (left axis) and the residuals (right axis) from a linear prediction of estimating Equation (2) of total births per day. Additionally, a linear fit is added including a 95% confidence interval (in blue) for the week before and after the policy change. ζ_i controls for day-of-week fixed effects for the canton of Geneva. The 60 days window reports the 30 days pre- and post policy change (black vertical line) on January 1, 2012, where the baby bonus got doubled from CHF 1000 to CHF 2000 due to a public initiative [Colour figure can be viewed at wileyonlinelibrary.com]

7 | CONCLUSION

We exploit a unique quasi-experimental setting in Switzerland that allows us to study the effect of birth allowances on fertility, newborn health, and birth-scheduling. In Switzerland, cantons are free to implement birth allowances. This gives rise to a lot of cantonal variation over time, which we use in an event study setting. Based on administrative data, we analyze various outcome measures—fertility rate, crude birth rate, birth weight, share of low-birth weight, infant deaths, stillbirths and the sex-ratio as a proxy for miscarriages.

To study birth-scheduling, we use a graphical time-series event study analysis based on daily birth counts. We base the birth-scheduling analysis on the fact that children were already in the womb when the policy introduction was announced. Thus, couples cannot react by becoming new parents, but they might shift the birth date to receive the baby bonus.

We do not find evidence for birth-scheduling. We argue that this results from several features, such as only minor changes in the amount and a small sample size due to a low cantonal birth count per day.

Looking at the effect of the baby bonus on fertility and newborn health, we find that the fertility rate and the birth weight increase. All significant effects are declining over time. While the effect on birth weight fades out but stays significantly different from zero, the effect on fertility is transitory. Furthermore, we find that mothers with nationalities from LMICs drive the fertility effect. We argue that this is an approximation for socio-economic status. The often fairly small baby bonus might still be an incentive for mothers with a low socio-economic status. Surprisingly, children from these mothers have a slightly higher birth weight than children from high-income country mothers. One might therefore speculate that the birth weight effect is driven by the former mothers′ selection into birth. However, the subgroup analysis shows that birth weight does not significantly increase for women from LMICs, but for women with citizenship from high-income countries. While we cannot provide conclusive evidence that the allowance itself leads directly to better newborn health, this finding rules out that compositional changes are the sole driver of the effect on birth weight. Finally, we find that the effect on birth weight is especially strong at the lower end of the distribution documented by a significant decline in the occurrence of low birth weight.

Other health outcomes, such as infant deaths, stillbirths and the sex ratio do not show significant changes. We argue that those are severe measures and unlikely to be affected by a small financial bonus in a rich economy.

In terms of the fertility rate, we do not find significantly different effects at the intensive or extensive margin. However, we see suggestive evidence that the point estimate for the fertility rate of the first child is slightly higher than for the second child, while the effect of the fertility rate of the second child is significantly different from 0. The fertility rate for three or more children does not react to the policy.

Compared to other countries, the Swiss baby bonus is a cheap intervention to temporarily increase fertility and permanently improve newborn health. Importantly, other studies (Almond et al., 2018; Carneiro et al., 2015; Schwandt, 2018) suggest that health measures at birth translate into meaningful later life outcomes such as higher earnings or lower welfare dependence. Thus, the efficiency of the program might be underestimated by only studying outcomes visible at birth. One potential limitation of the study is the external validity. Switzerland is a comparably rich country. Since we can show that there is effect heterogeneity by country of origin, it is also likely, that the effect differs between countries—or at least its magnitude.

Our results entail important policy advice. Providing a birth allowance can increase fertility, even when the effect is not long lasting. Crucially, our findings show a heterogeneity by citizenship status of the mother. With additional data, future research could investigate heterogeneity by socioeconomic status in more detail. Finally, an unintended, yet beneficial side-effect of this policy is the positive impact on newborn health.

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CONFLICT OF INTEREST

The authors have no conflicts of interest to declare.

DATA AVAILABILITY STATEMENT

The data that support the findings of this study are available from the Swiss Federal Statistical Office. Restrictions apply to the availability of these data, which were used under license for this study. Data are available at https://www.bfs.admin.ch/bfs/de/home/dienstleistungen/forschung/zugang-anonymisierte-einzeldaten.assetdetail.220225.html with the permission of the Swiss Federal Statistical Office (Swiss Federal Statistical Office, 2017).

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ENDNOTE

[1] A related strand of literature investigates tax incentives and birth scheduling (Dickert-Conlin and Chandra, 1999; Schulkind and Shapiro, 2014; LaLumia et al., 2015). For the US, these papers show that the tax scheme incentivizes parents to schedule births in late December instead of early January.

[2] 1 CHF equals roughly 1 USD.

[3] In a robustness check, we do, however, control for changes in child allowances and find similar results.

[4] The evolution of child allowances over time are depicted in Figure A.1 in the Appendix.

[5] Eligibility for child and education allowances depends on the age of the child, the educational track of the child, and the employment status of the parent.

[6] See for further information on announcement and implementation dates Table A.1 in the Appendix.

[7] We follow conventional definition to measure these two rates. The crude birth rate is the total number of births divided by the total population multiplied by 1000. The total fertility rate results from dividing the total number of births by the total number of fertile women aged 15–49 multiplied by 35, the total age range.

[8] As the data set on the outcome measures only starts in 1969, the variation before 1969 cannot be exploited in this paper and is therefore not shown in the graphs.

[9] For this part of the analysis we focus solely on the introduction and not on increases of the baby bonus. With our empirical approach at hand, a study of increases is not straightforward and possibly even problematic if future changes in bonus size are endogenous, that is depend on the success of the introduction.

