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Abstract

We study how parental leave benefit levels affect household labor supply, family income, and child outcomes, exploiting the Speed Premium (SP) in the Swedish leave system. The SP grants mothers higher benefits for a subsequent child without re-establishing eligibility through market work, if two births occur within a pre-specified interval. We use the spacing eligibility cutoffs in a Regression Discontinuity framework and find that the SP improves educational outcomes of the older child, but not of the younger. Impacts are likely driven by increased maternal time and the quality of maternal time relative to the counterfactual mode of care.

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1 Introduction

Family leave programs have become a salient feature of most industrialized countries' efforts to help parents reconcile the combination of market work and family responsibilities, and to promote infant and maternal health. However, the duration of paid leave entitlements and the level of compensation vary widely across countries. Central questions for policy are whether job protected and paid leave entitlements have desired consequences for the gender gradient in labor earnings and hours worked, as well as for children's development.

In this paper we study how parental leave benefit levels affect the labor supply decisions of mothers and their spouses. In addition, we assess whether policy-induced changes in the early home environment – in terms of parental time spent with children and the monetary resources of the family – affect the outcomes of children from birth into early adulthood. We exploit a feature of the Swedish parental leave (PL) system commonly referred to as the Speed Premium (SP). To qualify for wage-replaced PL benefits, parents must meet a work requirement of 240 consecutive days of employment before birth, and benefits are based on the salary received before birth.[1] Thus, part-time work and periods of non-work between births – which is common among new mothers – decrease the qualifying income and thereby the benefits received for a subsequent child. To protect the financial situation of families with young children born in close intervals it became legal practice during the 1970s to allow mothers to keep the PL benefit level for a subsequent child if the two children were born within a pre-specified interval. This automatic renewal of PL benefits without re-qualifying for them through market work is called the Speed Premium (SP) due to its incentives to space births in close intervals. Initially, the eligibility birth spacing interval that granted access to the SP was very short and biologically difficult to achieve. In 1980, the eligibility birth interval was increased to 24 months, and in 1986 it was further extended to 30 months.

The thresholds to SP eligibility generate experimental variation in maternity leave benefits, and we therefore use the 24- and 30-month birth spacing cutoffs as the treatment assignment in a Regression Discontinuity (RD) empirical framework. Focusing on couples with at least two children, we examine the effects of eligibility to higher benefits on maternal labor supply, spousal labor supply, and household disposable income. We also examine the consequences of maternal SP eligibility for children's educational and health outcomes. We separately analyze the effects on the *existing* child in the household at SP receipt, and the *new* child, the parental leave for whom is the target of the SP. Moreover, we perform a variety of heterogeneity analyses to probe potential mechanisms for any impacts on the couples' labor

[1] In Sweden, PL benefits are governmentally paid to both mothers and fathers, and financed through payroll taxes.

supply decisions and on children's outcomes. We use longitudinal administrative data with information on both parents and children, such as individuals' labor income, household disposable income, gender, hospital admissions in early life, birth outcomes (birth weight, Apgar scores, among other measures), cumulative GPA in 9^{th} grade, and college attendance by age 24.

Our paper makes three contributions to the literatures on PL policies, household and maternal labor supply, and child development. First, we study the effect of parental leave *benefit levels* on maternal labor supply, while being able to keep constant the total duration of leave entitlements and mandated job protection. The latter features of PL systems are the focus of most previous papers on family policies, looking either at the effect on *take-up* of leave, or on *subsequent* labor market outcomes of mothers.[2] We find that eligibility to higher PL benefits, all else equal, decreases mothers' labor supply as measured by their earnings from market work. While we lack data on leave-taking, we find no effect of the SP on mothers' total disposable income (net of taxes and transfers), suggesting that reduced labor earnings are compensated by parental benefits, and that women are not switching to unpaid leave. There is a reduction in both extensive and intensive margin labor supply of mothers, but only in the short-run (effects dissipate by the time the youngest child is two years old). These findings are in line with existing studies on family polices, which generally find that paid leave significantly increases the take-up, but that effects on women's long-run labor market outcomes are smaller or nonexistent (see Rossin-Slater, 2017, for a review of the literature). We complement these studies by showing that the level of monetary compensation matters for the take-up of leave. In particular, we find that mothers respond not only to current benefits, but also to *future* benefit levels. The SP eligibility status is based on expected due dates of the new child, which is determined by ultrasound scan during pregnancy or by the date of last menstruation. Thus, upon learning about SP eligibility, expecting mothers reduce their labor supply during what would otherwise have been a benefits-qualifying period of market work. This suggests that benefits based on pre-birth earnings have important implications for women's, and in particular mothers', labor market participation.

Our second contribution is to provide evidence on the cross-spousal effects of PL benefit levels, by estimating the effect of maternal SP receipt on the labor earnings of their partners. Several papers show that leave entitlement targeted to fathers increases the take-up of leave among males (see e.g. Dahl et al., 2014; Ekberg et al., 2013; Avdic and Karimi, 2018),[3] but few papers look at the intra-household labor

[2]See e.g., Ruhm (1998); Liu and Skans (2010); Dustmann and Schönberg (2012); Schönberg and Ludsteck (2014); Dahl et al. (2016); Baker and Milligan (2010); Lalive and Zweimüller (2009); Lalive et al. (2013); Bergemann and Riphahn (2015); Ruhm (1998); Olivetti and Petrongolo (2017).

[3]See also Rege and Solli (2010); Cools et al. (2015); Patnaik (ming).

supply decisions in this context.[4] If male and female time are substitutes in home production, a relative increase in the benefits received by the wife could have implications for her partner's time spent in market work vis-à-vis home production. We find that higher maternity benefits *increases* the labor supply of the partners of women in the top third of the earnings distribution. Thus, for these couples, the extent of within-household specialization increases as a result of the SP.

In our third contribution, we study the consequences of higher maternity leave benefits on children's health and educational outcomes. Our data allow us to follow children from birth up to early adulthood. We find that maternal access to the SP increases children's cumulative GPA in 9^{th} grade and increases their likelihood to have attended college by age 24. These impacts, however, only pertain to the *existing* child in the household at SP receipt; we find no effects for the *new* child, who is actually the target of the SP. This heterogeneity by birth parity exists for both the first- and second-born child in a two-child household, and for the second- and third-born child in a three-child household (where the mother is eligible for the SP with the third child). Thus, the SP benefits mainly the older child in the household. Our results on children's outcomes run counter to many previous studies examining extensions of paid leave, which find little evidence of improved child well-being (see e.g. Rossin-Slater, 2017; Baker and Milligan, 2008, 2010, 2015; Liu and Skans, 2010; Rasmussen, 2010; Dustmann and Schönberg, 2012; Dahl et al., 2016; Danzer and Lavy, 2017).[5] However, one central aspect of the setting in our study distinguishes it from that of other papers. We consider a change in benefit levels in families with *closely spaced births*. Buckles and Munnich (2012) show that a one year increase in birth spacing increases reading scores for the older sibling, but no effect of spacing on test scores for the younger sibling, which potentially operates via differential time investments of parents by birth order. For example, Price (2008) finds that parents spend more time with first-born than second-born children, and that this translates into lower reading test scores for the younger child. Our findings are in line with these studies, as our evidence suggests increased maternal time for the older sibling *before* the younger sibling is born.

Our results are robust to several sensitivity tests. First, we find no evidence that parents are able to perfectly time the spacing between two births, nor that there are any systematic differences in pre-determined characteristics at the eligibility threshold. The results are also robust to the functional form

[4]The exception are two recent papers. Moberg (2017) studies the effect of mothers' PL benefit levels on own and spousal parental leave take-up by exploiting the 30-month SP eligibility cutoff in Sweden; and Canaan (2016) who studies an extension of paid leave in France.

[5]Notice that, the *introduction* of short leave programs has been shown to improve the health and schooling achievement of children (Rossin, 2011; Carneiro et al., 2015; Stearns, 2015). There appears to be some heterogeneity in the impacts of such policies by families' socioeconomic (SES) background, with larger benefits for high SES children (see e.g Danzer and Lavy, 2017; Liu and Skans, 2010).

used to capture the underlying trends in the outcome variables. Moreover, we find no significant differences at the threshold in either of our main outcomes at the spacing cutoffs in years preceding the implementation of the SP (that is, at placebo cutoffs where no relation between eligibility to SP and outcomes should be detected). Finally, the impacts are robust to adjusted inference for multiple hypothesis testing.

We cannot provide a definitive answer about mechanisms; nevertheless, we provide suggestive evidence that children's schooling outcomes are likely improved via a combination of increased maternal time and family financial resources, and by switching from informal to maternal care before the age of two. We rule out that the effects operate via changes in family size or maternal employment in the long-term. No effects are found on birth outcomes of the new child, nor on the incidence of hospital visits for either the existing or new child.

In terms of policy implications, our results suggest that the automatic renewal of PL benefits *increases* inequality in three dimensions. First, the SP yields no improvements in women's long-term job continuity or earnings, nor in household income. However, in the short-run, it widens the gender earnings gap within the household, and potentially increases the extent of specialization in households of high socioeconomic status. Second, it is a regressive scheme in the sense that only children to mothers in the top third of the earnings distribution are positively affected in terms of their schooling outcomes. Third, the SP widens the birth order gap between siblings, because it only improves the outcomes of the older child.[6] The automatic renewal of PL benefits in Sweden thus contributes to part of the "birth order penalty". In particular, our findings suggest that meeting the SP requirements contributes to 31 percent of the gap between the compulsory schooling GPA of first- and second-born children born two years apart.

The rest of the paper is organized as follows: Section 2 describes the Swedish parental leave system and the Speed Premium rule. Therein, we also lay out the expected effects of the SP on parents' labor supply. Section 3 describes the data and the empirical framework. In Section 4 we present the main results from our analyses of the effects of the SP on parents and children. In Section 5 we probe possible mechanisms through which the SP affects children's outcomes. Finally, in Section 6 we discuss the conclusions that we draw from our analyses.

[6]The literature on birth order effects demonstrates that educational attainment and cognitive ability decreases with birth order (Black et al., 2005, 2011; Hotz and Pantano, 2015; Black et al., 2018). See also Björkegren and Svaleryd (2017) for evidence on birth order and health, and Breining et al. (2017) on birth order and delinquency.

2 Institutional Setting

The Swedish parental leave system was introduced in 1974 and initially offered six months of paid parental leave. The paid leave was sequentially extended over the subsequent decades, and to date parents are entitled to 16 months of paid leave per child (see Table A.1 for changes to different components of the parental leave system up to 2010). The mother and the father of a child are given half of the entitled days each (if they have joint custody), but have the option of transferring paid leave days between one another.[7] Individuals with sole custody of their child get the full number of paid leave days.

Parental leave (PL) benefits are divided into three components. The most important part consists of benefits that (for the time period studied) replaces 90 percent of parents' salary, up to an inflation-adjusted cap.[8] These wage-replaced benefits are conditioned on at least 240 days of employment before the birth of the child. Parents that do not fulfill the work requirement receive a relatively low, fixed, daily amount of PL benefits. Second, since 1978, part of the leave is compensated with a low, fixed daily amount. Third, since 1980, ten days of leave are given exclusively to the father, which he can use during the first 60 days after the birth of the child.

During the first 18 months after birth both parents are legally entitled to full-time job-protected leave, irrespective of whether they claim PL benefits. Moreover, paid leave benefits can be used on a part-time basis until the child is eight years old; parents can thus save paid leave to e.g., extend vacations.[9] In addition, parents have the right to a working-time reduction by up to 25 percent until the child turns eight years old, even if they have used up all their entitled PL benefits.

2.1 The Speed Premium Rule

To study the effect of parental leave benefits on labor supply and children's outcomes, we exploit a feature of the Swedish PL system that is commonly referred to as the Speed Premium (SP) rule. Wage-replaced PL benefits are calculated on the pre-birth earnings, i.e., during the qualifying period of em-

[7] In 1995, one month of paid leave became earmarked to each parent, so that fathers could not transfer all of their paid leave to the mother of the child (as was usually practice). This "daddy-month" was introduced to increase the incentives for take-up of parental leave among fathers. In 2002 and 2016, a second and third month, respectively, of paid leave were earmarked to each parent.

[8] Currently, the replacement rate is 80 percent of previous earnings (see Table A.1). Moreover, by some collective agreements employers top up the benefits to full replacement. The cap is set to 10 base amounts ("basbelopp"). During the time period studied, only about 1 percent of mothers had a pre-birth income that reached the cap. Thus, the effective replacement rate is similar for the vast majority of female workers in our sample.

[9] Workers must notify their employers at least two months in advance of any parental leave or work-time reduction, but the employer cannot deny the worker leave given that this requirement is met.

ployment before birth. Thus, part-time work or time out of the labor market between births decrease the benefits received for a subsequent child. To protect the financial situation of families with young children, it became legal practice during the 1970s to base the PL benefits for a subsequent child on the income earned before the preceding child, provided that the time interval between the births did not exceed the period of eligible leave plus six months. This administrative rule thus implied that mothers did not have to return to work between births and re-qualify for (higher) PL benefits, if two births were sufficiently closely spaced, and therefore became known as the speed premium.

In 1974, the eligibility birth interval was 12 months, but could in practice be extended by three months. Because entitlement to paid leave was extended to seven months in 1975, the eligibility birth interval for higher benefits (that is, to the SP) increased to 13–16 months. On January 1, 1980, the SP eligibility birth interval was extended to 24 months. A few years later, on January 1, 1986, the birth interval granting access to the SP was further extended to 30 months, and it also became statutory (Proposition, 1984; SfU, 1984).[10] In this paper, we focus on the SP rules after 1979, i.e., the 24- and the 30-month spacing rules, and use the spacing eligibility cutoffs as the treatment assignment in a Regression Discontinuity (RD) framework. We present the empirical strategy in detail in Section 3.2.

We note that, also in the absence of additional children, wage-replaced benefit levels are "protected" for some time after birth, so that the qualifying income does not decrease while on leave with a child. The SP merely extends this "protected period" with subsequent pregnancies. In particular, before 1986 the benefit level for a current child remained unchanged until the child turned 18 months; and from 1986, until the child turned 24 months. This is the protection period that ensures the same benefit level for a current child during the first 18 (24) months. The SP stipulates that, if by the time the protected benefit level expires the woman is pregnant again with an *expected* due date within six months from the protected period ($18 + 6 = 24$ months between 1980 and 1985; and $24 + 6 = 30$ months from 1986 onwards), the mother is entitled to receive the same PL benefits also for the new child (Proposition, 1984; SfU, 1984). Hence, eligibility to the higher benefits for the subsequent child via the SP is based on the *expected due date*, and not the *actual* date of birth. The fact that access to the SP is based on expected and not actual due dates has implications for the expected timing of any parental responses to the SP. We return to this discussion below in Section 2.3.

The SP rule creates economic incentives to space births in short intervals. Among mothers with at least one child, giving birth to a subsequent child outside of the eligibility spacing interval implies a

[10] Table A.2 shows the changes to the eligibility spacing interval over time.

lower benefit level for leave taken with the new child, compared to the scenario where the new child is born within 24 (30) months from the previous birth, after the introduction of the 1980 (1986) extension. Exploring the potential fertility timing effects of introducing the SP is beyond the scope of our paper, but for illustration, Figure A.1 shows the average age difference in months between a mother's first two children for different cohorts of second-born kids. Before 1980 there was an increasing trend in birth spacing, which almost instantaneously declined after the introduction of the 24-month rule in 1980, and continued to decline after the implementation of the 30-month rule in 1986.[11] We note that the RD framework that we use is based on the inability of parents to precisely manipulate the spacing of births around the eligibility cutoffs, and thus the identification of impacts of the SP will not be affected by changes in the trends in the spacing of births associated with the changes in the rules.

The SP in the Swedish system is similar to the "automatic renewal" in the Austrian system, described and studied in detail by Lalive and Zweimüller (2009). In their paper, they find that an extension of automatic renewal from one to two years expansion delays mothers' return to work, but that these negative labor supply effects are temporary. In addition, they find a differential impact by pre-birth wage levels on the labor market attachment of women, with high-wage women being more likely to return to work three years after the birth than low-wage mothers.[12]

2.2 Labor Market Conditions

We will focus on families with at least two children, and in which the second child was born between 1980 and 1989. During this period, Sweden had very low unemployment rates, and high employment rates (see Figure A.2). The labor market conditions were relatively similar across the two SP regimes (24- and 30-month), despite a small increase in real earnings over the decade. In our main analyses, we will pool samples of parents with children born under the respective SP regimes. However, in auxiliary analyses we will assess whether the SP has different impacts depending on the spacing eligibility threshold. Importantly, any potential differences in the impacts across the thresholds are unlikely to be driven by differences in labor market conditions. Finally, there were no other relevant changes in policies related

[11] Hoem (1990, 1993) study the fertility timing impacts of the SP and he shows that with an eligibility interval of two years or more, couples find it more manageable to fulfill the SP criteria and take advantage of the new benefit. Pettersson-Lidbom and Thoursie (2009) exploit the introduction of the 1980 SP rule to examine the effect of (shorter) birth spacing on children's outcomes and find adverse consequences for children.

