# Money Talks: How Increased Reimbursement Rates Impact Parental Benefit Uptake

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**Abstract**

In this thesis, I examine the relationship between the compensation rate and the take-up rate of parental benefits. I utilise a unique reform in the Swedish parental leave scheme dating back to 2006 as a natural experiment to study parents' responses to a small increase in the compensation rate of the so-called minimum level days. Unlike most parental benefit days, which are subject to a wage replacement scheme, minimum level days are compensated at a low flat rate, regardless of parents' previous earnings. Although the precise magnitude of my estimates should be interpreted with caution, I find that the overall take-up for a child increased following the reform. Therefore, my thesis provides empirical evidence supporting a positive relationship between the compensation rate and the take-up rate of parental benefits.

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## 1 Introduction

Individuals entering parenthood are often faced with the difficulty of balancing work and family responsibilities. Taking time off from work to care for a child often results in significant losses of income and missed career opportunities, particularly for mothers. Consequently, parents may opt for a quick return to work following childbirth, possibly at the expense of their child's well-being. In order to support the work-family balance, most industrialised countries have introduced some type of paid parental leave policy. However, the structure of these leave systems varies significantly. As policymakers employ legislation to encourage specific behaviours, the design of such systems often mirrors societal objectives.

Sweden has established itself as a pioneering country in expanding parental leave uptake. Over the past few decades, the Swedish parental leave scheme has undergone several extensions in the duration of parental leave entitlement. The system has also been reformed, specifically targeting dual-earning families to increase fathers' leave-taking. Notwithstanding these reforms, there are several paid parental leave days that go unused. While more common among fathers, mothers also forfeit some days (the Swedish Social Insurance Agency 2019). One plausible explanation for this is the high opportunity cost of not working. According to neoclassical labour supply theory, parents are more inclined to take parental leave when the cost of not working is low and the value of spending time with their children is high. While evidence supporting this idea exists, much of it is based on the introduction of paid parental leave.[1] Only few studies have examined causal responses to variations in the reimbursement rate of paid parental leave within existing schemes. This thesis adds to this small body of research.

The few studies investigating such responses suggest a positive relationship between compensation rates and leave-taking. For example, Kluve and Tamm (2013) studied a German reform in which the previous means-tested flat-rate compensation scheme was replaced by a wage replacement scheme, ultimately increasing the benefit level for the majority of parents. Their findings revealed that, on average, parents extended their leave. Similarly, Lapuerta et al. (2011) reached similar conclusions when examining variations in compensation levels across regions in Spain. However, due to different gender norms and societal objectives, these findings may not necessarily apply to Sweden.

The multifaceted parental leave system in Sweden has lent itself to a couple of studies in this area with Moberg (2018) studying the effects of the speed premium for second-time

[1] See Olivetti and Petrongolo (2017) for an overview of studies in high-income countries.

parents and Rosenqvist (2022) evaluating the effects of leave-taking following the introduction of the equality bonus. While both studies provide evidence of a positive relationship between compensation level and leave-taking in Sweden, these findings are conditional on couples having a second child or there being two parents involved in the upbringing of a child. This highlights the need for a study that examines responses to unconditional variations in compensation rates. Therefore, my thesis addresses this gap by investigating a unique reform that pertains to the compensation rate of minimum level days. Specifically, I study the response in take-up of parental benefits following an unconditional increase in the compensation level of the minimum level days which are compensated at a low flat rate.

Parents in Sweden are entitled to a total of 480 days of paid parental leave, out of which 390 are compensated at 80 % of parents' previous labour earnings, up to an inflation-adjusted cap. In addition, parents are eligible to take 90 days of leave that are compensated at a low flat rate, regardless of previous earnings. These 90 days are known as minimum level days. Single parents are entitled to the full 480 days of paid leave, while for couples, both income-related days and minimum level days are divided equally between the pair of parents. However, co-parents have the flexibility to transfer the full number of minimum level days between each other. This is in contrast to certain income-related days that are specifically allocated to each parent, often referred to as "daddy-months".

Prior to July 1, 2006, the compensation rate for the 90 minimum level days was 60 SEK per day. However, starting from that date, parents of children born on or after July 1, 2006, received an increased reimbursement rate of 180 SEK per day. The purpose of this increase was twofold: to improve the financial situation of women, who are the primary beneficiaries of these days, and to encourage fathers to take a greater share of these days (the Swedish Government 2006). In this thesis, I leverage the discontinuous nature of the minimum level days reform to examine the impact of becoming eligible for the higher reimbursement rate on the uptake of parental benefit days. However, exploiting this reform with the empirical setting and data at hand requires careful consideration of modelling assumptions. The cause for concern mainly lies in seasonal dependencies in the take-up of parental benefits.

To analyse the effects of the reform, I estimate the average difference in uptake between eligible and non-eligible children born in the reform year of 2006, while using children born in other years as a control to account for seasonality. I analyse administrative data containing information on the take-up of parental benefits for children born in Sweden from January 1, 2002, to June 30, 2008. However, this data does not include information on the specific type of day used, distinguishing between minimum level days and income-related days. I therefore complement my analysis with aggregate data from the Swedish Social Insurance Agency. This supplementary data allows me to study the birth month average take-up of parental benefits across all levels for children born in Sweden from 2005 to 2007.

Based on my findings, it appears that the policy reform had a positive impact on the uptake of minimum level days. When examining the cumulative take-up of minimum level days on a yearly basis for children, I observe an increase of 3.65 days during their first year of life following the reform. This uptake continued to rise over subsequent years, reaching an estimated effect of 9.70 days by the time the child turned five years old. In comparison to the mean take-up for non-eligible children born in 2006, this corresponds to a 20 % increase in minimum level days and a 2.5 % increase in total uptake, irrespective of day type. Moreover, an analysis of gender-specific responses reveals that mothers played a crucial role in driving the increased uptake of minimum level days for children, accounting for 80 % of the observed increase, while fathers contributed with 20 %. However, if one takes into account the relative difference in take-up between mothers and fathers, with fathers taking approximately 15 % of the minimum level days for a child, my findings suggest that the responses to the reform over gender are very similar.

However, it is not entirely clear whether these effects are solely attributable to the increase in compensation for minimum level days. Concurrent with the minimum level days reform, there was also an increase in the inflation-adjusted cap for income-related days, which applied to all income-related days taken from July 1, 2006. While it does not seem that the reform had a discontinuous effect on the take-up of parental benefits, it is possible that the reform influenced the take-up of minimum level days as parents became more mindful of optimising their leave-taking. Fortunately, both reforms share the common goal of increasing leave-taking, so regardless of the precise mechanism at play, the overall uptake of parental leave for children increased.

There are certain questions that I cannot address in this thesis due to data limitations. I am unable to estimate the effects of leave-taking for the entire entitlement period, which concludes when a child reaches the age of eight. Parents may choose to save some days for future use, such as attending school-related events when the child is older, and this aspect warrants further investigation. Additionally, it is important to note that minimum level days can be used on any day and does not require parents to take leave from work. Therefore, my findings may not necessarily be associated with a lower opportunity cost of not working. Nevertheless, my study provides evidence of a positive relationship between compensation rates and the utilisation of parental benefits.

The remainder of this paper is structured as follows: Section 2 provides a summary of the theoretical background and previous empirical research. Section 3 presents a description of the Swedish parental leave system and relevant reforms for this study. Section 4 provides an overview of my data and empirical strategy. In Section 5, I present the results and discuss robustness. Finally, I conclude with some final remarks in Section 6.

## 2 Theoretical Background and Previous Evidence

In this section, I outline theoretical considerations and previous evidence related to how parents respond to changes in compensation for paid parental leave. As mentioned earlier, according to standard neoclassical labour supply theory, parents are more likely to take parental leave when the cost of not working is low and the time spent with their children is highly valued. Therefore, it is plausible that increasing the reimbursement rate of parental benefits can reduce the cost of not working and make leave-taking more appealing. This theory can also explain differences in the responses of mothers and fathers. Since mothers generally earn less than fathers, their initial opportunity cost of not working may be lower relative to fathers'. Additionally, mothers may place a higher value on time spent with their children compared to fathers.

It is also worth considering the possibility of strategic choices within dual-earning families. The theory of housework specialisation within families, formulated by Becker (1985) can provide insights into the gender differences in responses. Given that mothers generally earn less than co-parenting fathers, if the mother takes leave from work, the couple's pooled income decreases less compared to the income loss if the father were to stay with the child. Therefore, on average, couples would be better off if mothers take leave. While this theory goes beyond the scope of my exploration, it is worth mentioning. A less complex explanation for these differences is the influence of existing gender norms regarding parenting.

As stated in the introduction, there is limited empirical research on the causal responses to variations in the reimbursement rate of parental benefits. In the context of evaluating a Swedish reform, I will focus on describing previous evidence from Sweden. First, I consider the evidence presented by Moberg (2018). She examines the effects of the "speed premium rule" on leave-taking. In Sweden, there is a wage replacement scheme where parents are compensated at 80 % of their pre-childbirth wage earnings. The speed premium rule allows parents who have a second child within 30 months of their first child to maintain the qualifying income they had for the first child. This rule has a significant impact on the level of compensation for parental benefits when having a subsequent child since many mothers reduce their working hours and, consequently, their wage earnings after having a child.

Moberg (2018) employs a fuzzy regression discontinuity design with the 30-month eligibility threshold as a source of variation in benefit level. She uses the spacing between the first and second child as a running variable and estimates a discontinuous jump in the compensation level of parental benefits. This approach allows her to estimate the causal response of a one-unit change in the parental leave benefit level on the utilisation of parental leave days. Her sample includes married or cohabiting parents who had their first and second child together between 1994 and 2009, excluding those without a qualifying income for their firstborn children. The results indicate that a 1 % (5 SEK) increase in the mother's benefit level per day extends her leave by 2.6 days (1 %). Fathers, on the other hand, respond by reducing their own leave time by 1.9 days, which accounts for approximately 75 % of the mother's increase. These findings suggest that a change in the benefit level not only affects the duration of leave for the recipient but also influences the division of leave between parents.

In another Swedish study, Rosenqvist (2022) examines the causal impact on leave-taking following the introduction of the equality bonus. This bonus provided a financial reward to parents of children born on or after July 1, 2008, based on their allocation of transferable parental leave days. By utilising a regression discontinuity design with a child's birthday as a running variable, Rosenqvist (2022) finds that the policy had a significant effect in reducing the absolute difference in the number of paid leave days between parents. This effect was primarily observed among couples who had fully utilised their reserved income-related days (60 days at the time of the equality bonus introduction).

Relevant to my thesis is that Rosenqvist (2022) also explored possible spillover effects for parents who changed their division of income-related days because of the bonus on the division of minimum level days. Restricting his analysis to include only couples who exhausted all reserved days, he considers two sub-samples: couples where mothers have the highest uptake of income-related days and couples where fathers have the highest uptake of the corresponding days. These samples are almost equal in size. No spillover effects were found in the former sample. Interestingly, in the latter sample, there was an increase of 3 days in the total take-up of minimum level days among couples where fathers had the highest take-up of income-related days. This increase was attributed to fathers increasing their use of minimum level days, with no notable change in mothers' usage. However, fathers did not utilise the full 45 level days, and mothers' usage remained below 45 days, suggesting no evidence of day transfer between parents. Although the estimate of 3 days lacks statistical significance, Rosenqvist (2022) suggests that the equality bonus might have prompted parents to be more attentive in optimising their usage.

The studies conducted by both Moberg (2018) and Rosenqvist (2022) collectively demonstrate a positive correlation between the compensation rate and the uptake of parental benefits in Sweden. They support the possibility of leave-taking being more likely if the opportunity cost of not working decreases with the increase in the compensation rate of parental benefits. Although their findings may not directly align with the theory of household specialisation, they are certainly not contradictory to it.

# 3 The Swedish Parental Leave System and Relevant Reforms

The Swedish parental leave system dates back to 1974 whereby maternity leave was replaced with flexible parental insurance giving both parents equal rights to take leave. At the outset, parents were initially entitled to 180 days of paid leave. Parents with previous labour earnings were compensated at 90 % of their income up to an inflation-adjusted cap. To facilitate the reading, I have previously only provided a simplified description of the compensation scheme, which explains the income-related days to be compensated as a percentage of parents' previous labour earnings. However, the income-related days are compensated as a percentage of parents' sickness benefit qualifying income, SGI (short for "sjukpenninggrundande inkomst"), which is specifically based on previous labour earnings. Roughly, SGI amounts to an individual's monthly wage times 12 up to a cap which is calculated based on some price base amounts ("prisbasbelopp"). The price base amounts vary to reflect inflation and are set annually. Parents without previous income received compensation at the guarantee level ("garantinivå"), which is a low flat rate.

The system has been subject to various reforms throughout the decades to follow. There have been considerable extensions in paid parental leave entitlement, mostly in terms of income-related days. However, with the extension of parental leave in 1978 came the minimum level days, that is, the days that are compensated at a low flat rate regardless of parents' previous income or labour market attachment. In contrast to the SGI level days which parents may only utilise for days they otherwise would have worked, the minimum level days can be utilised even on days parents would not have worked, such as weekends and national holidays. In 1986, parental leave was further extended to 360 days. Part of the expansion is captured by the increase in minimum level days, which had increased to 90 days. Apart from a minor deviation in 1994, the number of minimum level days has stayed the same.[2] The final extension of parental leave duration was implemented in 2002, when parents of children born on or after January 1, 2002, became entitled to take a total of 480 days of parental leave (380 income-related days and 90 minimum level days).

The system has also changed in flexibility. As of January 1, 2002, parents can take as little as one-eighth of a full benefit day. Parents have until their child finishes first grade or until their eighth birthday to use the days they are entitled to. Parents with joint custody are entitled to an equal amount of benefit days. Until 1995, parents used to be able to transfer all of their days between each other. However, in 1995, this changed as 30 SGI level days were reserved for each parent, introducing the first so-called daddy month. A second daddy month was introduced in 2002 so that 60 SGI level days were reserved for each parent and applied to parents with joint custody of a child born on or after January 1, 2002. Related to the reforms promoting a gender-equal division of parental leave is the introduction of the equality bonus ("jämställdhetsbonus"), which I briefly described in the previous section. This granted a cash transfer of up to a total of 13,500 SEK for co-parents who split their leave more equally. The equality bonus was implemented on July 1, 2008, and could be used by parents of children born on or after July 1, 2008.

The system has also been subject to changes in the reimbursement rate. As of 1998, income-related days have been compensated at 80 % of a parent's SGI. However, the first 180 days are conditioned. This condition states that to qualify for reimbursement at 80 % of SGI, parents must have worked for 240 days before the birth of their child. Those who have not worked for 240 days receive a benefit at a guaranteed level for the first 180 days. After this threshold, parents may be compensated at their SGI level, provided that they have one. The guaranteed level has increased over the years. In 2002, it increased from 60 SEK to 120 SEK for benefit days taken on or after January 1, 2002. The level increased again, from 120 SEK to 150 SEK, for days taken on or after January 1, 2003. Finally, it was raised to 180 SEK for days taken on or after January 1, 2004, and remained at this level until 2013, when it was raised to 225 SEK. Note that these increases were not conditioned by the date of birth of a child.

For the remainder of this section, I will focus on two reforms that took place in 2006. I start with the minimum level days reform which I utilise in this study. As mentioned in the introduction, the minimum level days used to be compensated at 60 SEK per day. The 60 SEK rate was set in 1978 and remained at this level for parents of children born before

[2] The minimum level days were partly phased out and removed to finance a child-care allowance scheme but were reintroduced six months later, replacing the child-care allowance scheme.

July 1, 2006. As of July 1, 2006, however, parents of children born on or after this date were compensated at 180 SEK per day and the rate has remained at this level since. A full exhaustion of the minimum level days for eligible parents translates into a compensation of 16 200 SEK. The corresponding number for non-eligible parents is 10 800 SEK.