[10] There are in total 26 cantons in Switzerland. As three of these cantons have been treated before the start of our data—always takers—we are left with 23 clusters in our analysis.

[11] The effects are also robust to adjusted inference for multiple hypothesis testing, that is all significant effects at the 5% level remain significant at the same level. We use the method described by Anderson (2008) to compute sharpened False Discovery Rate (FDR) q-values. This rate describes the expected proportion of rejections that are type 1 errors. We use this approach because of its (1) flexibility as p-values are the input variables, (2) power as, for example, the Bonferroni adjustment is very conservative, and (3) the method works well also when p-values are positively correlated with each other. The latter point, due to the event time structure, is relatively likely to hold in this setting. Results are available upon request.

[12] A large body of literature (see, e.g., Antecol and Bedard (2006); Biddle et al. (2007); Chiswick et al. (2008); Constant et al. (2018); McDonald and Kennedy (2005) and many more) shows the so called *healthy immigrant effect* stating that immigrants tend to be healthier than comparing native populations. However, the same strand of literature also states that the health advantage of immigrants declines with time spent in the host country. As we do not know anything about the time spent in Switzerland, we argue that if anything, we estimate lower bounds as mothers from LMICs might be positively selected.

[13] The birth weight for the four income country groups is: High-income 3325 grams, upper middle-income 3393 grams, lower middle 3279 grams, and low-income 3308 grams.

[14] The group specific fertility rates are calculated by dividing the number of births from a specific group by the number of fertile women in the whole country.

[15] Keep in mind that the measure of birth interval can only be calculated for higher order births excluding first births. This makes it harder to document a statistically significant effect.

[16] These newspapers include *Tages Anzeiger, Neue Zürcher Zeitung, Blick, St. Galler Tagblatt*, *24heures,* and *Le Temps*.

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APPENDIX

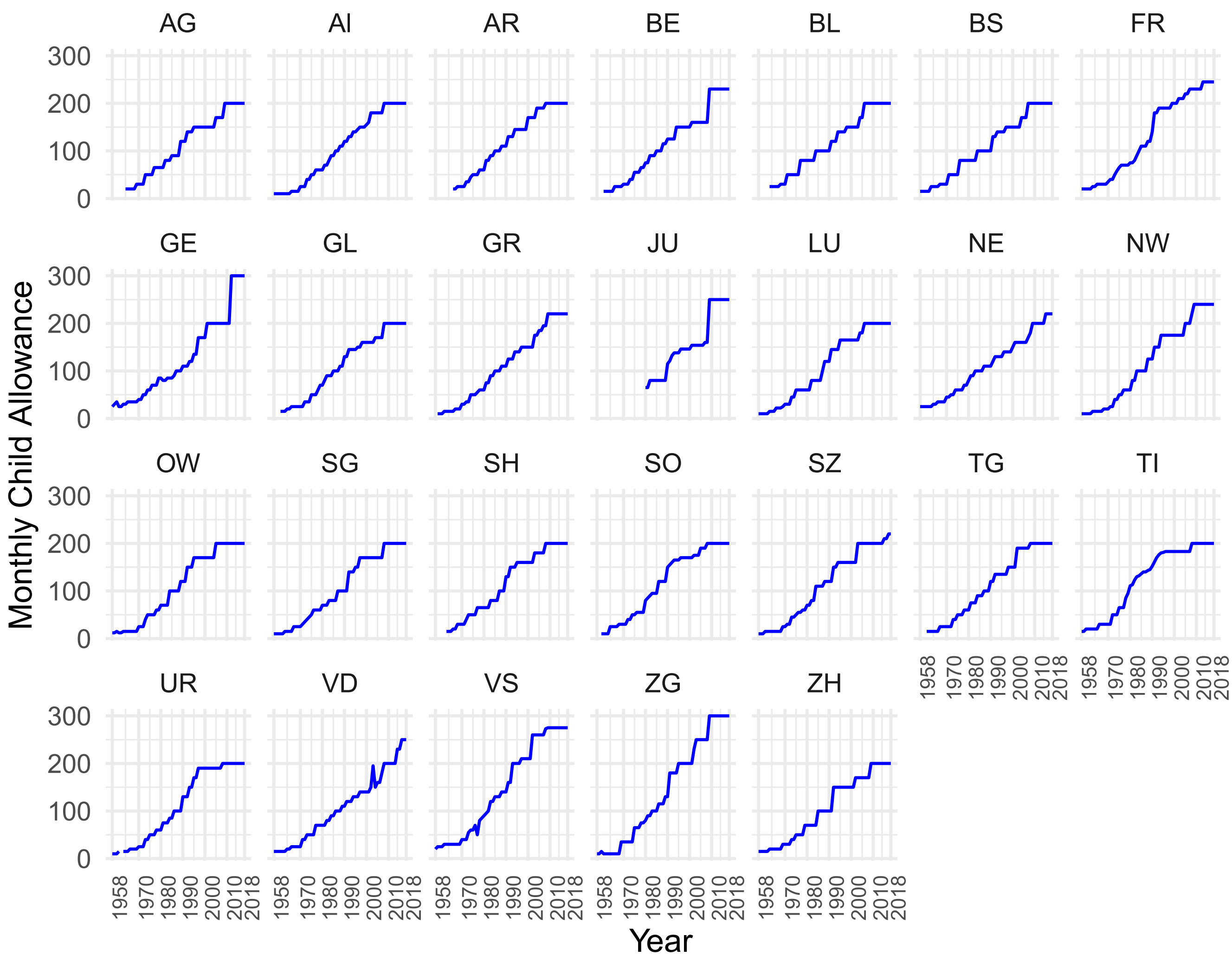

FIGURE A.1 Monthly child allowances per canton. This figure shows the amount of child allowances provided per child per month per canton in current year values [Colour figure can be viewed at wileyonlinelibrary.com]