[12] In the Austrian PL system, mothers must meet a work requirement to be eligible for paid leave. Mothers giving birth to a subsequent child within 3.5 months after the end of a previous PL spell are exempted form the work requirement and get an automatic renewal of PL benefits for the subsequent child. In 1990, paid parental leave was extended from one to two years, increasing the eligibility spacing interval for automatic renewal from 15.5 months to 27.5 months (Lalive and Zweimüller, 2009). This extension also increases the short-run higher-order fertility by 5 percentage points.

to family leave during the time period studied.

2.3 Expected Effects of the Speed Premium on Parental Labor Supply

The SP rule affects parents' PL benefit level, and thereby alters the relative gains from market work and time at home. Thus, the policy has implications for both the financial situation of the household as well as parents' time spent with their young children. Drawing on previous empirical and theoretical work on household labor supply, we discuss below the different margins of spousal labor supply that may respond to changes in the benefit level. In Section 3.2, we describe the empirical strategy that we employ to estimate the relationships discussed below. The discussion herein focuses on the case of families with one child and expecting a second, but can be generalized to the case of higher parity births.

First, because eligibility to the SP is determined based on the *expected* due date of the subsequent child, and not the *actual* date of birth, women will learn about their eligibility status during pregnancy. The near full replacement (of 90 percent pre-first-birth salary) without the need to re-qualify for the insurance through market work reduces the opportunity cost of time at home before the birth of the second child, that is, during what would otherwise have been the qualifying period. We therefore expect that eligible mothers will respond to the *future* benefit level by a downward shift in their labor supply before giving birth. In addition, higher PL benefits with the second child also lowers the opportunity cost of time at home *after* birth. We test these conjectures by estimating the difference in labor supply between mothers just eligible and just ineligible for the SP, in the immediate years surrounding second birth (both before and after). Moreover, human capital may erode during interruptions (Mincer and Polachek, 1974; Mincer and Ofek, 1982), with depreciation increasing in education and experience. If the cost of work disruptions differs along these dimensions, we expect heterogeneous impacts by mothers' earnings potential, with highly paid mothers being less likely to stop working or decrease their work hours by less when eligible to the SP relative to lower-paid mothers.

Secondly, while both mothers and fathers in Sweden have the same rights to job-protected and paid parental leave, the vast majority of the benefits is paid out to mothers, who can therefore arguably be considered the main caretakers of children.[13] Thus, the SP mainly (if not only) affects the mother's benefit level, since fathers effectively re-establish eligibility for paid leave with subsequent children by default. Nevertheless, the SP may have indirect effects – on *spousal* labor supply – through at least two channels. For example, one important factor concerns the relationship between male and female time

[13] In 1992, the male share of total parental leave take-up was approximately 10 percent (see e.g. Karimi et al., 2012).

within household production functions. If male and female time are substitutes in home production (Becker, 1981), then we expect that a lower opportunity cost of home time on the part of mothers may increase the extent of specialization within the household.[14] If a specialization channel exists, we should expect an increase in the labor supply of fathers in families where the mother gains eligibility to the SP. Moberg (2017) studies the 30-month speed premium rule in an RD setup similar to ours to estimate the effect of PL benefit levels on the spousal division of PL take-up during the 1990s. She finds that the speed premium increases the benefit level for mothers, and thereby increases her duration on leave. Fathers' benefit levels are unaffected, but they nevertheless decrease their parental leave duration as a response.

Secondly, even if the SP entails higher benefits compared to a scenario of being non-eligible, a maternal response in terms of decreased labor supply has an ambiguous net impact on mothers' and households' income. If households are liquidity constrained and the net impact of the SP on mothers' contribution to the household budget is negative, their spouses may increase their labor supply to insure against such income drops.[15] Because of the nearly full replacement rate on the benefits, we do not expect this insurance channel to be effective. Nevertheless, to study the scope for insurance we also study the impact of the SP on household disposable income, which includes not only total labor income of the couple but also the benefits and other transfers. If the husband's labor supply works as an additional source of compensation for the loss in maternal labor income (due to longer labor market absence), we should expect an increase in time spent in market work for husbands in low SES families.[16]

3 Data and Econometric Strategy

3.1 Data

We combine data from multiple Swedish longitudinal registers, focusing on families with at least two children. We use the multi-generational register that is a population-wide data set with links between parents and children, and information on date of birth, parity, and child gender. For each parent, we merge information on labor earnings, disposable income,[17] and year of birth from the LOUISE register, which includes the universe of individuals in Sweden aged 16–64 and covers the period 1970–2013.

[14] In the early 1990s, the father had higher earnings than the mothers in 85 percent of all households with at least two children (own calculations from the registry data used in the analyses).

[15] This would be similar to a so called "added worker effect" (see e.g. Cullen and Gruber, 2000).

[16] Olsson (2017) finds spousal responses in parental leave take-up of weaker employment protection rules (higher risk of job dismissal) in financially constrained couples.

[17] Both labor earnings and disposable earnings are collected by the Swedish Tax Authority. Throughout the paper all monetary values are measured in SEK as of 2013.

While individual variation in labor income can be generated both by changes in hourly wages and hours worked, short-run changes in labor income are more likely to be driven by changes in hours worked rather than hourly wage rates, which are contractually set. Therefore, and due to the lack of data on months or hours worked and hourly wages, we use labor income to measure parental labor supply. To get a more complete picture of potential changes to the overall financial situation of the household, we use the measure of disposable income at the household level. Disposable income includes income from market work and governmental transfers, such as PL benefits, net of taxes.

For each child, we extract information on the exact date of birth from the medical birth register. This register also includes birth weight, birth height, gestation (in weeks), Apgar scores, and a number of different medical diagnosis codes at birth. The medical birth register also includes variables related to the mother's pregnancy and delivery, such as weight and height at birth, and the predicted date of birth based on the last menstruation and ultrasound scans. Eligibility to the speed premium is based on the expected (predicted) due date, and not on actual date of birth. However, the medical birth register does not have full coverage for the predicted date of birth; only 50 percent of births have information on the expected due date based on last menstruation, and 20 percent based on ultrasound scan. Therefore, we calculate the spacing between the first two births based on *actual* date of birth, which is highly correlated with spacing calculated based on expected due dates from both ultrasound and last menstruation. In Section 3.2, we discuss this data issue in greater detail, and how we handle potential discrepancies between actual and expected due date in our identification strategy.

For children we also study early and later life health outcomes using the inpatient register, which covers the universe of all hospital visits during the years 1987–2005. In addition to diagnosis codes (ICD10-codes) the register reports the date of admission, indicators for planned/emergency care, and the duration of each hospital visits.

Lastly, for each child we match information on schooling outcomes from the grade-9 register, which is available from 1988 onward. We standardize (within graduation year) cumulative GPAs in 9^{th} grade to have mean 0 and unit standard deviation, so that the values are comparable across years.[18] We also extract an indicator for whether the student's grades qualifies them for high school.

[18]In Sweden, for the years of 1988 to 1997, the raw units of the GPA scores vary between 0 and 5 points, in intervals of 0.1. Since 1998 the GPA varies between 0 and 300 points, typically in intervals of 5 points.

3.2 Empirical Strategy

We use a Regression Discontinuity (RD) framework to study the impact of the SP benefit rule on parental labor supply, household income, and children's health and schooling outcomes. To this end, we use the fact that parents whose first two children were born within an interval of 24 or 30 months were subject to the automatic renewal of parental leave benefits, that is, access to full replacement without having to re-establish eligibility to paid leave through market work. Using data on families whose second child was born under the respective SP regimes we estimate the following regression model by OLS, pooling data for the two eligibility spacing thresholds:

$$y_{i,\tau} = \alpha + \mathbf{1}[t \leq c]\beta + \mathbf{1}[t > c]f_r(t-c) + \mathbf{1}[t \leq c]f_l(c-t) + \epsilon_i \tag{1}$$

where $y_{i,\tau}$ is the outcome of interest for parent or child i, measured at some follow up year τ after second birth. $t = \kappa - \kappa'$ is the spacing between the first two births, where κ' denotes the date of birth of the first child, and κ the birth date of the second child. c is the respective eligibility cutoff spacing threshold, i.e., 24 months in the first regime, and 30 months in the second regime. $\mathbf{1}[\cdot]$ is the indicator function that takes the value 1 if the spacing requirement is fulfilled and 0 otherwise, and f_l and f_r are unknown functions capturing underlying trends in the outcome variables over child spacing. If parents do not have precise control of the timing of birth, $\hat{\beta}$ provides an estimate of the effect of eligibility to the speed premium at the threshold. The assumption of imprecise control of birth timing implies that births are locally randomized around the cutoff. We perform a number of robustness checks to assess the validity of this assumption.

Equation (1) can be seen as the reduced form equation in a fuzzy regression discontinuity setup where the PL benefit level is the dependent variable in the first-stage equation. The setup is fuzzy and not sharp because not all individuals giving birth to two children within 24 (30) months get a higher PL benefit level; it depends on whether they re-entered the labor market before the subsequent birth. Because data on PL take-up and benefit levels exist only from 1988 onward, and is not available to us for this study, we are not able to re-scale the Intent-to-treat (ITT) parameter β by the first-stage estimate of the impact of SP eligibility on PL benefit levels. Therefore, we measure the incidence and duration of parental leave with changes in labor earnings (which do not include governmental transfers). When looking at the outcomes of children, we would wish to re-scale the ITT parameter with parental inputs. However, the exclusion restriction would fail in this setup, as the SP likely influences the outcomes of children through several channels; namely, parental inputs in terms of both time and material resources. Thus, we focus on the ITT effect of the SP in our analyses of both parental and child outcomes.

We use weekly birth data; the cutoff c is 104 and 130 weeks for the 24-month and 30-month regimes. Although eligibility to the SP is based on expected due date, we use actual birth spacing as the running variable due to incomplete coverage of expected due dates in the medical birth register. However, expected and actual birth spacing are highly correlated, and on average the difference between the expected due date (based on date of last menstruation) and actual date of birth is of 0.04 weeks (a quarter of a day). To account for measurement error in the running variable we cluster the standard errors by week of birth (Lee and Card, 2008). We use a linear specification with triangular weights, and test the sensitivity of our results to functional form.

Finally, as we study effects on a relatively large number of outcomes, we test which of the estimated impacts survive adjustment of inference for multiple hypotheses testing. We use the procedure in algorithms 4.1 and 4.2 of Romano and Wolf (2005), which accounts for testing several hypotheses simultaneously. Romano and Wolf (2005) propose an iterative rejection/acceptance procedure for a fixed level of significance. In the tables presented, we therefore mark in bold the coefficients that are still significant at a level of 10 percent. We use 1000 bootstrap replications to obtain the adjusted critical values.

Analysis Sample We restrict attention to mothers who were eligible for wage-replaced parental leave for their first child, i.e., who had a qualifying labor income before the birth of their first child. During our period of analysis, the PL benefits replaced 90 percent of foregone earnings, with eligibility based on the earnings in the 8 consecutive months before birth, or on 12 out of 24 months preceding the birth-related labor market withdrawal. Due to the annual nature of the data on labor earnings, we do not directly observe if individuals are eligible for wage-replaced leave. We must, therefore, rely on total annual labor earnings to approximate eligibility status (similar to the method used by Carneiro et al. (2015)). We use a value of 32,000 SEK as a threshold defining eligibility, which corresponds to the 10^{th} percentile of the annual income distribution for mothers in the year before giving birth to the first child. This likely results in an overestimate of the fraction of eligible mothers; it yields a slightly larger share of eligible mothers than in Carneiro et al. (2015) for a similar period in Norway. Reassuringly, we show below that our results are not driven by families in the left tail of the income distribution. In addition, because non-eligible mothers' incentives are unaffected by the speed premium, a larger fraction of non-eligible mothers in the analysis sample leads to attenuation bias in the estimation of the effect of the speed premium on spousal labor supply.[19]

In our main analyses, we focus on families in which the second child was born 1980–1989, and in which the mother was aged 20–39 when they gave birth to the first child.[20] For parents whose second child was born in 1980–1985 the 24-month rule applies, while the 30-month threshold applies to parents whose second child was born 1986–1989. Although we start with the first pair of children in the household, we also expand the sample to higher-parity children.[21]

As is practice in RD empirical frameworks (see Lee and Lemieux, 2010), we estimate equation (1) using data on families with a birth spacing in a neighborhood of the SP eligibility cutoff. In our main analyses, we use families with birth spacing in a range of 24 weeks on either side of the cutoff.

Table 1 shows summary statistics for the pooled sample of families whose second child was born under the 24- or 30-month regimes, and in which the first two children were born within 24 weeks above or below the SP eligibility cutoff.

On average, fathers earn a higher labor income than mothers, and there is an average spousal age difference of about 2.7 years. In the first row of Table 1 we also report mothers' earnings in the year before 1^{st} birth, which is the income on which PL benefits for the first child is calculated, as well as for the second child, if she is eligible for the SP. Comparing this income level with mothers' average income in the year before 2^{nd} birth, we note that the average gain of eligibility to SP is around SEK 63,600 (USD 7,200) ((157.479 - 93.919)$\times$0.9). This corresponds to 41 percent of baseline annual labor earnings for the mothers in our sample. Consistent with studies on birth order effects, first born children on average perform better than second-born children (see e.g. Black et al., 2005, 2011; Hotz and Pantano, 2015, for recent evidence). For example, first-born children have higher grade-9 cumulative GPA:s, they are more likely to be high school eligible by age 16 compared to second-born children, and they are also more likely to have attained some college education by age 24 compared to their younger siblings.

[19]We have tried to set the threshold at higher values to define eligibility, to which our main findings are robust.

[20]Among the cohorts in the analysis, only 0.01 percent of mothers gave birth to the first child at ages 40 or beyond, and 8 percent of gave birth to the first child before age 20.

[21]We also extract data on families with second births in 1977–1979 to be able to perform sensitivity analyses using the 24-month cutoff in years preceding the SP introduction. These analyses are discussed in section 4.

TABLE 1.
Summary statistics

	Mean	SD
Parental characteristics		
Mother's pre-birth income (1000s SEK)	157.479	340.315
Spousal pre-birth income-gap	30.560	308.458
Spousal age difference	-2.730	3.672
Mothers Age at 1^{st} birth	25.798	3.738
Fathers age at 1^{st} birth	28.536	4.523
Mother's income pre 2^{nd} birth	93.919	65.310
Father's income pre 2^{nd} birth	198.459	165.171
Mother's income post 2^{nd} birth	65.657	60.971
Father's income post 2^{nd} birth	211.494	133.397
Characteristics of first-born child		
Boy	0.515	0.500
Gestation (weeks)	39.622	1.793
Birth weight (grams)	3468.603	515.786
Pre-term	0.047	0.212
Low birth weight	0.032	0.176
Apgar 1 min	8.674	1.161
No Hosp visits age 10	0.045	0.329
Grade-9 GPA (standardized)	0.255	0.912
High school eligible	0.947	0.225
At most compulsory schooling at age 24	0.068	0.252
At most High school at age 24	0.464	0.499
Some college/college at age 24	0.468	0.499
Characteristics of second-born child		
Boy	0.515	0.500
Gestation (weeks)	39.516	1.604
Birth weight (grams)	3596.923	520.058
Pre-term	0.033	0.179
Low birth weight	0.020	0.141
Apgar 1 min	8.876	0.913
No Hosp visits age 10	0.046	0.405
Grade-9 GPA	0.156	0.908
High school eligible	0.936	0.244
At most compulsory schooling at age 24	0.067	0.249
At most High school age 24	0.508	0.500
Some college/college at age 24	0.425	0.494

NOTE.— Means and (standard deviations) of characteristics of parents, first-, and second-born children. The sample consists of all families whose second child was born 1980–1989 and whose first two children were born within 18–30 months from each other (24-month regime), and 24–36 months from each other (30-month regime); i.e., speed premium eligibility cutoff $+/-$ six months.

3.3 Covariate Balance and Manipulation of Birth Timing

Covariate Balance We start by presenting estimates of specification (1) for variables that are pre-determined at the time of the birth of the second child, and thus for which we should *not* expect to find a statistically significant discontinuity at the cutoffs. The covariate balance test yields information regarding the local randomization assumption needed for identification of β (Lee and Lemieux, 2010): if pre-determined covariates are unbalanced at the cutoff, this could suggest systematic manipulation of the birth spacing interval by parents. In Table 2 we present results from the covariate balance tests for a linear specification of the underlying trends captured by f_l and f_r. The table reports the point estimate for β, the standard error, and the t-statistic for a test of whether β is statistically significantly different from zero. In the bottom row of the table we also include the results from a test of joint significance from estimating a seemingly unrelated regression model including all pre-determined covariates (as in Deshpande, 2016). Even though a few individual covariates are statistically different from zero, the joint test suggests that there is little room for systematic timing of births. All covariate balance tests are also presented graphically in Figure A.3. Because we are simultaneously testing multiple hypotheses, we also adjust inference following Romano and Wolf (2005), which corresponds to testing 12 hypotheses simultaneously. We cannot reject the null that the coefficients reported in the table are all statistically zero at a significance level of 10 percent once we perform this adjustment.

