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Reducing the gender gap in parental leave through economic incentives?

Evidence from the gender equality bonus in Sweden

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Reducing the gender gap in parental leave through economic incentives?[a]

– Evidence from the gender equality bonus in Sweden

by

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Abstract

Using administrative data from Sweden, I study an internationally unique parental leave policy that rewarded parents with a financial bonus as a function of their division of paid parental leave. Results from a birthdate based regression discontinuity design show that the policy significantly reduced the absolute difference in days of paid leave between the parents. Since parents started earning bonus only after the exhaustion of the 60 reserved days for each parent, the response to the bonus was completely driven by the roughly 55 % of the couples who exhausted all reserved days. Within this group, the effect of the policy was particularly strong in the small group of parents where the father had the highest uptake, causing the effect on the mother-father difference in uptake to be insignificant. Labor market earnings and temporary parental leave (i.e., caring for the child when he/she is too sick to be in school/daycare center), which has been argued to be a good proxy for a parent's general childcare involvement beyond the first years after childbirth, were not significantly affected by the bonus. However, mothers who lowered (increased) their uptake of parental leave in response to the bonus policy displayed negative (positive) point estimates for temporary parental leave and positive (negative) point estimates for labor earnings. While a corresponding pattern for fathers could not be observed, for mothers, the results suggest a potentially important link between the length of the early parental leave and later allocation of time between home and market production.

Keywords: Parental leave, gender equality bonus, home production, regression discontinuity
JEL-codes: D13, J13, J16

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1 Introduction

A central question in modern labor markets, occupying many researchers and policymakers, is how to close the remaining, and persistent, gender gap in wages and income. There is growing consensus that family formation, rather than differences in human capital, is the main driver of the remaining gender gap (e.g., Adda, Dustmann and Stevens 2017; Angelov, Johansson and Lindahl 2016; Bertrand, Goldin and Katz 2010; Bütikofer, Jensen and Salvanes 2018; Ejrnæs and Kunze 2013; Kleven, Landais and Søgaard 2019), suggesting that measures to address the gap should be directed at equalizing and/or ameliorating the labor market consequences of starting a family. As Goldin (2014) points out, gender convergence likely involves a combination of increased paternal responsibility for home production and increased temporal flexibility in the workplace. While changes in the temporal flexibility of jobs presumably must occur organically and through negotiations between employers and employees, the role of fathers in the family might be affected by policies.

Accordingly, during the last decades, there have been government interventions in many countries aimed at increasing fathers' involvement in caring for newborn children as well as home production more generally and in the longer term. These interventions have mainly taken the form of earmarking days in the paid parental leave systems for the mother and father respectively (i.e., gender quotas or "daddy-month(s)").[1] With such policies in place paid parental leave days will be lost for the household if the father doesn't take up leave to care for the child, creating relatively strong incentives for the father to do just that. In line with the intentions of the reforms, evaluations of gender quotas have indeed consistently found substantial positive (negative) effects on fathers' (mothers') uptake of paid parental leave (e.g., *Germany*: Tamm 2019; *Iceland*: Olafsson and Steingrimsdottir 2020; *Norway*: Cools, Fiva and Kirkebøen 2015 and Rege and Solli 2013; *Quebec*: Patnaik 2019; *Spain*: Farré and González 2019; *Sweden*: Avdic and Karimi 2018 and Ekberg, Eriksson and Friebel 2013).

In this paper, I contribute with causal evidence from an internationally unique parental leave policy (the so-called gender equality bonus), introduced in Sweden in 2008, that incentivized a more equal division of paid parental leave without earmarking days. While daddy-months certainly have been effective in increasing the fathers' share of paid parental leave, gender quotas have also been criticized for being intrusive and potentially welfare-reducing because of the constrained choice-set that parents face (Ekberg, Eriksson and Friebel 2013). Investigating effects of policies aiming to achieve a more equal division without (further) reserving days is therefore important. The bonus policy I study, which was added to a system that already earmarked 60 days

[1] The list of countries that have introduced such policies include, e.g., Austria, Finland, France, Germany, Iceland, Norway, Spain and Sweden. The Canadian province Quebec also has reserved leave for fathers.

for each parent, rewarded parents with a financial bonus as a function of their division of the transferable days. The maximum bonus, rewarded to parents who took up 135 days each of the 270 transferable days, amounted to 13,500 SEK (≈1,220 EURO). Only parents of children born on or after July 1, 2008, were eligible for the bonus, making it possible to estimate causal effects using a regression discontinuity design.

I find that the policy significantly reduced the absolute difference in leave days between parents. Consistent with the bonus' construction, which required both parents to exhaust their 60 reserved days before any bonus payments could be earned, the response to the bonus was completely driven by the roughly 55 % of the couples who exhausted all reserved days. The average decrease in the absolute difference in this group was around 5 days which amounts to 3.3 % of the control mean. The reduction in the absolute difference in the uptake was particularly pronounced in the small group of parents where the father had the highest uptake. Since these parents faced opposite gender-specific incentives compared to the majority of couples, their strong response caused the overall effect of the bonus on the mother-father difference in uptake to be insignificant. Thus, even though the bonus led to a more equal division of paid parental leave at the household level, it did not affect aggregate differences in parental leave uptake between mothers and fathers.[2] Importantly, however, the results clearly show that parents reacted to the bonus despite its modest size, an active application requirement and slow feedback[3], suggesting that a bonus-system, on its own or combined with gender quotas, can be a potent policy instrument for affecting the division of paid parental leave.

Second, I contribute to the literature on the link between division of early parental leave and long-term division of home and market production by studying if parents who changed their uptake of paid parental leave because of the bonus in turn adjusted their more long-term childcare involvement. While the division of the early parental leave for newborn children is important and arguably holds an intrinsic value, a central idea behind the gender quotas and the gender equality bonus has been that a more equal division of paid parental leave during the first years after childbirth might affect the long-term division of home and market production through reduced gender specialization. For mothers, the arrival of the first child has been found to induce long-term reductions in labor supply (Angelov, Johansson and Lindahl 2016), and time-use studies consistently show that mothers devote much more time than fathers to housework and childcare (OECD 2010; Patnaik 2019). Mothers' extensive childcare responsibilities, in combination with

[2] In a previous short-term evaluation of the reform, Duvander and Johansson (2012) looked separately at the uptake of mothers and fathers through the child's second birthday and found insignificant effects. This finding was widely cited when the gender equality bonus was abolished and replaced by an additional daddy-month in 2016/2017.

[3] From January 1, 2012, the bonus was paid out automatically shortly after the generation of the bonus-days. Before that, parents needed to actively claim the bonus which was given in the form of a tax credit in the year after it was generated. Details are provided in section 2.2.

growing expectations of high maternal labor supply, have even been linked to higher rates of sickness absence (Angelov, Johansson and Lindahl 2020). Consequently, if parental leave policies aimed at achieving a more equal division of early parental leave can affect parents' long run allocation of resources to home and market production, they have potential to be crucial policy instruments for gender equality.

The causal evidence on this issue is, however, inconclusive. First, with respect to the division of housework and childcare duties, Kotsadam and Finseraas (2011), Patnaik (2019) and Tamm (2019) report results in line with the hypothesis that reserved parental leave for fathers induces fathers to take greater responsibility at home also in the longer run. By contrast, several other studies that also use plausibly valid identification strategies fail to find such effects (Ekberg, Eriksson and Friebel 2013[4]; Kluve and Tamm 2013; Rieck 2012). Second, papers examining the causal effect of daddy-months on mothers' and fathers' labor market outcomes have reached different conclusions. Ekberg, Eriksson and Friebel (2013), studying daddy-months in Sweden, do not find any effects on long-term wages and employment. In Norway, Rege and Solli (2013) found negative effects on fathers' earnings but no effects on mothers' earnings. Cools, Fiva and Kirkebøen (2015), also studying the Norwegian daddy-month, found negative but insignificant effects on fathers' earnings. In the Canadian province Quebec, Patnaik (2019), using time-use data, find that mothers exposed to a daddy-month reform worked more in the years following the initial leave period. There are also indications that fathers worked less but these estimates are insignificant. Finally, Tamm (2019), using data from Germany, does not find long-term effects of paternity leave on fathers' labor market outcomes.

Taken together, my interpretation of the findings above is that the division of early parental leave typically does affect long-run dynamics in the household, but that rich data is needed to detect the effects.[5] It should also be noted that virtually all causal evaluations of parental leave reforms are performed on the first cohort affected by the policy which leaves open the possibility of growing effects over time (see e.g. Dahl, Løken and Mogstad 2014).

I study this question using register data on labor earnings and paid *temporary* parental leave (TPL)[6] taken at a later stage to care for sick children that cannot be in daycare or in school. Eriksson and Nermo (2010) have shown that TPL is a good proxy for long-term childcare

[4] The finding in Ekberg, Eriksson and Friebel (2013) has, however, been questioned in Duvander and Johansson (2019) who claim that mothers who decreased their uptake of early parental leave because of the daddy-month subsequently took less leave to care for sick children.

[5] The fact that daddy-months have been found to affect marital stability and fertility corroborates this notion (Avdic and Karimi 2018; Farré and González 2019; Olafsson and Steingrimsdottir 2020).

[6] Paid TPL, which is part of the parental insurance system, becomes relevant when the initial, longer, parental leave period is over, and children have started attending formal childcare and both parents are back at work. If parents forego labor earnings because they need to be home caring for children who are too sick to be at the daycare center or in school, they can claim TPL benefits. Almost 80 % of the foregone earnings are replaced by the benefits and parent couples can claim a maximum of 120 days per child every year until the child turns 12 years old.

involvement. While I find insignificant effects of the bonus on these outcomes, mothers who lowered (increased) their uptake of parental leave in response to the bonus policy displayed negative (positive) point estimates for TPL and positive (negative) point estimates for labor earnings. For fathers, the pattern is less clear. Taken together, the results suggest, at least for mothers, a potentially important link between the length of the early parental leave and later allocation of time between home and market production.