Accompanying the minimum level days reform was an increase in the SGI cap for the compensation of income-related days. The SGI cap was previously set to 7.5 times the price base amount and had not changed since the introduction of parental benefits in 1974. However, as of July 1,y 2006, the SGI was capped at 10 times the price base amount. The reform was introduced to reflect the increase in real wages, as well as to create economic incentives for individuals with higher incomes, typically fathers, to take more leave. It applied to all SGI level days taken as of July 1, 2006, regardless of a child's date of birth. Using the 2006 price base amount, the increase implied that the maximum daily compensation of SGI level days change from approximately 653 SEK to 870 SEK, calculated as 80 % of the SGI cap divided by 365 days. Given the entire Swedish working population in 2006, 34.3 % of the male population and 14.9 % of the female population had incomes above the old cap. Only 9 % had incomes above the new cap (the Swedish Government 2006).

# 4 Data and Empirical Strategy

## 4.1 Data

The administrative data used in this study is based on several population-wide registers provided by Statistics Sweden. I use the multi-generational register to link all children born in Sweden to their biological parents. This register contains information on when these children were born in terms of birth week, birth month and birth year. I match this data to the longitudinal administrative data containing information on parents' baseline characteristics, the majority of which are recorded annually. I extract information on parents' age, gender, educational attainment, marital status and whether they are native-born. To this data, I add two income variables. The first variable contains information on individuals' earnings from employment, excluding income from self-employment, social transfers and similar. The second one contains information on all taxable labour income including earnings from both employment and self-employment as well as work-related social transfers such as sickness benefits and parental benefits.

I match this data to information on parents' take-up of parental leave benefits extracted from a register maintained by the Swedish Social Insurance Agency. Parental benefit usage is registered based on leave spells. These spells refer to periods of consecutive usage of parental benefits without interruption. Since parents may choose to take as little as 12.5 % of a full parental benefit day, these spells do not necessarily reflect the full amount of benefit days and are not corrected for weekends or national holidays. Therefore, I utilise another variable which provides information on the number of full parental benefit days that parents take within a spell. For my main measure, I calculate the cumulative sum of days taken in all spells that start within a given number of weeks from the birth week of a child. To clarify with an example, the cumulative uptake for a child during its first two years of life amounts to the number of parental benefits taken in all spells starting within 104 weeks from the child's birth week.

Most of the data is available from 1990 to 2020. However, the data on parental leave is only available from 1995 to 2012. By restricting my analysis to children born between January 1, 2002, and June 30, 2008, selected based on no other major reforms plaguing the analysis, I can measure the take-up for children up to their fourth birthday.[3] I will however also consider the uptake for children up to their fifth birthday for a restricted sample. Recall that parents may take parental benefits until their child finishes first grade or until their child's eighth birthday. This means that I do not observe the final number of parental benefit days taken for all children.

Another limitation is that I am not able to distinguish which type of days parents utilise, only the total number of days taken within a spell. I also lack information on parents' qualifying income, that is, their SGI level and whether they fulfil the requirements to be compensated at the SGI level for the first 180 days. As such, this data does not allow me to confirm that it indeed is the take-up of minimum level days that are affected by the reform. Therefore, I complement my analysis with birth month aggregate data provided by the Statistical Analysis Unit at the Swedish Social Insurance Agency (henceforth the Agency). This data covers all children born in Sweden from January 1, 2005, to December 31, 2007, for which leave was taken at some point. The data contains information on the number of children born in each month and the sum of parental benefit days paid out to mothers and fathers, respectively, on a yearly interval from the birthday of a child. Not only does this data contain information on the number of minimum level days taken for children, but also the number of income-related days paid out on the SGI level and guarantee level.

[3] Recall the 30 days extension in paid leave accompanied by the introduction of the second daddy month for children born as of January 1, 2002, and the introduction of the equality bonus for children born as of July 1, 2008.

Unfortunately, I am yet again limited to the uptake rates until 2012 due to a change in data collection procedures in 2013. Ultimately this data allows me to study take-up for children until their fifth birthday given the 2007 cohort.

I will consider a variety of samples based on the administrative data, for reasons I will return to later, but my main sample includes all single-birth children born to a biological mother and father in Sweden from January 1, 2002, to June 30, 2008; a total of 600 956 children. I have excluded the 53 411 children with missing values in parental characteristics or with parents that are not uniquely identified in the data from this sample, at least for now. Note that this restriction does not imply that a child has a pair of co-parents. A child can still have a pair of biological parents but with only one of the parents having custody, which is important to keep in mind since parents with sole custody are entitled to the full number of parental benefits granted for a child. In contrast to the Agency's data, I include children for which no leave is taken to study the possibility of the reform impacting responses on an extensive margin. Out of the 600 956 children, 3 525 have no leave taken for them. Further, note that I choose to include only single-birth children in this sample because parents of multiple-birth children face different rules than those outlined in the previous section which makes it difficult to precisely study responses to the reform.[4]

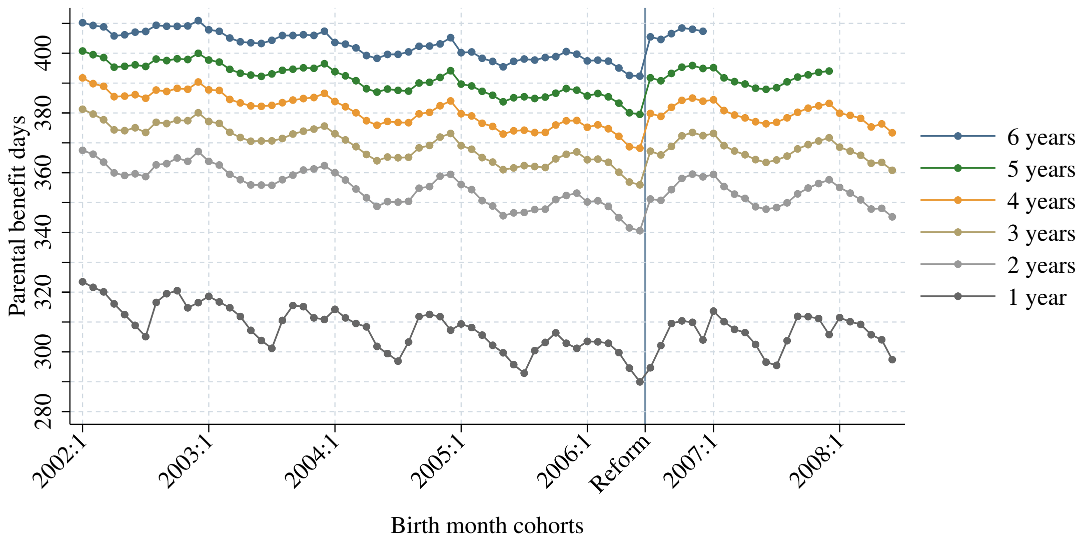
Regardless, multiple-birth parents also enjoyed an increase in the reimbursement rate and since the Agency's data includes multiple-birth children I will also consider a sample based on administrative data including multiple-birth children. The Agency's data contains information based on leave-taking for 311 639 children. When imposing what should be the same sample restrictions used to generate the Agency's data on the administrative data, I am left with 310 203 children. In this sample, I have not excluded the children with missing values in parental characteristics etc. Recall that the Agency's data included children for which leave is taken at some point. A possible explanation for the difference in sample size is that I drop children with leave taken in later years which I am not able to follow, while the Agency has this information and therefore includes those children. I am however not able to confirm this.

[4] To illustrate with an example, parents become entitled to a total of 480 income-related days and 180 minimum level days given twins. They may choose to take leave simultaneously, but if they do not and one of the parents registers leave-taking for only one of the children, it can look like there is no leave taken for the other.

### 4.2 Visual Inspection of Data

To familiarise with the data and empirical setting, I here present some time series plots of the data aggregated to the birth month level and discuss some properties with important implications for my estimation strategy. Making headway, I start with Figure 1 which is based on the administrative data for the 600 956 children born from January 1, 2002 to June 30, 2008. This figure shows the average number of days taken for a child, regardless of whether parents ever took leave. Specifically, it depicts the yearly cumulative sum of parental benefit days taken for children born in a particular month and year, divided by the number of children in that period.

Figure 1: Average Take-up of Parental Benefits for a Child

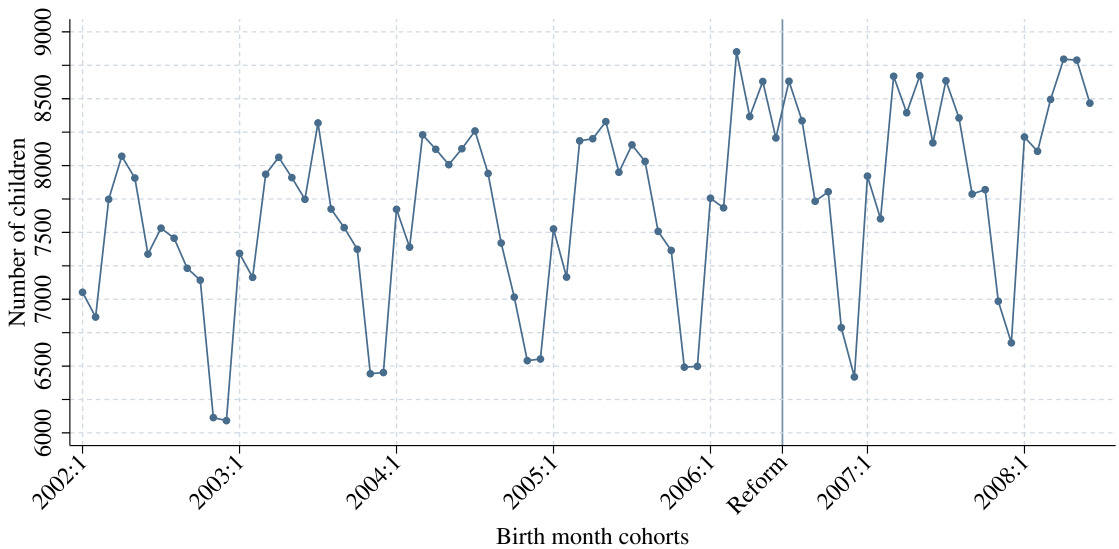



*Note:* The figure depicts the average cumulative take-up of parental benefits for a child based on administrative data containing information on the 600 956 children born to a biological mother and father in Sweden from January 1, 2002 to June 30, 2008.

One notable observation is that the take-up of benefit days stabilises during a child's second year of life. The main reason for this is that parents are entitled to their usual amount of work-related vacation days, which they can take under the condition of not using parental benefits simultaneously. Furthermore, we see that parents save some benefit days for later utilisation, which becomes evident when studying the uptake for the remaining years. Figure 1 also reveals a decreasing trend in the uptake of parental benefits with the level shifting downward yearly, albeit modestly. More strikingly is the clear seasonal pattern depending on a child's birth month. The average take-up in a given birth year typically starts at a high level for children born in January. The uptake then decreases during the spring and reaches the lowest levels during the summer months. The take-up increases for children born during the autumn and stops with December that on average takes the same number of days as the January cohort in the same year. This is not the case for the birth year 2006 where we find significant a jump in the level of take-up following the reform, indicated by the solid vertical line placed between June and July.

There are several plausible explanations for such seasonality. These include national holidays, vacation times and kindergarten admissions. The decreasing trend might relate to shifts in attitudes towards leave-taking or possibly the increase in real wages making it less attractive to take leave. In addition, these patterns could be related to a child's timing of birth and parental characteristics. Figure 2 uncovers clear seasonality in the number of single-birth children considered in the main sample. We also find an increasing trend in the number of children born.

Figure 2: Number of Children in the Main Sample

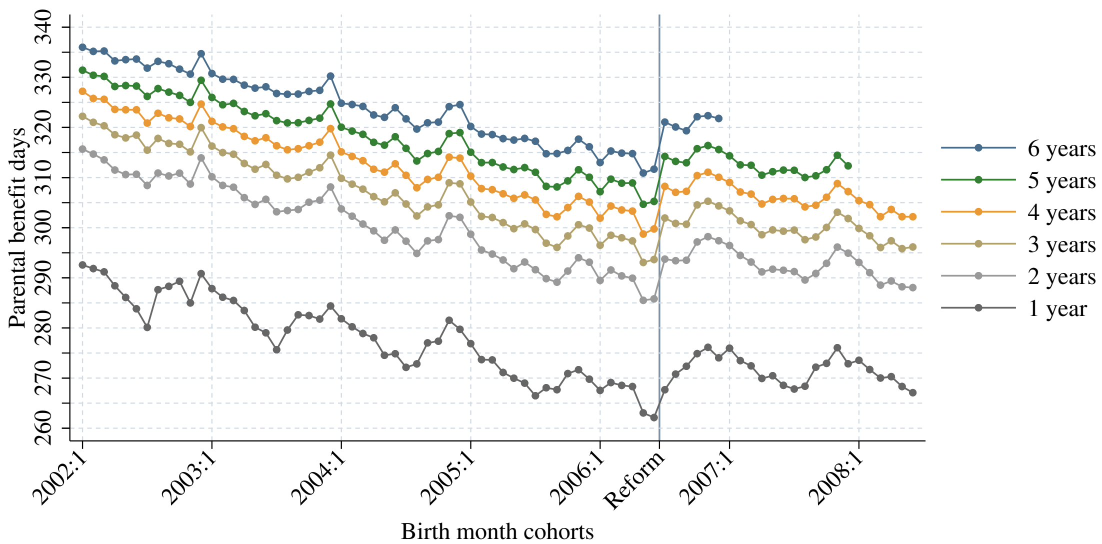



*Note:* The figure depicts the birth month aggregate number of children corresponding to the sample in Figure 1.

The number of children born in a given month is not necessarily a concern for modelling, but the composition of parents in the monthly cohorts can be. If parents differs in some sense related to the uptake of parental benefits, any estimated effect might as well just be the result of such differences if not modelled correctly. Indeed, Figure A1 in the Appendix demonstrates the possibility of this being a concern for my investigation of causal effects as I find a correlation between the number of children born and the share of children with native-born parents. In addition, I have included figures depicting the share of children with married parents and parental income measured in the year before the birth year of a child. The reason for including them is to illustrate that these variables are probably not good control variables. Recall that the majority of the variables are recorded yearly. As such, the differences found between children born early in the year and children born later in the year can just be the result of the time difference to the date that values are recorded.

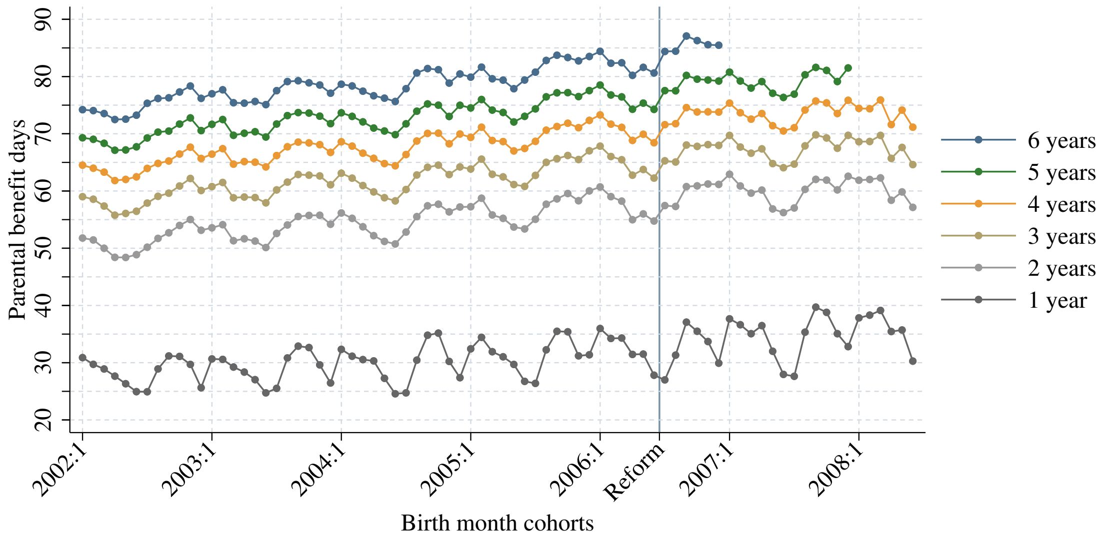
Consider next Figures 3 and 4 which depict the uptake of paid parental leave for mothers and fathers, respectively. Figure 3 discloses a remarkable similarity between mothers' take-up and total take-up for a child, depicted in Figure 1 above, although mothers' take-up is at lower levels. We see a shift in the level of take-up following the reform of similar magnitude to the shift in total take-up for a child in Figure 1. The same cannot be concluded for fathers when studying Figure 4. The uptake of fathers is slightly increasing over birth years and there is no clear shift in the level of take-up following the reform, at least not to the unaided eye. These differences bolster the necessity of studying the responses over genders and not only the total take-up for a child. Moreover, Figures 3 and 4 reveal, to no surprise, that mothers indeed take the lion's share of parent benefits.