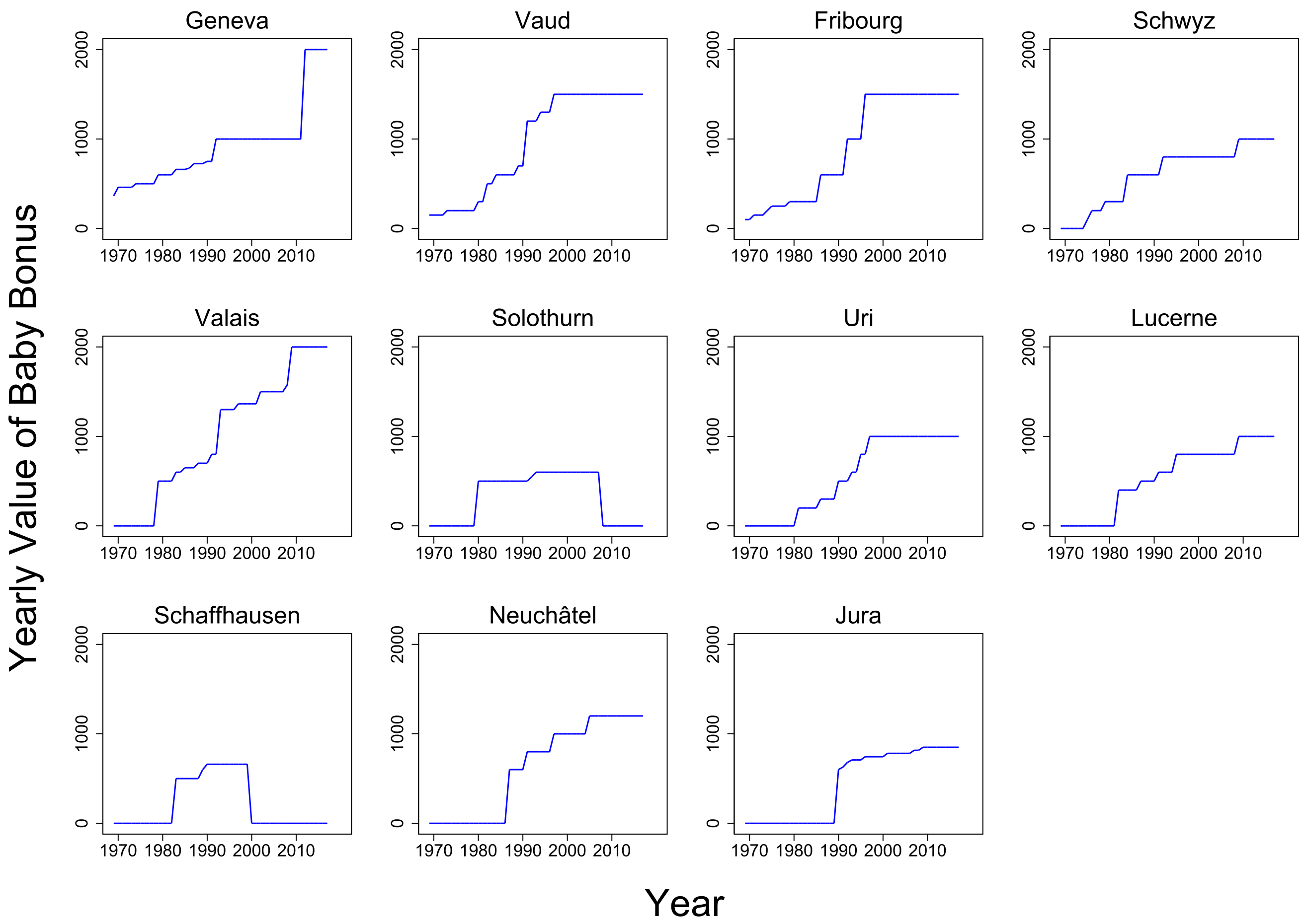

FIGURE A.2 Time variation of birth allowances by treated cantons. This figure shows the amount of birth allowances provided per child per canton in current year values. It only shows the movement over time for those cantons which ever introduced a baby bonus at one point in time. The ordering of the cantons is according to their introduction year of the birth allowance [Colour figure can be viewed at wileyonlinelibrary.com]