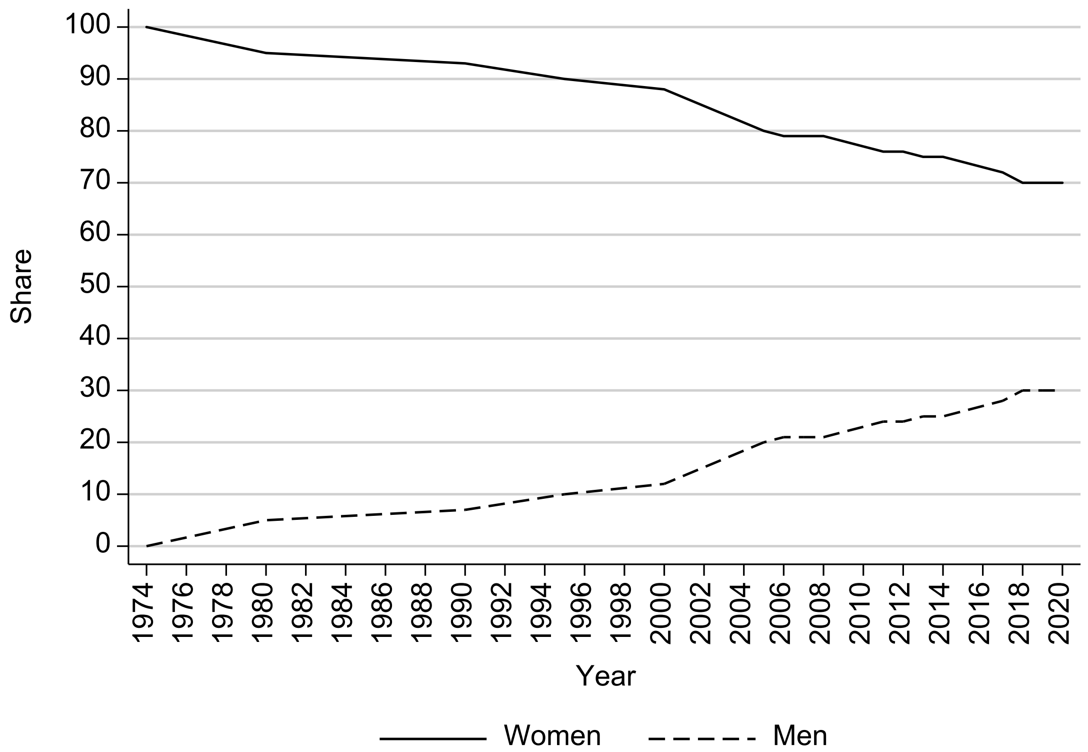
The rest of the paper is organized as follows: In section 2, I describe the institutional context and the details of the gender equality bonus. Section 3 contains a description of the data, while section 4 is concerned with the empirical strategy. Main results and heterogeneity analyses follow in section 5. Section 6 concludes.

2 Parental leave in Sweden and the gender equality bonus

2.1 Parental leave in Sweden: Background and description

The modern system of paid parental leave in Sweden was introduced in 1974. Mothers then took virtually all leave, but fathers' share of the paid leave has gradually increased over time reaching 30 % in 2020 (see Figure 1). In 1974, parents could in total take out 6 months of paid leave, and while each parent was entitled to three months of paid leave, the days were completely transferable (Swedish Social Insurance Agency (SSIA) 2011). During the first 15 years after introduction the number of paid parental leave days granted to parents of newborn children increased rapidly. Parents of children born in 1989 had access to 450 paid parental leave days (all still transferable). In 1995, one month was reserved for each parent, reducing the number of transferable days to 390. Further changes were made in 2002, when one additional month was reserved for each parent while the total number of paid days increased to 480, which is also the current level. The gender equality bonus, described in detail in subsection 2.2, was introduced in 2008 and later abolished and replaced by an additional third reserved month in 2016/2017.

Figure 1 Division of paid parental leave between men and women in Sweden, 1974–2020



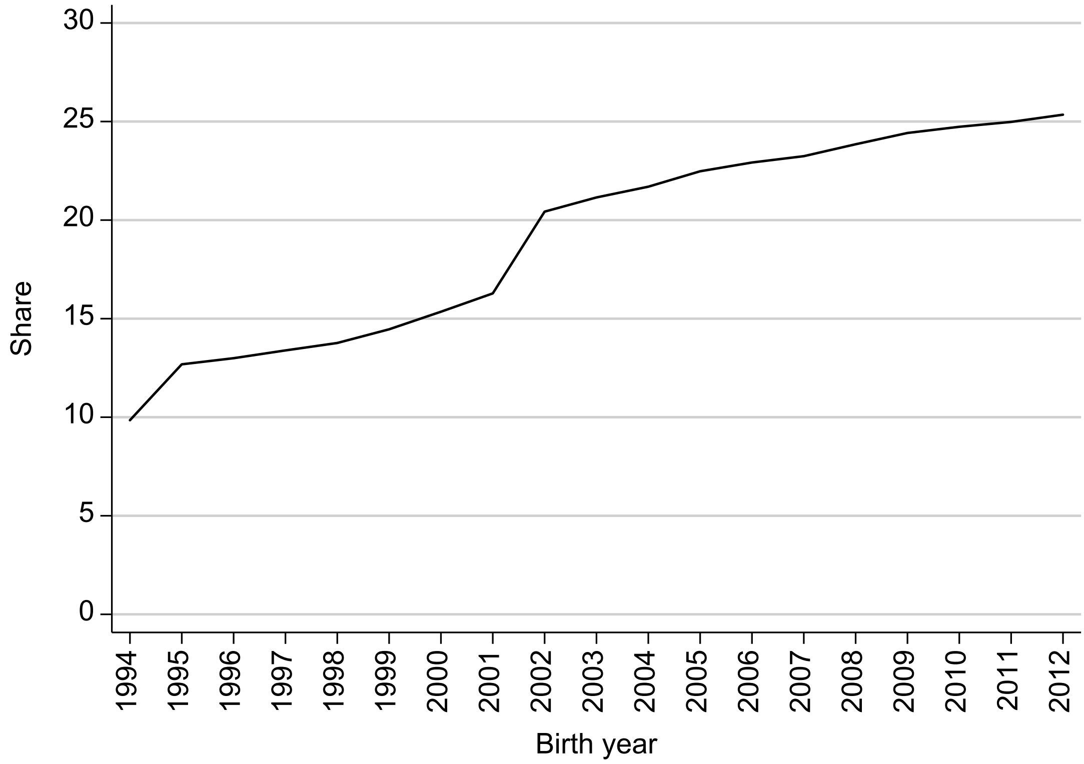
Note: The figure shows mothers' and fathers' share of all paid parental leave that was taken in a given year regardless of the age of the children. The numbers are from the Swedish Social Insurance Agency.

Note that the above description is concerned with *paid* parental leave. When the child is younger than 18 months, parents also have the right to be on job-protected *unpaid* parental leave (i.e. they can freely choose whether to take up benefits or not), and this practice is common. SSIA (2013) estimates that mothers (fathers) take 15.3 (3.8) months of parental leave (paid + unpaid) during the child's first two years, and that the share of paid leave is roughly 60 % for both parents.

Figure 2 below shows fathers' share of accumulated uptake of paid parental leave days through the child's 8th birthday (which marks the end of the paid parental leave eligibility period)[7] by birth year for children born 1994–2012. Effects of the reserved months, the first in 1995 and the second in 2002, are clearly visible in the figure while any impact of the gender equality bonus, introduced on 1 July 2008, is hard to discern using this measure. It is also interesting to note that fathers' share of the uptake trended upwards also between and after the daddy-month reforms, suggesting, but not confirming, dynamic effects of parental leave reforms.

[7] For children born from 2014, days can be used until the child's 12th birthday.

Figure 2 Fathers' share of finalized paid parental leave by birth year, 1994–2012



Note: The figure shows fathers' share of accumulated uptake of paid parental leave days through the child's 8th birthday (which marks the end of the paid parental leave eligibility period).

2.2 The gender equality bonus introduced July 1, 2008

As described above, parents of children born in 2008 could use 480 days of paid parental leave before the child's 8th birthday.[8] 390 days had a benefit level amounting to 80 % of pre-leave labor income up to a cap[9], while the benefit level of the remaining 90 days was set to 180 SEK for everyone. The two types of days are often referred to as qualifying income days (QI-days) and flat-rate days (FR-days). All FR-days could be transferred between the parents, while 120 QI-days were earmarked (60 for each parent).

The gender equality bonus, which applied to children born on or after July 1, 2008, rewarded parents with additional benefits as a function of their division of the 270 transferable QI-days.[10] A more equal division gave more so-called bonus-days with associated benefits that the parents

[8] The description here applies to singleton births.

[9] In 2008, the cap was set at 872 SEK per day. Following annual adjustments, the cap reached 942 SEK per day in 2016. For parents who have very low or zero pre-leave labor income the benefit is set at a fixed low level. In 2008, this benefit level was 180 SEK per day. It was later increased to 225 SEK in 2013 and further to 250 SEK in 2016. Parents might also be entitled to supplementary parental benefits paid by the employer. Thus, the effective replacement rate might be higher than 80 % for some parents.

[10] The center-right coalition pledged to introduce the gender equality bonus during the 2006 general election campaign in which they were victorious. A draft of the legislation was presented in the fall of 2007 and the final proposition was brought to parliament in March of 2008. The parliament approved the legislation in May, 2008, and the law came into effect on July 1, 2008.

could collect. Bonus-days were generated when the uptake of QI-days for the parent with the lowest uptake of QI-days exceeded 60 days (i.e. both parents needed to take up more than 60 QI-days to earn the family's first bonus-day). The formula for the calculation of bonus-days was given by Equation (1):

$$max[0, min(mother's\ uptake, father's\ uptake) - 60] \quad (1)$$

Since the maximum value of the lowest uptake of QI-days was 195, achieved when parents took up 195 days each, the maximum number of bonus-days was 135 (195-60). The benefit level of a bonus-day was 100 SEK (tax-free) for almost all parents, meaning that a couple could increase their parental leave income with 13,500 SEK if they took full advantage of the bonus.