Figure 3: Average Take-up of Parental Benefits by Mothers




*Note:* The figure depicts the average cumulative take-up of parental benefits by mothers for the 600 956 children in the main sample.

Figure 4: Average Take-up of Parental Benefits by Fathers




*Note:* The figure depicts the average cumulative take-up of parental benefits by fathers for the 600 956 children in the main sample.

Finally, I also consider plots based on the aggregate data from the Agency. To avoid overwhelming the reader with more figures at this point, I display them in the Appendix. However, I will briefly comment on them here. Figures A4-A12 depict the average take-up of minimum level days, SGI level days, and guarantee level days, in total for a child and the uptake by mothers and fathers, respectively. The figures depicting the take-up of minimum level days reveal a discontinuous shift in the level of average uptake following the reform, similar to the shift found in the figures above. Studying the plots separately for mothers and fathers suggests that this jump is mainly accounted for by mothers, but there is a possibility of a slight increase in the level for fathers as well. Furthermore, we can observe that the cumulative take-up by mothers is above the 45 days assigned to each parent given joint custody. It is unclear whether this is the result of parents transferring days between each other. There may be single mothers entitled to the full 90 days, which could account for the high levels.

Upon studying the figures depicting the take-up of SGI level days, I do not see any jumps in the take-up rates following the reform. This suggests that the rise in the SGI cap did not impact take-up in a discontinuous manner related to a child's birth month. Next, I consider the figures depicting the average take-up of days compensated at the guarantee level. I find a clear negative correlation between the take-up of guarantee level days and

SGI level days, which is logically related to the condition that parents have to work for 240 days before the birth of their child to qualify for reimbursement at the SGI level for the first 180 days. This is further supported by the possibility of a decreasing trend in parental incomes within a given birth year, as depicted in Figure A3. However, a note of caution is warranted since it might just be a time-lagged difference, as previously explained. It does not seem to be a discontinuous jump in the take-up level for all birth month cohorts following the reform, but it is rather difficult to confirm based on only three years of data. Nonetheless, we see a clear seasonality in the take-up of guarantee level days, given a child's birth month. This seasonality must be taken into account when exploring the causal effects of the minimum level days reform.

In conclusion, the plots generated from the Agency's aggregate data suggest that the discontinuous shift in the level of leave-taking based on administrative data is likely due to an increase in the take-up of minimum level days. Furthermore, the plots clearly show seasonality not only in overall leave-taking but also in parental characteristics and those eligible for reimbursement at the SGI level.

### 4.3 Empirical Strategy

The discontinuous nature of the minimum level days reform is an attractive feature in learning the average casual response to becoming eligible to receive a higher reimbursement rate of parental benefits. However, exploiting this reform to estimate the average causal effect on the take-up of parental benefits given the empirical setting and data at hand requires careful consideration of modelling assumptions. To help make sense of issues faced and related modelling choices, I start by formulating the ideal population regression model and take it from there. I let $Y_i$ denote the observed take-up of parental benefits, or "outcome", of child $i$, for $i = 1, 2, ..., N$. Utilising the binary property of minimum level days reform, I define an eligibility indicator, $Eligible_i$, to take the value 1 for a child born on or after July 1, 2006, and 0 otherwise; formally, $Eligible_i = 1[\text{child's birthday} \geq \text{July 1, 2006}]$. Provided the simplifying assumption of eligibility effects being constant for all children, the average causal effect of interest could in the ideal case be recovered by estimating the following linear regression equation

$$Y_i = \alpha + \rho Eligible_i + \varepsilon_i \tag{1}$$

with $\alpha$ denoting the intercept, $\rho$ the average causal effect and $\varepsilon_i$ the regression residual. Given that a child's eligibility status is randomised, or as if randomised, estimating Equation

1 could potentially have given us a good idea of the average treatment effect. However, without a continuous measure of time of birth to employ a proper regression discontinuity design, this equation is of little help with regard to returning an estimate of $\rho$ which is not biased by confounding factors. The main concern here is of course issues related to the composition of individuals on either side of the eligibility cutoff and that seasonality will plague the estimate.

To disentangle the seasonal effects from an estimate of the causal effect of interest, I have to precisely control for all factors affecting the take-up of parental benefits. However, because I only have data for the birth week of a child and the seasonal factors affecting the take-up of parental benefit can vary at a daily level, for example, with a national holiday in the middle of the week, I am unable to exactly control for such factors. Regardless, it would be very difficult, not to say impossible, to gather all relevant information on such factors, including, for example, kindergarten admissions which vary at the kindergarten level. Instead, I assume the seasonal factors to be fixed given a child's birth season. For my main analysis, I let this season be the birth month to facilitate a comparison between the estimates based on administrative data and the Agency's data. To check on robustness, I will also consider a model with birth week fixed effects, but more on this later.

My strategy aims to estimate the difference in average take-up between eligible and non-eligible children born in the reform year 2006. To purge the estimate of seasonal effects, I include children born in other years and estimate a model with birth month and birth year dummy variables similar to the one in Liu and Nordström Skans (2010). The regression equation of interest is

$$Y_{imy} = \alpha + \mu_m + \mu_y + \rho Eligible_{imy} + \varepsilon_{imy} \tag{2}$$

with $\mu_m$ and $\mu_y$ denoting the full set of dummy controls for birth months, $m$, and birth years, $y$, respectively. $\alpha$ denotes the intercept and $\varepsilon_{imy}$ the error term. Finally, $\rho$ denotes the effect of interest. Under the assumption that the seasonal effects are equal across birth years, the estimated difference in take-up of parental benefits should reflect the average causal response of becoming eligible to receive a higher reimbursement rate of the minimum level days.

Recall that we found considerable seasonality in the number of children born and several of the parental characteristics which might be related to parents' responses to the reform. By including the full set of birth month and birth year dummies, I also aim to control for the seasonality in parental characteristics which otherwise could have biased the estimate. Indeed, Buckles and Hungerman (2013) illustrate that controlling for the season of birth can make all the difference. To assess the robustness of my model, I will also consider a version of Equation 2 for which I include a vector of control variables for parental characteristics. If the estimates are impacted in a significant way upon including controls, it would imply that my estimates might be biased by omitted variables.

Estimating Equation 2 on individual-level data using OLS regression will return the desired estimates but accompanied by unreliable standard errors. The cause of concern is the underlying group structure in the model. More specifically, my model essentially depends on the average take-up in a given birth cohort, i.e. the unique combination of birth month and birth year. The concern is that of cohort-specific shocks, which the birth month and birth year controls would fail to account for. Given the existence of such shocks, the random errors of individuals could be correlated within birth month cohorts, yielding biased and inconsistent standard errors (Moulton 1986). To account for the possibility of cohort-specific shocks plaguing inference, I cluster the standard errors on the birth month cohort level when estimating the model with individual-level data.

Whether the clustered standard errors have good properties depends on the number of cohorts considered in the model. When presenting the results, I will start with the 600 956 children born to a biological mother and father in Sweden from January 1, 2002, to June 30, 2008. With birth month capturing seasonality, this leaves me with 78 clusters, all containing a sizeable number of children as depicted in Figure 2. As such, I expect the standard errors to have good properties. I will however consider other samples by imposing a couple of restrictions for reasons I will explain in the next section. It is worth mentioning that these restrictions result in a decrease in the sample size which can be a concern for the robustness of the standard errors, but I will discuss these issues when presenting the results.

Recall that the administrative data contains information only on the total number of days taken for a child and does not distinguish between day types. In order to examine whether the reform actually affected the up-take of minimum level days, I will consider a birth month average version of Equation 2. This enables me to estimate the reform effects based on the Agency's data which contains information on the birth month average take-up of minimum level days, SGI level days and guarantee level days, out of which I only expect the minimum level days to be impacted. The birth month average version of Equation 2 can be written as

$$\bar{Y}_{my} = \alpha + \mu_m + \mu_y + \rho Eligible_{my} + \bar{\varepsilon}_{my} \tag{3}$$

with a similar interpretation of the coefficients as the previous model. Note that estimating Equation 3 by weighted least squares using the number of children in each birth month as weights return the very same estimates as those returned by an estimation of Equation 2 on the individual level. Therefore, I estimate Equation 3 using weighted regression. I do this not only for the Agency's data containing information for the 311 639 children born from 2005 to 2007 but also for the 310 203 children in the administrative data to check on the robustness of my findings. Since the data used to estimate Equation 3 is collapsed on the birth month level, I no longer have to worry about within-cohort correlation. Hopefully, the finite-sample properties of regression with normal errors will kick in (Angrist and Pischke 2009). However, given the limited number of 36 birth month cohorts raises doubts about the robustness of the standard errors.

## 4.4 Samples

My main outcomes of interest are the yearly cumulative take-up of minimum level days and take-up in total, regardless of compensation level. The administrative data allows me to study the reform effect on total uptake for a child while the Agency's data allows me to study the take-up of minimum level days specifically. Since the Agency's data is aggregated to birth month level and contains only 36 observations I cannot profoundly evaluate the robustness of my results based on this data. Instead, I will extrapolate some important findings based on the administrative data to get a sense of the validity of the results based on the Agency's data. As mentioned above, I will first consider the 600 956 single-birth children born to a biological mother and father in Sweden from January 1, 2002, to June 30, 2008. Recall that the Agency's data includes multiple-birth children and that their parents face different rules. By excluding multiple-birth children, I examine whether the inclusion of these children affect the results significantly. I have also excluded the 53 411 children with missing values in parental characteristics or with parents that are not uniquely identified in the data to facilitate a comparison of the estimates returned when estimating the model with and without parental controls. Table A1 in the Appendix summarises the statistics for the 600 956 children. Note that I have divided the sample into two groups based on eligibility status so that readers better understand the composition of children on either side of the reform cutoff.

As alluded to above, I will also consider a more restricted sample based on the 600 956 children as a point of departure. The first restriction concerns the 3 525 children for which no leave is taken, or at least for the time that I can follow the take-up. It is not impossible that some of the parents simply do not find it worthwhile to take paid parental leave or do not have sufficient knowledge of the system. However, a more reasonable explanation is that the parents are not insured and covered by the Swedish social insurance system, which they would not be if they, for example, just moved back after working abroad for some time. Since it is not very interesting to study the null effect of parents who might not even be entitled to take leave, I will drop the 3 525 children for which no leave is taken. This will also give me an idea of how much the null take-up of children influence the results, which is important since the Agency's data includes only children with leave taken at some point.

I will also impose a sample restriction related to possible sibling-related spillover effects. Approximately 50 % of the children in the sample are siblings. Since parents cannot take leave for several children at the same time, it is reasonable to assume that they will choose to take leave for the child whom they receive the highest compensation for. However, since I lack a measure of parents' SGI levels I am unable to examine this further. Instead, I impose a restriction including only children to first-time parents, or at least the first child born to either parent as of 1990. In doing so, I also limit possible information bias such that first-time parents might be more aware of the new compensation rate of the minimum level days relative to parents with previous children who might think that the compensation rate remains at 60 SEK like for their preceding children. There are 269 342 children with a first-time father and 265 225 with a first-time mother. Note that children are not necessarily first-born to both of their parents. Including only the children with a pair of first-time parents and for which leave is taken at some point leaves me with 239 958 children.

Finally, I consider some other samples related to robustness checks and the above restrictions and concerns. Since I have laid out my reasoning, it should be easy to follow my arguments as we make progress through the remainder of this paper. Therefore, I save the readers from redundant descriptions at this point.

# 5 Results

In this section, I first present the reform effects on total take-up for children based on the administrative data and investigate the above discussed concerns. I then move on to the results based on the Agency's data which allows me to study the reform effects on take-up of minimum level days. In the third section, I evaluate the key identifying assumption.

Finally, I conclude the section with various robustness checks.

### 5.1 Reform Effects on Total Uptake of Parental Benefits

Before I start interpreting the results of the cumulative take-up for children, I will first discuss what type of effect the estimates reflect. It is not very likely that the minimum level days reform impacted any extensive margin since the majority of the income-related days are compensated at higher levels. Nevertheless, I examine the matter by estimating my main model with a binary variable indicating whether leave is ever taken for a child during its first four years of life. I do this for my main sample of consisting of 600 956 children and consider also the full sample of 654 367 children without dropping the 53 411 children with missing values in parental characteristics etc. The latter contains 8 067 children for which no leave is taken. I do this in total and for mothers and fathers separately. I find only minuscule estimates, non of which are larger than the absolute value 0.006, all accompanied by p-values well above the 0.10 significance level. As such, the reform likely affected only the intensive margin. Further, this implies that dropping children with null uptake should not be problematic.

Table 1 summarises the estimated reform effects on the cumulative take-up of parental benefits for the two samples based on administrative data. The first two panels display the estimated $\rho$ in Equation 2 including only birth month and birth year dummy controls. The third and final panel displays the estimated effects for the birth month and birth year fixed effects model but with additional controls for parental characteristics. Readers wishing to compare these estimates to estimates without any birth month and birth year controls, that is, the estimates of Equation 1, may visit Table A2 in the Appendix. The estimates of the latter are ultimately just the difference in means between eligible and non-eligible. To no surprise, the estimates are negative for take-up by mothers, reflecting the overall decrease in take-up over the years as found in the above figures. Fathers, on the other hand, have positive estimates, reflecting the increase in their take-up over the years.

Returning to Table 1, I start with the first panel displaying the estimated effects for the 600 956 children in the main sample. The estimated difference in total take-up during a child's first year of life following the reform amounts to 6.97 days, which is significant at the 0.001 significance level, just like all other estimates for total take-up and take-up by mothers. Studying the cumulative take-up for the following years, we see that the effect increases by 1.82 days to 8.79 days by the time the child turns two years old. The take-up increases by 0.10 days for the third year relative to the second, now amounting to 8.89 days.

Finally, during the child's fourth year of life, the estimated effect is 9.67 days, an increase of 2.47 days from the first year.

When partitioning the total take-up for a child by the take-up of mothers and fathers respectively, we see that it is the take-up by mothers that account for the overall increase. Interestingly, I find that fathers decrease their uptake by 1.05 days during a child's first year of life, as suggested by the estimate which is significant at the 0.05 significance level. It is not clear why this is the case. A possible explanation is that it results from mothers prolonging their leave during a child's first year of life so that fathers start taking leave later. Judging by the estimates for fathers' take-up for the remaining years, which are roughly null for the second and third year and finally 0.48 days during the fourth year, it does not seem that the reform affected fathers' overall uptake.