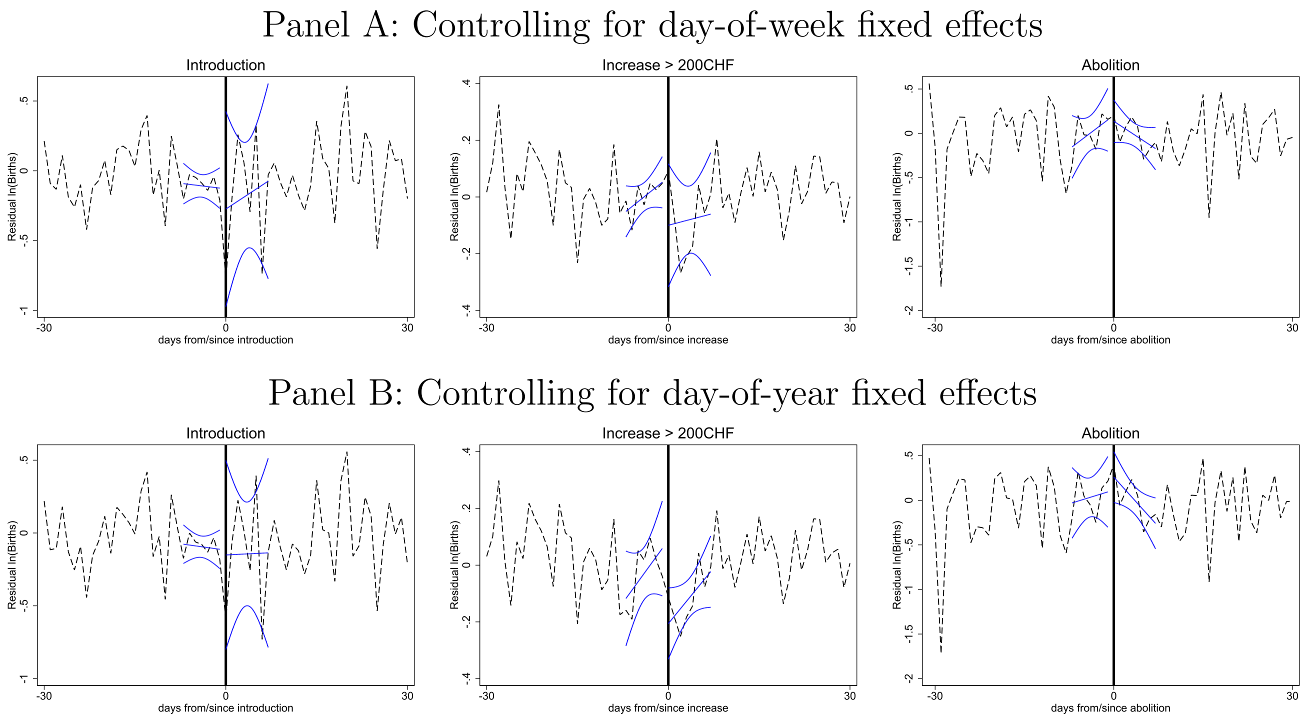

FIGURE A.3 Birth-scheduling log specification event study. This figure shows the residuals (in dashed black line) from a linear prediction of estimating Equation (2) of the log of total births per day and a linear fit including a 95% confidence interval (in blue) for the week before and after the policy change. Panel A reports the residual when ζ_i controls for day-of-week fixed effects and Panel B when ζ_i controls for day-of-year effects. The three event studies combine either all introductions, increases above CHF 200, or abolition across cantons and time [Colour figure can be viewed at wileyonlinelibrary.com]

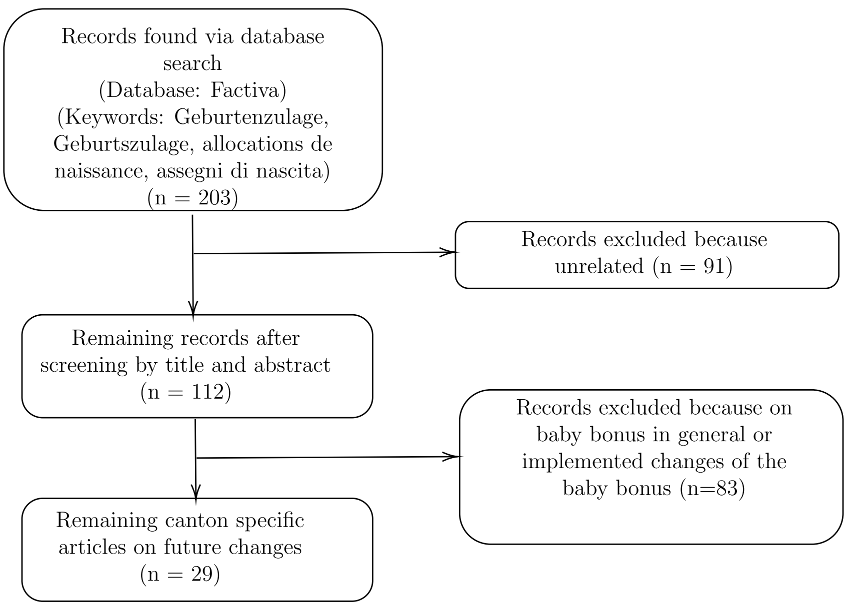

FIGURE A.4 Media search. This figure shows the results of the media search related to the keywords “Geburtenzulage,” “Geburtszulage,” “allocations de naissance,” “assegni di nascita” on Factiva

TABLE A.1 Overview policy changes in detail

Canton	Announcement date	Implementation date
Geneva	14.03.1969	01.05.1969
Fribourg	15.12.1970	01.01.1971
Vaud	27.11.1972	01.01.1973
Geneva	12.06.1973	01.07.1973
Fribourg	24.09.1973	01.01.1974
Schwyz	09.05.1974	01.07.1974
Fribourg	29.10.1974	01.01.1975
Schwyz	05.12.1975	01.01.1976
Schwyz	25.09.1977	01.01.1978
Valais	01.12.1977	01.01.1978
Solothurn	12.06.1978	01.01.1979
Geneva	12.10.1978	01.01.1979
Fribourg	10.10.1978	01.07.1979
Vaud	18.09.1979	01.01.1980
Uri	28.09.1980	01.01.1981
Lucerne	10.03.1980	01.07.1981
Vaud	13.11.1981	01.01.1982
Schaffhausen	24.06.1982	01.07.1982
Geneva	07.03.1982	01.07.1982
Valais	12.11.1982	01.01.1983
Schwyz	20.10.1983	01.01.1984
Vaud	12.12.1983	01.01.1984
Valais	16.11.1984	01.01.1985
Geneva	15.02.1985	01.04.1985
Fribourg	25.09.1985	01.01.1986
Uri	08.10.1985	01.01.1986
Geneva	25.06.1986	01.01.1987
Neuchatel	20.10.1986	01.01.1987
Lucerne	14.11.1986	01.01.1987
Valais	13.11.1987	01.07.1988
Schaffhausen	06.06.1988	01.07.1988
Vaud	09.11.1988	01.01.1989
Jura	24.02.1989	01.07.1989
Uri	08.06.1989	01.01.1990
Geneva	27.09.1989	01.01.1990
Schaffhausen	06.11.1989	01.01.1990
Valais	28.09.1990	01.01.1991
Vaud	30.11.1990	01.01.1991