Collection of benefits associated with bonus-days generated before 2012 required an active application to the SSIA. Before 2012, the SSIA calculated parents' bonus-days on a calendar year basis. In early February 2009–2012, the SSIA informed parents about how many bonus-days they had earned during the preceding calendar year. Only the parent with the highest uptake of QI-days at the end of the preceding calendar year was entitled to the bonus, and collection of the bonus required applying to the SSIA before March 1.[11] In the application, the parent with the highest uptake of QI-days needed to provide evidence that he or she had worked or studied when the other parent took out leave and generated the bonus-days. The benefit level of the bonus-days was a function of labor income and/or study benefits up to a cap of 100 SEK per day, which was binding for almost all parents who worked or studied.[12] After approval of the application, the SSIA informed the Tax Authorities which gave the bonus in form of a tax credit. Thus, it was a relatively cumbersome process to collect the bonus and the parents first saw the benefits of their behavior long after the generation of the bonus-days.

Recognizing the administrative burden of parents, and responding to an early evaluation of the bonus that showed insignificant effects on mothers' and fathers' uptake of QI-days after 18 months (SSIA 2010), the government made adjustment in the gender equality bonus law that took effect from January 1, 2012. The adjustments applied to all children born on or after July 1, 2008 (i.e. not only to children born on or after January 1, 2012). Collection of benefits associated with bonus-days generated from January 1, 2012, no longer required an active application. Instead, the

[11] Consider the following example: The mother and father use 90 and 70 QI-days respectively in the first calendar year. The mother has the highest uptake and is therefore eligible for the bonus. The SSIA informs the mother that she can collect benefits from 10 (70-60) bonus-days. After the second calendar year accumulated uptake of QI-days is 120 for the mother and 150 for the father. The father now has the highest uptake and is therefore eligible for the bonus. The SSIA calculates the number of bonus-days that the father can collect benefits from by subtracting the 60 reserved days and the 10 bonus-days from the first calendar year from the mother's uptake, i.e. 120-60-10=50.

[12] Parents who had a qualifying income of at least 6,000 SEK per month were rewarded the full bonus of 100 SEK.

SSIA automatically made a bonus payment shortly after the generation of the bonus-days (typically the next month). The work and study requirement was also abolished, and the benefit level was set to 100 SEK per bonus-day for everyone. An additional change was that both parents now were entitled to the bonus, so that a bonus-day rewarded parents with 50 SEK each. Arguably, the changes made the bonus system more accessible and salient for parents. The gender equality bonus was later abolished, and no bonus-days could be generated after December 31, 2016.

In summary, parents of children born just after July 1, 2008, were eligible for the bonus throughout the entire parental leave eligibility period which ended when the child turned 8. During the first three and a half years after the birth the parents were subject to the original, more complicated, version of the bonus after which the adjusted version of the bonus came into effect. For these parents, the uptake of QI-days that took place before 2012 amounted to 93 and 69 percent of the total uptake of QI-days for mothers and fathers respectively. Thus, the evaluation in the current paper, which builds on the July 1 cutoff for identification, should primarily be interpreted as an evaluation of the original bonus system.

2.3 Theoretical considerations when estimating effects of the bonus

As briefly noted in the introduction and shown in Table 1 below, fathers have higher uptake of QI-days than mothers in about 8 % of the couples in the sample. Consequently, this small minority of parent couples faces opposite gender-specific incentives compared to the majority of couples. While all parents are incentivized by the bonus to increase the QI-uptake of the person with the lowest QI-uptake, the gender of this person varies between the two groups of parent couples just mentioned above. Thus, outcomes such as the mother's uptake, the father's uptake and the mother-father difference in uptake are subject to opposing forces, inevitably leading to an underestimation of the response to the bonus (as in Duvander and Johansson 2012). While these gender-specific uptake measures are interesting in terms of examining the reform's effect on aggregate gender differences in uptake, they are not necessarily informative about the full responses to the reform. I, instead, suggest using the absolute difference in uptake of QI-days between the parents as the main outcome as this measure captures the full response to the bonus.

Another important aspect is that marginal increases in the lowest QI-uptake only generated bonus-days if the lowest QI-uptake was at least 60 days. The lowest QI-uptake was above 60 days in about 55 % of the cases (see Table 1 below), meaning that almost half of the couples never reached the point where marginal increases in the lowest QI-uptake generated bonus-days. While effects of the gender equality bonus in this group cannot be ruled out (maybe they intended to reach 60 days but never made it), it is arguably likely that the bonus mainly affected parent couples who at some point reached the 60-day limit. It is therefore highly relevant to divide the sample by the 60-day limit and focus on parents who reached this limit. There are no indications that the bonus affected the probability of reaching the 60-day limit suggesting that splitting the data by this limit is unproblematic.

3 Data

I combine administrative registers from Statistics Sweden and the SSIA linked by pseudonymized personal identifiers to construct the analysis dataset. The key registers from Statistics Sweden are a population event register (*Historiska folkbokföringsregistret*) containing, among other things, exact dates of all births, deaths, immigrations and emigrations in Sweden since 1969, and the multigenerational register (*Flergenerationsregistret*) where parent-child relations can be observed. The focus is on parents of children born in Sweden in 2008, but I also use data from adjacent years for descriptions and specification tests. From Statistics Sweden, I also source annual individual-level information on, e.g., education, income sources and civil status available in the *Louise*-registers which cover all individuals aged 16–74 who reside in Sweden on December 31 the specific year. To this data I add information on the parents' uptake of paid parental leave days from registers compiled by the SSIA. The parental leave data cover all leave spells up to and including 2020, allowing me to observe how many days the parents have used when the child turns 8 which marks the end of the paid parental leave eligibility period.

I make several sample restrictions to achieve a relevant analysis dataset. First, the identities of both parents must be known. Second, the analysis is restricted to singleton births since special rules apply in the case of a multiple birth. Third, children who die or emigrate at some point during the paid parental leave eligibility period are not included in the analysis. Finally, I require that both parents are observed in the *Louise*-registers each year during an interval ranging from one year before the birth to eight years after the birth. This restriction ensures that the evaluation of the gender equality bonus largely is focused on working-age parents who reside in Sweden throughout the study period. In addition, the restriction makes it possible to add potential outcome variables from the *Louise*-registers for all parents. Taken together, the sample restrictions reduce the original sample of children by roughly 11 %.

Table 1 describes characteristics of the main sample, i.e. children born in 2008, divided by eligibility for the gender equality bonus. We can note that parents of children born after July 1 are significantly different from parents of children born before July 1, both in terms of predetermined characteristics and in terms of outcomes related to parental leave uptake. Parents on the bonus-side of the cutoff are younger, have lower pre-birth schooling and income, and are less likely to be born in Sweden. Thus, they tend to be socioeconomically weaker than parents of children born in the first half of 2008.[13] With respect to uptake of QI-days, in the lower part of the table, patterns are consistent with an effect of the gender equality bonus, with lower (higher) uptake for the mother (father) on the bonus-side, but causal claims are clearly premature given the imbalances in the predetermined characteristics. Note that both parents take up more than 60 QI-days in about 55 % of the couples, meaning that 55 % of couples on the bonus-side of the cutoff were eligible for at least some bonus benefits (although we don't know if they collected the benefits).

It is very uncommon to have a completely equal uptake of QI-days, but it is interesting to note that the father has the highest uptake in about 8 % of the cases. Consequently, there are parent couples in which mothers, because of the bonus, have incentives to increase their QI-uptake at the expense of fathers' uptake. The opposite incentive structure in this small minority of parents needs to be taken into account in the empirical analysis to capture the full response to the bonus. Interestingly, mothers in couples in which the father has the highest uptake have significantly lower pre-birth income than other mothers while fathers in these couples have significantly higher pre-birth income than other fathers. Thus, the difference in the benefit level for the QI-days between the father and the mother is particularly large in these couples which could be a possible explanation for the unusual uptake pattern we observe. [14]

[13] That parents of children born in the second half of the year are socioeconomically weaker than parents of children born in the first half of the year is true for most, if not all, years in Sweden.

[14] Mothers in couples in which the father has the highest uptake are also significantly less likely to be born in Sweden than other mothers while fathers in these couples, if anything, are slightly more likely to be born in Sweden than other fathers. This observation could also contribute to the uptake pattern we see in these couples since parents born in Sweden generally take up more days of paid parental leave (ISF 2013).

Table 1 Descriptive statistics for the main sample

Column:	**(1)**	**(2)**
Children:	**Born < July 1**	**Born ≥ July 1**
Covariates:		
Age at birth (mother)***	31.08	30.60
Age at birth (father)***	33.93	33.45
Years of schooling in t-1 (mother)***	13.05	13.00
Years of schooling in t-1 (father)***	12.60	12.55
Mother born in Sweden***	0.815	0.806
Father born in Sweden**	0.813	0.807
Mother's income in t-1 (1,000 2018 SEK)***	200.68	189.09
Father's income in t-1 (1,000 2018 SEK)***	318.64	305.81
First-born child (mother)***	0.439	0.458
First-born child (father)***	0.438	0.456
Married***	0.432	0.403
Outcomes:		
Mother's uptake of QI-days***	283.30	281.29
Father's uptake of QI-days***	90.16	92.26
Mother-father QI difference***	193.14	189.02
Absolute QI difference***	209.47	205.39
Lowest QI-uptake > 60	0.545	0.549
Mother's QI-uptake > Father's QI-uptake***	0.920	0.913
Mother's QI-uptake = Father's QI-uptake***	0.004	0.005
Mother's QI-uptake < Father's QI-uptake***	0.076	0.081
Number of children	50,405	46,813

Note: The statistics are based on parents of children born in 2008. Columns 1–2 give the mean values of the indicated variables for parents of children born in the first and second half of 2008 respectively. */**/*** indicate that the mean values in columns 1–2 are significantly different from each other at the 10/5/1 percent level.

