DISSERTATION SERIES 2022:2

Essays in Swedish family policy

Malin Tallås Ahlzén

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Academic dissertation for the Degree of Doctor of Philosophy in Economics at Stockholm University has been publicly defended on Friday 23 September 2022 at 10.00 in Nordenskiöldsalen, Geovetenskapens hus, Svante Arrhenius väg 12.
Essay II ; Human Capital Effects of Opportunities for One-on-one Time with Parents: Evidence from a Swedish Childcare Access Reform, has been published by IFAU as working paper 2022:02 and Swedish report 2022:02.

ISSN 1651-4149

Swedish Institute for Social Research 108

Stockholm University

Essays in Swedish Family Policy

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Doctoral Thesis in Economics at Stockholm University, Sweden 2022

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Abstract

Parental Leave Quotas: Peer Effects and Workplace Related Costs

In this paper, I estimate whether the introduction and expansion of parental leave quotas in Sweden triggered spillovers at the workplace level. Using a regression discontinuity design, I find that the introduction of the quota did not affect the uptake of parental leave of male coworkers. However, the expansion of the reform appears to increase the days of parental leave taken by male coworkers when the child is relatively young. For both reforms, the response is similar across workplaces that differ in terms of costs of parental leave. The lack of spillovers from the first reform is consistent with the introduction of the quota being more distorting.

Human Capital Effects of Opportunities for One-on-one Time with Parents: Evidence from a Swedish Childcare Access Reform

We study the effects of increased opportunities for one-on-one time with a parent during infancy on the human capital formation of children. To this end, we exploit a nationwide reform that mandated Swedish municipalities to offer childcare access for infants' older siblings, while parents were on parental leave to care for their infants. Survey data on childcare enrollment show that the reform had a significant impact on the childcare enrollment of older siblings. Using rich administrative data, we estimate intention-to-treat effects in a differences-in-differences setting, comparing infants with and without siblings of childcare age, pre- and post-reform, in municipalities that were affected by the reform. We find no robust overall effects on the children's sixth grade test scores, but we find evidence of positive effects on test scores for boys, driven by sons of less than university educated mothers. There is no corresponding overall effect for girls, but we find suggestive evidence of positive effects for daughters of highly educated mothers. Exploring potential pathways, we find no evidence of changes in quantity of parental time during infancy, pointing instead towards the role of improved quality of parent-child interactions as a result of less competition for parental time. We also find that improvements in physical and mental health in school age may have contributed to the positive effect for boys.

Seasonality of Childcare Enrollment

In this paper, I establish that childcare enrollment varies systematically over the year, which translates into differences in the age at childcare enrollment. The pattern aligns with the seasonal variation in childcare supply, implied by the institutional structure of the childcare system. I find the strength of the seasonality in childcare enrollment to differ with the socioeconomic status of the parents. High-status parents exhibit more variation in enrollment across the year, likely because their financial resources allow them to wait for peaks in supply when higher quality childcare is available. I examine possible consequences of these results for household and child outcomes and discuss equality implications.

Keywords: *public economics, labor economics, gender, parental leave, quotas, childcare, cognitive development.*

Stockholm 2022
http://urn.kb.se/resolve?urn=urn:nbn:se:su:diva-207713

ISBN 978-91-7911-962-1
ISBN 978-91-7911-963-8
ISSN 0283-8222

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ESSAYS IN SWEDISH FAMILY POLICY

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ISBN print 978-91-7911-962-1
ISBN PDF 978-91-7911-963-8
ISSN 0283-8222

Printed in Sweden by Universitetsservice US-AB, Stockholm 2022

To Björn, Alvin and
Melker

Acknowledgements

It seems unreal to finally be done with this work, it has taken a lot of time and effort, and would not have been possible without the extraordinary support of my main supervisor Anne Boschini. Thank you for not giving up on me! I am so grateful for everything you have done, especially the encouragement and consideration. For believing in me, help me to stay focused and to emphasize the importance of a healthy work-life balance. Also, to share your valuable insights and great knowledge of the field, while encouraging me to take my own decisions. I also want to express my sincere gratitude to my co-supervisors. Erik Lindqvist, thank you for your constructive comments. I am impressed with your knowledge, your humble personality and patience. Lisa Laun, thank you for your generosity; for inviting me to IFAU, looking out for me and for teaching me how research come about. You are a role model.