The second panel displays the estimated reform effects based on the restricted sample including only children to a pair of first-time parents and for which leave is taken at some point during the first four years. We see that the findings remain largely the same when comparing these estimates to those in the first panel. However, note that the new estimates are somewhat larger. Apart from this possibly being explained by dropping the children for which no leave is taken, it might also result from sibling spillover effects and information bias as previously discussed. I examine these possibilities by estimating my model with three sub-samples based on the main sample. Table A3 in the Appendix displays the results of this exploration. First, I exclude only the children for which no leave is taken and find, to no surprise, that the estimated effects are larger; approximately 0.2 days larger than the main ones. Second, I sub-set children with a pair of first-time parents but impose no restriction regarding leave ever being taken for the child. Studying these estimates suggest that it is the first-time parents, particularly mothers, that mainly account for the increase in Table 1 with estimates that are approximately 0.8 days larger. This suggests that sibling-related spillover effects should be of little concern. However, the increase might be related to information bias. I examine the matter further by sub-setting children with a pair of second-time parents. The estimates are only scantly lower than the main ones, which might have to do with some eligible parents not being aware of the new compensation rate. Regardless, the fact that the estimated effects remain largely the same for the variety of samples suggests that the estimates for the main sample are not likely that biased by differences in information and siblings.

Table 1: Reform Effects on Uptake of Parental Benefits

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample (600 956): | | | | |
| Total take-up for child | 6.97*** | 8.79*** | 8.89*** | 9.67*** |
| | (0.65) | (0.69) | (0.71) | (0.67) |
| Take-up by mother | 8.02*** | 8.75*** | 8.88*** | 9.19*** |
| | (0.71) | (0.74) | (0.76) | (0.75) |
| Take-up by father | -1.05* | 0.03 | 0.01 | 0.48 |
| | (0.48) | (0.48) | (0.43) | (0.39) |
| Restricted sample (239 958): | | | | |
| Total take-up for child | 7.64*** | 9.62*** | 9.73*** | 10.37*** |
| | (0.92) | (0.78) | (0.75) | (0.71) |
| Take-up by mother | 8.70*** | 9.39*** | 9.64*** | 9.75*** |
| | (0.99) | (1.00) | (1.01) | (1.02) |
| Take-up by father | -1.07 | 0.22 | 0.10 | 0.62 |
| | (0.72) | (0.56) | (0.54) | (0.57) |
| Including controls for the restricted sample (239 958): | | | | |
| Total take-up for child | 7.20*** | 9.35*** | 9.47*** | 10.13*** |
| | (0.95) | (0.81) | (0.76) | (0.72) |
| Take-up by mother | 8.16*** | 8.91*** | 9.18*** | 9.32*** |
| | (0.94) | (0.95) | (0.97) | (0.98) |
| Take-up by father | -0.97 | 0.43 | 0.30 | 0.82 |
| | (0.69) | (0.50) | (0.48) | (0.52) |

*Notes:* This table displays the estimated effects of the reform on the take-up of parental benefits for the two samples based on administrative data. The first two panels display the estimated coefficient $\rho$ in Equation 2 with birth month and birth year fixed effects. The third panel displays the estimated effects for the restricted sample upon including control variables indicating whether the child's parents are married, native-born, have a post-secondary education, parents' age and parents' yearly incomes and wage earnings measured the calendar year before the child's birth year (in addition to birth month and birth year fixed effects). The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth month level, a total of 78 clusters.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

However, there is a possibility of omitted variables biasing the estimates. To examine this, I have estimated my fixed effects model with a vector of controls indicating whether the child's parents are married, have a post-secondary education, are native-born, their age and yearly incomes and wage earnings. I do so for both the main sample and the restricted sample but choose to display only the latter as these estimates are the ones that are impacted the most. The results are displayed in the final panel in Table 1. Comparing these estimates to the estimates returned for the very same sample but without any additional controls (displayed in the second panel), we see that the estimated effects are not impacted to a significant degree. The estimated effects for mothers decrease by approximately 0.6 days and increase for fathers by approximately 0.2 days. These differences are very small in comparison to the corresponding estimates without any fixed effects, displayed in Table A2. In the latter, mothers' estimates increase by approximately 2.5 days and fathers' decrease by approximately 1 day upon including parental control variables. This suggests that the fixed effects model is successful in capturing at least some of the seasonality related to parental characteristics and timing of birth. However, without additional control variables, I cannot further assess the severity of omitted variable bias.

While the estimates change only scantly, it is still interesting to understand why this is. Trying out various specifications, I find that it is the education variables that account for the differences. Apart from this indicating that parents with higher education take fewer benefit days, it suggests that my simplifying assumption of constant treatment effects does not hold perfectly. I have included the estimated effects based on the main sample but partitioned the children by their parents' educational attainment. The results are displayed in Table A4 in the Appendix. We see that the reform effects on children with lower-educated mothers are roughly twice the size of the effects estimated for children with higher-educated mothers. A possible explanation for this is that lower-educated mothers earn on average less than higher-educated mothers and that their opportunity costs differ for this reason. Another explanation is the existence of differences in gender norms regarding parenting associated with educational attainment.

### 5.2 Reform Effects on Uptake of Minimum Level Days

It would of course be interesting to further analyse heterogeneous treatment effects by estimating other models. However, recall that the administrative data does not provide information on which type of days parents take. As such, it is difficult to say whether it is the uptake of minimum level days that are impacted. To assess this, I must use the Agency's data which is aggregated to the birth month level and provides no additional information on parental characteristics. Since this data does not allow me to study heterogeneous responses, I continue my exploration assuming constant treatment effects and estimate the birth month average version of my model, captured by Equation 3.

Recall that the Agency's data includes only the 311 639 children born in Sweden during 2005 and 2007 for which leave is taken at some point, although without the restriction of single-birth children. For consistency, I also include multiple-birth children and construct a similar sample based on the administrative data, resulting in a total of 310 203 children. Table 2 displays the results, starting with the estimates based on the Agency's data and ending with the administrative data. The Agency's data allows me to study the average take-up of all types of days, in total and by mothers and fathers respectively. To apprehend the magnitude of the effect, I will relate my estimates to the mean take-up for non-eligible children born in the reform year, i.e. children born between January 1, to June 30, 2006. The baseline means are summarised in Table A5.[5]

Studying the estimated effects based on the Agency's data in Table 3, we see that the total take-up of minimum level days increases by 3.65 days during a child's first year of life following the reform. Relative to the baseline mean of 8.16 minimum level days, this translates into a 45 % increase, but relative to the total take-up of 249.81 days, this corresponds to a 1.5 % increase. Next, we see that the take-up increases by 4.64 days during a child's second year, now amounting to 8.29 days, and continues to increase, although only slightly and ends with the estimate of 9.70 days by the time the child turns five. For the latter years, the relative effects amount to approximately a 20 % increase in terms of minimum level days and 2.5 % in terms of total uptake.

Judging by the estimates of mothers and fathers respectively, it is still the mothers that account for the majority of the increased take-up of minimum level days. However, now that we can study the take-up of minimum level days specifically, we see that the responses over gender are not very different. The take-up of minimum level days by mothers during a child's first year increased by 3.42 days which corresponds to a 44 % (1.5 %) increase relative to the baseline mean of 7.70 minimum level days (226.92 days in total take-up). During a child's fifth year, the take-up by mothers increased to 7.88 days, which translates to a 20 % (2.5 %) increase. Looking at the take-up of minimum level days by fathers, we have an increase of 0.23 days during a child's first year of life, which corresponds to a 50 % (1%) increase relative to the baseline mean of 0.46 minimum level days (22.89 days in total take-up), and during a child's fifth year fathers' take-up increased to 1.81 days, corresponding to a 25 % (2.4 %) increase. Therefore, if one takes into account the relative difference in take-up over gender, we see that the responses to the reform are quite similar.

Note that the statistically significant estimates of the total take-up for children are larger than the estimates of the minimum level days, especially during a child's first year of life.

[5] I have also included the means corresponding to the main sample for readers wishing to compare the findings in the first panel in Table 1. But to summarise, the total take-up for eligible children increased by roughly 2.5 % following the reform.

The estimates on the other levels roughly add up to the estimated effects for total take-up. Although not statistically significant, these estimates illustrate that the estimated effects based on administrative data should be interpreted with caution. The magnitude of the estimated effect during a child's first year of life based on administrative data is particularly questionable since more than half of the effect might be accounted for by the increase in uptake of SGI level days. While the estimated difference of 4.51 days is not statistically significant, it might be the result of the rise in the SGI cap, even though the reform did not have a discontinuous nature. However, the difference in take-up of SGI level days then decreases to 1.73 days the following year and continues to decrease until it reaches 0.75 days by the time the child turns five. Therefore, it does not seem that either of the two reforms affected the take-up of SGI level days discontinuously. But without a variable indicating day type in the administrative data, I cannot confirm my suspicion.

To get a sense of how much I can extrapolate from my findings based on administrative data, I now compare the estimates based on the Agency's data to those based on the 310 203 children in the administrative data, displayed in the final panel of Table 2. We see that the estimates based on the Agency's data are up to 1.8 larger for the total take-up and the take-up by mothers, and up to 0.5 larger for take-up by fathers. A plausible explanation for such inconsistency relates to the differences in how parental leave is measured. The Agency has information on children's exact birthdays and can therefore measure the leave-taking more exactly while the calculation for children in the administrative data is based on the number of days taken for leave spells starting within some weeks from a child's birth week. It is however not impossible that this inconsistency is also accounted for by differences in the individuals included in the respective samples, since they differ by 1 436 children. Moreover, when dropping multiple-birth children from the administrative sample, the estimates decrease by up to 0.3 days. It is therefore unclear how much multiple-birth children affect the results, but at least they increase the estimated effects.

Despite the small inconsistencies, the fact that the results are very similar using the two data sets suggests that we can extrapolate some important findings based on administrative data to the results based on the Agency's data. More specifically, it should not be problematic to drop children without any leave taken nor include multiple-birth children. Moreover, it seems omitted variables are of little concern as well as information bias and sibling-related spillover effects. However, recall that these estimates hinge on the assumption that seasonal effects are equal across birth years. Therefore, I explore the validity of this assumption in the next sub-section.

Table 2: Estimates Based on the Agency's Data

| Cumulative take-up (years): | 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|---|
| Data from the Agency (311 639 children) | | | | | |
| Minimum level days for child | 3.65*** | 8.29*** | 8.73*** | 9.38*** | 9.70*** |
| | (0.22) | (0.35) | (0.34) | (0.36) | (0.33) |
| SGI level days for child | 4.51 | 1.73 | 0.79 | 0.98 | 0.75 |
| | (2.52) | (1.18) | (1.14) | (1.21) | (1.15) |
| Guarantee level days for child | 0.24 | 0.64 | 0.79 | 0.94 | 0.89 |
| | (0.77) | (0.80) | (0.80) | (0.81) | (0.81) |
| Total take-up for child | 8.39*** | 10.66*** | 10.32*** | 11.30*** | 11.34*** |
| | (2.27) | (0.86) | (0.89) | (0.96) | (0.90) |
| Minimum level days by mother | 3.42*** | 6.97*** | 7.22*** | 7.64*** | 7.88*** |
| | (0.19) | (0.31) | (0.31) | (0.33) | (0.31) |
| SGI level days by mother | 4.68 | 1.95 | 1.44 | 1.27 | 1.23 |
| | (2.38) | (1.30) | (1.30) | (1.33) | (1.32) |
| Guarantee level days by mother | 0.32 | 0.58 | 0.71 | 0.82 | 0.83 |
| | (0.73) | (0.74) | (0.72) | (0.73) | (0.73) |
| Total take-up by mother | 8.42*** | 9.50*** | 9.37*** | 9.73*** | 9.94*** |
| | (2.15) | (1.03) | (1.11) | (1.17) | (1.13) |
| Minimum level days by father | 0.23*** | 1.32*** | 1.52*** | 1.74*** | 1.81*** |
| | (0.05) | (0.14) | (0.14) | (0.15) | (0.16) |
| SGI level days by father | -0.18 | -0.22 | -0.65 | -0.29 | -0.48 |
| | (0.40) | (0.53) | (0.55) | (0.51) | (0.52) |
| Guarantee level days by father | -0.08 | 0.06 | 0.08 | 0.12 | 0.07 |
| | (0.09) | (0.10) | (0.11) | (0.13) | (0.13) |
| Total take-up by father | -0.03 | 1.16 | 0.95 | 1.57** | 1.40* |
| | (0.39) | (0.58) | (0.56) | (0.50) | (0.53) |
| Administrative data (310 203 children) | | | | | |
| Total take-up for child | 6.61*** | 8.90*** | 8.82*** | 9.62*** | 9.75*** |
| | (0.85) | (0.59) | (0.62) | (0.65) | (0.57) |
| Take-up by mother | 7.32*** | 8.11*** | 8.19*** | 8.47*** | 8.62*** |
| | (0.93) | (0.79) | (0.82) | (0.86) | (0.80) |
| Take-up by father | -0.82 | 0.68 | 0.52 | 1.05 | 1.03 |
| | (0.67) | (0.59) | (0.56) | (0.51) | (0.54) |

*Notes:* This table displays the estimated $\rho$ in Equation 3 for birth month cohort average take-up based on the Agency's data (estimates on the first 12 rows) and administrative data (final three rows). The samples include all children born in Sweden from 2005 to 2007, including multiple-birth children, for which leave is taken at some point. The columns indicate the cumulative take-up over a child's first five years of life. All estimates include birth month and birth year controls and the regression is weighted by the number of children in each cohort. Normal standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

### 5.3 Evaluation of the Key Identifying Assumption

To assess whether the seasonal effects are equal across birth years, I examine a transformation of my data. Recall Equation 3, the birth month average equation

$$\bar{Y}_{my} = \alpha + \mu_m + \mu_y + \rho Eligible_{my} + \bar{\varepsilon}_{my}$$

which is merely estimated with a single time series consisting of birth month average uptakes ordered consecutively in time. The idea is to take the seasonal difference, that is, the 12-month lagged difference, of the time series to eliminate the birth month seasonal effects. In terms of my model, this translates to

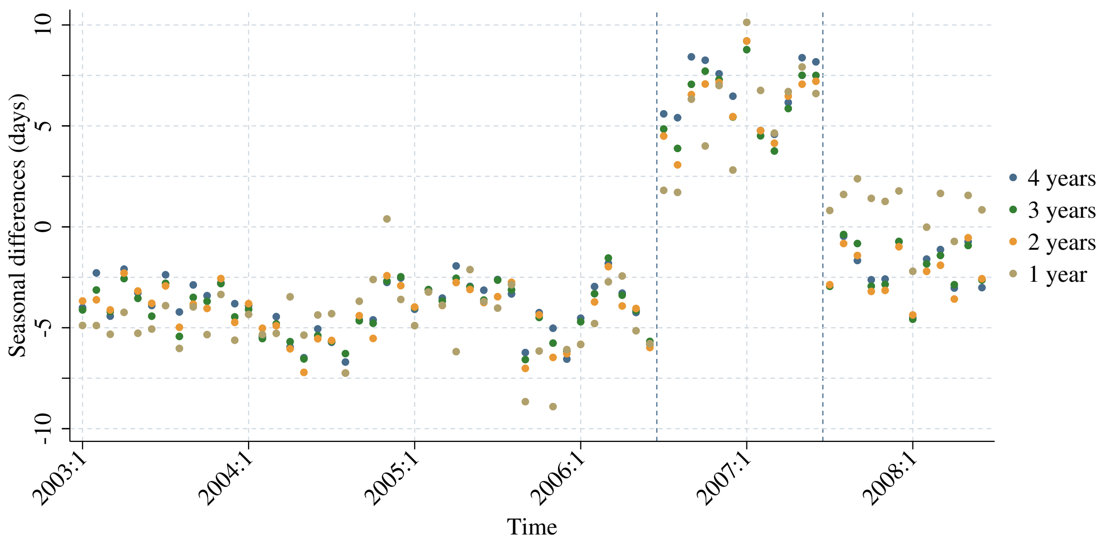
$$\bar{Y}_{my} - \bar{Y}_{m(y-1)} = \mu_y - \mu_{(y-1)} + \rho(Eligible_{my} - Eligible_{m(y-1)}) + \bar{\varepsilon}_{my} - \bar{\varepsilon}_{m(y-1)} \quad (4)$$

where the transformation eliminates not only the birth month fixed effects, $\mu_m$, but also the intercept, $\alpha$. Note that when taking the seasonal difference of the eligibility indicator it becomes an indicator taking the value 1 only from July 2006 to June 2007, and 0 otherwise. Intuitively, I expect to find reform effects, $\rho$, only for the seasonal differences from July 2006 to June 2007, that is, the only differences involving a pair of eligible and non-eligible cohorts. For all other periods, it is a manner of taking the difference between a pair of non-eligible cohorts or a pair of eligible cohorts.