4 Empirical strategy

Resting on the assumption of randomness of births around July 1, 2008, I estimate the causal effect of the gender equality bonus on different measures of uptake of QI-days using a regression discontinuity design. In the empirical model, outlined in Equation (2) below, I regress the outcome for child i on the child's normalized birth date (July 1 is 0, July 2 is 1 and so on) and a dummy for being exposed to the gender equality bonus. The linear relation between the outcome and the child's normalized birth date is allowed to be different on the two sides of the cutoff.

$$Y_i = \beta_0 + \beta_1 Bonus_i + \beta_2 BD_i + \beta_3 (Bonus_i * BD_i) + \varepsilon_i \qquad (2)$$

The parameter of interest is β_1 which captures potential discontinuities in the linear relation between the outcomes and the child's normalized birth date at the July 1 cutoff. In the main specification, I use a uniform kernel and a 90 day bandwidth. This specification generally gives more sensible slopes and more conservative estimates than alternative specifications with smaller bandwidth and/or triangular kernel. Results from a wide range of different specifications are presented in Appendix A. Following Kolesár and Rothe (2018), I use robust standard errors.

4.1 Was the timing of births around July 1, 2008, random?

Eligibility for the gender equality bonus offered a possibility for parents to increase their income from parental leave benefits. Thus, all parents, to some extent, had incentives to strategically time births to the bonus-side of the July 1 cutoff. Incentives to strategically time births were particularly strong for parents who, regardless of eligibility for the bonus, planned to divide their QI-days equally. Strategic timing of births around the cutoff, which would compromise the validity of the regression discontinuity design, is therefore a potential concern and must be carefully investigated. Following convention in the RD-literature, I check for manipulation of the running variable (i.e. the child's birth date) by examining the evolution of births and predetermined parental characteristics across the cutoff (results are presented in Appendix B).

In panel A of Figure B1, I show the number of births per day (in my analysis sample) during the period June–July 2008. There are clearly more births on July 1 than on June 30, but there are many "jumps" between days in the relatively sluggish histogram, making it hard to differentiate between manipulation and normal variation. The McCrary density test (McCrary 2008), which was constructed for smoothly behaving continuous running variables, reports a significant increase in the number of births at the cutoff, but the test's usefulness in this context can be questioned. Using another test designed for discrete running variables, which puts the difference between the values exactly at the cutoff in relation to differences between other adjacent values of the running variable, instead delivers a p-value of 0.213 (Frandsen 2017). The evolution of births per 7-day period around the cutoff (panel B of Figure B1) is also not indicative of strategic timing of births.

Another way of testing the fundamental assumption of randomness of births around the July 1 cutoff is to apply Equation (2) to predetermined parental characteristics. In Table B1, I present estimates of β_1 from such estimations. I look for potential discontinuities in the mothers' and fathers' age, schooling, income and likelihood of being Swedish-born. Reassuringly, all estimates in Table B1 are insignificant, suggesting that parents who planned to divide their QI-days equally did not manipulate births to take place on the bonus-side of the cutoff. To test this even more directly, I have also predicted the absolute difference in the uptake of QI-days using the characteristics listed above. To do so, I first regressed the absolute difference in the uptake of QI-days on all parental characteristics using children born in 2007 and then used the estimation results to predict the absolute difference in the uptake of QI-days for parents of children born in 2008.

Estimating Equation (2) on this predicted absolute difference in the uptake of QI-days yields a positive estimate of β_1 that is small and insignificant. Thus, there are no indications that parents with characteristics predictive of an equal division of QI-days (i.e. small absolute difference in uptake) manipulated births to take place on the bonus-side of the cutoff.

In summary, we cannot reject the null hypothesis of no discontinuities in predetermined parental characteristics at the cutoff which supports the validity of the regression discontinuity design.

5 Results

5.1 Main results

Estimates of β_1 from applying Equation (2) to the absolute difference in the uptake of QI-days between the parents are presented in Table 2. As discussed in section 2.3, marginal increases in the lowest QI-uptake only generated bonus if the lowest QI-uptake was at least 60 days. Thus, I split the data by the 60-day limit and perform separate analyses for parent couples above and below this limit. The bonus did not increase the probability of reaching the 60-day limit (it only affected the probability of reaching higher levels), suggesting that it is unproblematic to split the data in this way (see estimates in Figure A1). As expected, we note that there are no significant effects among parent couples below the 60-days threshold (panel B). For parents above the threshold, however, we observe a substantial negative effect on the absolute difference in the uptake of QI-days (panel A).[15] The negative estimate of 5 days is significant at the 1 % level and amounts to a bit more than 3 % of the control mean of the absolute difference.[16] The data underlying this estimate are visualized in Figure A2. In the remainder of the paper, I will solely focus on parent couples above the 60-days threshold.

When the same exercise is repeated for children born in 2007 and 2009, the resulting estimates are small and insignificant (see columns 3–4 in Table A2). The fact that we cannot reject the null hypothesis of no discontinuity in the absolute difference at the July 1 cutoff in 2007 and 2009 strengthens the notion that the significant 2008-estimate reflects a genuine response to the gender equality bonus. Reassuringly, we also find an insignificant effect on the predicted absolute difference in the uptake of QI-days (see column 5 in Table A2).

To check the robustness of the results, I have also estimated effects on the absolute difference in the uptake using a difference-in-differences (DD) model. In this specification, I use data on

[15] Average effects for the overall sample are presented in Table A1. The effect on the absolute difference is significantly negative also in this sample.

[16] Estimating the RD-model with a triangular kernel instead of a uniform kernel gives a somewhat more negative estimate (see column 2 of Table A2).

children born in 2007–2009 and investigate if the difference before-after July 1 in 2008 is different from the corresponding difference in the adjacent years. The result in column 6 of Table A2 confirms the significantly negative estimate from the baseline RD-model. The DD-estimate is, however, markedly smaller.

The negative effect on the absolute difference in panel A of Table 2 is further robust to substantial variations in the bandwidth (see Figure A3). If anything, the estimate becomes more negative for bandwidths that are shorter than the baseline of 90 days, but confidence intervals are then also relatively wide. To address any remaining concern about strategic timing of births around the cutoff, I have also estimated the model on a sample that excludes children born in the days just before and just after July 1, 2008 (i.e. a so-called “donut-RD”). The results are not sensitive to such an exclusion (see Figure A4).

Can the negative effect on the absolute difference in panel A of Table 2 be driven by multi-child parent couples equalizing the uptake of QI-days for bonus children while widening the difference in the uptake for pre-bonus children? For example, parents of children born just after July 1, 2008, who had children before might decrease the absolute difference in the uptake for the bonus child and increase it for the older children (if they have remaining days to make adjustments). Similarly, parents of children born just before July 1, 2008, who have additional children later on might increase the absolute difference in the uptake for the pre-bonus child and decrease it for younger children. Such reshuffling of days between children could lead to the negative effect seen in panel A of Table 2. I address this issue in Table A3. While the negative effect on the absolute difference in our baseline 2008 sample indeed is driven by multi-child parent couples (who constitute about 75 % of the sample), the results are inconclusive with respect to effects on the absolute difference in the uptake of QI-days for siblings. In the baseline specification with a uniform kernel, there is a positive and marginally significant effect on the absolute difference for siblings giving some support to the reshuffling mechanism. However, the effect on the uptake for siblings virtually disappears when a triangular kernel is used suggesting that the existence of a “reshuffling-effect” is highly uncertain. Taken together, while reshuffling of days between children cannot be ruled out as one mechanism behind the negative effect on the absolute difference in our baseline 2008 sample, it does not appear to be the main driver.

Table 2 Main results

Column:	(1)	(2)	(3)	(4)
Uptake measure:	Absolute difference	Mother-father difference	Mother	Father
A. Lowest uptake > 60				
Effect of bonus	-5.01***	-0.87	0.05	0.93
	(1.91)	(2.56)	(1.33)	(1.29)
Control mean	153.01	133.03	258.24	125.20
Observations	27,589	27,589	27,589	27,589
B. Lowest uptake < 61				
Effect of bonus	0.41	2.35	1.35	-1.00
	(1.39)	(2.58)	(1.74)	(1.20)
Control mean	277.94	266.02	312.88	46.86
Observations	22,871	22,871	22,871	22,871

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth.

In panel A of Table 2, we also see a negative tendency for the mother-father difference, but the estimate is much smaller and insignificant underscoring the importance of considering the absolute difference in QI-uptake between the parents to capture the full response to the bonus. The difference between the highly significant effect on the absolute difference and the small and insignificant effects on the other uptake outcomes is, as previously discussed, caused by the fact that the other outcomes are subject to opposing forces. While most mothers (fathers) have incentives to decrease (increase) their uptake of QI-days, there is a small minority of couples with reversed incentives, namely couples where the father has the highest uptake. To see this more clearly, I have estimated the models in panel A of Table 2 by the gender of the main user. Table 3 shows the results from this exercise.[17] In parent couples where the mother had the highest uptake and the father an uptake above 60 days (a group that constitutes almost 50 % of the total sample), we observe an effect of the bonus on the difference between the parents of about 4 days (panel A). The effect is significant on the 5 % level and amounts to 2.4 % of the control mean. As expected, both parents adjusted their uptake but the fathers' response was more pronounced (and significant). Thus, for a large portion of the parent couples the gender equality bonus indeed pushed fathers to take greater childcare responsibility which was the explicit aim of the reform.

[17] There are no indications that the bonus affected the gender of the main user. This is consistent with the reform's construction that gave parents incentives to reduce the gap between the main user and the second user but not shift those positions altogether.