Given the results from the balancing tests we proceed with the linear specification in our main analysis, but we also provide estimates using local linear regression with varying bandwidths as robustness checks. Moreover, we also follow Lee and Lemieux (2010) who propose goodness-of-fit tests as an ancillary in the selection of the polynomial function. These specification diagnostics and other robustness tests are presented in Section 4.

Strategic Manipulation of Birth Timing In our context, the identifying assumption requires parents to have imprecise control of the exact date of birth, even if they will have some influence on the timing of birth. Thus, while some parents will aim to meet the eligibility criteria by planning their fertility timing, which would show up as a larger mass in births to the left of the cutoff, the assignment to treatment at the cutoff is random if parents have imprecise control of the exact date of birth. Because eligibility to the SP is based on the expected due date, one potential concern is that parents with due dates close to the cutoff misreport the date of last menstruation in order to be eligible for the SP. The covariate balance tests (Table 2) suggest that there is no systematic correlation between pre-determined variables at the cutoff and thus there is little concern for strategic manipulation. Nevertheless, to corroborate this evidence we perform two additional checks. First, we estimate the difference at the cutoff between actual and expected due date of the second child, for the sub-sample of parents in our data for which we have information on expected date of birth (based on the date of last menstruation and based on ultrasound scans). Figure A.4 plots the average difference (in days) between expected and actual date of second birth in each weekly bin of birth spacing in a 24-week window around the eligibility cutoffs. There is no statistically significant difference at the thresholds using either measure of expected delivery date.

TABLE 2.
Covariate balance tests for the regression-discontinuity specification

	Point Est.	SE	t
Spousal pre-birth income gap	9.027	7.326	1.232
Spousal age difference	-0.066	0.035	-1.846
Mother's age at 1^{st} birth	0.061	0.051	1.192
Father's age at 1^{st} birth	0.122	0.056	2.191
Mother's height at 1^{st} birth	-0.064	0.117	-0.548
Mother's weight at 1^{st} birth	-0.103	0.203	-0.507
Gestational length (weeks), 1^{st}	0.033	0.019	1.793
Birth weight (grams), 1^{st}	-0.958	8.014	-0.120
Pre-term birth ($<$ 37 weeks), 1^{st}	-0.009	0.003	-2.905
Low birth weight ($<$ 2500 grams), 1^{st}	-0.005	0.003	-1.842
Birth height (cm), 1^{st} child	-0.011	0.029	-0.374
Apgar 1 above 8, 1^{st} child	-0.002	0.006	-0.256
Joint χ^2			10.679
p-value			0.557

NOTE.— The reported point estimates are the estimates of β in equation (1) for a set of pre-determined spousal (parental) characteristics as well as for characteristics at birth for first-born children (which are pre-determined at assignment to treatment), using a linear trend specification with triangular weights. The sample consists of all families whose second child was born 1980–1989, and whose first two children were born within 18–30 months from each other (24-month regime), and 24–36 months from each other (30-month regime); i.e., speed premium eligibility cutoff $+/-$ six months.

Second, Figure A.5 presents the results from tests of the discontinuity at the cutoff in the density of the running variable, proposed by McCrary (2008), using daily birth data, and separately for the two different SP regimes. The underlying density functions are continuous at the cutoffs in both regimes.[22] This evidence of inability to exactly manipulate the timing of births is also corroborated by the small average difference between the expected due date (based on date of last menstruation) and actual date of birth mentioned above (less than one day). In addition, the majority of the births in Sweden during the 1980s were spontaneous vaginal deliveries. Among all births in Sweden between 1980 and 1989, only 11 percent were delivered via C-section (with a slight declining trend during the decade); although no figures on induction rates are available prior to 1990, in 1991 (the first year to which data are available) 93 percent of all births started spontaneously.[23] As a comparison, Dahl et al. (2016) report an induction rate of 12 percent in Norway in 1993.

Taken together, the results from the different tests presented in this section suggest that there should be little concern for strategic manipulation of the assignment variable at the cutoff. The mass in births, however, lies to the left of the cutoff. Therefore, we allow for separate trends on each side of the cutoff.

[22] This result is robust to alternative bandwidths.

[23] All statistic information about the annual number of births and delivery mode can be found at the Socialstyrelsen website: http://www.socialstyrelsen.se/publikationer2018/2018-1-7.

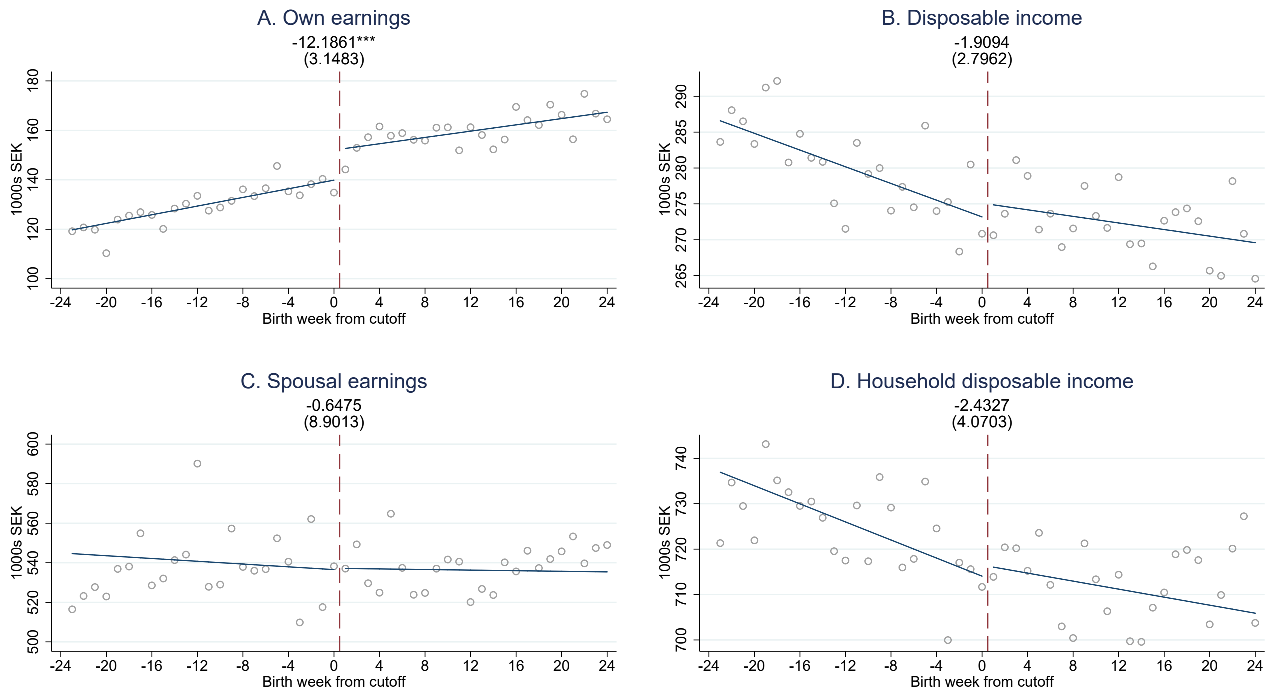
4 Effects of the Speed Premium Eligibility on Parents and Children

4.1 The Speed Premium and Household Labor Supply

As we discuss in Section 2.3, eligibility to the SP potentially affects both the time spent at home and the material resources available to parents when their children are young. The effect of the SP on the labor supply of mothers vis-à-vis fathers may be of a counteracting nature, rendering the net impact on household income *a priori* ambiguous. We therefore analyze the impact of the SP separately on the labor supply of each parent (as measured by their labor income), and on the household's total disposable income.

We start with a graphical analysis where, for each spouse and for the household, respectively, we pool income across the immediate years surrounding and including the year of second birth, that is, for the time periods $\tau = \kappa - 1$ to $\tau = (\kappa + 1)$, where κ denotes the birth year of the second child. Figure 1 plots the average of these outcomes in each weekly bin of the assignment variable in a 24-week window around the SP cutoff. In each graph we also display the RD estimate of the discontinuity at the threshold, based on specification (1). The results show that eligibility to the SP decreases the labor supply of mothers of around SEK 12,000, on average. There are no effects on mothers' disposable income, nor on spousal earnings or the household's total income. Thus, the higher PL benefits induced by the SP seem to fully compensate for mothers' earnings loss from market work.

FIGURE 1.
The effect of the SP on spousal labor earnings and household income



NOTE.— Data is based on the sample described in Table 1. The labor earnings of mothers, fathers, and the disposable income measures are each pooled around the year of the second birth, that is, $\tau = \kappa - 1$ to $\tau = (\kappa + 1)$, where κ denotes the birth year of the second child. The figure reports the point estimate of β in equation (1) using a linear trend specification and triangular weights. Estimated discontinuities at the cutoff are indicated at the top of the vertical line at the cutoff in each graph. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

In Figure A.6 and Figure A.7 we show the results from the separate analysis of the effect of the SP in the 24- and 30-month regimes. We add this analysis for the following reason. Due to the longer eligibility spacing interval under the 30-month regime, mothers have had a longer time to return to work after the first child compared to mothers who gave birth under the 24-month regime. Thus, the financial incentives may not be equally strong across the two regimes. In fact, comparing the income level with mothers' average income in the year before 2^{nd} birth, we note that the average gain of eligibility to the SP is around SEK 65,8000 in the 24-month regime, and around SEK 57,3000 in the 30-month regime. These numbers correspond to 42 and 36 percent of baseline annual labor income for mothers in the respective regimes.[24] The results presented in Figure A.6 and Figure A.7 show that for mothers, the decrease in labor supply is of similar magnitude across both regimes. Under the 24-month regime, there is also a statistically significant positive effect on the total household disposable income, of around SEK 11,000. Whether this effect is driven by mothers' disposable income or fathers' earnings is unclear however, as both are imprecisely estimated. Despite a somewhat larger financial incentive under the 24-

[24]Calculations based on sample presented in Table 1, separately for mothers to children born under the 24- and 30-month regimes.

month regime, we conclude that the SP has similar effects on mothers' earnings across the two different eligibility cutoffs, and will hereon therefore present results on the pooled sample of mothers to children born under the respective SP regimes.

Next, we break down the earnings estimates by pre- and post-birth earnings to analyze the timing of the labor supply responses. We estimate the specification (1) for labor income earned in the calendar year before second birth to assess whether mothers respond to the SP by reducing her labor supply during what would have been the qualifying period in the absence of the SP. Ideally, we would have a less crude measure of the timing of labor supply adjustments than that offered by calendar-year earnings. The annual nature of our income data implies that for women who give birth late in a calendar year, earnings measured in the calendar year before birth will understate a downward adjustment in market work during pregnancy. Similarly, earnings effects in the calendar year of birth will be a combination of pre-birth and year-of-birth labor supply responses, depending on when during the calendar year the child is born. Nevertheless, estimating the effects separately for the different years surrounding birth should provide some insights as to whether mothers react to both current and future benefit levels. Importantly, these features of the data do not pose issues for identification given that calendar month of birth is evenly distributed across birth spacing (we return to this in a robustness check below).

Thus, we perform separate analyses for pre- and post-second birth labor earnings and income, where post-birth income aggregates earnings over the calendar year of (second) birth and the calendar year after, that is, over time periods $\tau = \kappa$ and $\tau = (\kappa + 1)$. Again, we study the impacts separately for mothers and fathers, and on the household disposable income.

Table 3 presents the results. First, column (1) shows that mothers respond to future PL benefit levels; labor supply of mothers in the year before second birth is reduced by nearly four percent relative to baseline earnings. Second, column (2) shows that labor supply also declines immediately post-birth for mothers. Third, columns (3) and (4) show no impact on father's labor income. Fourth, household disposable income does not decrease in the pre-birth year (column 5), when there is a reduction in maternal labor earnings, suggesting that mothers use paid maternity leave between the first two births and not unpaid leave, or get transfers from other government programs (for example, sickness insurance); however, there is a statistically significant positive effect of the SP on the household's post-birth disposable income (column 6). All estimates that are statistically significant survive adjustment to testing simultaneously all the six coefficients in the table (Romano and Wolf, 2005).

The positive effect on household income in the absence of a positive effect on spousal labor supply

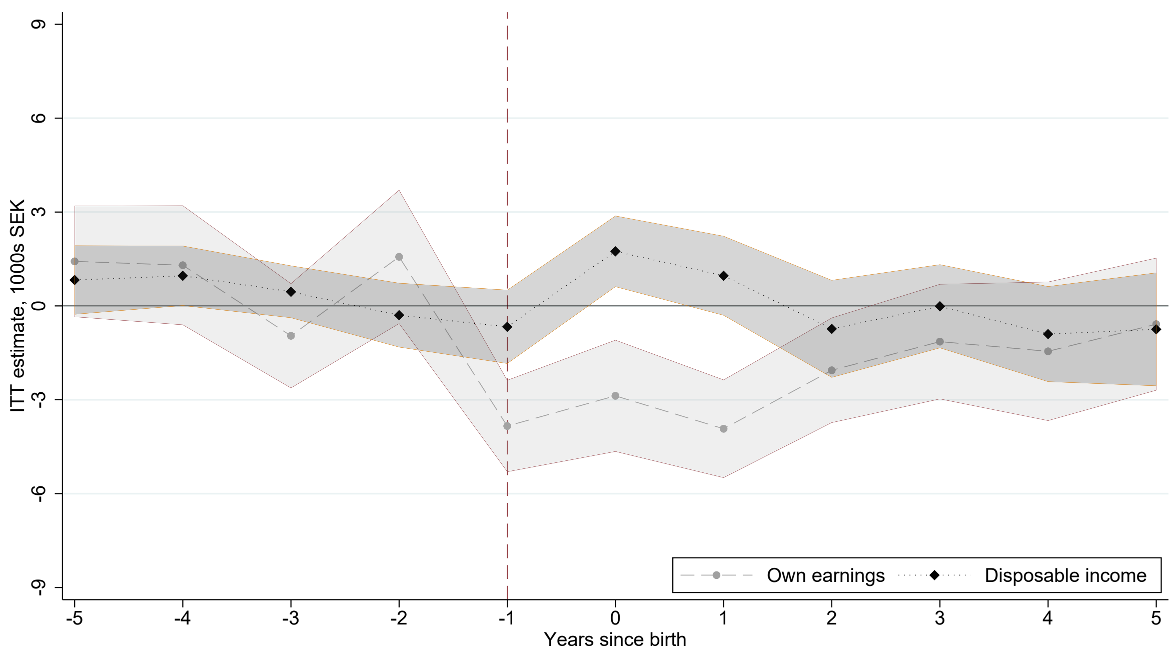
TABLE 3.
The effect of the SP on spousal labor earnings and household income

	(1)	(2)	(3)	(4)	(5)	(6)
	Mother's income		Father's income		HH disp. income	
Years since birth, 2^{nd} birth	Pre-birth	Post birth	Pre-birth	Post birth	Pre-birth	Post birth
Treated ($\hat{\beta}$)	**-3.838*****	**-6.756*****	0.466	2.920	0.344	**6.824*****
	(0.729)	**(1.232)**	(1.498)	(3.151)	(0.964)	**(1.852)**
Observations	71,622	71,290	71,574	71,296	71,950	71,950
Mean of outcome	99.07	129.2	198.3	410.5	249.7	534.3
$\hat{\beta}/Mean$	-0.0387	-0.0523	0.0024	0.0071	0.0014	0.0128

NOTE: Data is based on the sample described Table 1. The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The pre-birth outcome is measured in time period $\tau = (\kappa - 1)$, where κ denotes the calendar year of birth of the second child. The post-birth outcomes aggregates earnings over the time periods $\tau = \kappa$ and $\tau = (\kappa + 1)$. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p < 0.1$.