A nice feature of this exploration is that we can visually study whether the assumption seems to hold. It also allows us to visualise the reform effects. Figure 5 presents the transformations of the total take-up for a child based on the main sample using administrative data, that is, the transformations of the time series depicted in Figure 1. The data points are divided into three segments. The first segment includes the seasonal differences involving a pair of non-eligible cohorts, starting with the difference of $(\bar{Y}_{2003:1} - \bar{Y}_{2002:1})$ labelled "2003:1". The data points within the distinct horizontal lines are the seasonal differences involving a pair of eligible and non-eligible cohorts; the only data points for which the transformed eligibility indicator takes the value 1. The final segment includes data points of the seasonal difference involving a pair of eligible cohorts.

Studying Figure 5, we see that the data points prior to the reform are almost exclusively negative. This reflects the yearly decreasing trend in the take-up of parental benefits found in Figure 1. Following the reform, we find a significant jump in the level with the data points now taking only positive values, clearly illustrating the impact of the reform on the take-up of benefits. In the final segment, we again find negative values, suggesting that the downward sloping trend persisted following the reform. However, it seems that the take-up decreases less relative to the periods prior to the reforms since the data points are closer to zero and some are even positive during a child's first year of life. Thus, we might be dealing with a structural break in the trend following the reform, but it is rather difficult to confirm this with the limited number of observations.

Figure 5: Seasonal Difference for Total Take-up of Parental Benefits for a Child

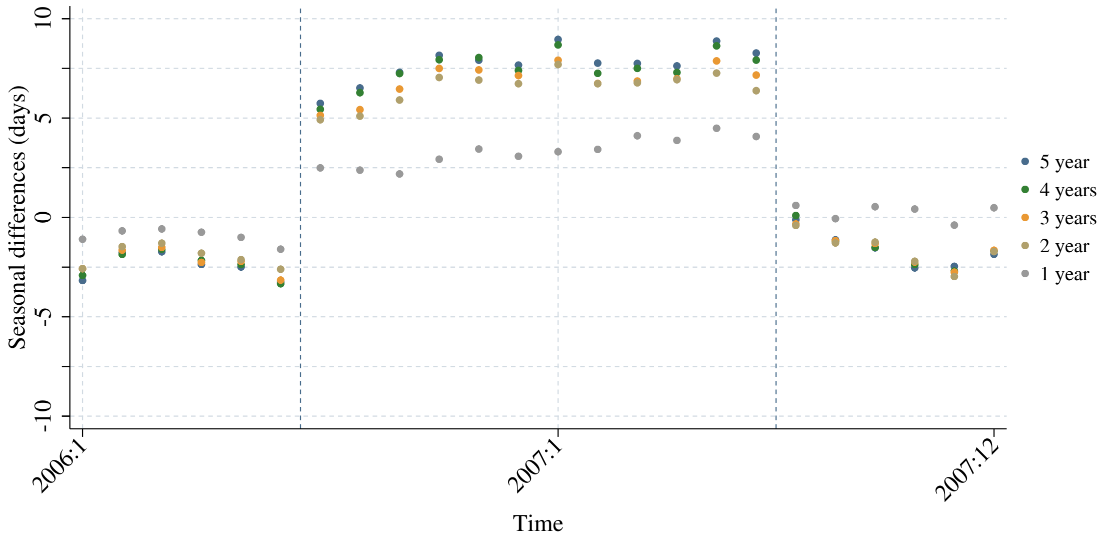



*Note:* This figure depicts the seasonal differenced time series of birth month average take-up of parental benefits for the main sample consisting of 600 956 children based on administrative data. The distinct horizontal lines indicate the periods in which the transformed eligibility indicator takes the value 1.

I now return to the evaluation of the key identifying assumption. Had the seasonal effects been equal across birth years, taking the seasonal difference should have resulted in the data points being ordered on horizontal lines given yearly intervals but allowing for a change in the level for the periods from July 2006 to June 2007. Put differently, if the seasonal effects were constant over years, taking the difference between any pair of seasonal differences within a birth year should equal zero. Studying Figure 5, we see that the idealised assumption does not hold perfectly since the data points follow a fluctuating pattern. This suggests that the precise magnitude of my estimates should be interpreted with caution, especially the estimates for take-up during a child's first year as they vary more relative to the other years. To see this, visit Figure A13 in the Appendix in which I have excluded the transformation of take-up during the first year.

I do the same for the take-up of minimum level days based on the Agency's data. The transformations are depicted in Figure 6. It seems that the differences fluctuate less in comparison to the transformations depicted in Figure 5. This is not very surprising since the raw data plot for the uptake of minimum level days depicts relatively less seasonality compared to the corresponding plots for uptake of SGI level days and guarantee level days (see Figures A4, A7 and A10 in the Appendix). Alas, with only 36 observations in the raw data and 24 remaining after taking the seasonal difference it is difficult to evaluate the underlying assumption further. Nevertheless, the striking change in the level of differences for the eligible periods compared to the rest clearly illustrates that the reform indeed impacted the uptake of minimum level days.

Figure 6: Seasonal Difference for Total Uptake of Minimum Level Days for a Child




*Note:* This figure depicts the seasonal differenced time series of birth month average take-up of minimum level days for the 311 639 children based on the Agency's data. The distinct horizontal lines indicate the periods in which the transformed eligibility indicator takes the value 1.

While the purpose of this exploration is mainly to evaluate the key identifying assumption, I also estimate the eligibility effects based on Equation 4. This estimation will not return the same point estimates as those returned by the original model since I now use transformed time series. Regardless, they should be similar in magnitude. Note that because I have taken the seasonal difference of the time series, by construction there is now autocorrelation in the error structure. Applying the Newey-West estimator could potentially address this issue, but the limited number of 66 observations raises concerns about the reliability of the standard errors. Instead, I estimate my model using OLS because the normal standard errors are larger than those corrected for autocorrelation. But readers may note that the significance level changes only for the estimates of father's take-up during the first and fourth year, as they become significant subject to the Newey-West estimator.

The results are presented in Table 3. To save space, I display only the estimates corresponding to those in the first panel in Table 1, that is, the estimated effects for the main sample but similar conclusions are made for other samples. Comparing the estimates returned by the transformed model to those returned by the original model, we see that the estimates are very similar in magnitude, but with some variability as expected. I would like to emphasise the importance of this finding. Not only does it strengthen the validity of my main findings, but the fact that I roughly get the same results using only 66 observations in contrast to the 600 956 children implies that the variation of interest can be captured on birth month level, which is the level available to me in the Agency's data.

Table 3: Reform Effects Given the Seasonal Differenced Model

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample: | | | | |
| Total take-up for child | 6.99*** | 9.21*** | 8.97*** | 9.72*** |
| | (1.12) | (0.87) | (0.87) | (0.81) |
| Take-up by mother | 7.89*** | 8.57*** | 8.60*** | 8.89*** |
| | (1.24) | (1.19) | (1.21) | (1.16) |
| Take-up by father | -0.91 | 0.64 | 0.36 | 0.84 |
| | (0.54) | (0.61) | (0.60) | (0.62) |

*Notes:* This table displays the estimated reform effects captured by Equation 4 which includes only birth year controls. The model is estimated using the seasonal differenced data of birth month average take-up for the main sample consisting of 600 956 children, resulting in an estimation involving a total of 66 observations. Normal standard errors in parentheses.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

### 5.4 Robustness

In this final sub-section, I further assess the robustness of my findings with a series of specification checks and a falsification test. However, before doing so I will first briefly discuss the robustness of the standard errors. Although the estimated effects returned by the main models with birth month and birth year fixed effects are based on cross-sectional data of children entering the models only once, clustering the standard errors might not suffice. The cause of concern is the possibility of autocorrelation remaining in the error structure related to, for example, the siblings in the data. Rest assured, I have chosen to display my results which are accompanied by the largest standard errors, that is, the cluster robust standard errors.

Applying a Newey-West estimator on the cohort average version of my main model for the samples considered in Table 1 lowers the standard errors by 0.2 days on average, naturally with the point estimates remaining at their exact level by estimating with weights based on the number of children in each birth month cohort, as long as no parental controls are included. The results are displayed in Table A6 in the Appendix. The only difference in terms of statistical significance is that a couple of estimates for take-up by fathers change. The standard errors are not very sensitive to the selection of lag order. Similar conclusions are drawn for the estimates in Table 2.

Recall that I excluded the 53 411 children from the main sample if they have missing values in parental characteristics or parents that are not uniquely identified in the data to facilitate a comparison of the estimates returned when estimating the model with parental controls. Dropping these children can be problematic for the validity of my findings if there is a systematic difference between parents with missing values that affect the estimated effects. To assess this issue, I estimate my model including the 53 411 children in addition to the 600 956 children in the main sample. Note that 7 029 children in this sample do not have a biological father registered and therefore miss values for paternal uptake and characteristics. I therefore start with a sample consisting of children born to a biological mother and move on to a sample consisting of children with a pair of biological parents. I further impose restrictions in terms of leave being taken during the first four years and that the child has a pair of first-time parents. I do this in an additive step-wise manner to see whether the previous findings hold. The results are displayed in Table A7 in the Appendix. We see that the results remain largely the same, suggesting that dropping the 53 411 children is not problematic.

Next, I check whether the results remain the same when estimating my main model but controlling for birth week instead of birth month. Table A8 in the Appendix displays the results for the same samples as in Table 1. We see that the results are very similar to the ones in Table 1, although with smaller standard errors which are now based on 338 clusters, possibly reflecting that weekly data captures seasonality better. I also check whether the extrapolation is heavily influenced by the observations closest to the reform date. I do this by excluding children born in June and July in any year and find that the results are not very sensitive to the exclusion of them. To see this, visit Table A9 in the Appendix. Moreover, binning the endpoints in different ways does not change the results significantly. Finally, I also estimate my model with an artificial eligibility cutoff set to July 1, 2005, and exclude the children born after June 30, 2006, to limit the actual effects of the minimum level days reform. The results are displayed in Table A10 in the Appendix. I find only small estimates, the majority of which are accompanied by very large p-values. There are a couple of significant estimates, but we know from before that the idealised assumptions do not hold perfectly. I have also estimated other artificial cutoffs on both sides of July 1, 2006, and find only very small and mostly non-significant estimates. It is very encouraging that no other artificial treatment effects are even close to the magnitude of the estimated reform effects.

Overall, the various specification checks and falsification tests in this section corroborate the validity of my main results. However, it is important to keep in mind that my findings hold conditionally on birth season and birth year fixed effects. Given richer data, it would be beneficial to employ a conventional regression discontinuity design to check whether the estimated effects hold locally for children born close to the cutoff on July 1, 2006. I have estimated the average treatment effects for a couple of local randomisation approaches based on a child's birth week and birth month. While I do find reform effects of similar magnitude to the above findings, these strategies are rather unwieldy given the size of the mass points in the discrete running variables. Since I am unable to thoroughly evaluate the robustness of the results based on the local randomisation approach, I see no point in displaying the results. I will conclude and discuss some remaining questions in the next and final section of this thesis.

## 6 Conclusion

The Swedish parental leave scheme is one of the most generous in the world, particularly in terms of the number of paid parental leave days available to parents after having a child. However, a significant number of parental benefit days remain unused. This could be due to the high opportunity cost of not going to work. To delve deeper into this possibility, my study examines the impact of increasing the reimbursement rate for minimum level days on the uptake of parental benefits.

Based on quasi-experimental designs, my findings indicate a positive relationship between the compensation rate and utilisation of the minimum level days. I find that the take-up of minimum level days increased by 3.65 days during a child's first year of life following the minimum level days reform. The uptake increased further over subsequent years, reaching an estimated effect of 9.70 days by the time the child turned five years old. Relative to the mean take-up of non-eligible children born in 2006, this corresponds to a 20 % increase in minimum level days and a 2.5 % increase in total uptake, irrespective of day type. However, the precise magnitude of my estimates should be interpreted with caution since it seems that the key identifying assumption of seasonal effects being equal across years does not hold perfectly. Additionally, due to the increase in the inflation-adjusted cap for compensation of income-related days accompanying the reform, it is unclear whether the estimated effects can be solely attributed to the increase in the compensation rate of minimum level days. Nevertheless, the overall uptake of parental leave increased. Unfortunately, I was unable to study leave-taking for the entire entitlement period, which ends with a child's eighth birthday. It is possible that parents are saving days for future use, as indicated by the steady increase over the years. With access to more detailed data, it would be beneficial to explore the full potential of the reform effects.

I have also studied the responses over gender. My findings suggest that mothers are the ones that account for the majority of the total increase in take-up of minimum level days for a child, approximately 80 % of the increase. However, if one takes into account the relative difference in take-up between mothers and fathers, with fathers taking approximately 15 % of the minimum level days for a child, my findings suggest that the responses to the reform over gender are very similar. Unfortunately, I cannot observe whether parents transfer days between each other, making it difficult to evaluate the results relative to the findings of Moberg (2018) and Rosenqvist (2022). Further examination is needed to understand the responses within dual-earning families.

In summary, my study provides additional evidence in support of a positive relationship between the compensation rate and the uptake of parental benefits in Sweden. Contrasting the previous evidence derived conditional on co-parenting or parents having a second child, my results show that this relationship holds unconditionally. Furthermore, my findings indicate that parents respond even to a small increase in the compensation level of parental benefits. Despite the responses of mothers and fathers being very similar when taking into account the relative difference in take-up, mothers are still the ones who increase their take-up the most in absolute terms. Thus, whether the increase in the compensation level of minimum level days strengthens the financial situation for mothers, as was the intention of the reform, remains to be investigated. It could very well be the case that it added fuel to the existing gender gap in incomes and wages.

## Acknowledgements

I would first and foremost like to thank my supervisor, Peter Skogman Thoursie, for his commitment and valuable advice throughout this process, and for making it possible to work with such detailed data. I would also like to thank Andreas Madestam for the helpful comments and feedback in the thesis seminar. A final thanks is extended to the Statistical Analysis Unit at the Swedish Social Insurance Agency for providing the additional data.

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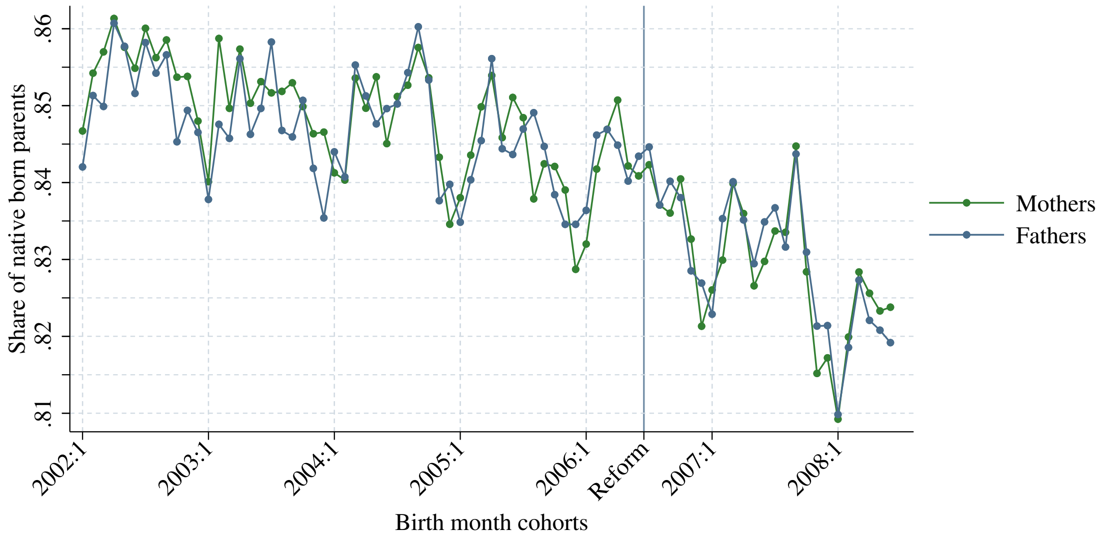
# A Appendix

This section contains additional material in the form of figures and tables which I have referred to throughout the thesis.