Among parents where the father had the highest uptake and the mother an uptake above 60 days, the bonus instead pushed *mothers* to take up more days (panel B). Mothers in this group used around 8 more QI-days because of the bonus and fathers displayed a corresponding decrease. Thus, the response to the bonus was around four times as strong in this subsample, but in the opposite direction. Since the baseline absolute difference in uptake was much smaller for these parent couples, the estimated reduction amounts to almost 20 % of the control mean. The fact that the benefit level of the bonus-days was 100 SEK per day for virtually all parents might offer an explanation for the particularly strong response in this group. To generate bonus-days, the parent with the lowest uptake of QI-days needed to take up additional QI-days and thereby forego labor earnings (or earnings from other activities). Consequently, incentives to take up additional days and earn bonus decreased in outside income of the parent with the lowest uptake of QI-days. Mothers in the sample in panel B had much lower pre-birth income than fathers in the sample in panel A, suggesting that the stronger response to the bonus in the sample in panel B can, at least partly, be explained by stronger economic incentives.

Table 3 Effects by the gender of the main user

Column:	**(1)**	**(2)**	**(3)**	**(4)**
Uptake measure:	**Absolute difference**	**Mother-father difference**	**Mother**	**Father**
A. Mother>Father>60				
Effect of bonus	-3.89**	-3.89**	-1.41	2.48**
	(1.94)	(1.94)	(1.04)	(0.97)
Control mean	162.08	162.08	272.78	110.70
Observations	24,394	24,394	24,394	24,394
B. Father>Mother>60				
Effect of bonus	-15.99***	15.99***	8.14***	-7.84***
	(4.86)	(4.86)	(2.53)	(2.67)
Control mean	84.93	-84.93	149.09	234.02
Observations	3,195	3,195	3,195	3,195

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth.

5.2 Effects on the distribution of the lowest QI-uptake

Consistent with the reform's construction, we learned from Table 2 that only parent couples who exhausted all their reserved days responded to the bonus. Thus, the bonus pushed couples with an already relatively high degree of gender equality towards even more gender equality, thereby contributing to a polarization of gender-equality between parent couples. But how did the response vary by gender equality among parents who exhausted all their reserved days? One way to examine this question would be to estimate heterogeneous effects by the level of the lowest uptake, e.g., effects for parent couples where the lowest uptake is above 120 days or some other level. However, such an analysis is compromised by the fact that the bonus increased the probability of the lowest uptake exceeding certain high-level thresholds (see Figure A1). Instead, to avoid this endogeneity problem, I have estimated quantile regressions for the lowest uptake of QI-days. Focusing on percentiles 50, 75, 90 and 95, Table 4 presents the findings from this analysis. First, we note that the bonus did not affect the median of the lowest uptake which sits at around 64 days both before and after the reform. This is consistent with the earlier result that the bonus did not increase the likelihood of having a lowest uptake above 60 days (see Figure A1). The bonus did, however, significantly increase the level of the lowest uptake at percentiles 75, 90 and 95. The increase was particularly large at the 90th percentile where the bonus pushed up the uptake by more than 7 days from a baseline level of 157 days. The increase at percentiles 75 and 95 was roughly 5 days from baseline levels of 116 and 175 days respectively.[18] Thus, the main contributors to the reform effect were parent couples who even in the absence of the bonus policy would have had a very equal division of paid parental leave. Parent couples just above the 60-days threshold, on the other hand, did not respond to the bonus even though marginal increases in the lowest uptake would have generated bonus payments also for this group. Overall, the results from this heterogeneity analysis suggest that the economic incentives for equal division of paid parental leave provided by the bonus mainly affected decisions of parents with relatively strong preferences for equal division. For parents with other preferences, the economic incentives appear to have been too weak.

Table 4 Effects on the distribution of the lowest uptake of QI-days

Column:	**(1)**	**(2)**	**(3)**	**(4)**
Percentile:	**50**	**75**	**90**	**95**
Effect of bonus	0.70	4.82**	7.23***	5.22***
	(1.17)	(2.07)	(1.71)	(1.35)
Control value	64	116	157	175
Observations	50,460	50,460	50,460	50,460

Note: The table shows estimates of β_1 from quantile regressions using Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control value is the value for children born in June 2008 The model is estimated with a uniform kernel and a 90 day bandwidth.

5.3 The timing of the effect

In Sweden, children can start attending formal childcare from age 1. About 55 % of the children in the cohorts we study attended formal childcare when they were 18 months old. By the age of

[18] Note that the highest possible value of the lowest uptake of QI-days was 195 days.

3, the number reached 93 % (Hall and Lindahl 2018). One could argue that the division of paid parental leave before the child starts attending childcare is particularly important for gender equality in the household, since leave during this period is likely to reflect time spent alone at home with the small child. Paid parental leave that is saved during the first years and instead used at older ages is more likely to reflect days taken to extend family holidays or guarantee leave on specific dates. It is therefore interesting to investigate the timing of the total bonus effect over 8 years that we have documented so far. Such an examination can potentially also be informative about the importance of the adjustments of the bonus that came into effect from January 1, 2012, i.e. when children born around July 1, 2008, were three and a half years old. However, since we don't know how the timing of the effect would have looked in the absence of the adjustments such assessments will be uncertain. Again, following the discussion above, I restrict the sample to parent couples in which both parents used more than 60 QI-days over 8 years and evaluate effects of the bonus on accumulated uptake of QI-days between ages 1–8. Effects are estimated separately by the gender of the main user. Results are presented in Table 5.

In parent couples where the mother had the highest uptake (panel A), about 77 % of the total effect is present already at age 3 (3/3.9).[19] Thus, even though the overall response is relatively small in this group it appears to be concentrated to the pre-childcare period. The mothers' response seems to be finalized at age 4 while the fathers' response continues to grow until age 7.

In parent couples where the father had the highest uptake (panel B), the entire effect exists already at age 1. The effect actually seems to gradually decrease after this point, but the confidence intervals are also very large for this group suggesting that we cannot rule out the possibility that the effect remains stable after age 1. The positive effect on the mother's uptake appears to grow slightly over time but it is still clear that the bonus primarily affected the division of paid parental leave during the pre-childcare period for this group of parents.

[19] The complete absence of effects consistent with incentives of the bonus at age 1 in this group might be explained by biological factors, such as breastfeeding, being important determinants of leave division during the first year.

Table 5 Effects on uptake of QI-days at different ages

Column:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
Age:	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**
A. Mother>Father>60								
Mother-father diff.	1.8	-2.0	-3.0	-3.5*	-3.9*	-3.6*	-3.8*	-3.9**
	(1.9)	(2.2)	(2.1)	(2.1)	(2.0)	(2.0)	(2.0)	(1.9)
Control mean	180.2	173.9	172.5	170.7	169.3	167.9	166.0	162.1
Mother's uptake	1.4	-0.6	-1.2	-1.4	-1.6	-1.4	-1.3	-1.4
	(1.5)	(1.4)	(1.3)	(1.3)	(1.2)	(1.2)	(1.1)	(1.0)
Control mean	203.8	241.8	247.9	252.3	256.3	260.4	265.1	272.8
Father's uptake	-0.4	1.3	1.9	2.1*	2.4**	2.1*	2.6**	2.5**
	(0.9)	(1.3)	(1.2)	(1.2)	(1.1)	(1.1)	(1.0)	(1.0)
Control mean	23.6	67.9	75.5	81.5	87.0	92.5	99.2	110.7
Observations	24,394	24,394	24,394	24,394	24,394	24,394	24,394	24,394
B. Father>Mother>60								
Mother-father diff.	21.0***	18.1***	17.0***	16.2***	16.6***	16.3***	14.8***	16.0***
	(6.7)	(6.4)	(6.2)	(6.1)	(5.9)	(5.6)	(5.3)	(4.9)
Control mean	12.8	-50.6	-60.8	-66.6	-70.5	-74.7	-78.6	-84.9
Mother's uptake	6.0	6.3	7.1*	7.7**	7.8**	7.5**	7.6***	8.1***
	(4.1)	(3.9)	(3.7)	(3.6)	(3.4)	(3.1)	(2.9)	(2.5)
Control mean	103.5	118.8	122.8	127.1	131.7	136.8	141.9	149.1
Father's uptake	-14.9***	-11.8**	-9.9**	-8.5*	-8.8**	-8.8**	-7.2**	-7.8***
	(5.0)	(5.0)	(4.7)	(4.4)	(4.1)	(3.7)	(3.2)	(2.7)
Control mean	90.8	169.5	183.6	193.8	202.2	211.5	220.5	234.0
Observations	3,195	3,195	3,195	3,195	3,195	3,195	3,195	3,195

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth.

5.4 Spillover-effects to other parts of the parental insurance

As mentioned in section 2.2, paid parental leave can come in form of QI-days or FR-days. So far, because of the construction of the gender equality bonus, we have only examined effects on the uptake of QI-days. The 90 FR-days, which give 180 SEK per day for all parents regardless of income, are, in contrast to the 390 QI-days, completely transferable between the parents. Also, the gender equality bonus did not apply to the division of the FR-days. A natural question is then whether parents who changed their division of the QI-days because of the bonus, in turn adjusted their division of the FR-days. I investigate this matter in Table 6. Again, I restrict the sample to parent couples in which both parents used more than 60 QI-days over 8 years and estimate separate effects by the gender of the main user. For ease of comparison, I reprint the effects on the parents' uptake of QI-days in the table.

In parent couples where the mother had the highest uptake of QI-days over 8 years (panel A), the effects on the uptake of FR-days are small and insignificant. Thus, for both mothers and fathers (and for the parent couple together), we cannot reject the null hypothesis of a zero effect on the uptake of FR-days. Interestingly, we note that the bonus increased the total uptake of QI-days in this group of parents (column 5).

In parent couples where the father had the highest uptake (panel B), there are also no significant effects on the uptake of FR-days. However, for fathers we do see a relatively large opposite-signed point estimate. Since the mothers also have a positive point estimate for the FR-days, the effect on the parent couples' total uptake of FR-days is a little more than 3 days. In this context, it should be noted that it is common that parents don't exhaust all their 90 FR-days, despite the fact that the FR-days can be used on weekends, i.e. they can be used to increase income rather than to guarantee leave. Thus, by not exhausting the 90 FR-days, parents leave money on the table. It is possible that the bonus generally made these parents more attentive to possible ways of optimizing their uptake, such as increasing their uptake of FR-days and leaving less money on the table.