(Continues)

TABLE A.1 (Continued)

Canton	Announcement date	Implementation date
Neuchatel	03.12.1990	01.01.1991
Jura	04.12.1990	01.01.1991
Geneva	12.12.1990	01.01.1991
Lucerne	18.12.1990	01.01.1991
Fribourg	18.02.1991	01.03.1991
Jura	16.04.1991	01.10.1991
Solothurn	15.10.1991	01.01.1992
Schwyz	08.12.1991	01.01.1992
Valais	06.04.1992	01.01.1993
Jura	20.09.1992	01.01.1993
Solothurn	12.11.1992	01.01.1993
Uri	08.12.1992	01.01.1993
Vaud	26.11.1993	01.01.1994
Lucerne	13.09.1994	01.01.1995
Uri	28.09.1994	01.01.1995
Fribourg	13.11.1995	01.01.1996
Jura	21.11.1995	01.01.1996
Valais	11.09.1996	01.01.1997
Vaud	24.09.1996	01.01.1997
Uri	13.11.1996	01.01.1997
Neuchatel	27.11.1996	01.01.1997
Schaffhausen	05.09.1999	01.01.2000
Jura	31.10.2000	01.01.2001
Valais	23.09.2001	01.01.2002
Neuchatel	01.12.2004	01.01.2005
Jura	26.11.2006	01.01.2007
Valais	31.10.2007	01.01.2008
Solothurn	16.11.2007	01.01.2008
Valais	11.09.2008	01.01.2009
Schwyz	28.09.2008	01.01.2009
Jura	25.11.2008	01.01.2009
Lucerne	28.11.2008	01.01.2009
Geneva	23.06.2011	01.01.2012

Notes: This table gives detailed information on every policy change regarding birth allowances in Switzerland starting in 1969 and informs about the announcement date and about the implementation date of each stated change.

TABLE A.2 Event study estimates with wild cluster bootstrapped standard errors

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
Event time estimate: δ_τ									
−5	−0.026	−0.350	−1.184	−0.085	47.483	−0.007	0.037	0.140	−0.034
	(0.6346)	(0.5145)	(0.5606)	(0.6877)	(0.2142)	(0.3143)	(0.9730)	(0.8458)	(0.2302)
−4	−0.018	−0.254	0.094	0.002	7.167	−0.004	0.971	−1.071	−0.018
	(0.1471)	(0.1321)	(0.9039)	(0.3243)	(0.7968)	(0.4074)	(0.2743)	(0.2032)	(0.4535)
−3	0.026	0.064	−0.371	−0.023	15.172	−0.006	−0.949	0.021	−0.026
	(0.9009)	(0.8899)	(0.1091)	(0.7878)	(0.3343)	(0.4004)	(0.5145)	(0.9830)	(0.1632)
−2	0.020	0.079	0.015	−0.022	14.659	−0.001	−0.073	0.031	−0.017
	(0.7207)	(0.7928)	(0.9389)	(0.2733)	(0.6066)	(0.5255)	(0.9129)	(0.9630)	(0.3794)
0	0.089	0.189	−0.475	−0.474***	93.340**	−0.015	1.151	0.331	0.049
	(0.4545)	(0.6076)	(0.6396)	(0.2332)	(0.0300)	(0.1001)	(0.4484)	(0.6907)	(0.2663)
1	0.110*	0.423	−0.332	−0.415*	74.973**	−0.011**	2.099	1.027	0.027
	(0.0982)	(0.2492)	(0.7738)	(0.082)	(0.0250)	(0.0431)	(0.1341)	(0.4545)	(0.2252)
2	0.089	0.344	−0.048	−0.343*	83.869**	−0.013**	1.180	1.708	−0.006
	(0.3483)	(0.3914)	(0.9510)	(0.0551)	(0.0310)	(0.0422)	(0.3904)	(0.4194)	(0.7347)
3	0.109*	0.541	−0.207	−0.317*	79.970*	−0.020***	1.558	0.628	0.025
	(0.074)	(0.2793)	(0.7968)	(0.0721)	(0.0501)	(0.0060)	(0.2012)	(0.5285)	(0.4374)
4	0.036	0.061	−0.479	−0.307	65.582**	−0.011**	0.739	−0.232	−0.020
	(0.7457)	(0.8539)	(0.3984)	(0.1461)	(0.0160)	(0.0487)	(0.4034)	(0.7638)	(0.2222)
5	0.068	0.324	0.024	−0.233*	49.411***	−0.009*	1.549	1.112	−0.006
	(0.2683)	(0.2943)	(0.9770)	(0.0861)	(0.0070)	(0.0791)	(0.1902)	(0.1532)	(0.5976)
6	0.038	0.151	−0.421	−0.217*	33.970**	−0.005*	1.780	0.222	0.014
	(0.7337)	(0.7738)	(0.4064)	(0.0630)	(0.0310)	(0.0652)	(0.2002)	(0.7658)	(0.2342)
7	−0.013	−0.174	−0.848	−0.153	24.522	−0.002	−0.031	−0.159	0.014
	(0.5986)	(0.4705)	(0.2913)	(0.1982)	(0.1441)	(0.5345)	(0.9640)	(0.8128)	(0.3674)
8	−0.019	−0.175	−0.428	−0.038	19.108*	−0.004	1.536*	−0.242	0.001
	(0.4545)	(0.238)	(0.3704)	(0.5325)	(0.0881)	(0.3183)	(0.0671)	(0.7938)	(0.9610)