I also want to thank Erica Lindahl, for carefully reading my work and to make insightful comments at the final seminar. And thank you Anna Sjögren, for what turned out to be a great collaboration. You are truly inspiring and I've learned a lot from you.

I'm grateful to all my colleagues at SOFI, I feel privileged to have been a part of this warm research environment. I believe that the combination of kindness and great research at SOFI is quite rare. Thank you Markus Jäntti, I really appreciate your leadership and thanks for checking in now and then. I'm especially grateful to Matthew Lindquist for welcoming me to SOFI and for being always so friendly. Anders Stenberg and Karin Edmark, thank you for your consideration and encouragement. I also want to express my gratitude to Johanna Rickne, you are just super woman. And ofcourse Elma Sose, Katarina Hagelin, Maria Mårtensson, Tara Nabavi, Julio Lundborg, Daniel Rossetti and Anne Jensen, thank you for all the administrative work that you do, always with a smile.

I also want to thank my previous coworkers at IFN. I would not have applied to the PhD program if it wasn't for you. Special thanks to Lars Persson and Joakim Tåg for your enthusiasm and encouragement.

I feel fortunate to have met amazing people during the PhD program. Fellow PhD's at SOFI, the Department of Economics and IIES who made the University a place of joy. Svante, Mattias, Sreyashi, Ricardo, John and Jon–no way I would have passed the first year courses if it wasn't for you. It was a struggle, but I choose to remember the good times and there are many! To this I think we all owe Christine a special thanks, the hostess of all times. Your generosity has no limits and I'm grateful to have you a close friend (and constant roommate). Many thanks to Roza, for just being you–you are a rock! And Xueping, never have I meet a more curious and inspiring person. Also, thank you Iman, the friendliest person ever. Thank you for offering support and laughs at any time at SOFI. And more recently, Ulrika and Anna for your company at the University.

Stockholm University also brought me Ellinor, Matilda, Johanna and Noora from the master program, I really appreciate your company and our dinners have been a real highlight.

I'm very grateful to have the best of friends. Anna, Cissi, Emilie, Malin, Natti, Sofo and Sillen. Thank you for all the good times -You are simply the best!

And of course, thank you to my family. I love you all!

Last but not least, I am grateful for my awesome husband, who (tries to) keep me sane and grounded and reminds me about the important things in life. And to our kids, who are challenging at times (as they should), but has provided all the joy and meaning to endure.

Contents

Introduction

From an international perspective, family policy in Sweden is well developed with high qualitative childcare and a generous parental leave scheme. The goals of the Swedish family policy include economic security and physical well-being, gender equality, and children's rights (Haas, 1996). Swedish family policy developed already in the 1930s, during the rise of the Swedish welfare state, to support families and raise fertility (Lundqvist, 2013). The extensive family policy is considered to have contributed to Sweden's low poverty rates among children and to enable a high female labor force participation, despite relatively high fertility rates (Duvander, 2008).

This thesis consists of three essays, all related to the family policy in Sweden and its potential policy spillovers, i.e., possibly unintended effects beyond the immediate purpose. The first two essays are empirical, estimating spillovers of parental leave quotas and childcare access. The third chapter is descriptive, characterizing the seasonal variation in childcare enrollment.