Table 6 Effects on uptake of FR-days

Column:	(1)	(2)	(3)	(4)	(5)	(6)
Uptake measure:	Mother QI	Mother FR	Father QI	Father FR	Total QI	Total FR
A. Mother>Father>60						
Effect of bonus	-1.41	0.15	2.48**	0.52	1.07**	0.67
	(1.04)	(0.86)	(0.97)	(0.70)	(0.53)	(0.78)
Control mean	272.78	44.33	110.70	26.87	383.48	71.21
Observations	24,394	24,394	24,394	24,394	24,394	24,394
B. Father>Mother>60						
Effect of bonus	8.14***	0.54	-7.84***	2.56	0.30	3.09
	(2.53)	(2.39)	(2.67)	(1.96)	(1.88)	(2.26)
Control mean	149.09	42.68	234.02	24.07	383.11	66.75
Observations	3,195	3,195	3,195	3,195	3,195	3,195

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth.

5.5 Effects on indicators of gender equality in the household

In the previous sections, we have established that the gender equality bonus led to a more equal division of paid parental leave at the household level. The effect was completely concentrated to parent couples in which both parents used more than 60 QI-days; a group constituting roughly 55 % of the total sample. While the bonus, just as the reserved months introduced in the decades before the bonus, had a direct effect on the uptake of QI-days, we must further investigate if there in turn were indirect effects on more fundamental long-term indicators of gender equality in the household. Direct effects on the uptake of paid parental leave days are important and arguably hold an intrinsic value, but a central argument behind both the bonus and the reserved months was that a more equal division of paid parental leave during the first years after childbirth should affect long-term division of home production and gender equality in the labor market.

While a natural way to study labor market effects of the bonus is to examine labor market earnings for men and women, capturing division of home production is more challenging. Eriksson and Nermo (2010), however, pioneered the use of the division of the uptake of paid *temporary* parental leave (TPL) days as a proxy for gender equality in the home. Paid TPL, which is part of the parental insurance system, becomes relevant when the initial, longer, parental leave period is over, and children have started attending formal childcare and both parents are back at work. If parents forego labor earnings because they need to be home caring for children who are too sick to be at the daycare center or in school, they can claim TPL benefits. Almost 80 % of the foregone earnings are replaced by the benefits and parent couples can claim a maximum of 120 days per child every year until the child turns 12 years old. Eriksson and Nermo (2010) combined register data on the division of paid TPL with survey data on parents' self-reported division of home production and found a clear correlation that was robust to several specification tests. Accordingly, Ekberg, Eriksson and Friebel (2013) used mothers' and fathers' uptake of paid TPL when they studied indirect effects of the first reserved month in the Swedish parental insurance introduced in 1995. While they concluded that mothers' and fathers' uptake of paid TPL was unaffected by the first daddy-month reform (which substantially affected the uptake of QI-days), the result was later questioned in Duvander and Johansson (2019) who found small but significantly negative effects on mothers' uptake consistent with long-term effects of the decrease in mothers' uptake of paid *standard* parental leave.

In Table 7 below, I show estimates of β_1 from applying Equation (2) to mothers' and fathers' uptake of paid TPL. Since the parental leave data cover all leave spells up to and including 2020, I can observe how many days the parents have used when the child turns 12 which marks the end of the paid TPL eligibility period. Thus, accumulated uptake of days through the child's 12^{th} birthday is the outcome in Table 7. Again, I restrict the sample to parent couples in which both parents used more than 60 QI-days and estimate separate effects by the gender of the main user. For ease of comparison, I reprint the effects on the parents' uptake of QI-days in the table.

There are no significant effects on the uptake of paid TPL. For mothers, however, the sign of the point estimates follows the direction of the direct effects on QI-uptake as hypothesized. For fathers, the pattern is more enigmatic with the TPL-estimates being opposite-signed compared to the QI-estimates. To some extent, these results resemble the findings in Duvander and Johansson (2019) who found that negative effects on mothers' uptake of QI-days, triggered by the introduction of the first reserved month in 1995, were accompanied by small but significantly negative effects on mothers' uptake of TPL, while similar indirect effects on fathers' uptake of TPL could not be observed.

Table 7 Effects on uptake of TPL-days

Column:	(1)	(2)	(3)	(4)	(5)
Uptake measure:	Mother QI	Mother TPL	Father QI	Father TPL	Total TPL
A. Mother>Father>60					
Effect of bonus	-1.41	-0.51	2.48**	-0.57	-1.08
	(1.04)	(0.76)	(0.97)	(0.60)	(1.10)
Control mean	272.78	29.60	110.70	20.15	49.75
Observations	24,394	24,394	24,394	24,394	24,394
B. Father>Mother>60					
Effect of bonus	8.14***	0.99	-7.84***	3.25	4.24
	(2.53)	(1.97)	(2.67)	(2.03)	(3.27)
Control mean	149.09	22.67	234.02	22.51	45.17
Observations	3,195	3,195	3,195	3,195	3,195

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth.

Next, in Table 8, I examine effects of the bonus on labor earnings for mothers and fathers. In the eight columns, I study accumulated income between the start of the calendar year of the child's first birthday (i.e., January 1, 2009) and the end of the calendar year when the child turns 1–8 years old. All effects, in both groups of parents, are insignificant. For parent couples where the mother had the highest uptake of QI-days and the father an uptake above 60 days (panel A), the effects are also relatively small, although the positive direction of the effects on the mother's income is consistent with the negative effects on paid parental leave previously documented. While the estimates for parent couples where the father had the highest uptake and the mother an uptake above 60 days are also insignificant (panel B), they are much larger than in the other group and go in the hypothesized direction for both mothers and fathers. For mothers, we observe fairly large negative effects in line with the substantial increase in paid parental leave that we saw in Table 7, and for fathers we see positive effects consistent with the decrease in the uptake of QI-days. The main take-away from Table 8, however, is that we cannot reject the null hypothesis of no effect of the bonus on mothers' and fathers' labor earnings.

Table 8 Effects on labor income

Column:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
End of year:	1	2	3	4	5	6	7	8
A. Mother>Father>60								
Mother's income	4,763	4,588	9,557	12,063	13,304	13,168	11,149	7,973
	(2,961)	(6,514)	(9,967)	(13,561)	(17,524)	(21,667)	(26,091)	(30,822)
Control mean	136,515	364,503	585,828	835,777	1,116,330	1,418,505	1,741,405	2,086,888
Father's income	-2,836	92	1,673	5,519	7,099	3,364	3,718	-6,155
	(4,909)	(9,673)	(14,656)	(19,438)	(24,538)	(30,074)	(36,010)	(42,272)
Control mean	318,077	684,229	1,066,924	1,459,905	1,871,732	2,308,236	2,759,480	3,229,094
Observations	24,394	24,394	24,394	24,394	24,394	24,394	24,394	24,394
B. Father>Mother>60								
Mother's income	-4,456	-9,132	-25,186	-45,930	-54,640	-74,133	-91,166	-115,755
	(11,787)	(24,462)	(36,981)	(50,696)	(64,789)	(79,667)	(95,588)	(112,639)
Control mean	161,146	365,389	581,011	833,079	1,104,968	1,397,514	1,711,905	2,057,315
Father's income	4,711	14,735	28,842	39,009	58,919	71,137	84,371	101,625
	(10,567)	(21,254)	(32,659)	(43,916)	(55,531)	(67,311)	(79,901)	(93,336)
Control mean	226,581	533,024	858,579	1,191,771	1,546,868	1,922,295	2,311,906	2,721,212
Observations	3,195	3,195	3,195	3,195	3,195	3,195	3,195	3,195

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth. Estimates and means are in SEK.

6 Conclusion

I examined the causal impact of a parental leave policy in Sweden, the so-called gender equality bonus, that rewarded parents with a financial bonus as a function of their division of transferable parental leave days and found significant effects in line with the incentives. Evaluating the accumulated uptake of paid parental leave days through the child's 8th birthday, which marks the end of the paid parental leave eligibility period, I find that the bonus significantly reduced the absolute difference in uptake between the parents. The response to the bonus was completely driven by the roughly 55 % of the couples who exhausted all reserved days (60 for each parent). This pattern was theoretically expected since marginal increases in the uptake of QI-days for the parent with the lowest uptake only generated bonus if the uptake was at least 60 days. In this group, the average decrease in the absolute difference was around 5 days amounting to 3.3 % of the control mean. The reduction in the absolute difference was particularly pronounced in the very small group of parents where the father had the highest uptake, causing the overall effect of the bonus on the mother-father difference in uptake to be insignificant. Thus, even though the bonus led to a more equal division of paid parental leave at the household level, it did not affect *aggregate* differences between mothers and fathers.

However, in couples where the mother had the highest uptake and the father an uptake above 60 days (a group that constitutes almost 50 % of the total sample), there was actually a significant decrease in the mother-father difference of almost 4 days. A substantial part of this effect was observable already at age 3. Thus, for a large portion of parent couples the gender equality bonus indeed pushed fathers to take a little more childcare responsibility, also during the particularly important pre-daycare period. From this first set of results, I conclude that parents clearly reacted to the specific economic incentives provided by the bonus despite its modest size, an active application requirement and sluggish feedback, suggesting that a bonus-system, on its own or combined with gender quotas, can be a potent policy instrument for affecting the division of paid parental leave.

Did parents who changed their division of QI-days because of the bonus in turn adjust their more long-term division of home and market production, which was the ultimate aim of the reform? To investigate this, I estimated effects of the bonus on the uptake of paid TPL, which has been suggested to be a good proxy for gender-equality with respect to home production, and labor earnings. While I found insignificant effects of the bonus on these outcomes, mothers who lowered (increased) their uptake of parental leave in response to the bonus policy displayed negative (positive) point estimates for TPL and positive (negative) point estimates for labor earnings. For fathers, the pattern was less clear. Taken together, the results suggest, at least for mothers, a potentially important link between the length of the early parental leave and later allocation of time between home and market production.