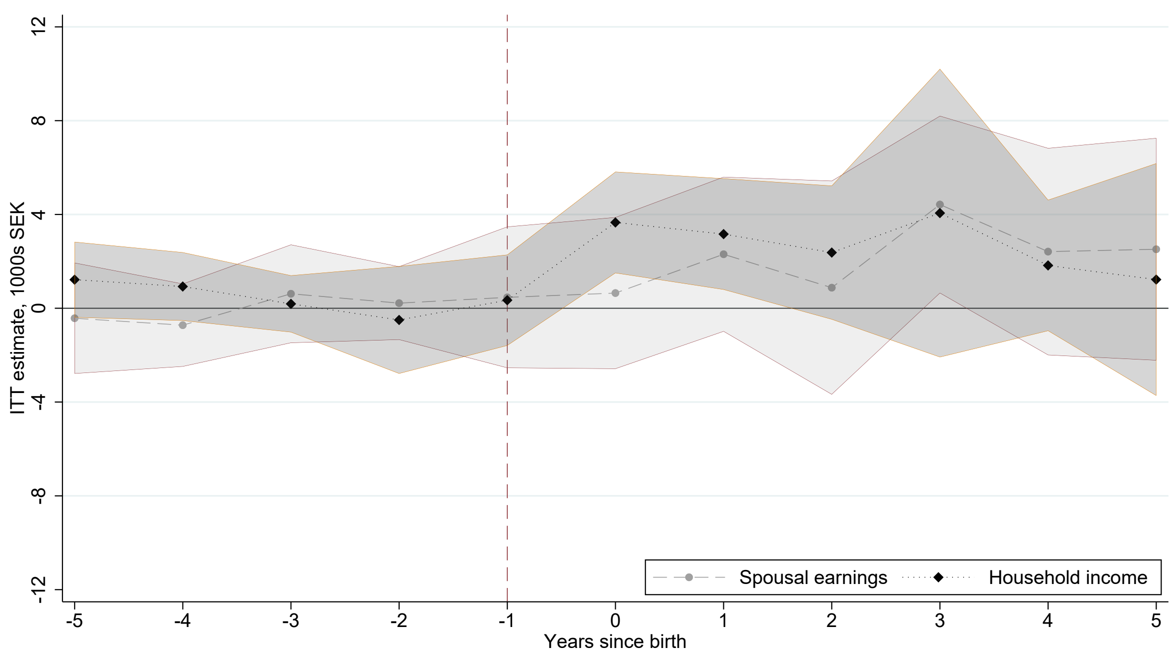
may seem puzzling at first glance. In Figure 2 we estimate the effect of the SP on *annual* earnings and disposable income of mothers, from five years before second birth up to five years after. The estimates for years $\tau = (\kappa - 5)$ to $\tau = (\kappa - 2)$ serve as specification- or *placebo* tests, as we should not expect any differences in earnings over this time period between mothers of different spacings. Results show that there are indeed no effects of the SP on earnings in the pre-treatment period, but that eligibility to the SP has an instantaneous impact on mothers' labor supply, which lasts up to two years after second birth. Moreover, the disposable income of just-eligible mothers is significantly higher in the year of birth, compared to the counterfactual scenario of being just-noneligible, and marginally significant the year after. Thus, the positive effect on household income pooled over the full post-birth period is likely driven by the increase in mothers' total income. This latter conjecture is corroborated in Figure 3, where household total income is shown to be statistically significantly positive in the year of birth and the year after birth. For fathers, there is no apparent effect of their spouse gaining access to the SP. Again, there are no differences in the pre-treatment outcomes across treatment status, suggesting that we are indeed capturing the causal effect of the SP on earnings.

FIGURE 2.
Temporal pattern of the effect of the SP on own labor earnings and disposable income



NOTE.— Data is based on the sample described in Table 1. The vertical line indicates the time at which SP eligibility is revealed. Each point in the graph pertains to the point estimates point estimate of β in equation (1) on yearly earnings (and income) from year $\tau = (\kappa - 5)$ to $\tau = (\kappa + 5)$, along with the 95-percent confidence intervals, estimated with a linear trend specification and triangular weights. Standard errors are clustered at the birth week level.

FIGURE 3.
Temporal pattern of the effect of the SP on spousal labor earnings and household disposable income



NOTE.— Data is based on the samples described in Table 1. The vertical line indicates the time at which SP eligibility is revealed. Each point in the graph pertains to the point estimates point estimate of β in equation (1) on yearly earnings (and income) from year $\tau = (\kappa - 5)$ to $\tau = (\kappa + 5)$, along with the 95-percent confidence intervals, estimated with a linear trend specification and triangular weights. Standard errors are clustered at the birth week level.

TABLE 4.
The effect of the SP on maternal labor market participation

Years since 2nd birth	(1) $\tau = (\kappa - 1)$	(2) $\tau = \kappa$	(3) $\tau = (\kappa + 1)$	(4) $\tau = (\kappa + 2)$	(5) $\tau = (\kappa + 3)$	(6) $\tau = (\kappa + 4)$
Treated ($\hat{\beta}$)	**-0.010*****	-0.013**	**-0.016*****	**-0.011****	-0.004	-0.007
	(0.004)	(0.006)	**(0.005)**	**(0.005)**	(0.003)	(0.005)
Observations	71,622	71,625	71,603	71,816	71,749	71,711
Mean of outcome	0.954	0.913	0.892	0.918	0.916	0.918
$\hat{\beta}/Mean$	-0.0109	-0.0139	-0.0182	-0.0122	-0.0046	-0.0072

NOTE: Data is based on the sample described Table 1. The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The outcome is measured at various points in time, τ, relative to second birth, the timing of which is denoted κ. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p < 0.1$.

In Table 4, we study whether the decrease in maternal labor income is driven in part by a reduction in extensive margin labor supply. Labor market participation is here defined as having positive labor earnings. We find that there is a 1 percentage point decrease in mothers' participation in the year before second birth, and a 1.3, 1.6 and 1.1 percentage point decrease in the year of birth, year after, and two years after, respectively[25] Thereafter, the point estimates are close to zero and not statistically significant. Hence, part of the earnings decrease is driven by mothers who exit - or fail to return to - market work entirely during the calendar year before second birth. This implies an opportunity for more exclusive time with the first-born child before the arrival of his or her younger sibling. Moreover, the results also suggest increased time spent at home with the new child.

4.1.1 Additional Robustness Checks

In addition to the lack of differences in pre-treatment outcomes shown in Figure 2 and Figure 3, we present the results from an additional placebo test. Specifically, we use data on couples where the second child was born in 1977–1979; before the 24-month SP came into effect. We set the fake cutoff to 24 months, and estimate the discontinuity at the cutoff in the mothers' and fathers' earnings. The results are presented in Figure A.8 and show that, reassuringly, there is no discontinuity at the cutoff in the placebo sample.

Because some individual pre-determined covariates were statistically significantly different from zero at the cutoff in Table 2, we test the sensitivity of the results to the inclusion of these pre-determined

[25] The coefficients for the year before, after and two years after second birth, i.e., in columns (1), (3) and (4) of Table 4 survive adjustment to multiple hypotheses testing.

covariates in the estimations. Table A.3 presents results from estimating equation (1) on earnings, controlling for the pre-birth spousal income-gap, the spousal age difference, and the spouses' age at first childbirth. Our main findings are robust to including these pre-determined control variables.

We have worked with the global linear specification with triangular weights in our analyses. As a specification test, we follow Lee and Lemieux (2010) and report the polynomial choice from a bins test by estimating models with a full set of birth week dummies together with different parametric trends. The recommended polynomial – presented in the first column of Table A.4 and Table A.5 – is the specification in which the set of bin dummies are jointly insignificant. For all but one outcome in the 24-month and 30-month regimes, the bins test recommend a linear specification. In the tables we reiterate the point estimates using the global linear specification, and provide local linear estimates (last three columns) as a comparison. As seen, the global and local estimates are very similar across all outcomes. In Figure A.9, we also report local linear point estimates and confidence intervals for mothers earnings for a wide range of bandwidths. The estimates show that our baseline estimate is highly robust to the choice of functional form for bandwidths that are in the close neighborhood around the optimal bandwidth (i.e., the bandwidth that minimizes the mean square error).

Summing up, the findings presented in this section suggest that mothers' leave-taking behavior responds to parental leave benefit levels. This is consistent with a recent paper by Moberg (2017) who studies the effect of parental leave benefit levels on the take-up of parental leave by both spouses during the 1990s, exploiting the 30-month SP threshold. She finds that the 30-month speed premium increases mothers' benefit levels for the second child, which in turn increased mothers' duration of leave with the *second* child. Additionally, we find that mothers also respond to *future* benefits, and reduce their labor supply during what would have been the qualifying period in the absence of SP eligibility. We also find that the SP fully compensates for the labor earnings decline, rendering even net positive effects on household income in the post-birth period. In the full sample, we find no effects on spousal labor supply.[26] In the next section, we analyze potential heterogeneous effects across socioeconomic status.

[26]There is relatively little evidence on the fathers' response to increased parental leave benefits, and the findings are mixed: Dahl et al. (2016) find no impacts on fathers' labor supply for Norway; Canaan (2016) finds that expansion of paid leave in France has no impacts of the extensive margin of fathers' labor, but it reduces absences related to sick-leave; and Bana et al. (2018) find that higher benefits increase the labor supply of mothers and fathers post-birth labor supply. However, these three studies focus on heterogeneous settings from the most (Dahl et al., 2016) to the least generous (Bana et al., 2018).

4.1.2 Heterogeneous Effects by Mother's Position in the Earnings Distribution

As we discuss in Section 2.3, the expected responses of parents to the SP may vary according to their SES, which we can proxy by maternal pre-birth income. Thus, we analyze heterogeneous impacts on the earnings of both partners and on household income by the mother's position in the (maternal) pre-birth earnings distribution. We divide the sample in three equally large groups, based on mothers' income being in the lowest, middle, and top third of the income distribution. While both own and spousal earnings are higher among mothers in the top-income group, the average spousal earnings difference is the largest among mothers in the lowest income group: women in the bottom third of the income distribution make 64,000 SEK less than their partners in the year before first birth, while mothers in the top third of the income distribution make only 3,600 SEK less than their husbands. Thus, while the couples in the low SES group may be more financially constrained than couples in the top, the division of labor may be more gender equal in the top group.

Table 5 presents the heterogeneous impacts of the SP by maternal SES. First, we note that the maternal labor supply responses are qualitatively similar across the income groups; mothers eligible to the SP reduce their labor supply both before and after second birth irrespective of their position in the earnings distribution. Quantitatively, the earnings reductions are – relative to baseline earnings – larger in the lower end of the distribution. Second, for the two lower income tertiles, the earnings reduction in the year before birth is not fully compensated for by the SP or other government transfers, as the household disposable income is significantly lower in that year. At the same time, we do not see a compensating behavior on the part of fathers in these households, suggesting that spousal labor supply does not act as an insurance to reduced maternal income in our context. Third, in couples where the mother belongs to the top third of the earnings distribution, however, spousal labor supply responses are *positive*, suggesting that there is some substitution between male and female time in home production in this group. Quantitatively, the husbands' increase in earnings is similar in amount to the reduction in the wives' earnings (see columns (2) and (4) of Panel C); but unlike the impact on maternal earnings, the effect on father's income post-birth of the second child is no longer statistically different from zero once we account for the fact that we simultaneously test 18 hypotheses. The net impact on household disposable income in top income group is positive; an increase of around two percent. In sum, for the high-income households, the SP potentially increases the specialization within the household (Becker, 1981; Rosen, 1983). While the total effect on household income is positive, this effect is only short-lived (see Figure 3).

TABLE 5.
Heterogeneous effects of the SP on spousal labor earnings and household income by mother's position in the earnings distribution

	(1)	(2)	(3)	(4)	(5)	(6)
	Mother's income		Father's income		HH disp. income	
Years since birth, 2^{nd} birth	Pre-birth	Post birth	Pre-birth	Post birth	Pre-birth	Post birth
A. Lowest income group (tertile 1)						
Treated ($\hat{\beta}$)	**-5.006*** **	**-7.015*** **	-1.196	-0.125	-2.847**	0.415
	(1.306)	**(2.179)**	(2.476)	(6.867)	(1.366)	(3.112)
Observations	20,959	20,810	20,944	20,832	21,074	21,074
Mean of outcome	69.95	87.03	174.5	361.9	225	487.9
$\hat{\beta}/Mean$	-0.0716	-0.0806	-0.0069	-0.0003	-0.0127	0.0009
B. Middle income group (tertile 2)						
Treated ($\hat{\beta}$)	**-3.775*** **	**-5.315*** **	-2.318	-4.949	-2.517*	1.974
	(1.341)	**(1.892)**	(2.393)	(4.453)	(1.312)	(3.515)
	(0.472)	(1.194)	(2.135)	(2.305)	(0.909)	(3.029)
Observations	22,402	22,295	22,379	22,302	22,510	22,510
Mean of outcome	89.49	108.3	192.4	395.4	240.3	512.2
$\hat{\beta}/Mean$	-0.0422	-0.0491	-0.0121	-0.0125	-0.0105	0.0039
C. Highest income group (tertile 3)						
Treated ($\hat{\beta}$)	-3.633*	**-8.933*** **	3.155	9.953**	**4.302*** **	**14.200*** **
	(1.857)	**(2.704)**	(3.991)	(3.928)	**(1.352)**	**(2.907)**
Observations	28,261	28,185	28,251	28,162	28,366	28,366
Mean of outcome	129	178.1	221.3	459.7	276.1	587.6
$\hat{\beta}/Mean$	-0.0282	-0.0502	0.0143	0.0217	0.0156	0.0242

NOTE: Data is based on the sample described Table 1. The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The pre-birth outcome is measured in time period $\tau = (\kappa - 1)$, where κ denotes the calendar year of birth of the second child. The post-birth outcomes aggregates earnings over the time periods $\tau = \kappa$ and $\tau = (\kappa + 1)$. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p < 0.1$.

4.2 The Speed Premium and Children's Outcomes

We study three groups of outcomes for children. First, we analyze impacts on schooling performance in 9^{th} grade and the likelihood of having attended college by age 24. Schooling performance is measured through two variables: an indicator for high school eligibility upon finishing 9^{th} grade (that is, after finishing compulsory schooling), and the cumulative GPA (standardized within graduation year to have a mean of zero and unit standard deviation). Secondly, we analyze two sets of health outcomes. In particular, we examine outcomes at birth for the second-born child. Because eligibility to the SP is revealed to mothers during pregnancy, the knowledge of a better financial situation could potentially lead to reduced maternal stress, which in turn could have beneficial impacts for children's outcomes at birth (see

TABLE 6.
The effect of the SP on the second-born child's outcomes at birth

	(1) Birth weight (grams)	(2) Gestation (weeks)	(3) Low birth weight	(4) Preterm birth	(5) Birth height (cm)	(6) Apgar-1	(7) Apgar-5
Treated ($\hat{\beta}$)	-4.332	0.011	0.001	-0.001	-0.003	-0.007	0.002
	(9.518)	(0.020)	(0.002)	(0.003)	(0.048)	(0.011)	(0.011)
Observations	71,685	71,783	71,685	71,783	71,484	71,153	68,318
R-squared	0.000	0.000	0.000	0.000	0.000	0.000	0.001
Mean of outcome	3,598	39.51	0.0206	0.0337	50.61	8.871	9.632
$\hat{\beta}/Mean$	-0.0012	0.0003	0.0541	-0.0311	0.0000	-0.0008	0.0002

NOTE: Data is based on the sample described in Table 1. The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p < 0.1$.

e.g. Black et al., 2016; Currie and Rossin-Slater, 2013; Aizer et al., ming; Persson and Rossin-Slater, 2018, for recent evidence on in utero exposure to maternal stress and its implications for child health). We also analyze health outcomes at later ages as measured by hospital admissions at ages 10 and 14.

In Table 6 we present the results for the second-born children's outcomes at birth. There are no effects of the SP on birth weight, gestation, birth height, or Apgar scores. Thus, the SP does not affect the outcomes at birth for the new child. Next, we look at hospital admissions, presented in columns (4)–(5) and (9)–(10) for the first- and second-born child, respectively, in Table 7. Apart from a significant effect on the likelihood of being admitted to the hospital at age 14 for the first-born child of 0.6 percentage points, there are no effects on hospital admissions at either age or parity. However, we note that hospital admission is a crude measure of health; it only measures the most severe health events.

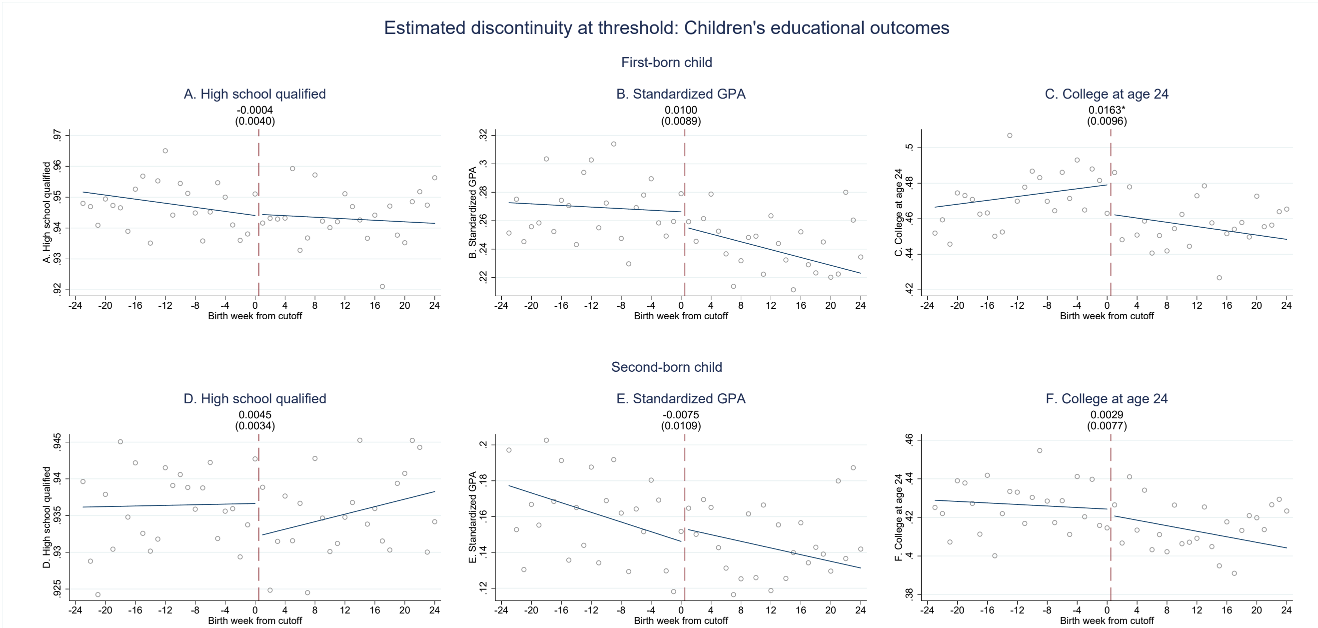
Finally, columns (1)–(3) and (6)–(8) of Table 7 present the results on the schooling performance for the first- and second-born child, respectively. There is no impact on the likelihood of the compulsory schooling grades being sufficient to qualify for high school for either child, nor any impacts on the cumulative GPA. In terms of college attendance, first-born children are found to be 1.6 percentage points more likely to having attended college by age 24 if their mother was eligible for the SP. Taken together, these results suggest that first-born children to mothers with SP eligibility are more likely to attain higher education, but that there are no impacts for the younger child. Figure 4 shows the results on the schooling outcomes graphically.