Parental leave

The maternity leave insurance was replaced by the shared parental leave insurance in 1974. The reform made Sweden the first country in the world to offer paid parental leave also to fathers (Duvander, 2008). The six months of paid parental leave in 1974 has increased to 480 days, which can be used flexibly until the child is eight years old (*Försäkringskassan*, 2022).[1] Despite the long history of gender neutral parental leave policies, fathers take less parental leave than mothers. From 0.5 percent of all days in 1974, the share taken by fathers has increased to 30 percent today, and this is largely attributed to reforms targeting gender equality.

To encourage a higher uptake of parental leave of fathers, one month of parental leave benefits was reserved to each parent in 1995. The reform applied to all children born January 1 and later, and was followed by a second month in 2002. In the first essay of this thesis, **Parental**

[1] Parental leave can be saved for twelve years for children born after 2014, which is outside the scope of this paper.

leave quotas: Peer effects and workplace related costs, I estimate spillovers from the introduction and expansion of parental leave quotas in Sweden. More specifically, I estimate whether *fathers* take more parental leave, if their male coworker (*peer*) is exogenously induced by the reform to take more parental leave. Reform spillovers between peers have not been studied in the context of Sweden before, even though they contribute to the complete reform effect.[2] Lessons from the Swedish case are interesting also from an international perspective, given that the European parliament recently mandated all member states to reserve at least 2 months of paid parental leave to each parent.[3]

Using rich administrative data, I estimate reduced form effects in a regression discontinuity (RD) model where the birthdate of the peer's child relative to the date of the reform, determines the treatment status of the father. The empirical specification follows Dahl et al. (2014), who found extensive margin effects from implementation of the Norwegian parental leave quota. The Swedish setting is considerably different with a relatively high uptake already before implementation of the first quota, and I instead estimate effects on the number of days.

I find that the introduction of the quota did not spill over and affect the uptake of parental leave of fathers, beyond the direct effect. However, the expansion of the reform appears to affect the timing of parental leave taken by treated fathers, such that a larger share of their total parental leave is taken when the child is relatively young. I find no evidence of workplace specific reform response; there is no significant heterogeneity with respect to monetary and normative costs of parental leave, both which are suggested by the literature to matter for fathers' uptake of parental leave (e.g., Becker, 1991; Akerlof and Kranton, 2000).

The lack of spillovers from the introduction of parental leave quotas is consistent with the introduction being perceived more negatively, both in terms of design and gender norms at the time of implementation. The findings suggest that increasing the days reserved for each parent can trigger a positive trend increasing the parental leave of fathers taken

[2]Effects on parental leave uptake and other outcomes for those directly affected by the Swedish reforms has been studied by Eriksson, 2005; Johansson, 2010; Duvander and Johansson, 2012; Duvander and Johansson, 2013; Ekberg et al., 2013; Försäkringskassan, 2015; Avdic and Karimi, 2018; Försäkringskassan, 2019; Duvander et al., 2020; Avdic et al., 2022; Ginja et al., 2020b

[3]Directive 2019/1158.

relatively early, which is the type of parental leave from which we would expect to find effects on continued childrearing responsibilities.

Childcare

1973 marks the beginning of public child care provision in Sweden. At this time, the benefits of childcare outside the household were widely questioned by working and upper middle class households but as the educational purpose of public child care became more emphasized in the mid 1990s, it was increasingly considered an important complement to parental care and to be valuable in the socialization process of children (Bergqvist and Nyberg, 2001). Since 2002, childcare is guaranteed to all children from the age of one, at a highly subsidized cost. The purpose is to equalize social inequality by enabling labor force participation among mothers and to integrate and prepare children for school (Lundin et al., 2008).

Consequences of childcare access is estimated in the second essay, **Human capital effects of opportunities for one-on-one time with parents: Evidence from a Swedish childcare access reform**, co-authored with Anna Sjögren. In 2001-2003 there was a comprehensive childcare access reform, which in 2002 obliged all municipalities to provide childcare for at least 15 hours per week, to children whose parents were on parental leave. Before the reform, municipalities could decide themselves whether to offer childcare to these children, and most did not. This implied increased access to childcare for older siblings following the reform, in municipalities that were previously restrictive. For the infant sibling, the same reform implied increased opportunities for one-on-one time with a parent during infancy and we study the human capital effects of this. The quantity and quality of parental time investments in early childhood has previously been shown to be important for human capital development (Fiorini and Keane, 2014; Hsin and Felfe, 2014; Del Bono et al., 2016; Fort et al., 2020, Ginja et al., 2020a). This paper adds to the existing literature by exploring effects of *exogenously* increased opportunities during *infancy*.