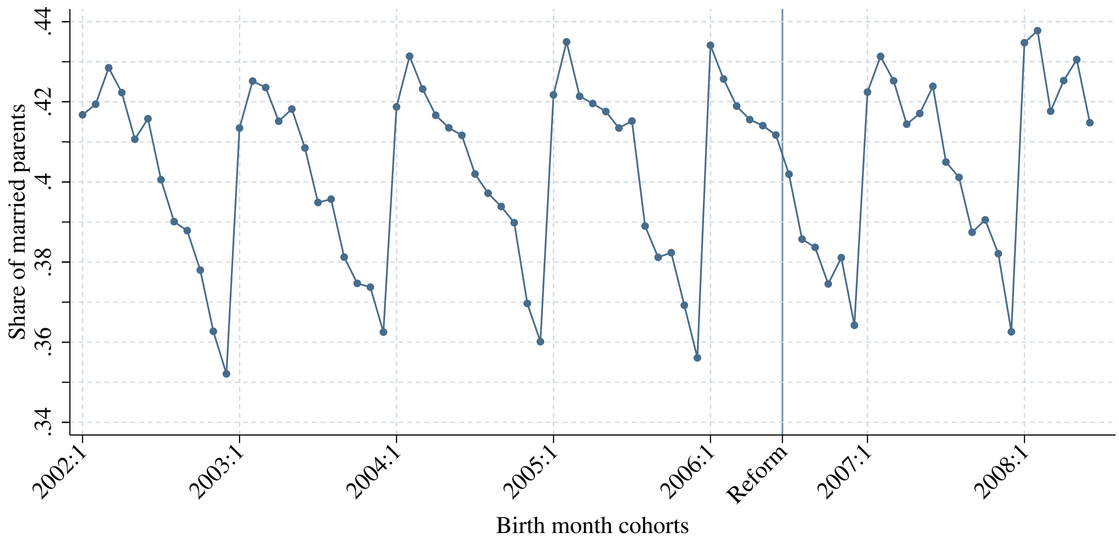
## A.1 Figures

Figure A1: Share of Native-born Parents



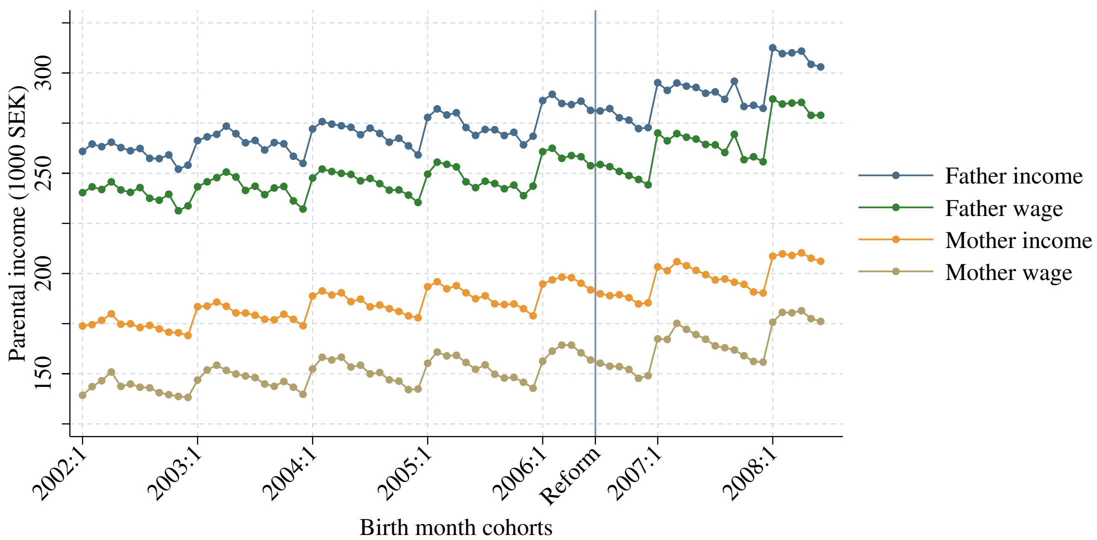

*Note:* The figure depicts the share of children in the main sample (600 956 children) with a native-born mother and father, respectively.

Figure A2: Share of Children with Married Parents



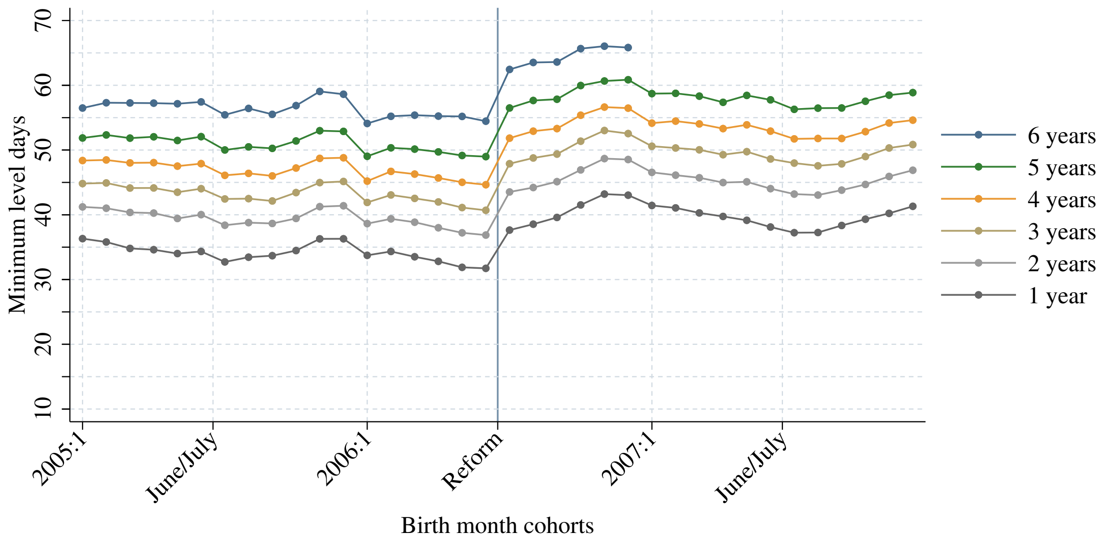

*Note:* The figure depicts the share number of children with married parents in the main sample (600 956 children). The marital status of parents is based on the value at the end of the calendar year prior to a child's birth year. Note that the seasonal patterns might just be time-lagged differences.

Figure A3: Average Parental Income



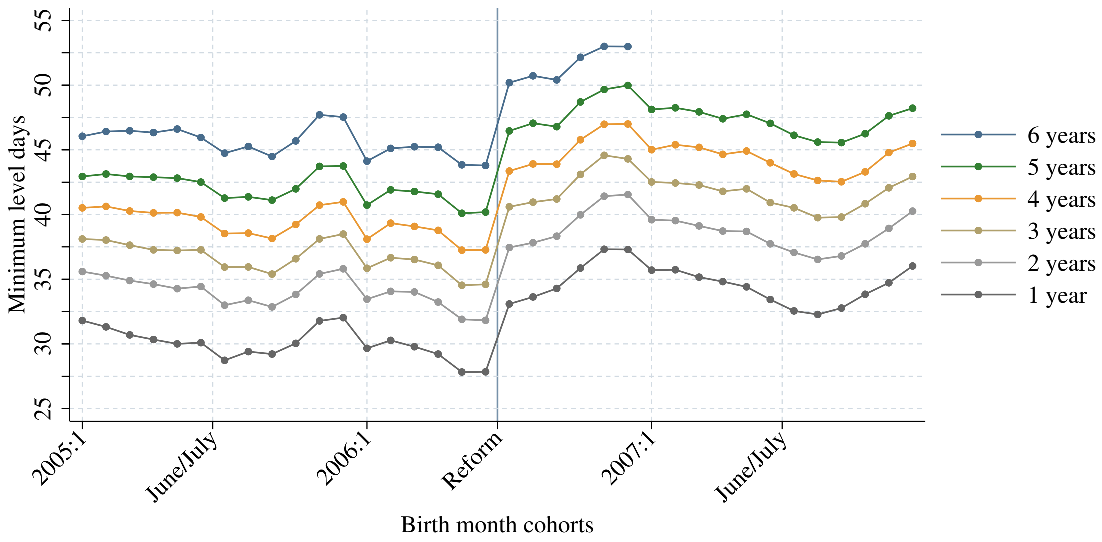

*Note:* This figure depicts the average income of mothers and fathers based on the main sample (600 956 children). *Income* contains information on all taxable labour income including earnings of both employed and self-employed as well as work-related social transfers. *Wage* contains information on individuals' earnings from employment, excluding income from self-employment, social transfers and similar. The values are observed in the calendar year prior to a child's birth year.

Figure A4: Average Take-up of Minimum Level Days for a Child



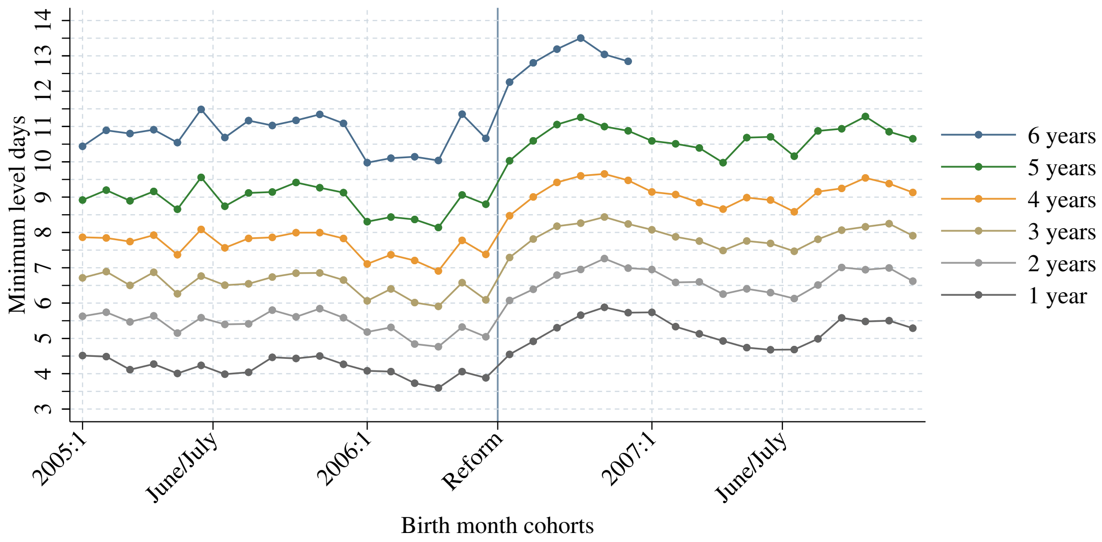

*Note:* This figure is based on the Agency's data of average take-up of minimum level days for a child. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A5: Average Take-up of Minimum Level Days by Mothers



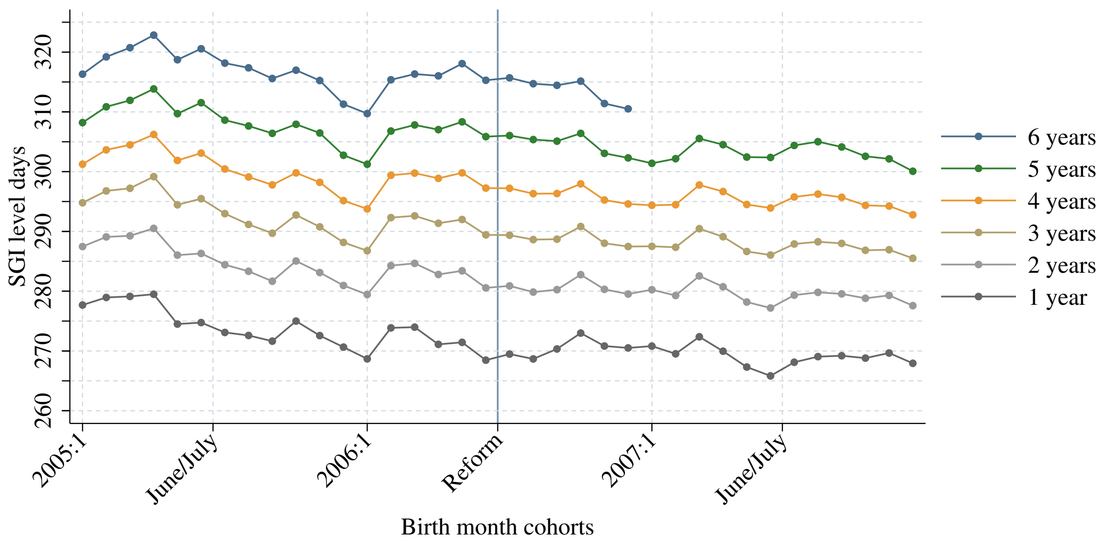

*Note:* This figure is based on the Agency's data of average take-up of minimum level days by mothers. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A6: Average Take-up of Minimum Level Days by Fathers



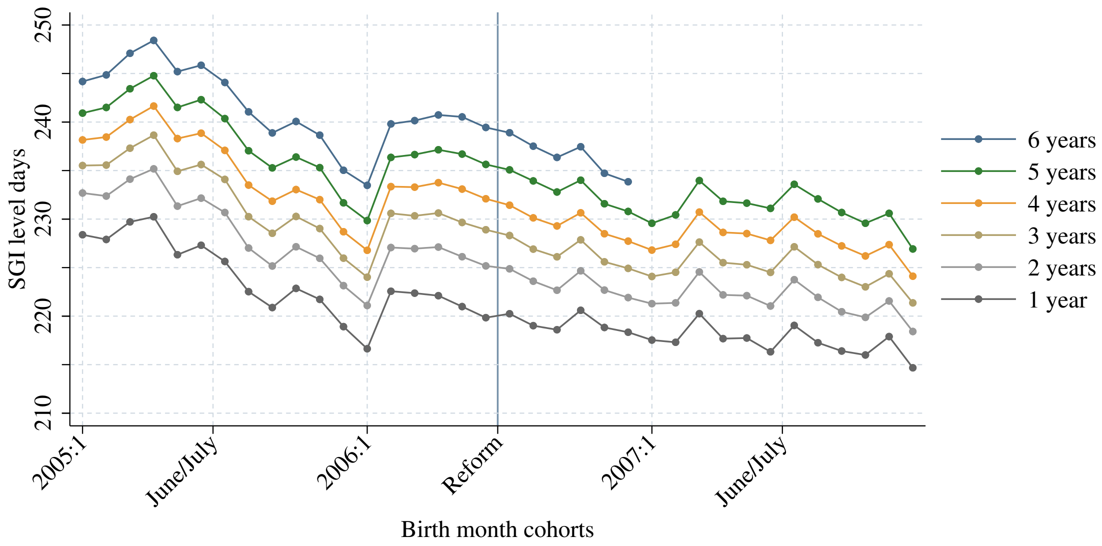

*Note:* This figure is based on the Agency's data of average take-up of minimum level days by fathers. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A7: Average Take-up of SGI Level Days for a Child