TABLE 7.
The effect of the SP on children's education and health outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	First Child					Second Child				
	High school qualified	Standard. GPA	College at age 24	Hospitalization age 10	Hospitalization age 14	High school qualified	Standard. GPA	College at age 24	Hospitalization age 10	Hospitalization age 14
Treated ($\hat{\beta}$)	-0.000	0.010	0.016*	-0.001	0.006**	0.005	-0.007	0.003	0.004	-0.001
	(0.004)	(0.009)	(0.010)	(0.002)	(0.002)	(0.003)	(0.011)	(0.008)	(0.003)	(0.004)
Observations	46,692	70,191	69,591	71,950	71,950	58,686	69,856	69,288	71,950	71,950
Mean of outcome	0.943	0.245	0.458	0.0357	0.0333	0.934	0.146	0.415	0.0346	0.0349
$\hat{\beta}/Mean$	-0.0005	0.0410	0.0355	-0.0413	0.169	0.0048	-0.0514	0.0069	0.116	-0.0192

NOTE: Data is based on the sample described in Table 1. The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE 4.
The effect of the SP on children's educational outcomes



NOTE.— Data is based on the sample described in Table 1. The figure reports the point estimate of β in equation (1) using a linear trend specification and triangular weights. Estimated discontinuities at the cutoff are indicated at the top of the vertical line at the cutoff in each graph. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Placebo Analyses and Robustness to Functional Form As for the parental outcomes, we perform placebo tests using the 24-month threshold as a fake cutoff for cohorts born before the 24-month rule came into effect, that is, for families in which the second child was born in 1977–1979. The results are presented in Figure A.10 and show that there are no significant discontinuities at the cutoffs for either the GPA or college attendance outcomes, for neither parity, and the point estimates for the first-born child are of opposite sign than our main estimates.

Moreover, we reiterate the bins test (Lee and Lemieux, 2010) for the recommended polynomial for children's outcomes. Results are presented in Panel B of Table A.4 and Table A.5 and show that for all outcomes the recommended specification is a linear one. Again, local linear estimates are very similar to those obtained with a global linear specification.

4.2.1 Results for Higher-Parity Births

So far, we have focused on families with at least two children and used the spacing between the first two children to assess the effects of the SP on the siblings' outcomes. Next, we study whether we can generalize our findings on education outcomes to higher-parity children. To do so, we sample all *second*- and *third*-born children in couples with at least three kids, and where the mother was *not* eligible for the

TABLE 8.
The effect of the SP on children's education outcomes: higher parity children

	(1)	(2)	(3)	(4)	(5)	(6)
		Second child			Third child	
	High school qualified	Standardized GPA	College at age 24	High school qualified	Standardized GPA	College at age 24
Treated ($\hat{\beta}$)	0.005	**0.061****	0.036*	-0.002	0.020	0.031*
	(0.011)	**(0.025)**	(0.020)	(0.014)	(0.041)	(0.016)
Observations	10,272	14,568	14,507	12,629	14,659	14,544
Mean of outcome	0.919	0.0665	0.391	0.911	0.00296	0.377
$\beta/\hat{Mean}$	0.0053	0.9180	0.0925	-0.0026	6.788	0.0834

NOTE: Data is based on families with at least three children and whose third child was born 1980–1989 and where the mother was not eligible for the SP for the first two children. The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

SP with her first two births, but eligible for PL benefits with her second child.[27] The results, presented in Table 8, show that the SP positively affects the compulsory schooling cumulative GPA by 6 percent of a standard deviation and increases the likelihood of college attendance by 3.6 percentage points, for the lower birth order child (second-born child). In this sample, we also find a statistically significant effect on college attendance for the younger child. One reason for why the effect sizes are quantitatively larger than in the two-children case could be that additional parental investments may matter more in a larger family size, where per-child resources are diluted compared to households with fewer children. Nevertheless, we conclude that, overall, also these findings suggest that it is mainly the *existing*, or lower birth order, child that benefits from the additional parental resources induced by the SP.

Because the results are qualitatively similar across birth parities, from hereon, we pool the samples of first- and second- and second- and third-born children in all remaining analyses of the effect of the SP on children's outcomes. In Table A.6, we re-estimate the effects of the SP on the schooling outcomes of the lower- and higher- birth order child on this pooled sample. Results show an increase in GPA of two percent of a standard deviation and an increase in likelihood of college attendance of two percentage points, for the lower-birth order child. There are no effects on the schooling outcomes for the higher-birth order child in this pooled sample.

Why does the SP benefit only the older child? It is possible that later born children spend less time with their parents, on average, than earlier born children and that they have to share that parental time

[27] We exclude mothers eligible for the SP after her first child to make sure that we calculate the correct qualifying income for wage replaced leave. That is, to whom we can use the labor income before the birth of the second child as a proxy for eligibility to paid leave on the wage-replaced level.

with the older sibling. If time spent with the mother in the first years of life is more important than when children are older, later-born children will benefit less from parental time since that time is competed for with an older sibling (Markus and Zajonc, 1977; Zajonc, 1976; Price, 2008).[28] Moreover, recent work finds that birth order effects on cognitive and non-cognitive outcomes are driven by differential parental investments across children of different parities after their birth (Pavan, 2016; Black et al., 2018). The automatic renewal of PL benefits in Sweden thus seems to contribute to part of the "birth order penalty", potentially by increasing the gap in maternal time between older and younger children.

4.2.2 Heterogeneity by Child Gender

Earlier studies on the effects of maternity leave durations on the outcomes of children find that boys tend to benefit more from parental care compared to alternative care than do girls (see e.g. Danzer and Lavy, 2017). Table 9 shows the impact of the SP on the educational outcomes of boys and girls. Results show a significant effect in the likelihood of college attendance for the lower-birth order boys, which survives adjustment to multiple hypotheses testing. Results for lower-birth order girls are not significant. For higher-birth order children, there are no statistically significant effects for either boys or girls, and the point estimates are all close to zero. Thus, we find weak evidence that boys benefit more than girls. Again, our results suggest that mainly lower birth order kids benefit from the SP.

4.2.3 Additional Robustness Checks

In Table A.7 we repeat the exercise of adding pre-determined covariates to the estimations for children's outcomes. In addition to controlling for parental pre-determined characteristics, we also include dummies for children's calendar year of birth, to account for potential shocks common to all children born in a given year, and – more importantly – calendar month of birth which may be related to educational outcomes via school starting age (see e.g. Buckles and Hungerman, 2013). The results show that the impacts of the SP on the schooling outcomes of the lower-birth order child are robust to including control variables; the coefficients are quantitatively and qualitatively unchanged. Finally, in Figure A.11, we show that the results on college attendance of the lower-birth order child is robust to functional form; for bandwidths close to the optimal bandwidth, local linear point estimates are similar to that from the linear specification.

We conclude that our analyses, taken together, suggest a positive effect of the SP on the schooling

[28] de Haan (2010) finds that the birth order effects do not vary with the spacing between births.

TABLE 9.
The effect of the SP on children's education outcomes: heterogeneous effects by gender

	(1)	(2)	(3)	(4)	(5)	(6)
	Lower birth order child			Higher birth order child		
	High school qualified	Standardized GPA	College at age 24	High school qualified	Standardized GPA	College at age 24
A. Boys						
Treated ($\hat{\beta}$)	-0.004	0.018	**0.023*** **	0.008	0.004	0.009
	(0.007)	(0.014)	**(0.008)**	(0.008)	(0.016)	(0.009)
Observations	29,192	43,511	43,315	36,629	43,388	43,160
Mean of outcome	0.926	0.0307	0.375	0.919	-0.0427	0.343
$\hat{\beta}/Mean$	-0.0044	0.573	0.0619	0.0088	-0.0921	0.0274
B. Girls						
Treated ($\hat{\beta}$)	0.006	0.023	0.017	-0.002	-0.004	0.009
	(0.005)	(0.014)	(0.012)	(0.004)	(0.018)	(0.009)
Observations	27,757	41,224	40,778	34,670	41,109	40,670
Mean of outcome	0.952	0.406	0.522	0.941	0.294	0.479
$\hat{\beta}/Mean$	0.0059	0.0562	0.0327	-0.0017	-0.0122	0.0183

NOTE: Data is based on the samples described in Table 1, pooled with the corresponding sample of families with at least three children (higher parity kids). The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

outcomes of the older child of two successive siblings, despite that the higher PL benefits are realized during the parental leave with the younger child.

5 Heterogeneity and Potential Mechanisms

From the analyses presented in the previous section, we draw three main conclusions. First, mothers respond to both *future* and *current* PL benefit levels by lowering their labor supply. For mothers in the lower two thirds of the earnings distribution, there are no changes in household disposable income in the year before subsequent birth (once we adjust inference for multiple hypotheses testing). For children to these mothers, the SP thus potentially increases time spent with the mother, but does not alter the financial capacity of the household. Secondly, for mothers in the top third of the earnings distribution, we observe a compensating behavior on the part of spouses, the labor income of whom increases. The total impact of higher maternal benefits on the household disposable income in this group is positive. Third, there is an improvement in the long-run schooling outcomes for children whose mother was just eligible to SP relative to children of just-ineligible mothers. However, the SP affects only the child existing in the household at SP eligibility receipt, that is, the lower birth order child. In this section, we probe the potential mechanisms through which children's schooling attainment benefits from maternal SP receipt. Specifically, we consider parental time and monetary resources, the quality of the alternative mode of care, and diluted or increased resources via completed family size. Our data and empirical strategy do not allow us to explicitly separate between these channels. Instead, we perform subgroup analyses to arrive at tentative conclusions about which mechanisms are likely to be more important.

5.1 Parental Time and Monetary Resources

First, we consider parental time investments in the form of maternal time (vis-à-vis labor supply) and household financial resources. In Table 5 we found that the SP has different effects on the financial situation of the household, but similar effects on maternal labor supply across groups defined by the mother's position in the earnings distribution. In particular, in families of high SES mothers, in addition to an increase in maternal time at home, the SP also increases household disposable income, which is likely driven by the increased labor supply of the fathers in these households. Thus, there is potentially some substitution between male and female time at home in this group. In Table 10, we investigate whether the heterogeneous responses by maternal SES are carried through to children's outcomes. For lower-birth order children to high SES mothers there is a positive effect of the SP on 9^{th} grade GPA of 4.5 percent of a standard deviation, and a 4.2 percentage point increase in the likelihood of college attendance by age 24 (surviving adjustment for multiple hypotheses testing at 10 percent significance level). For the younger child, there is a 1.6 percentage point increase in college attendance. There are no effects for children to low- or middle-income mothers. The magnitude in the increase in maternal time (measured by their labor earnings) is similar for mothers in the top and bottom SES groups (see Table 5), suggesting that the quality of maternal time may differ by SES. There are, however, alternative explanations for the differential impacts of maternal time by SES other than the quality of maternal care. For example, it is possible that there are complementarities between maternal time and household income, which could explain the larger impacts for children of top-income mothers. On the other hand, it seems unlikely that the additional family resources dramatically affect children given that the improvements in child outcomes are found in a group of financially unconstrained families, and given that the increase in household resources is statistically significant but economically small (2 percent; see column (6) of Table 5).

Another way to assess the importance of parental investments is to study heterogeneous effects

TABLE 10.
The effect of the SP on children's education outcomes: heterogeneous effects by mother's position in the earnings distribution

	(1)	(2)	(3)	(4)	(5)	(6)
	Lower birth order child			Higher birth order child		
	High school qualified	Standard. GPA	College at age 24	High school qualified	Standard. GPA	College at age 24
A. Low income group (tertile 1)						
Treated ($\hat{\beta}$)	-0.001	-0.009	0.005	0.006	-0.016	0.000
	(0.007)	(0.017)	(0.011)	(0.009)	(0.024)	(0.015)
Observations	16,649	24,998	24,816	20,858	24,843	24,746
Mean of outcome	0.906	-0.0107	0.349	0.891	-0.0895	0.317
$\hat{\beta}/Mean$	-0.0014	0.879	0.0134	0.0062	0.175	0.000
B. Middle income group (tertile 2)						
Treated ($\hat{\beta}$)	0.000	0.011	0.005	-0.001	-0.016	0.004
	(0.008)	(0.017)	(0.017)	(0.008)	(0.025)	(0.012)
Observations	17,926	26,871	26,682	22,580	26,822	26,621
Mean of outcome	0.934	0.132	0.397	0.926	0.0361	0.361
$\hat{\beta}/Mean$	0.0005	0.0813	0.0132	-0.0007	-0.441	0.0098
C. High income group (tertile 3)						
Treated ($\hat{\beta}$)	0.000	0.045**	**0.042*** **	0.004	0.015	0.016**
	(0.005)	(0.021)	**(0.012)**	(0.005)	(0.022)	(0.008)
Observations	22,389	32,890	32,600	27,877	32,850	32,465
Mean of outcome	0.968	0.457	0.563	0.963	0.355	0.521
$\hat{\beta}/Mean$	0.0005	0.0989	0.0745	0.0040	0.0436	0.0314

NOTE: Data is based on the samples described in Table 1, pooled with the corresponding sample of families with at least three children (higher parity kids). The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p <$ 0.1.

across the two SP regimes, that is, separately for children whose mothers received the SP under the 24- and 30-month regimes. In Figure A.6 and Figure A.7, we show that the SP has similar effects on mothers' labor supply across the two regimes, but that the SP under the 24-month regime also leads to an improvement in household income. In Table 11 we present results from the analysis of the effect of the SP on children separately for the two SP-regimes. The results show positive effects of the SP on the schooling outcomes (of lower birth order children) only under the 24-month regime, which could again be indicative of complementarities between family resources and maternal time. We note that the estimates in the table are imprecise, and that we cannot rule out that the eight hypotheses tested in Table 11 are jointly zero at 10 percent of significance once multiple inference is accounted for. When we jointly test the four hypotheses in columns (1) and (2) (i.e., for the lower parity child) we reject the null of no im-

TABLE 11.
The effect of the SP on children's education outcomes: heterogeneous effects by spacing eligibility thresholds

	(1)	(2)	(3)	(4)
	Lower birth order child		Higher birth order child	
	Standardized GPA	College at age 24	Standardized GPA	College at age 24
A. 24-month regime				
Treated ($\hat{\beta}$)	0.035**	0.029**	0.002	0.009
	(0.014)	(0.011)	(0.017)	(0.008)
Observations	43,824	43,471	43,779	43,443
Mean of outcome	0.230	0.460	0.145	0.427
$\beta/\hat{Mean}$	0.154	0.0625	0.0159	0.0211
B. 30-month regime				
Treated ($\hat{\beta}$)	0.004	0.011	-0.004	0.008
	(0.017)	(0.014)	(0.021)	(0.009)
Observations	40,935	40,627	40,736	40,389
Mean of outcome	0.194	0.429	0.0912	0.386
$\beta/\hat{Mean}$	0.0204	0.0257	-0.0430	0.0216

NOTE: Data is based on the sample described in Table 1, pooled with the corresponding sample of families with at least three children (higher parity kids). The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p < 0.1$.

pact for children in the 24-month regime for these results.[29] However, we can not credibly rule out other potential explanations for the heterogeneous impacts across the two regimes. Two obvious alternative mechanisms are the spacing of children, which is six months shorter at the threshold in the 24-month regime, and the younger age of the older child at SP receipt across the two regimes.

Nevertheless, we believe that maternal time, and the quality of parental time, are more likely channels than the small positive income shock or the age at SP receipt, mainly for two reasons. First, the impacts found on household income show that meeting the eligibility requirement for the SP only increases household income temporarily. Due to data limitations we can not directly study the impacts on measures of household consumption, which would allow to understand possible changes in household expenditures. But, in line with findings from the consumption literature, it is unlikely that the temporary albeit positive income shock is transferred to household consumption and parental investments (Blundell et al., 2008; Carneiro and Ginja, 2016). Second, for the existing child in the household, the SP induces a longer consecutive period of exclusive maternal time, before the arrival of a younger sibling, in both SP regimes.