(Continues)

TABLE A.2 (Continued)

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LinTrends	Yes	Yes	No	Yes	Yes	Yes	Yes	Yes	Yes
N (canton × years)	636	636	678	862	678	678	862	862	862
R2	0.952	0.937	0.857	0.993	0.719	0.694	0.79	0.672	0.157
Mean dependent	1.63	11.87	37.15	28.86	3342	0.053	7.84	5.13	0.94

Notes: This table shows coefficients for δ_τ from Equation (1) for $\tau \in [-5, 8]$ for each specific outcome variable. The event year represents the year relative to the introduction of the baby bonus. The omitted category is event time $\tau = -1$. Estimates in the canton-year cell are weighted corresponding to the following structure: fertility rate with number of fertile women; crude birth rate with total population; birth interval, age mother, birth weight, share low birth weight, death rate, stillbirth rate, and sex ratio with number of births. Values in parenthesis indicate the wild-cluster bootstrapped p-values. Stars indicate significance levels of *0.10 **0.05 ***0.01.

Abbreviation: FE, fixed effect.

TABLE A.3 Event study estimates without abolishing cantons

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
Event time estimate: δ_τ									
−5	−0.027	−0.265	−1.184	−0.085	46.348**	−0.008***	0.596	0.081	−0.047*
	(0.039)	(0.207)	(1.632)	(0.053)	(13.619)	(0.002)	(1.422)	(0.953)	(0.019)
−4	−0.014	−0.162	0.094	0.002	5.410	−0.004	1.013	−0.963*	−0.029
	(0.010)	(0.114)	(0.369)	(0.057)	(12.342)	(0.003)	(1.154)	(0.369)	(0.015)
−3	0.031	0.147	−0.371	−0.023	22.718**	−0.008***	−1.828	−0.452	−0.037*
	(0.045)	(0.276)	(0.192)	(0.038)	(7.095)	(0.002)	(1.141)	(1.201)	(0.015)
−2	0.029*	0.158	0.015	−0.022	7.594	−0.002	−0.345	0.309	−0.025
	(0.012)	(0.115)	(0.257)	(0.063)	(29.031)	(0.002)	(1.094)	(1.058)	(0.020)
0	0.064	0.237	−0.475	−0.474***	92.283**	−0.014**	2.052	1.344**	0.044
	(0.053)	(0.299)	(0.848)	(0.110)	(24.301)	(0.004)	(1.916)	(0.460)	(0.023)
1	0.099*	0.554*	−0.332	−0.415**	69.616***	−0.012***	2.873***	1.837	0.027
	(0.046)	(0.259)	(0.916)	(0.112)	(16.197)	(0.003)	(0.539)	(1.263)	(0.019)
2	0.080	0.446	−0.048	−0.343**	81.385***	−0.013**	2.081*	1.820	−0.017
	(0.048)	(0.280)	(0.581)	(0.106)	(20.790)	(0.004)	(0.960)	(1.072)	(0.016)
3	0.111*	0.719*	−0.207	−0.317**	79.729***	−0.021***	2.444	1.419*	0.029
	(0.052)	(0.322)	(0.757)	(0.099)	(20.313)	(0.002)	(1.192)	(0.669)	(0.024)
4	0.017	0.066	−0.479	−0.307**	69.246***	−0.014**	0.901	−0.071	−0.020
	(0.052)	(0.334)	(0.461)	(0.098)	(17.258)	(0.004)	(0.859)	(0.981)	(0.015)
5	0.060	0.386	0.024	−0.233*	46.752***	−0.010***	2.399***	1.230	−0.003
	(0.042)	(0.274)	(0.810)	(0.092)	(7.463)	(0.002)	(0.619)	(0.688)	(0.011)
6	0.038	0.235	−0.421	−0.217*	38.340**	−0.007***	2.105*	0.506	0.021
	(0.050)	(0.338)	(0.490)	(0.081)	(10.394)	(0.002)	(0.945)	(0.741)	(0.011)
7	−0.010	−0.097	−0.848	−0.153	29.290*	−0.003	0.433	0.317	0.014
	(0.032)	(0.211)	(0.706)	(0.080)	(12.355)	(0.002)	(0.833)	(0.596)	(0.020)
8	−0.010	−0.080	−0.428	−0.038	16.158	−0.005	1.743*	0.467	0.013
	(0.032)	(0.220)	(0.409)	(0.045)	(9.792)	(0.003)	(0.812)	(0.833)	(0.021)

(Continues)

TABLE A.3 (Continued)

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LinTrends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (canton × years)	617	617	655	830	655	655	830	830	830
R2	0.952	0.94	0.857	0.993	0.715	0.692	0.827	0.673	0.158
Mean dependent	1.63	11.87	37.15	28.86	3342	0.053	7.84	5.13	0.94

Notes: This table shows coefficients for δ_τ from Equation (1) for $\tau \in [-5, 8]$ for each specific outcome variable. The event year represents the year relative to the introduction of the baby bonus. The omitted category is event time $\tau = -1$. Estimates in the canton-year cell are weighted corresponding to the following structure: fertility rate with number of fertile women; crude birth rate with total population; birth interval, age mother, birth weight, share low birth weight, death rate, stillbirth rate, and sex ratio with number of births. Robust standard errors (shown in parentheses) are clustered at the cantonal level and significance levels are indicated by *0.05 **0.01 ***0.001.