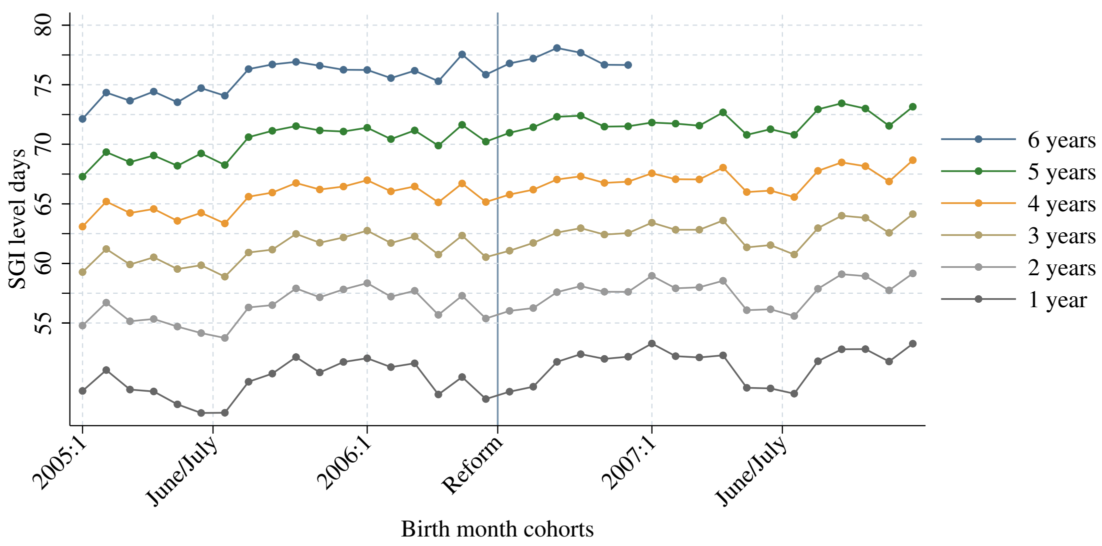

*Note:* This figure is based on the Agency's data of average take-up of SGI level days for a child. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A8: Average Take-up of SGI Level Days by Mothers



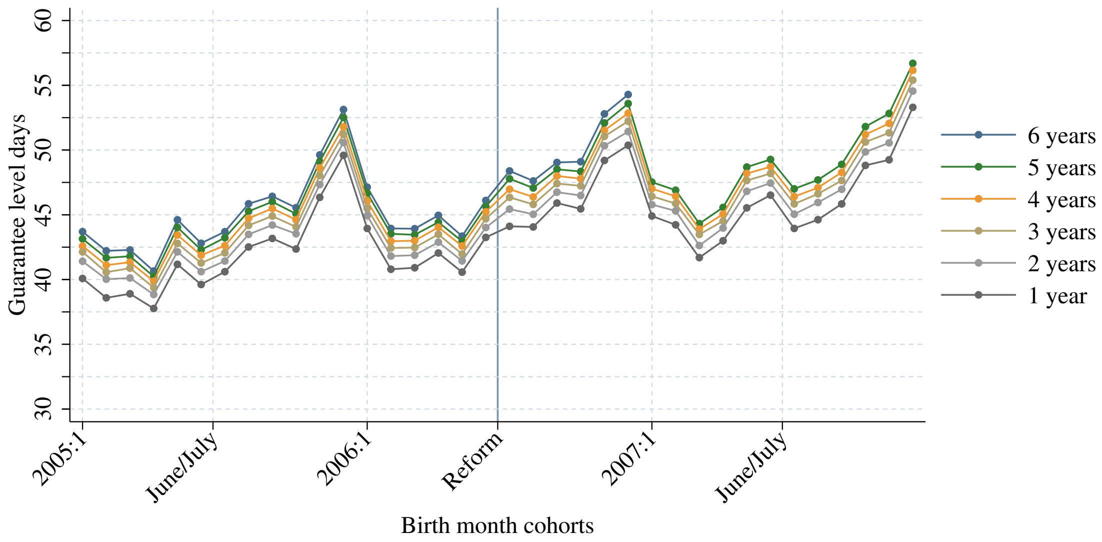

*Note:* This figure is based on the Agency's data of average take-up of SGI level days by mothers. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A9: Average Take-up of SGI Level Days by Fathers



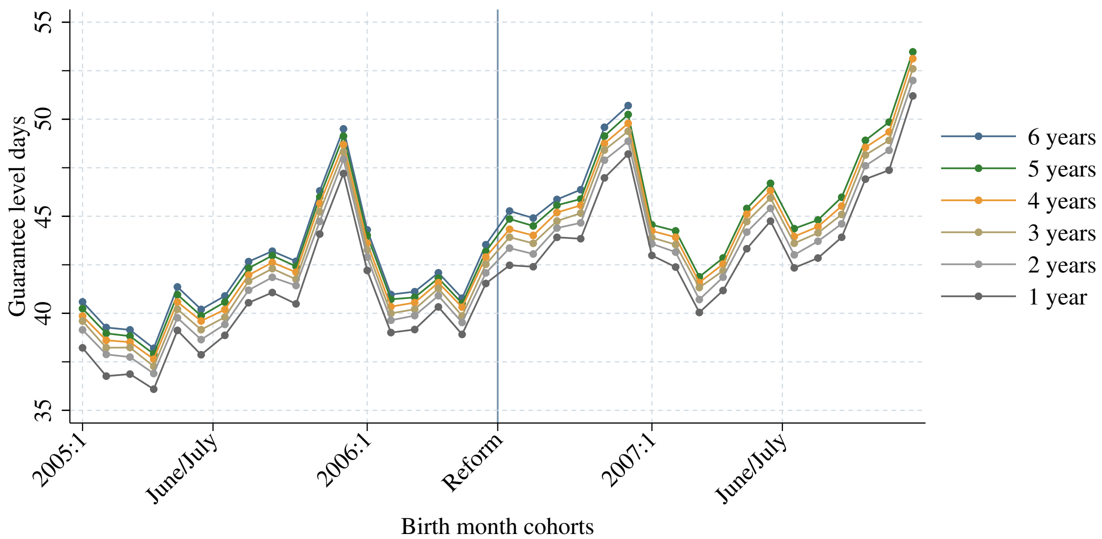

*Note:* This figure is based on the Agency's data of average take-up of SGI level days by fathers. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A10: Average Take-up of Guarantee Level Days for a Child



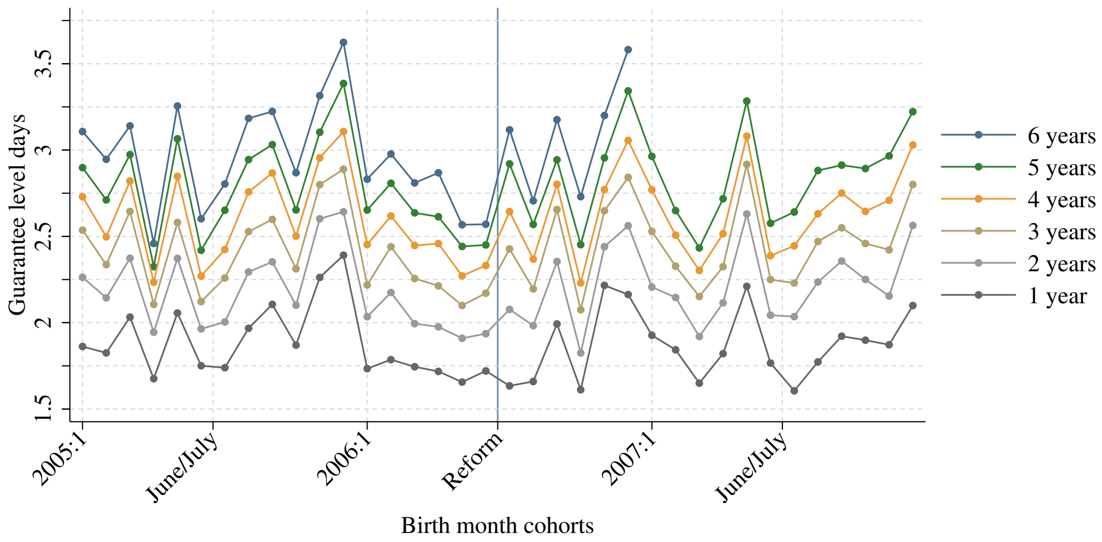

*Note:* This figure is based on the Agency's data of average take-up of guarantee level days for a child. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A11: Average Take-up of Guarantee Level Days by Mothers



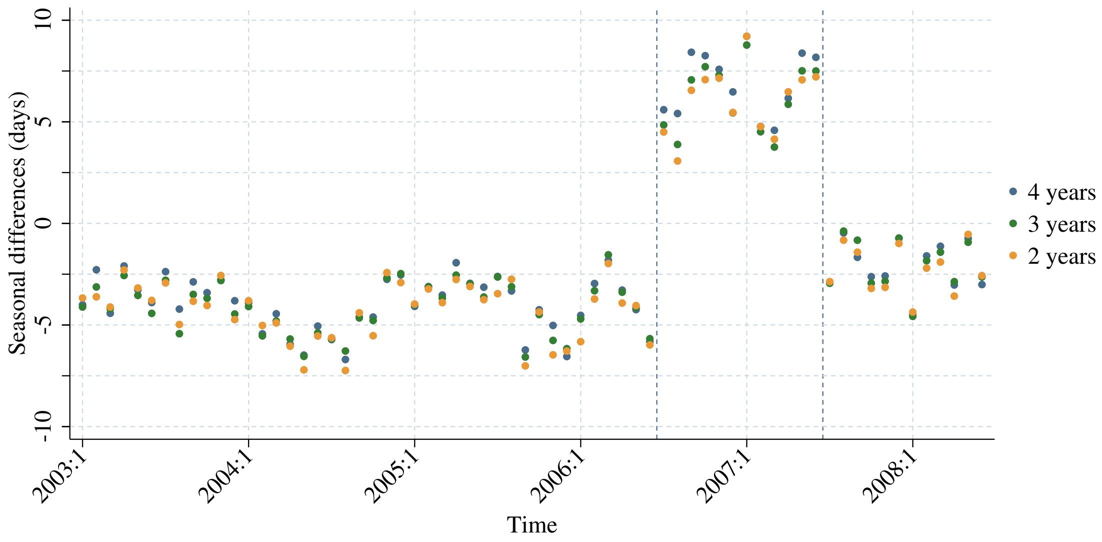

*Note:* This figure is based on the Agency's data of average take-up of guarantee level days by mothers. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A12: Average Take-up of Guarantee Level Days by Fathers




*Note:* This figure is based on the Agency's data of average take-up of guarantee level days by fathers. The sample consists of the 311 639 children born in Sweden from 2005 to 2007.

Figure A13: Seasonal Differences Excluding Take-up During First Year




*Note:* This figure corresponds to Figure 5 but excludes the transformed data for take-up during a child's first year of life.

## A.2 Tables

Table A1: Descriptive Statistics of the Main Sample

| | Non-Eligible | | Eligible | |
|---|---|---|---|---|
| Variable | Mean | SD | Mean | SD |
| No take-up for child | 0.01 | 0.07 | 0.01 | 0.08 |
| No take-up by mother | 0.01 | 0.12 | 0.02 | 0.12 |
| No take-up by father | 0.16 | 0.37 | 0.17 | 0.37 |
| Total take-up for child, 1 | 307.87 | 90.05 | 305.87 | 92.15 |
| Total take-up for child, 2 | 355.14 | 78.95 | 352.57 | 82.34 |
| Total take-up for child, 3 | 369.34 | 76.33 | 367.35 | 79.99 |
| Total take-up for child, 4 | 380.64 | 73.66 | 379.59 | 77.41 |
| Take-up by mother, 1 | 277.83 | 99.17 | 271.42 | 101.76 |
| Take-up by mother, 2 | 300.72 | 92.45 | 292.56 | 96.60 |
| Take-up by mother, 3 | 307.66 | 90.54 | 300.03 | 94.86 |
| Take-up by mother, 4 | 313.14 | 88.77 | 306.10 | 93.10 |
| Take-up by father, 1 | 30.01 | 55.66 | 34.42 | 59.81 |
| Take-up by father, 2 | 54.37 | 63.31 | 59.95 | 67.13 |
| Take-up by father, 3 | 61.63 | 64.84 | 67.27 | 68.81 |
| Take-up by father, 4 | 67.45 | 65.81 | 73.42 | 69.90 |
| Married parents | 0.40 | 0.49 | 0.40 | 0.49 |
| Native-born mother | 0.85 | 0.36 | 0.83 | 0.38 |
| Native-born father | 0.85 | 0.36 | 0.83 | 0.38 |
| Age mother | 30.82 | 4.90 | 31.06 | 5.04 |
| Age father | 33.50 | 5.86 | 33.81 | 6.01 |
| Post-secondary mother | 0.44 | 0.50 | 0.49 | 0.50 |
| Post-secondary father | 0.37 | 0.48 | 0.40 | 0.49 |
| Income mother | 184 | 113 | 199 | 125 |
| Income father | 270 | 201 | 292 | 234 |
| Wage mother | 150 | 126 | 166 | 137 |
| Wage father | 246 | 205 | 266 | 233 |

*Notes:* This table summarises the descriptive statistics for the 600 956 children with non-missing values in parental characteristics. Share of eligible children equals 32 %. *No take-up* is a binary variable taking the value 1 if no leave is taken for the child during its first four years of life. *Married parents* is a binary variable taking the value 1 if the child's parents were married in the calendar year prior to its birth. *Age* measures parent's age in years at the birth week of a child (using information on parent's birth year and birth week). *Education* is a binary variable taking the value 1 for parents with post-secondary education. *Income* contains information on yearly taxable labour income (1000 SEK)including earnings of both employed and self-employed as well as work-related social transfers. *Wage* contains information on individuals' yearly earnings (1000 SEK) from employment, excluding income from self-employment, social transfers and similar. The variables on the final six rows are all recorded in the calendar year prior to the child's year of birth.

Table A2: Estimates Without Birth Month and Birth Year Controls

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample (600 956): | | | | |
| Total take-up for child | -2.00 | -2.58* | -1.98 | -1.05 |
| | (1.58) | (1.22) | (1.09) | (1.00) |
| Take-up by mother | -6.42*** | -8.16*** | -7.63*** | -7.03*** |
| | (1.22) | (1.27) | (1.21) | (1.17) |
| Take-up by father | 4.41*** | 5.58*** | 5.64*** | 5.97*** |
| | (0.89) | (0.60) | (0.54) | (0.51) |
| Restricted sample (239 958): | | | | |
| Total take-up for child | 0.22 | -0.65 | 0.06 | 0.90 |
| | (1.83) | (1.26) | (1.07) | (0.96) |
| Take-up by mother | -6.09*** | -8.12*** | -7.64*** | -7.20*** |
| | (1.34) | (1.36) | (1.29) | (1.24) |
| Take-up by father | 6.30*** | 7.47*** | 7.70*** | 8.08*** |
| | (1.11) | (0.73) | (0.71) | (0.68) |
| Including controls for the restricted sample (239 958): | | | | |
| Total take-up for child | 2.34 | 0.71 | 1.43 | 2.19* |
| | (1.77) | (1.20) | (1.00) | (0.89) |
| Take-up by mother | -3.53** | -5.63*** | -5.23*** | -4.89*** |
| | (1.18) | (1.18) | (1.11) | (1.06) |
| Take-up by father | 5.86*** | 6.34*** | 6.66*** | 7.08*** |
| | (1.09) | (0.70) | (0.66) | (0.63) |

*Notes:* This table displays the estimates corresponding to those in Table 1 but for the model without birth month and birth year fixed effects. The final panel displays the estimates of $\rho$ in Equation 1 when including the same vector of controls as in Table 1. The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth month level, a total of 78 clusters.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A3: Estimates Based on Various Sub-Samples