[29]Only at a significance level of 20 percent would we reject the null of no impact on standardized GPA and college attendance at age 24, when testing all the eight hypotheses in Table 11 simultaneously.

Alternative Mode of Care The additional maternal time seems only to matter for children's development under the early SP regime. For the period under study, there is no individual data on child care enrollment. However, we can leverage aggregate data on national enrollment rates in child care for children of different ages (Figure A.12). For both 1- and 2-year old's, the national enrollment rate is higher under the 30-month regime.[30] For the period of 1980–1985 (24-month regime) the child care enrollment rate among 1-year old's (which is the corresponding age of the older child in the household at treatment receipt) was 11–20 percent, whereas during 1986–1989 the enrollment rate among 2-year old's (corresponding to the age of the older child in the household at SP receipt under the 30-month regime) was over 30 percent. Thus, the alternative to maternal care under the 24-month regime is more likely to be informal outside-the-home care or family day care compared to under the 30-month regime, which may explain the stronger positive impacts of maternal time under the former. For example, Gupta and Simonsen (2010) find that different types of non-parental care have very different impacts on children's outcomes. In particular, preschool seems to outperform family day care for the overall population of Danish children in their data.[31]

Subsequent Fertility One possible channel through which the SP could affect children is family size: a larger or smaller family size could dilute or increase the amount of resources each child in the family can receive. To understand if gaining eligibility to the SP affects subsequent fertility, we estimate model (1) using completed fertility as the dependent variable. We find no effects on family size either in the sample of two-child households, or in the pooled sample of two- and three-child households.[32] Thus, a change in family size is not likely to be a channel through which the SP affects children's educational outcomes.

5.2 Summary

There are a number of channels through which children may be affected by the SP. The guarantee of higher PL benefits with the new child may lower maternal stress during pregnancy, which has been shown to have adverse effects on children at birth. However, we rule out the maternal stress channel since we find no effects on the birth outcomes of the younger child, and no impacts on the younger child in terms of schooling outcomes. Second, the SP seems to increase the amount of time children spend with

[30] During the 1980s there was a steady expansion of the supply of child care in Sweden.

[31] See, e.g., Blau and Currie (2006); Currie (2001); Ruhm (2004) for surveys of the literature on modes and quality of care and child development.

[32] These results are available from the authors upon request.

their mother, and this effect is similar across both the existing and new child, and across maternal SES. Nevertheless, only the older child benefits, and only the older child in families with a high SES mother. This suggests that the exclusivity and quality of maternal time may matter for children's development. Finally, there are some heterogeneous effects of the SP on the household's total monetary resources, but the effects on the disposable income of the household are relatively small and short-lived. We argue that it is unlikely that a short-run positive income shock in already financially unconstrained couples would dramatically affect children, and conclude that increased maternal time is a more likely channel.

6 Conclusions

The stated goals of family policies in industrialized countries are to promote child and maternal health, and to reduce gender inequalities in labor market participation and earnings. However, the duration of mandated leave with job protection and the level of compensation vary across countries, reflecting the lack of consensus on the optimal design of such programs. Key policy questions are whether paid leave entitlements have beneficial impacts for women's labor market outcomes and for children, and whether such effects differ by the generosity of leave entitlements. To address these questions, we exploit a feature of the Swedish parental leave (PL) system that generates variation in mothers' leave benefits, holding constant the duration of paid leave and job protection. The feature that we study is called the Speed Premium (SP), and it grants mothers higher PL benefits for a subsequent child without re-qualifying for benefits by going back to work, provided that the spacing between the births of the two children is below a pre-specified threshold. The spacing eligibility threshold was set to 24 months in 1980–1985, and expanded to 30 months in 1986 (where it remains today). The SP carries clear incentives for mothers to reduce their labor supply, while its potential effects on family income are a priori ambiguous by virtue of the higher PL transfers and potential spousal labor supply responses. We thus study the impact of the SP on all three margins (own and spousal labor supply, and family income) in the period surrounding the birth of the new child, in couples with at least two children. We also examine the effects on children in both the short- and long-run, separately for the child existing in the household and for the new child, respectively. While the SP targets the benefit levels received for the new child, it generates incentives for lower market work before the younger sibling is born, as eligibility status is based on expected due dates and therefore revealed during pregnancy through ultrasound scans or date of last menstruation. Thus, it potentially affects the older child's time spent with the mother before getting a new sibling.

We implement a Regression Discontinuity strategy, comparing couples whose birth spacing is just below and just above the eligibility threshold, and find three main results. First, just-eligible mothers respond to both *future* and *current* benefit levels. Removing the earnings qualification for subsequent birth immediately reduces mothers' hours worked, as do higher *realized* parental transfers. On average, there are no effects on family income, suggesting that the higher benefits compensate for the earnings reduction. Second, the partners of high-earning mothers respond to their wife's benefit level by increasing their labor supply, potentially increasing the extent of specialization within the household. For these couples, the family income is improved but this effect dissipates by the time the youngest child is two years old. Third, we find that maternal access to the SP increases children's compulsory schooling performance and their likelihood of college attendance by age 24. The effects on children, however, only pertain to the *existing* child in the household; we find no effects for the *new* child. This heterogeneity by birth order exists across the first- and second-born child in a two-child household, and across the second- and third-born child in a three-child household (where the mother is eligible for the SP with the third child). Thus, the SP mainly benefits the older child in the household. Due to the lack of studies changing household resources at a margin similar to ours, it is difficult to compare the effect sizes of our findings to others in the literature. Nevertheless, in the Swedish context Cesarini et al. (2016) find no impacts on the birth and educational outcomes of children of lottery winners, despite a decrease in the labor supply of women (Cesarini et al., 2017). Studies on the introduction and expansion of paid leave in Norway find no impacts on college attendance of exposed children (Carneiro et al., 2015; Dahl et al., 2016), despite an increase in the likelihood of college attendance associated with the expansion of coverage of child care for 3–6 year old's in Norway (Havnes and Mogstad, 2011). One central aspect of the setting in our study that distinguishes it from that of other related papers is that we consider a change in maternal time in families with closely spaced children.

The results presented here have several policy implications. Our findings clearly suggest that parental benefits tied to pre-birth earnings have significant effects on women's short-run labor market attachment. In particular, the automatic renewal of PL benefits yields no improvements in women's long-run job continuity or earnings, nor in household income. In the short-run, however, it widens the gender earnings gap within the household, and potentially increases the extent of female specialization in home production and male specialization in market work. Second, the scheme is regressive as it mainly benefits children to high SES mothers. Finally, the SP widens the birth order penalty, as it only benefits the older child in the household.

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A Appendix

TABLE A.1.
Changes to the Swedish parental leave system over time

Year	Total paid days	Wage-replaced days	Replacement rate,%	SEK/day if SGI= 0	Flat rate days	SEK/day, flat rate
1974	180	180	90	25	0	0
1975	210	210	90	25	0	0
1976	210	210	90	25	0	0
1977	210	210	90	25	0	0
1978	270	240	90	32	30	32
1979	270	240	90	32	30	32
1980	360	270	90	37	90	37
1981	360	270	90	37	90	37
1982	360	270	90	37	90	37
1983	360	270	90	48	90	48
1984	360	270	90	48	90	48
1985	360	270	90	48	90	48
1986	360	270	90	48	90	48
1987	360	270	90	48	90	48
1988	360	270	90	60	90	60
1989	450	360	90	60	90	60
1990	450	360	90	60	90	60
1991	450	360	90	60	90	60
1992	450	360	90	60	90	60
1993	450	360	90	60	90	60
1994[a]	450	360	90	64	90/0	60/0
1995[b]	450	360	80	60	90	60
1996[c]	450	360	75	60	90	60
1997	450	360	75	60	90	60
1998	450	360	80	60	90	60
1999	450	360	80	60	90	60
2000	450	360	80	60	90	60
2001	450	360	80	60	90	60
2002[d]	480	390	80	120	90	60
2003	480	390	80	150	90	60
2004	480	390	80	180	90	60
2005	480	390	80	180	90	60
2006[e]	480	390	80	180	90	60/180
2007	480	390	80	180	90	180
2008	480	390	80	180	90	180
2009	480	390	80	180	90	180

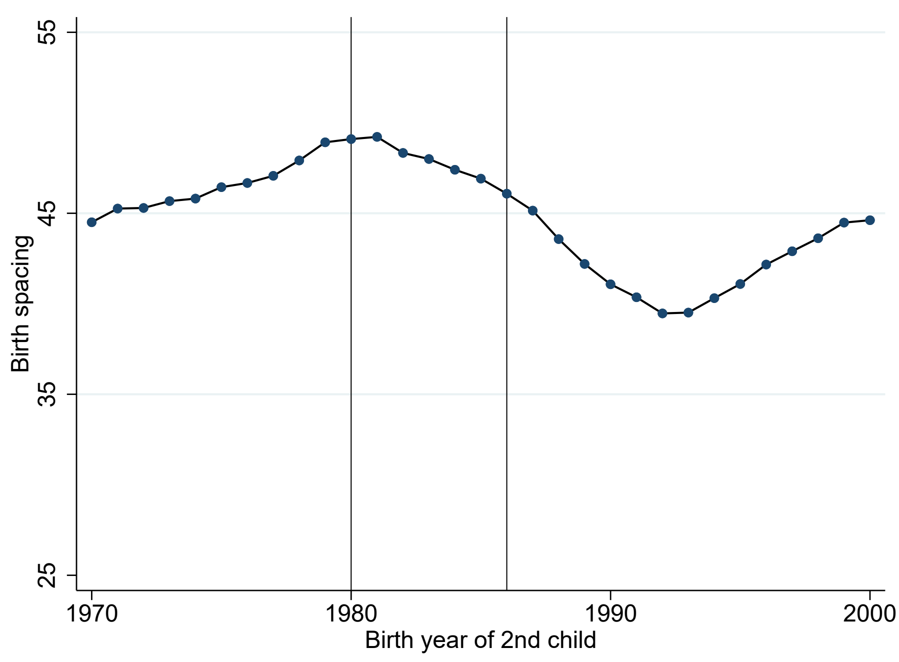
NOTE: The table shows the changes to the Swedish parental leave system since its introduction in 1974. a) During the second half of 1994, the flat-rate days were temporarily abolished for children older than one year. b) The first "daddy-month" was introduced for parents to children born on or after January 1st, 1995. For the 30 days of reserved leave, the replacement rate remained at 90 percent of previous earnings. c) For the 30 days of reserved leave, the replacement rate remained at 80 percent of previous earnings. d) The second "daddy-month" was introduced, targeting parents to children born on or after January 1, 2002. e) The flat rate was set to 180 SEK/day from July 1, 2006 onwards. SGI indicates that the individual has fulfilled the work requirement that entitles them to wage-replaced parental leave. (*Source*: National Insurance Board).

TABLE A.2.
Changes to the speed premium eligibility birth interval over time

Year	Eligibility interval	Total paid PL
1974	12–15 months	6 months
1975–1977	13–16 months	7 months
1978–1979	16–18 months	9 months
1980–1985	24 months	12 months
1986–	30 months	12–16 months

NOTE: The table shows the birth spacing intervals that makes parents eligible for the speed premium, for different time periods. Initially, the eligibility interval was set to statutory duration of paid leave, plus 6 months, which could be extended by up to 3 months. In 1980, the speed premium rule became statutory and the eligibility interval set to 24 months, and further extended to 30 months in 1986. Total paid PL days for the period 1986 onwards is 12–16 months due to gradual extensions of the eligibility for paid leave from 1989 onwards.

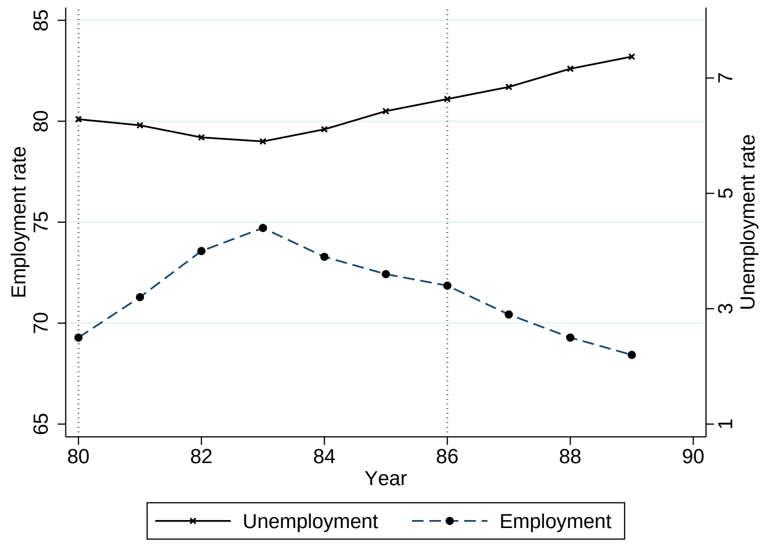
FIGURE A.1.
Spacing between first and second birth over time



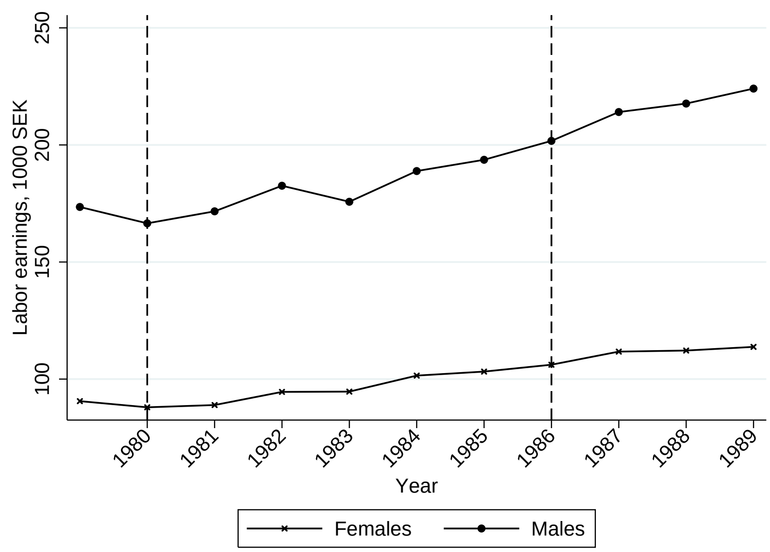
NOTE: The figure shows the average age difference - in months - between the first and second child, by second child birth cohort. The sample includes the full population of mothers whose first child was born 1970 or later.