Abbreviation: FE, fixed effect.

TABLE A.4 Event study estimates controlling for child allowances

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
Event time estimate: δ_τ									
−5	−0.041	0.285	−1.075	−0.083	49.447**	−0.008***	0.203	0.126	−0.033
	(0.043)	(0.169)	(1.702)	(0.051)	(13.889)	(0.002)	(1.115)	(0.886)	(0.018)
−4	−0.029	0.271	0.205	0.003	9.434	−0.005	1.128	−1.077	−0.018
	(0.017)	(0.156)	(0.326)	(0.056)	(13.317)	(0.003)	(0.998)	(0.650)	(0.015)
−3	0.019	0.172	−0.271	−0.022	17.379	−0.007*	−0.859	0.015	−0.026
	(0.048)	(0.167)	(0.227)	(0.038)	(18.247)	(0.003)	(1.304)	(0.920)	(0.015)
−2	0.018	−0.113	0.019	−0.022	14.857	−0.001	−0.019	0.028	−0.017
	(0.018)	(0.132)	(0.258)	(0.064)	(26.996)	(0.002)	(0.848)	(1.117)	(0.018)
0	0.050	−0.495	−0.522	−0.470***	95.224***	−0.016**	1.554	0.296	0.051**
	(0.051)	(0.352)	(0.845)	(0.110)	(24.262)	(0.005)	(1.126)	(0.790)	(0.014)
1	0.081	−0.494	−0.349	−0.409**	77.337***	−0.012**	2.275***	0.968	0.030
	(0.044)	(0.351)	(0.915)	(0.114)	(19.459)	(0.003)	(0.574)	(1.118)	(0.016)
2	0.066	−0.314	−0.042	−0.337**	86.816***	−0.014**	1.300	1.653	−0.002
	(0.048)	(0.298)	(0.578)	(0.108)	(22.398)	(0.004)	(0.734)	(0.854)	(0.015)
3	0.094	−0.162	−0.170	−0.308**	83.785***	−0.021***	1.425	0.547	0.030
	(0.050)	(0.348)	(0.766)	(0.102)	(20.578)	(0.003)	(0.909)	(0.816)	(0.022)
4	0.023	−0.497	−0.452	−0.302**	68.656**	−0.012*	0.763	−0.278	−0.018
	(0.042)	(0.290)	(0.497)	(0.097)	(19.091)	(0.005)	(0.883)	(0.815)	(0.012)
5	0.054	−0.095	0.024	−0.231*	51.267***	−0.010***	1.664**	1.090	−0.005
	(0.035)	(0.258)	(0.827)	(0.095)	(10.451)	(0.002)	(0.570)	(0.549)	(0.011)
6	0.023	−0.200	−0.447	−0.219*	34.373**	−0.006*	2.066*	0.236	0.013
	(0.043)	(0.291)	(0.486)	(0.080)	(11.189)	(0.002)	(0.894)	(0.675)	(0.011)
7	−0.025	−0.442*	−0.877	−0.155	24.394	−0.002	0.271	−0.138	0.012
	(0.033)	(0.177)	(0.699)	(0.079)	(12.685)	(0.002)	(0.708)	(0.594)	(0.017)
8	−0.025	−0.312	−0.443	−0.039	19.020*	−0.004	1.683*	−0.231	0.001
	(0.028)	(0.165)	(0.406)	(0.044)	(9.134)	(0.003)	(0.649)	(0.862)	(0.022)

(Continues)

TABLE A.4 (Continued)

	(1) Fertility rate	(2) Crude birth rate	(3) Birth interval	(4) Age mother	(5) Birth weight	(6) Share low birth weight	(7) Death rate	(8) Stillbirth rate	(9) Sex ratio
Child allowance	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Canton FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
LinTrends	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
N (canton × years)	636	636	678	862	678	678	862	862	862
R2	0.952	0.94	0.857	0.993	0.719	0.974	0.827	0.672	0.159
Mean dependent	1.63	11.87	37.15	28.86	3342	0.053	7.84	5.13	0.94

Notes: This table shows coefficients for δ_τ from Equation (1) for $\tau \in [-5, 8]$ for each specific outcome variable. The event year represents the year relative to the introduction of the baby bonus. The omitted category is event time $\tau = -1$. Estimates in the canton-year cell are weighted corresponding to the following structure: fertility rate with number of fertile women; crude birth rate with total population; birth interval, age mother, birth weight, share low birth weight, death rate, stillbirth rate, and sex ratio with number of births. Robust standard errors (shown in parentheses) are clustered at the cantonal level and significance levels are indicated by *0.05 **0.01 ***0.001.

Abbreviation: FE, fixed effect.