In summary, consistent with the economic incentives provided by the bonus, the policy led to a more equal division of paid parental leave among parents who would have had a relatively equal division even in the absence of the bonus policy. Importantly, for mothers in these couples, there are tentative indications that the bonus had indirect effects on earnings and the uptake of paid TPL. While the indirect effects were small (as were the direct effects), the suggested existence of a link between the length of the early parental leave and later allocation of time between home and market production is important since it suggests that reforms targeting the division of early parental leave can have long-term consequences for gender equality. Small indirect effects for the first cohort affected by a reform, as in the current case, might also translate into larger effects for later cohorts if, as is often the case, the direct effect of the reform grows over time.

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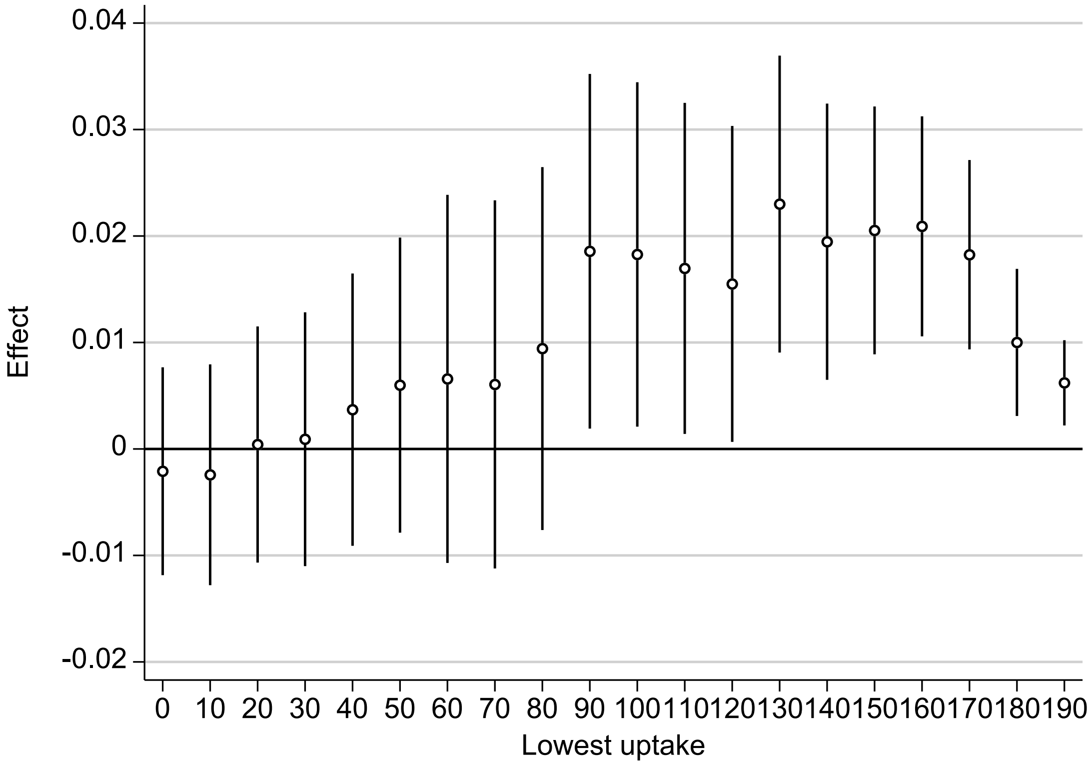
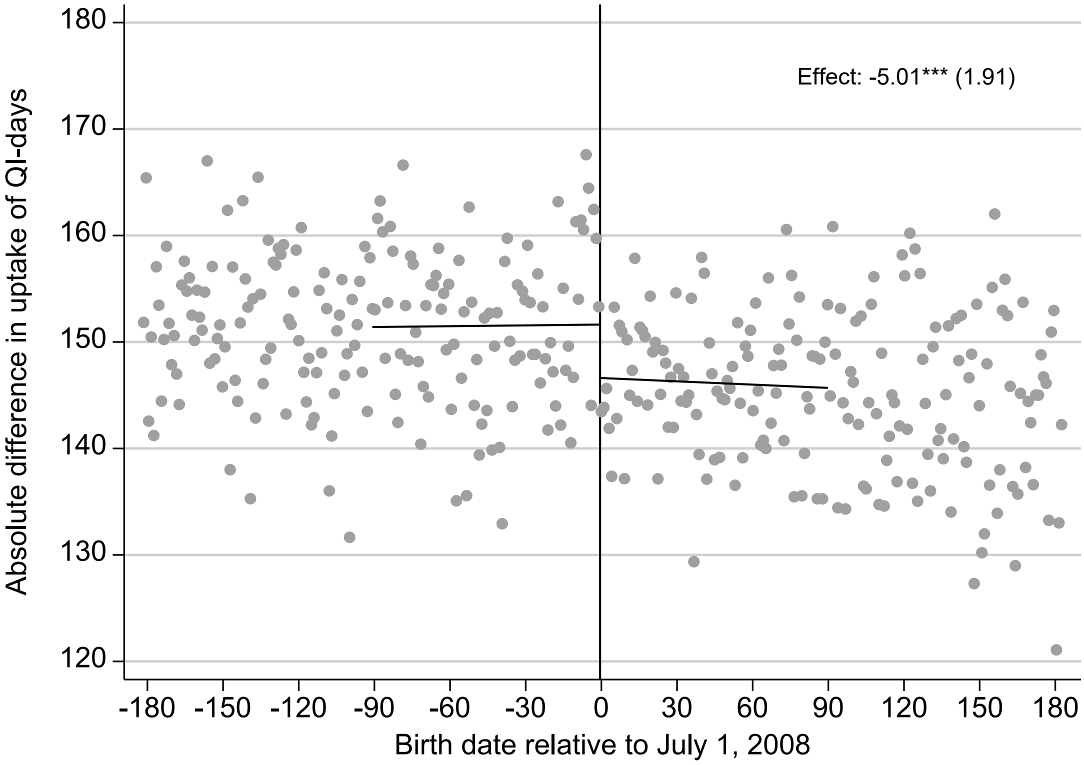
Appendix A Additional results

Figure A1 Effects on the probability that the lowest QI-uptake is above certain thresholds



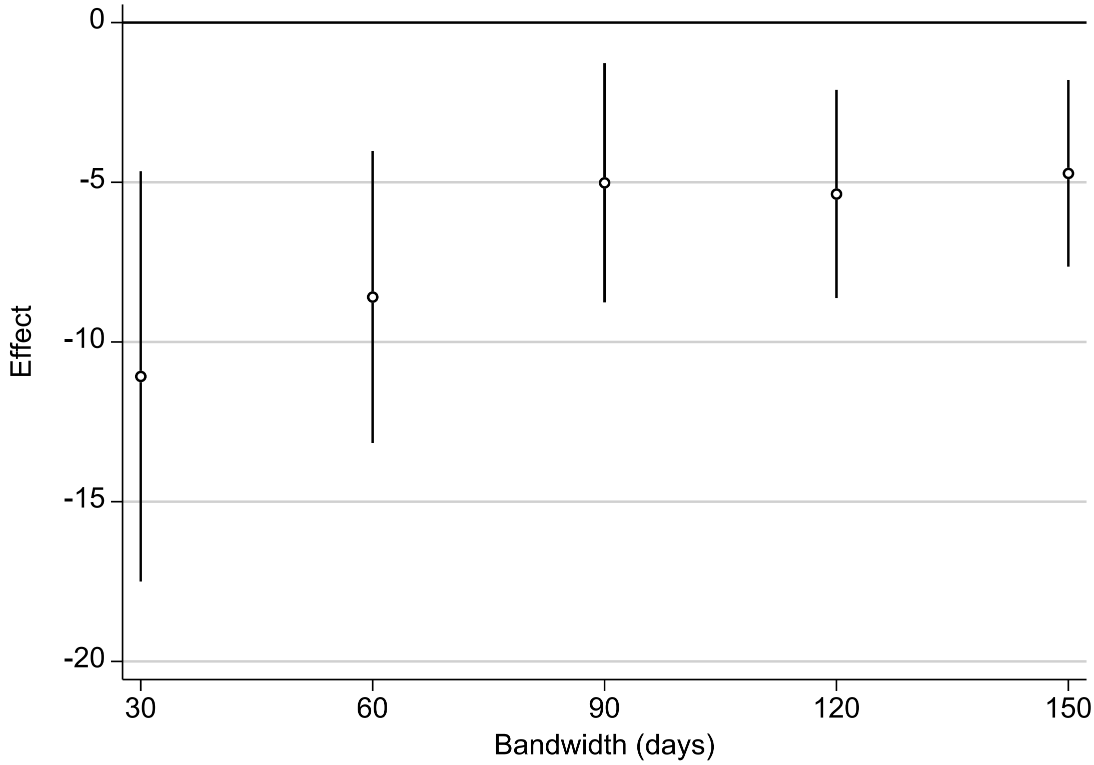
Note: The figure shows estimates of β_1 from Equation (2) with 95 % confidence intervals. The outcome takes the value 1 if the lowest uptake is higher than X days. The model is estimated with a uniform kernel and a 90 day bandwidth.