FIGURE A.2.
Labor market conditions during the 1980s

(A)
Employment and Unemployment

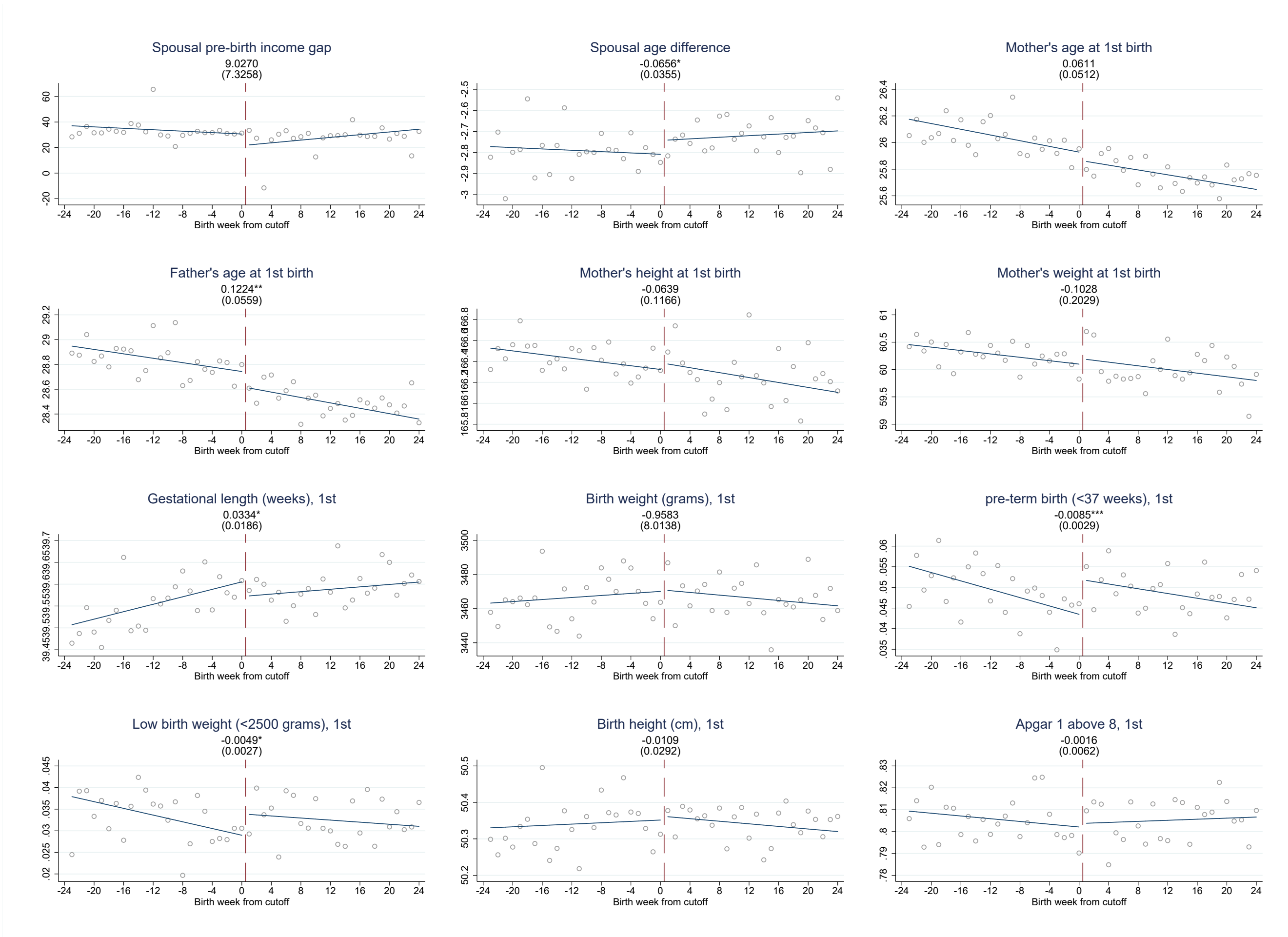


(B)
Labor Earnings



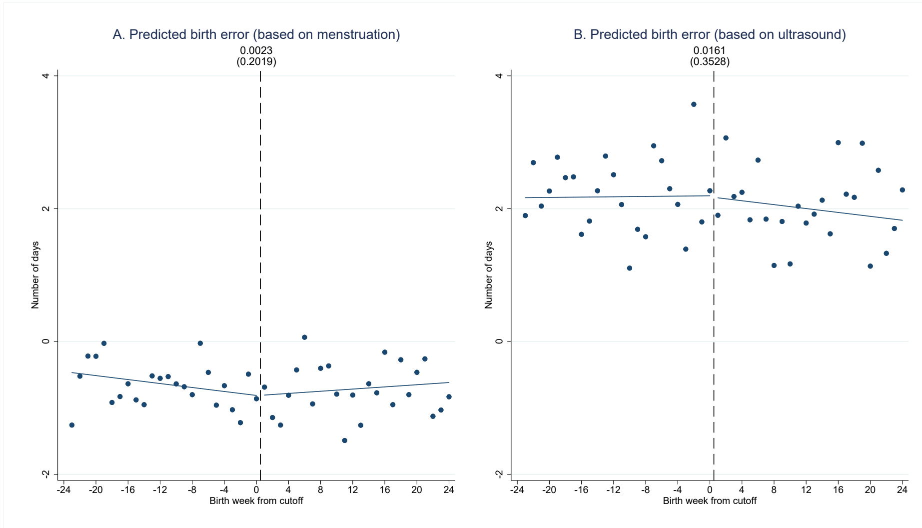
NOTE: The upper panel shows the trend in national employment- and unemployment rates over the time period studied, with figures from the Swedish Labor Force Survey's. The lower panel shows the average annual labor income of females and males (including zero-earners) based on our register data.

FIGURE A.3.
Covariate balance tests



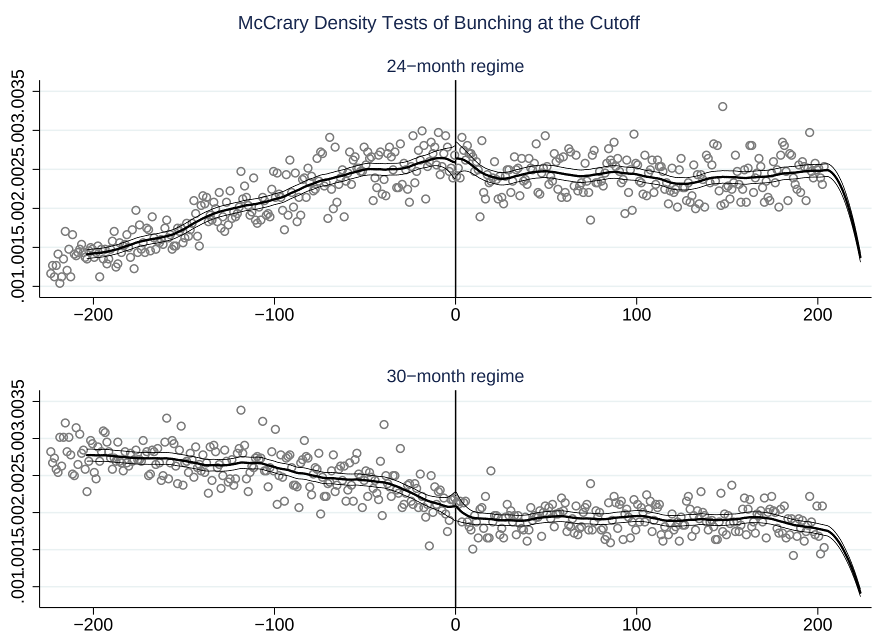
NOTE: Data is based on the sample described in Table 1. The circles show the mean of the outcome in each weekly bin of the running variable. Estimated discontinuities at the cutoff (estimates of β in equation (1) are indicated at the top of the vertical line at the cutoff in each graph. The estimates are based on a linear specification with triangular weights. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE A.4.
Strategic manipulation of birth timing: difference between expected and actual date of birth



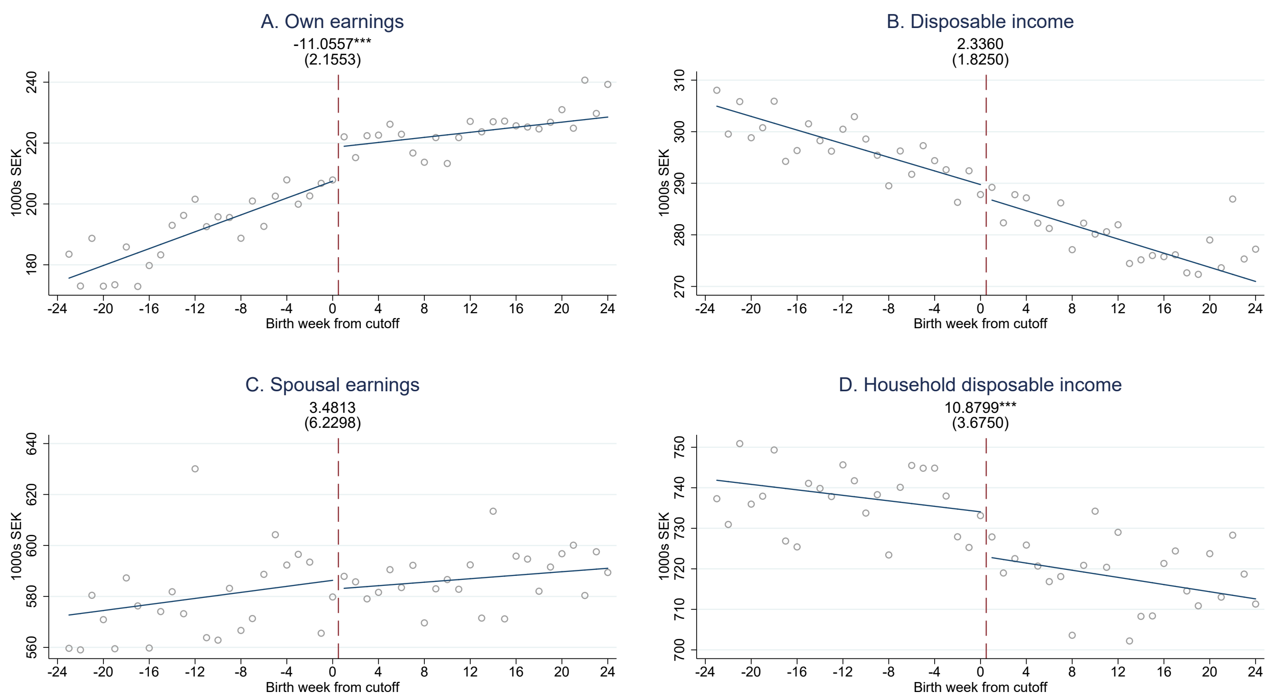
NOTE: Data is based on the sample described in Table 1. The circles show the mean of the outcome in each weekly bin of the running variable. Estimated discontinuities at the cutoff (estimates of β in equation (1) are indicated at the top of the vertical line at the cutoff in each graph. The estimates are based on a linear specification with triangular weights. Expected due date is based on the date of last menstruation in the left-hand side graph, and on an ultrasound scan in the right-hand side graph. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE A.5.
Strategic manipulation of birth timing: density of the running variable



NOTE: Each circle shows the average frequency of births in each daily bin of the running variable. The solid lines represent estimated density functions of birth spacing, with 95-percent confidence intervals. The bin size is 1 day and the bandwidth 20 days.

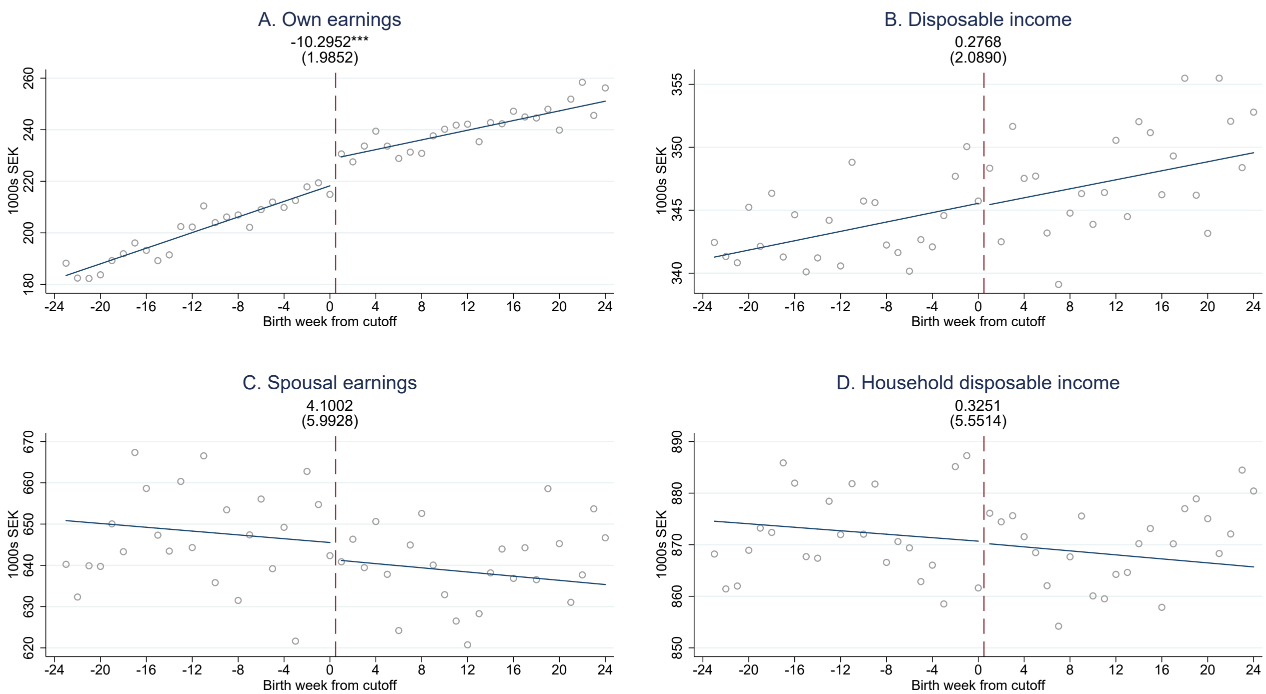
FIGURE A.6.
The effect of the SP on spousal labor earnings and household income: 24-month regime



NOTE.— Data is based on the sample of families whose second child was born under the 24-month regime (born 1980–1985). The labor earnings of mothers, fathers, and the disposable income measures are each pooled around the year of the second birth, that is, $\tau = \kappa - 1$ to $\tau = (\kappa + 1)$, where κ denotes the birth year. The figure reports the point estimate of β in equation (1) using a linear trend specification and triangular weights. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

FIGURE A.7.

The effect of the SP on spousal labor earnings and household income: 30-month regime



NOTE.— Data is based on the sample of families whose second child was born under the 30-month regime (born 1986–1989). The labor earnings of mothers, fathers, and the disposable income measures are each pooled around the year of the second birth, that is, $\tau = \kappa - 1$ to $\tau = (\kappa + 1)$, where κ denotes the birth year. The figure reports the point estimate of β in equation (1) using a linear trend specification and triangular weights. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.3.

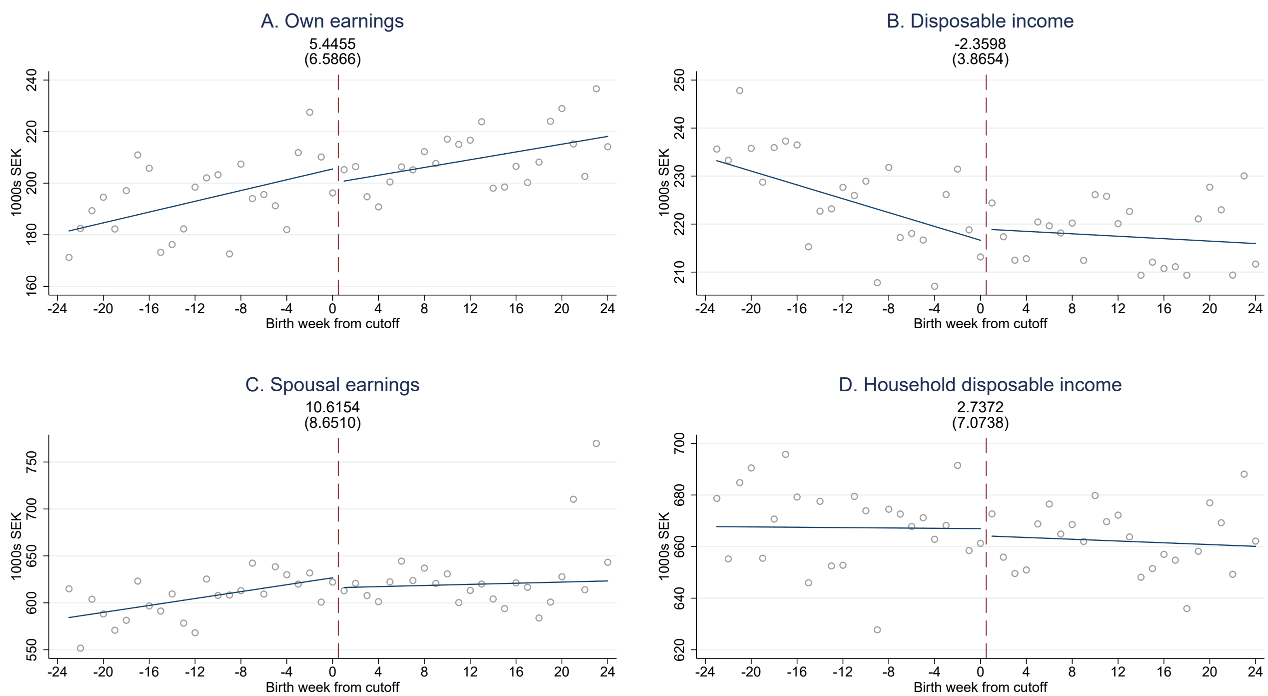
The effect of the SP on spousal labor earnings and household income: controlling for pre-determined characteristics

	(1) Mother's income	(2)	(3) Father's income	(4)	(5) HH disp. income	(6)
Years since birth, 2^{nd} birth	Pre-birth	Post birth	Pre-birth	Post birth	Pre-birth	Post birth
Treated ($\hat{\beta}$)	**-3.926*****	**-6.902*****	-0.249	1.041	-0.126	**5.833*****
	(0.690)	**(1.219)**	(1.324)	(2.950)	(0.805)	**(2.025)**
	(2.234)	(3.945)	(2.986)	(8.316)	(2.733)	(4.509)
Observations	70,932	70,607	71,001	70,791	71,250	71,250
Mean of outcome	99.12	129.1	198.7	411.4	250	535.3
$\hat{\beta}/Mean$	-0.0396	-0.0535	-0.0013	0.0025	-0.0005	0.0109

NOTE: Data is based on the sample described Table 1. The table reports the point estimate of β from equation (1) using a linear trend specification and triangular weights. Included covariates are the within-household earnings gap between the spouses (pre-first birth), the spousal age difference, and the age at first birth for both the mother and the father. The pre-birth outcome is measured in time period $\tau = (\kappa - 1)$, where κ denotes the calendar year of birth of the second child. The post-birth outcomes aggregates earnings over the time periods $\tau = \kappa$ and $\tau = (\kappa + 1)$. The coefficient estimates displayed in bold are significant at 10 percent level when inference is adjusted for multiple hypotheses testing. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p < 0.1$.

FIGURE A.8.