TABLE A.5 Fertility and birth weight by citizenship of mother

	Fertility rate		Birth weight	
	(1) High-income	(2) LMIC	(3) High-income	(4) LMIC
−5	−0.049	0.023*	47.808**	−29.463
	(0.05)	(0.01)	(14.71)	(141.66)
−4	−0.036	0.017*	9.581	−248.572
	(0.02)	(0.01)	(12.11)	(343.67)
−3	0.016	0.010	15.998	−240.484
	(0.05)	(0.01)	(15.64)	(294.62)
−2	0.019	0.001	14.035	13.205
	(0.02)	(0.01)	(26.19)	(110.70)
0	−0.063	0.152***	94.278***	165.105
	(0.05)	(0.01)	(24.53)	(98.26)
1	−0.033	0.143***	75.224***	170.447
	(0.05)	(0.01)	(19.76)	(92.46)
2	−0.049	0.137***	84.411**	187.007*
	(0.05)	(0.01)	(23.86)	(76.86)
3	−0.008	0.117***	79.178**	110.081
	(0.05)	(0.01)	(22.93)	(143.68)
4	−0.067	0.103***	64.218**	260.095***
	(0.04)	(0.01)	(19.72)	(63.98)
5	−0.013	0.081***	48.037***	119.716
	(0.03)	(0.01)	(11.76)	(68.73)
6	−0.025	0.063***	31.955*	150.339*
	(0.04)	(0.01)	(12.76)	(59.94)
7	−0.048	0.036***	22.330	115.592*
	(0.03)	(0.00)	(13.59)	(55.20)
8	−0.041	0.022***	19.217	−29.219
	(0.03)	(0.00)	(9.50)	(94.70)
Canton FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LinTrends	Yes	Yes	Yes	Yes
N (canton × years)	636	636	678	678
R2	0.974	0.982	0.717	0.33

Notes: This table shows coefficients for δ_τ from Equation (1) for $\tau \in [-5, 8]$ on fertility and birth weight by citizenship of the mother. The event year represents the year relative to the introduction of the baby bonus. The omitted category is event time $\tau = -1$. Estimates in the canton-year cell are weighted with the number of fertile women for the fertility rate and with the number of births for birth weight. Robust standard errors (shown in parentheses) are clustered at the cantonal level and significance levels are indicated by $^{*}0.05$ $^{**}0.01$ $^{***}0.001$.

Abbreviations: FE, fixed effect; LMIC, low- and middle-income country.

TABLE A.6 Maternal age at birth by citizenship of mother

	Maternal age at birth	
	(1) High-income	**(2) LMIC**
−5	0.023	0.277
	(0.05)	(0.36)
−4	0.081	1.130
	(0.05)	(0.56)
−3	0.029	−0.377
	(0.04)	(0.79)
−2	0.003	−1.035
	(0.06)	(1.14)
0	−0.167	−0.107
	(0.09)	(0.78)
1	−0.139	0.172
	(0.09)	(0.71)
2	−0.079	0.272
	(0.09)	(0.85)
3	−0.085	0.320
	(0.09)	(0.89)
4	−0.101	−0.207
	(0.09)	(0.63)
5	−0.084	−0.128
	(0.09)	(0.46)
6	−0.094	0.053
	(0.08)	(0.33)
7	−0.087	0.601
	(0.08)	(0.92)
8	0.003	−0.290
	(0.04)	(0.36)
Canton FE	Yes	Yes
Year FE	Yes	Yes
LinTrends	Yes	Yes
N (canton × years)	862	862
R2	0.995	0.816

Notes: This table shows coefficients for δ_τ from Equation (1) for $\tau \in [-5, 8]$ on maternal age by citizenship of mother. The event year represents the year relative to the introduction of the baby bonus. The omitted category is event time $\tau = -1$. Estimates in the canton-year cell are weighted with the number of births. Robust standard errors (shown in parentheses) are clustered at the cantonal level and significance levels are indicated by $^{*}0.05$ $^{**}0.01$ $^{***}0.001$.

Abbreviations: FE, fixed effect; LMIC, low- and middle-income country.

TABLE A.7 Fertility by child rank

	Fertility rate			
	(1) Rank 1	(2) Rank 2	(3) Rank 3	(4) All
−5	−0.011	−0.025*	−0.007	−0.026
	(0.05)	(0.01)	(0.01)	(0.04)
−4	−0.022	−0.008	0.003	−0.018
	(0.01)	(0.01)	(0.01)	(0.02)
−3	0.014	−0.013	0.003	0.026
	(0.03)	(0.01)	(0.01)	(0.05)
−2	−0.011	0.005	−0.011	0.020
	(0.01)	(0.00)	(0.01)	(0.02)
0	0.055	0.038*	−0.011	0.089
	(0.03)	(0.02)	(0.01)	(0.05)
1	0.068	0.021	−0.000	0.110*
	(0.03)	(0.01)	(0.01)	(0.04)
2	0.072*	0.027	−0.017	0.089
	(0.03)	(0.01)	(0.01)	(0.05)
3	0.087*	0.024	−0.012	0.109*
	(0.04)	(0.02)	(0.01)	(0.05)
4	0.049	0.008	−0.024*	0.036
	(0.03)	(0.01)	(0.01)	(0.04)
5	0.049	0.030*	−0.008	0.068
	(0.03)	(0.01)	(0.01)	(0.04)
6	0.048	0.002	−0.011	0.038
	(0.03)	(0.02)	(0.01)	(0.04)
7	0.005	0.008	−0.021*	−0.013
	(0.03)	(0.02)	(0.01)	(0.03)
8	−0.018	0.003	−0.005	−0.019
	(0.03)	(0.01)	(0.00)	(0.03)
Canton FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
LinTrends	Yes	Yes	Yes	Yes
N (canton × years)	636	636	636	636
R2	0.913	0.939	0.952	0.952

Notes: This table shows coefficients for δ_τ from Equation (1) for $\tau \in [-5, 8]$ on fertility by child rank. The event year represents the year relative to the introduction of the baby bonus. The omitted category is event time $\tau = -1$. Estimates in the canton-year cell are weighted with the number of fertile women. Robust standard errors (shown in parentheses) are clustered at the cantonal level and significance levels are indicated by *0.05 **0.01 ***0.001.

Abbreviations: FE, fixed effect.