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample (600 956): | | | | |
| Total take-up for child | 6.97*** | 8.79*** | 8.89*** | 9.67*** |
| | (0.65) | (0.69) | (0.71) | (0.67) |
| Take-up by mother | 8.02*** | 8.75*** | 8.88*** | 9.19*** |
| | (0.71) | (0.74) | (0.76) | (0.75) |
| Take-up by father | -1.05* | 0.03 | 0.01 | 0.48 |
| | (0.48) | (0.48) | (0.43) | (0.39) |
| Main sample, excluding children without take-up (597 431): | | | | |
| Total take-up for child | 7.14*** | 9.00*** | 9.10*** | 9.89*** |
| | (0.63) | (0.61) | (0.60) | (0.56) |
| Take-up by mother | 8.18*** | 8.93*** | 9.06*** | 9.37*** |
| | (0.68) | (0.68) | (0.68) | (0.68) |
| Take-up by father | -1.04* | 0.06 | 0.04 | 0.51 |
| | (0.48) | (0.48) | (0.44) | (0.39) |
| Main sample, including only children with first-time parents (241 215): | | | | |
| Total take-up for child | 7.82*** | 9.82*** | 9.95*** | 10.60*** |
| | (0.92) | (0.81) | (0.81) | (0.78) |
| Take-up by mother | 8.86*** | 9.56*** | 9.81*** | 9.94*** |
| | (0.97) | (1.00) | (1.01) | (1.01) |
| Take-up by father | -1.04 | 0.27 | 0.15 | 0.68 |
| | (0.71) | (0.56) | (0.55) | (0.59) |
| Main sample, including only children with second-time parents (194 773): | | | | |
| Total take-up for child | 6.88*** | 7.75*** | 7.83*** | 8.66*** |
| | (0.88) | (0.86) | (0.85) | (0.85) |
| Take-up by mother | 7.86*** | 8.23*** | 8.12*** | 8.47*** |
| | (0.93) | (0.94) | (0.96) | (1.01) |
| Take-up by father | -0.99 | -0.48 | -0.29 | 0.19 |
| | (0.60) | (0.77) | (0.72) | (0.69) |

*Notes:* This table presents the estimated effects for various sub-samples of the main sample. All estimates include birth month and birth year controls. The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth month level, a total of 78 clusters.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A4: Heterogeneous Responses by Educational Attainment

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Both parents lower educated (266 887): | | | | |
| Total take-up for child | 7.98*** | 9.97*** | 10.46*** | 11.68*** |
| | (0.88) | (0.80) | (0.75) | (0.74) |
| Take-up by mother | 9.01*** | 9.83*** | 10.37*** | 10.86*** |
| | (1.07) | (0.95) | (0.933) | (0.94) |
| Take-up by father | -1.06 | 0.13 | 0.08 | 0.82 |
| | (0.54) | (0.65) | (0.61) | (0.56) |
| Mother lower educated, father higher educated (60 249): | | | | |
| Total take-up for child | 10.17*** | 11.69*** | 11.40*** | 11.36*** |
| | (1.61) | (1.71) | (1.88) | (1.86) |
| Take-up by mother | 10.92*** | 11.83*** | 11.69*** | 11.80*** |
| | (1.73) | (1.73) | (1.95) | (1.96) |
| Take-up by father | -0.78 | -0.19 | -0.34 | -0.50 |
| | (1.11) | (1.21) | (1.08) | (1.11) |
| Mother higher educated, father lower educated (105 827): | | | | |
| Total take-up for child | 4.56*** | 6.16*** | 5.62*** | 6.17*** |
| | (1.08) | (1.05) | (0.18) | (1.18) |
| Take-up by mother | 5.73*** | 5.86*** | 5.49*** | 5.73*** |
| | (1.37) | (1.10) | (1.10) | (1.10) |
| Take-up by father | -1.17 | 0.31 | 0.14 | 0.46 |
| | (0.98) | (1.03) | (1.11) | (1.02) |
| Both parents higher educated (167 993): | | | | |
| Total take-up for child | 4.60*** | 6.85*** | 6.96*** | 7.55*** |
| | (1.12) | (0.97) | (1.07) | (1.03) |
| Take-up by mother | 5.36*** | 6.48*** | 6.46*** | 6.69*** |
| | (1.10) | (1.17) | (1.23) | (1.18) |
| Take-up by father | -0.75 | 0.40 | 0.51 | 0.88 |
| | (0.75) | (0.73) | (0.75) | (0.77) |

*Notes:* This table presents the estimated effects for sub-samples of the main sample (600 956 children). All estimates include birth month and birth year controls. The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth month level, a total of 78 clusters.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A5: Summary of Baseline Means

| Cumulative take-up (years): | 1 | 2 | 3 | 4 | 5 |
|---|---|---|---|---|---|
| The Agency's data: | | | | | |
| Minimum level days for child | 8.16 | 33.00 | 38.16 | 41.88 | 45.59 |
| SGI level days for child | 203.68 | 271.26 | 282.53 | 290.75 | 298.14 |
| Guarantee level days for child | 37.97 | 41.92 | 42.83 | 43.43 | 43.98 |
| Total take-up for child | 249.81 | 346.18 | 363.52 | 367.06 | 387.72 |
| Minimum level days by mother | 7.70 | 29.10 | 33.08 | 35.71 | 38.30 |
| SGI level days by mother | 182.24 | 220.75 | 225.59 | 229.02 | 232.06 |
| Guarantee level days by mother | 36.98 | 40.19 | 40.82 | 41.19 | 41.55 |
| Total take-up by mother | 226.92 | 290.05 | 299.49 | 305.92 | 311.91 |
| Minimum level days by father | 0.46 | 3.90 | 5.08 | 6.18 | 7.29 |
| SGI level days by father | 21.44 | 50.51 | 56.94 | 61.73 | 66.08 |
| Guarantee level days by father | 0.99 | 1.73 | 2.00 | 2.23 | 2.43 |
| Total take-up by father | 22.89 | 56.14 | 64.02 | 70.14 | 75.80 |
| Administrative data, given the Agency's restrictions: | | | | | |
| Total take-up for child | 299.33 | 345.35 | 360.14 | 371.81 | 382.73 |
| Take-up by mother | 267.49 | 289.61 | 297.05 | 302.86 | 308.37 |
| Take-up by father | 31.80 | 55.70 | 63.05 | 68.90 | 74.30 |
| Administrative data, given the main sample restrictions: | | | | | |
| Total take-up for child | 298.94 | 346.00 | 360.82 | 372.45 | |
| Take-up by mother | 266.42 | 288.74 | 296.16 | 301.91 | |
| Take-up by father | 32.51 | 57.23 | 64.64 | 70.51 | |

*Notes:* This table presents the mean take-up for non-eligible children born in the reform year, i.e., the children born from January 1, to June 30, 2006. The Agency's data contains information on 53 869 children. The first panel based on administrative data contains information on 53 679 children for which leave is taken at some point, including multiple-birth children. The final panel based on administrative data contains information on 49 494 children. The latter are subject to the main sample restrictions, that is, children being single-born and having parents that do not have missing values in parental characteristics etc.

Table A6: Estimates Accompanied by Newey-West Standard Errors

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample (600 956): | | | | |
| Total take-up for child | 6.97*** | 8.79*** | 8.89*** | 9.67*** |
| | (0.57) | (0.46) | (0.47) | (0.48) |
| Take-up by mother | 8.02*** | 8.75*** | 8.88*** | 9.19*** |
| | (0.62) | (0.55) | (0.56) | (0.55) |
| Take-up by father | -1.05** | 0.03 | 0.01 | 0.48 |
| | (0.31) | (0.40) | (0.34) | (0.31) |
| Restricted sample (239 958): | | | | |
| Total take-up for child | 7.64*** | 9.62*** | 9.73*** | 10.37*** |
| | (0.88) | (0.62) | (0.56) | (0.51) |
| Take-up by mother | 8.70*** | 9.39*** | 9.64*** | 9.75*** |
| | (0.73) | (0.56) | (0.56) | (0.55) |
| Take-up by father | -1.07** | 0.22 | 0.10 | 0.62 |
| | (0.39) | (0.43) | (0.38) | (0.35) |

*Notes:* This table displays the estimates from a weighted regression for a birth month average take-up version of Equation 2 with cohort size as weights. The regression is based on the same samples as in Table 1, with 78 birth month groups. The columns indicate the cumulative take-up over a child's first four years of life. All estimates include birth month and birth year controls. The estimates are accompanied by Newey-West standard errors with the lag order set to 4.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A7: Estimates Including Children with Missing Values

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Given biological mother (654 367): | | | | |
| Total take-up for child | 6.22*** | 7.84*** | 7.96*** | 8.69*** |
| | (0.64) | (0.70) | (0.71) | (0.66) |
| Take-up by mother | 7.28*** | 8.03*** | 8.16*** | 8.36*** |
| | (0.61) | (0.66) | (0.69) | (0.68) |
| Take-up by father (647 338) | -1.19** | -0.29 | -0.28 | 0.22 |
| | (0.42) | (0.42) | (0.39) | (0.36) |
| Given above restriction + biological father (647 338): | | | | |
| Total take-up for child | 6.52*** | 8.05*** | 8.17*** | 8.92*** |
| | (0.61) | (0.67) | (0.68) | (0.65) |
| Take-up by mother | 7.59*** | 8.23*** | 8.35*** | 8.59*** |
| | (0.62) | (0.65) | (0.67) | (0.68) |
| Take-up by father | -1.19** | -0.29 | -0.28 | 0.22 |
| | (0.42) | (0.42) | (0.39) | (0.36) |
| Given above restrictions + take-up within 4 years (639 271): | | | | |
| Total take-up for child | 7.24*** | 8.90*** | 9.06*** | 9.84*** |
| | (0.61) | (0.58) | (0.56) | (0.53) |
| Take-up by mother | 8.26*** | 8.95*** | 9.09*** | 9.35*** |
| | (0.65) | (0.64) | (0.64) | (0.66) |
| Take-up by father | -1.13** | -0.17 | -0.15 | 0.38 |
| | (0.42) | (0.41) | (0.38) | (0.34) |
| Given above restrictions + first-time parents (261 981): | | | | |
| Total take-up for child | 7.31*** | 9.26*** | 9.42*** | 9.98*** |
| | (0.85) | (0.78) | (0.75) | (0.72) |
| Take-up by mother | 8.33*** | 9.12*** | 9.37*** | 9.39*** |
| | (1.00) | (1.05) | (1.04) | (1.05) |
| Take-up by father | -1.12 | 0.04 | -0.05 | 0.49 |
| | (0.64) | (0.49) | (0.48) | (0.54) |

*Notes:* This table displays the estimated $\rho$ in the main model, that is, Equation 2, for the sample of 654 367 children without dropping the 53 411 children with missing values in parental characteristics etc. Note that 7 029 children do not have a biological father. The estimates on the third row in the first panel are corrected for those children. The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth month level, a total of 78 clusters.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A8: Estimates with Birth Week Effects

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample (600 956): | | | | |
| Total take-up for child | 6.89*** | 8.77*** | 8.87*** | 9.65*** |
| | (0.63) | (0.62) | (0.60) | (0.59) |
| Take-up by mother | 7.92*** | 8.70*** | 8.83*** | 9.14*** |
| | (0.75) | (0.73) | (0.73) | (0.73) |
| Take-up by father | -1.03* | 0.06 | 0.04 | 0.51 |
| | (0.43) | (0.46) | (0.47) | (0.45) |
| Restricted sample (239 958): | | | | |
| Total take-up for child | 7.66*** | 9.66*** | 9.76*** | 10.39*** |
| | (0.91) | (0.73) | (0.71) | (0.68) |
| Take-up by mother | 8.71*** | 9.43*** | 9.67*** | 9.78*** |
| | (1.05) | (0.95) | (0.95) | (0.95) |
| Take-up by father | -1.06 | 0.23 | 0.10 | 0.62 |
| | (0.65) | (0.63) | (0.65) | (0.66) |
| Including controls for the restricted sample (239 958): | | | | |
| Total take-up for child | 7.22*** | 9.39*** | 9.51*** | 10.16*** |
| | (0.83) | (0.70) | (0.69) | (0.66) |
| Take-up by mother | 8.17*** | 8.95*** | 9.21*** | 9.34*** |
| | (0.96) | (0.88) | (0.89) | (0.90) |
| Take-up by father | -0.96 | 0.44 | 0.30 | 0.81 |
| | (0.64) | (0.61) | (0.63) | (0.64) |

*Notes:* This table displays the estimates of a birth week version of Equation 2 based on the same samples considered in Table 1. The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth week level, a total of 338 clusters.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A9: Sensitivity Test, Excluding Children Born in June or July

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample (495 419): | | | | |
| Total take-up for child | 6.93*** | 8.41*** | 8.40*** | 9.26*** |
| | (0.77) | (0.77) | (0.77) | (0.73) |
| Take-up by mother | 8.01*** | 8.59*** | 8.69*** | 9.05*** |
| | (0.83) | (0.83) | (0.86) | (0.87) |
| Take-up by father | -1.09 | -0.19 | -0.29 | 0.21 |
| | (0.55) | (0.55) | (0.48) | (0.44) |
| Restricted sample (198 110): | | | | |
| Total take-up for child | 8.02*** | 9.65*** | 9.62*** | 10.23*** |
| | (0.99) | (0.78) | (0.75) | (0.69) |
| Take-up by mother | 9.00*** | 9.50*** | 9.62*** | 9.75*** |
| | (1.11) | (1.11) | (1.17) | (1.18) |
| Take-up by father | -0.99 | 0.14 | 0.00 | 0.48 |
| | (0.84) | (0.66) | (0.63) | (0.66) |
| Including controls for the restricted sample (198 110): | | | | |
| Total take-up for child | 7.63*** | 9.39*** | 9.37*** | 9.99*** |
| | (1.04) | (0.81) | (0.77) | (0.71) |
| Take-up by mother | 8.52*** | 9.07*** | 9.21*** | 9.35*** |
| | (1.04) | (1.03) | (1.10) | (1.12) |
| Take-up by father | -0.90 | 0.31 | 0.16 | 0.63 |
| | (0.80) | (0.59) | (0.55) | (0.59) |

*Notes:* This table displays the estimated $\rho$ in Equation 2 corresponding to the samples considered in Table 1 but dropping the 105 537 children born in June or July in any birth year. I have kept the sample labels to indicate which restrictions are imposed on the data. Note that the number of birth month controls drop to 10 when imposing this restriction but the birth year controls remain the same. The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth month level, a total of 65 clusters.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A10: Falsification Test, Artificial Cutoff at July 1, 2005

| Cumulative take-up (years): | 1 | 2 | 3 | 4 |
|---|---|---|---|---|
| Main sample (408 693): | | | | |
| Total take-up for child | -1.22 | -1.22** | -0.88 | -1.02* |
| | (0.61) | (0.45) | (0.44) | (0.44) |
| Take-up by mother | -1.22 | -0.88 | -0.85 | -0.78 |
| | (0.82) | (0.58) | (0.58) | (0.56) |
| Take-up by father | -0.00 | -0.33 | -0.02 | -0.23 |
| | (0.49) | (0.37) | (0.38) | (0.37) |
| Restricted sample (163 895): | | | | |
| Total take-up for child | -0.25 | -0.50 | -0.02 | -0.05 |
| | (0.78) | (0.60) | (0.59) | (0.58) |
| Take-up by mother | -0.01 | 0.39 | 0.60 | 0.88 |
| | (1.00) | (0.85) | (0.76) | (0.74) |
| Take-up by father | -0.23 | -0.88 | -0.59 | -0.91 |
| | (0.55) | (0.57) | (0.59) | (0.55) |
| Including controls for the restricted sample (163 895): | | | | |
| Total take-up for child | -0.97 | -0.89 | -0.39 | -0.41 |
| | (0.72) | (0.60) | (0.61) | (0.59) |
| Take-up by mother | -0.80 | -0.27 | -0.03 | 0.29 |
| | (0.97) | (0.77) | (0.75) | (0.71) |
| Take-up by father | -0.16 | -0.60 | -0.34 | -0.67 |
| | (0.55) | (0.48) | (0.51) | (0.47) |

*Notes:* This table displays the estimates for a falsification test based on a version of Equation 2. The eligibility indicator is changed to take the value 1 for all children born as of the July 1, 2005 and considers sample of children born from January 1, 2002 to June 30, 2006. I have kept the sample labels to indicate which restrictions are imposed on the data. All estimates include birth month and birth year controls. The columns indicate the cumulative take-up over a child's first four years of life. The standard errors, in parentheses, are clustered at birth month level, a total of 54 clusters.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$