Placebo estimates of the effect of the SP on spousal labor earnings and household income



NOTE.— Data is based on the sample of families whose second child was born before the 24-month rule came into effect (born 1977–1979). The labor earnings of mothers, fathers, and the disposable income measures are each pooled around the year of the second birth, that is, $\tau = \kappa - 1$ to $\tau = (\kappa + 1)$, where κ denotes the birth year. The figure reports the point estimate of β in equation (1) using a linear trend specification and triangular weights. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.4.
Specification diagnostics: 24-month regime

Variable	Bins-test	Linear			Local linear		
		Point est.	SE	t-stat	Point est.	SE	t-stat
A. Parental outcomes							
Mother's pre-birth earnings	2	-4.4287	1.1855	-3.7356	-4.0307	0.9917	-4.0644
Mother's post-birth earnings	1	-6.6283	2.0274	-3.2694	-5.9967	1.6938	-3.5404
Father's pre-birth earnings	1	-0.0087	3.6631	-0.0024	0.0021	2.4034	0.0009
Father's post-birth earnings	1	2.9258	4.3003	0.6804	2.9832	5.4321	0.5492
HH pre-birth disposable inc.	1	-0.3098	1.5307	-0.2024	-0.4121	1.6103	-0.2559
HH post-birth disposable inc.	1	11.1898	2.7445	4.0771	11.4155	2.6774	4.2637
B. Child outcomes							
9^{th} grade GPA, first-born	1	0.0219	0.0168	1.3080	0.0232	0.0147	1.5782
College by age 24, first-born	1	0.0194	0.0090	2.1547	0.0203	0.0139	1.4604
9^{th} grade GPA, second-born	1	-0.0040	0.0165	-0.2410	-0.0029	0.0185	-0.1568
College by age 24, second-born	1	0.0038	0.0090	0.4185	0.0038	0.0100	0.3800

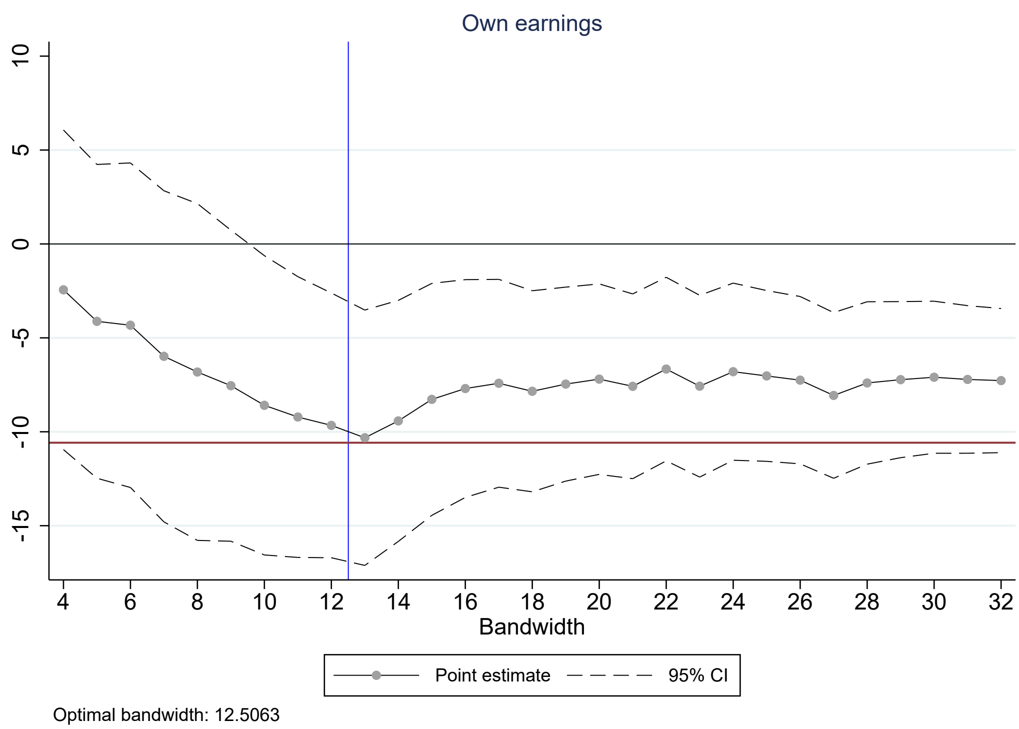
NOTE: The first column reports the polynomial choice from a bins test by estimating models with a full set of birth week dummies together with the different parametric trends (order 1 to order 4 polynomials), and where the recommended polynomial is the specification in which the set of bin dummies are jointly insignificant, for each outcome variable listed in each row. The remaining columns report point estimates, standard errors and t-statistics of the effect of the SP on the outcome variable listed in each row using global linear and a local linear estimation, respectively. The local linear estimates are based on a triangular kernel and uses a bandwidth of 24 weeks.

TABLE A.5.
Specification diagnostics: 30-month regime

Variable	Bins-test	Linear			Local linear		
		Point est.	SE	t-stat	Point est.	SE	t-stat
A. Parental outcomes							
Mother's pre-birth earnings	2	-3.6854	1.1241	-3.2787	-3.3294	1.0328	-3.2237
Mother's post-birth earnings	1	-6.5821	1.5345	-4.2895	-6.3895	1.3533	-4.7214
Father's pre-birth earnings	1	1.1692	2.0345	0.5747	1.2036	1.9386	0.6209
Father's post-birth earnings	1	2.8507	4.0832	0.6982	2.6744	4.7682	0.5608
HH pre-birth disposable inc.	1	0.1220	1.4119	0.0864	0.1425	1.8517	0.0770
HH post-birth disposable inc.	1	0.2031	2.8413	0.0715	0.2136	4.3351	0.0493
B. Child outcomes							
9^{th} grade GPA, first-born	1	-0.0023	0.0172	-0.1331	-0.0015	0.0206	-0.0728
College by age 24, first-born	1	0.0130	0.0098	1.3332	0.0135	0.0125	1.0800
9^{th} grade GPA, second-born	1	-0.0087	0.0175	-0.4937	-0.0110	0.0207	-0.5314
College by age 24, second-born	1	0.0029	0.0096	0.3017	0.0037	0.0100	0.3700

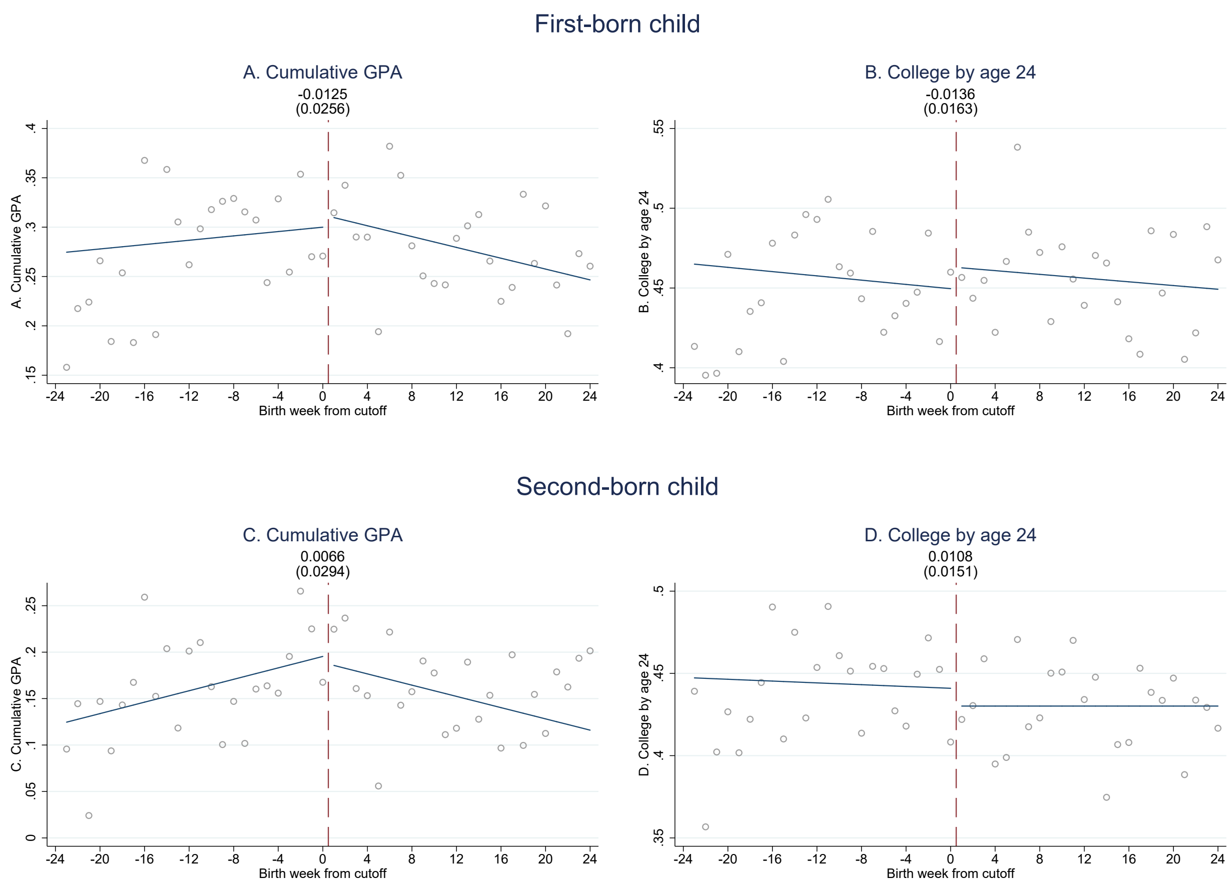
NOTE: The first column reports the polynomial choice from a bins test by estimating models with a full set of birth week dummies together with the different parametric trends (order 1 to order 4 polynomials), and where the recommended polynomial is the specification in which the set of bin dummies are jointly insignificant, for each outcome variable listed in each row. The remaining columns report point estimates, standard errors and t-statistics of the effect of the SP on the outcome variable listed in each row using global linear and a local linear estimation, respectively. The local linear estimates are based on a triangular kernel and uses a bandwidth of 24 weeks.

FIGURE A.9.
Local linear estimates of the effect of the SP on mothers' labor earnings: varying bandwidth



NOTE: Each point pertains to the local linear estimate of β in equation (1) with varying bandwidths. The solid red line represents the point estimate from a global linear specification with triangular weights. The vertical blue line shows the optimal bandwidth for the non-parametric estimates. 95 percent confidence intervals for the local linear estimates are indicated by the dashed lines.

FIGURE A.10.
Placebo estimates of the effect of the SP on children's educational outcomes



NOTE.— Data is based on the sample of families whose second child was born before the 24-month rule came into effect (born 1977–1979). The figure reports the point estimate of β in equation (1) using a linear trend specification and triangular weights. Clustered standard errors (at birth week) in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

TABLE A.6.
The effect of the SP on children's educational outcomes: pooled higher- and lower-order parity families

	(1)	(2)	(3)	(4)	(5)	(6)
	Lower birth order child			Higher birth order child		
	High school qualified	Standardized GPA	College at age 24	High school qualified	Standardized GPA	College at age 24
Treated ($\hat{\beta}$)	0.001	0.020**	**0.020****	0.003	-0.002	0.008
	(0.004)	(0.010)	**(0.008)**	(0.005)	(0.014)	(0.007)
Observations	56,964	84,759	84,098	71,315	84,515	83,832
Mean of outcome	0.939	0.214	0.446	0.930	0.121	0.409
T_SD	0.000743	0.0200	0.0203	0.00348	-0.00178	0.00825
$\beta/\hat{Mean}$	0.0008	0.0936	0.0455	0.0038	-0.0147	0.0202

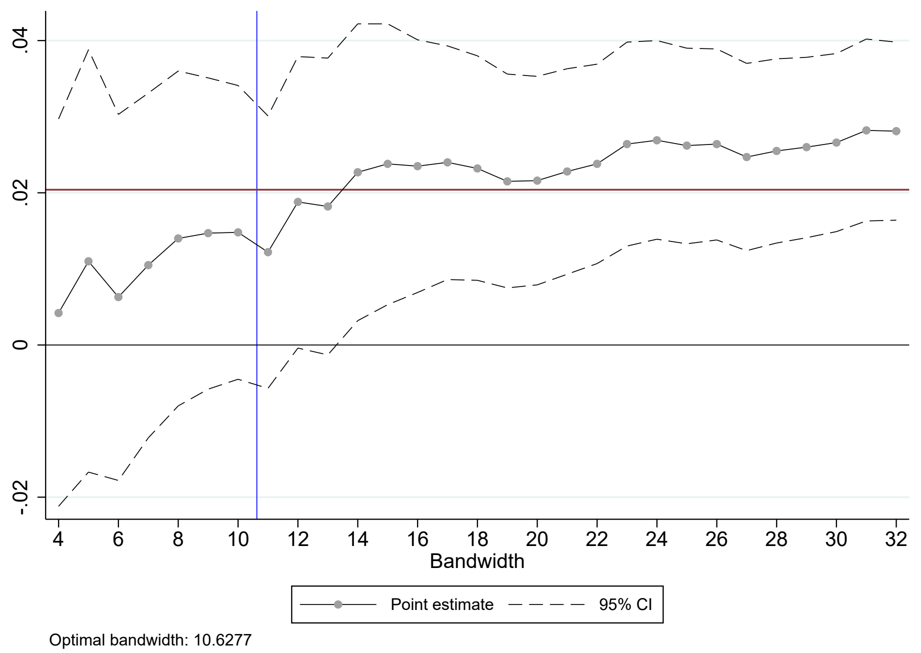
NOTE: Data is based on the sample described in Table 1 pooled with the corresponding sample of families with three children, whose third child was born in the relevant years. The table reports the point estimate of β from equation (1) using a linear trend specification with triangular weights. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p < 0.05$, * $p < 0.1$.

TABLE A.7.
The effect of the SP on children's educational outcomes: controlling for pre-determined characteristics

	(1)	(2)	(3)	(4)
	Lower birth order		Higher birth order	
	Standard. GPA	College at age 24	Standard. GPA	College at age 24
Treated ($\hat{\beta}$)	**0.022****	**0.020****	-0.001	0.008
	(0.009)	**(0.008)**	(0.013)	(0.006)
Observations	83,960	83,301	83,732	83,057
Mean of outcome	0.217	0.448	0.124	0.409
$\beta/\hat{Mean}$	0.0998	0.0457	-0.0112	0.0202

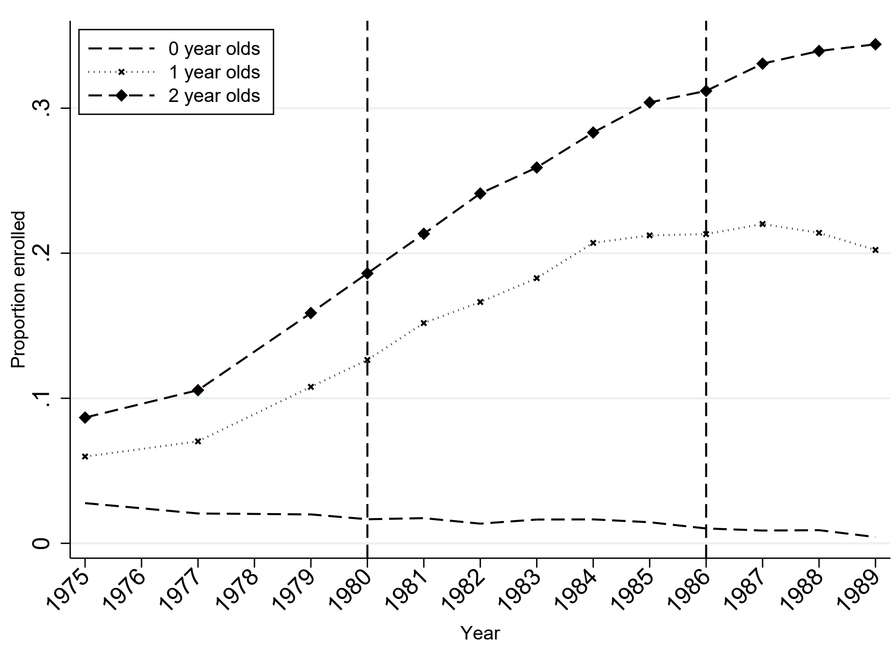
NOTE: Data is based on the sample described in Table 1 pooled with the corresponding sample of families with three children, whose third child was born in the relevant years. Included covariates are the within-household earnings gap between the spouses (pre-first birth), the spousal age difference, the age at first birth for both the mother and the father, and children's calendar-year and month of birth. The table reports the point estimate of β from equation (1) using a linear trend specification with triangular weights. The mean of the outcome variable is calculated on the control group. Clustered standard errors (at birth week level) in parentheses. *** p <0.01, ** $p <$ 0.05, * $p < 0.1$.

FIGURE A.11.
Local linear estimates of the effect of the SP on college attendance of the lower-birth order child: varying bandwidth



NOTE: Each point pertains to the local linear estimate of β in equation (1) with varying bandwidths. The solid red line represents the point estimate from a global linear specification with triangular weights. The vertical blue line shows the optimal bandwidth for the non-parametric estimates. 95 percent confidence intervals for the local linear estimates are indicated by the dashed lines.

FIGURE A.12.
Proportion children enrolled in child care by child age and year



SOURCE: Authors' illustration using data contained in the 1989 survey of child care modes in SCB (1989).
NOTE: The figure shows the fraction of children aged 0, 1, and 2 enrolled in child care (nationally) during the time period studied.