Figure A2 Average absolute difference in the uptake of QI-days by date of birth



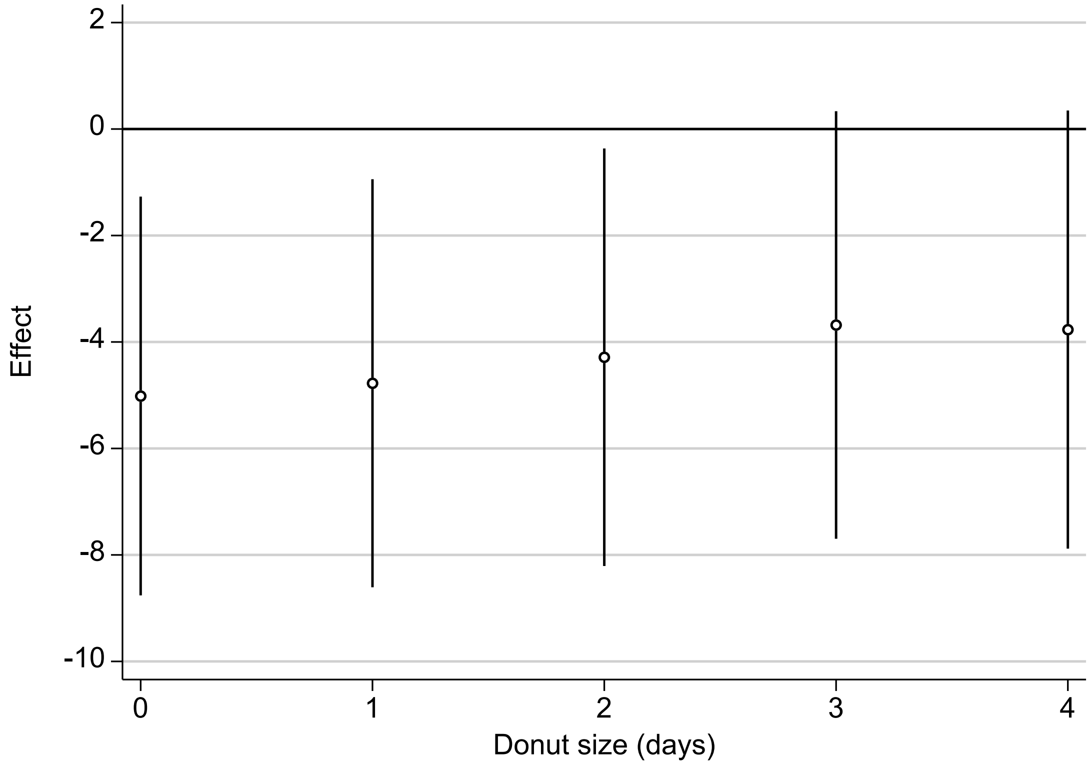
Note: The figure is based on children born in 2008. The sample is restricted to parent couples where the lowest uptake of QI-days is greater than 60 days. The model is estimated with a uniform kernel and a 90 day bandwidth.

Figure A3 Effects for different bandwidths



Note: The figure shows estimates of β_1 from Equation (2) with 95 % confidence intervals using different bandwidths. The sample is restricted to parent couples where the lowest uptake of QI-days is greater than 60 days. The model is estimated with a uniform kernel.

Figure A4 Donut regression discontinuity estimates



Note: The figure shows estimates of β_1 from Equation (2) with 95 % confidence intervals. The sample is restricted to parent couples where the lowest uptake of QI-days is greater than 60 days. The model is estimated with a uniform kernel and a 90 day bandwidth.

Table A1 Average effects in the overall sample

Column:	(1)	(2)	(3)	(4)
Uptake measure:	Absolute difference	Mother-father difference	Mother	Father
Effect of bonus	-3.36**	-0.25	0.30	0.55
	(1.66)	(2.17)	(1.18)	(1.13)
Control mean	210.51	194.25	283.39	89.14
Observations	50,460	50,460	50,460	50,460

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth.

Table A2 Specification tests (sample is always lowest uptake > 60 days)

Column:	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Specification:	**Baseline**	**Triangular kernel**	**Placebo 2007**	**Placebo 2009**	**Predicted abs. diff**	**DiD**
Effect of bonus	-5.01***	-7.66***	-1.18	2.04	1.14	-2.54**
	(1.91)	(2.09)	(1.89)	(1.90)	(0.71)	(1.17)
Control mean	153.01	153.01	156.68	144.36	199.67	153.01
Observations	27,589	27,589	26,976	28,585	27,589	83,150

Note: Columns 1–5 show estimates of β_1 from Equation (2) (triangular kernel in column 2). Column 6 shows a difference-in-differences estimate. Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008. The model is estimated with a uniform kernel and a 90 day bandwidth.

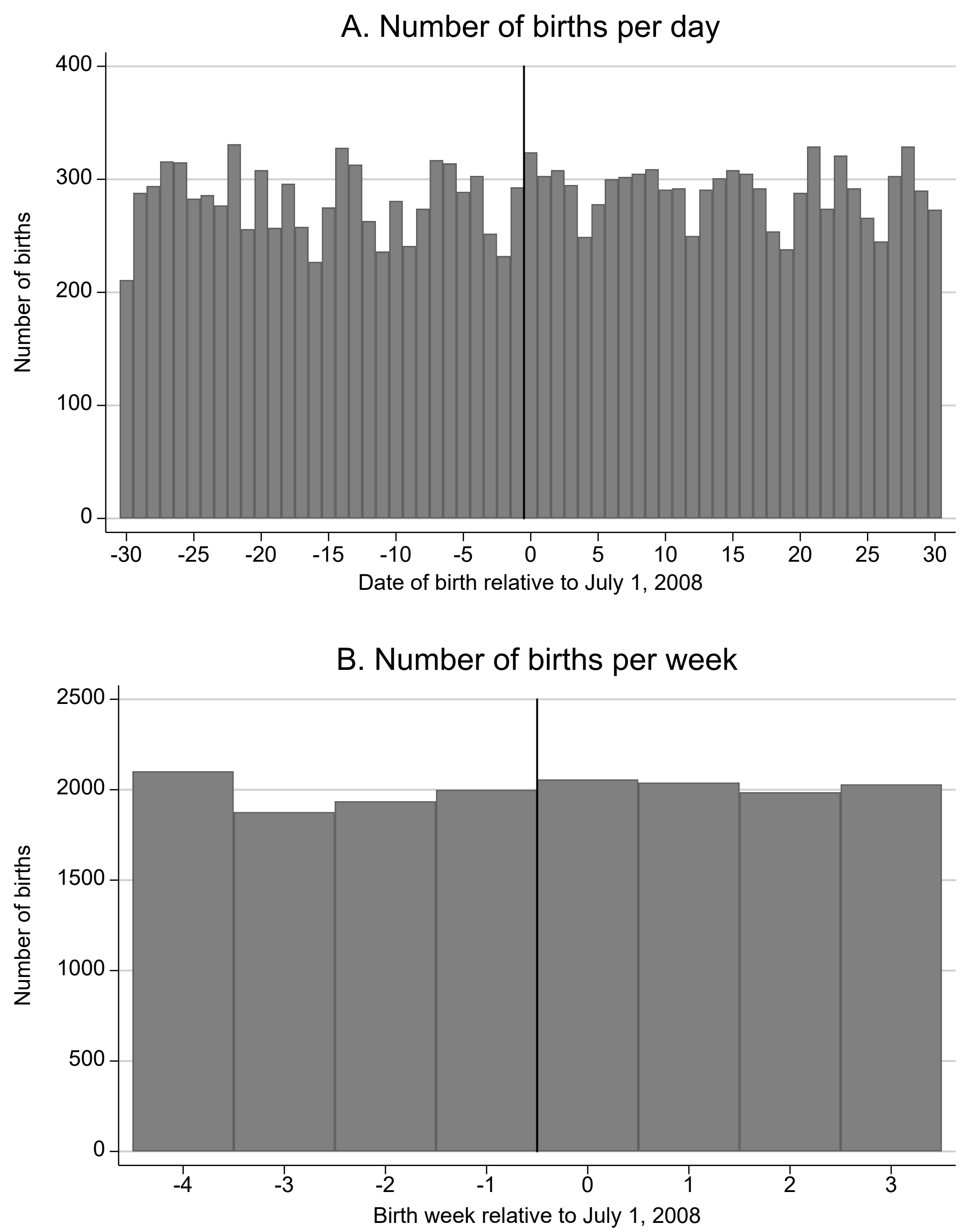
Table A3 Heterogeneous effects by number of children for couples with lowest QI-uptake above 60

Column:	**(1)**	**(2)**
Uptake measure:	**Absolute difference 2008 child**	**Absolute difference other children**
Panel A. Parent couples with one child born during 2004–2012		
Effect of bonus (uniform kernel)	1.07	N/A
	(3.94)	
Effect of bonus (triangular kernel)	1.88	N/A
	(4.31)	
Control mean	153.74	N/A
Observations	6,677	N/A
Panel B. Parent couples with more than one child born during 2004–2012		
Effect of bonus (uniform kernel)	-7.05***	4.12*
	(2.18)	(2.34)
Effect of bonus (triangular kernel)	-10.82***	0.30
	(2.38)	(2.55)
Control mean	152.78	169.75
Observations	20,912	23,900

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The control mean is the mean for children born in June 2008 or the mean for siblings of children born in June 2008. The model is estimated with a 90 day bandwidth.

Appendix B Validity of the regression discontinuity model

Figure B1 Births in June–July 2008



Note: The figure is based on children born in June–July 2008. July 1, 2008, was a Tuesday.

Table B1 RD-estimates on predetermined parental characteristics

Column:	**(1)**
Bandwidth (days):	**90**
Outcome:	
Schooling (mother)	-0.02
	(0.04)
Schooling (father)	-0.03
	(0.04)
Swedish-born (mother)	-0.00
	(0.01)
Swedish-born (father)	0.00
	(0.01)
Age at birth (mother)	-0.06
	(0.09)
Age at birth (father)	0.00
	(0.11)
Income_{t-1} (mother)	-2,434.87
	(2,779.53)
Income_{t-1} (father)	-2,552.32
	(4,154.41)
Predicted absolute difference	0.48
	(0.56)
Observations	50,460

Note: The table shows estimates of β_1 from Equation (2). Robust standard errors are in parentheses and */**/*** refers to statistical significance at the 10/5/1 percent level. The prediction is based on children born in 2007. The model is estimated with a uniform kernel and a 90 day bandwidth.