We estimate reduced form effects in a differences-in-differences (DD) setting, comparing infants with and without siblings of childcare age, before and after the reform, in municipalities that were affected by the reform. The restrictiveness of childcare access before the reform is determined using the Parental survey in 1999, conducted by the National Agency for Education. The survey response allows us to calculate the average difference in enrollment between working parents and parents on parental leave for each municipality, and rank accordingly. The top quintile group (with a difference of 82 percentage points) constitutes the reform municipalities, our main analysis sample.

We find no overall effects on the children's human capital, measured as the sixth grade test scores. There is however evidence of positive effects on test scores for boys, driven by sons of less than university educated mothers. There is no corresponding overall effect for girls, but suggestive evidence of positive effects for daughters of highly educated mothers. As we explore potential mechanisms, we find no evidence of changes in quantity of parental time during infancy (such as the timing of childcare enrollment), or human capital spillovers from the older sibling. Instead, we find improvements in physical and mental health in school age for boys. The findings are consistent with improved quality of parent-infant interactions *per se*, as a result of less competition for parental time.

The third essay, **Seasonality of Childcare Enrollment**, is a descriptive paper. Here, I characterize how the number of childcare enrollments varies systematically over the year with peaks in August and January, a pattern that aligns with the seasonal variation in childcare supply implied by the institutional structure of the childcare system. Although childcare enrollment is possible throughout the year, the match between households and childcare providers is presumably worse when supply is scarce (SOU, 2013), and consequently parents are incentivized to enroll their child in childcare during high supply.

The date of childcare enrollment is not available and therefore proxied using detailed data on payments of parental leave benefits. Although the proxy suffices for descriptive purposes, the lack of enrollment data implies that causal effects cannot be captured. Yet, the descriptive analysis provides insights regarding consequences of the structure of the childcare system, to which most Swedish parents are exposed.

I find that more than 50 percent of all children are enrolled in either August or January, a pattern that cannot be explained by variations in birthrates or household characteristics. I also observe that the seasonal variation in childcare enrollments is stronger for children of parents with high socioeconomic status, likely because they can afford to delay childcare enrollment until supply is high and higher quality childcare is available. Meanwhile, financially constrained households have a restricted choice set and the seasonal supply of childcare can aggravate social inequality.

The seasonality in childcare enrollments translates into differences in the age at childcare enrollment by birth month. While there is an extensive literature using the timing of birth to capture effects of school starting age (e.g., Fredriksson et al., 2021; Black et al., 2011), how the timing of birth affects the age at childcare enrollment is less explored. Yet, the literature suggests that the variation in age at childcare enrollment is potentially important for the human capital accumulation of children (e.g., Drange and Havnes, 2019; Fort et al., 2020). The age at childcare enrollment, and consequently the duration of parental leave, can also affect the household in the short- or medium time horizon. The impact on the household is explored in a tentative analysis and the findings are consistent with the uptake of fathers' parental leave being a function of total length of parental leave. Meanwhile, there is no indication of consequences for fertility, marital stability or earnings trajectory.

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Chapter 1

Parental Leave Quotas

Peer Effects and Workplace Related Costs*

*A previous version of this paper was circulated under the title "Peer effects and parental leave of fathers". I want to thank my main supervisor Anne Boschini for endless support and valuable feedback. I also want to thank Erica Lindahl, Katrine Vellesen Løken, Ashley Craig, Erik Lindqvist and Lisa Laun, colleagues at SOFI and seminar participants at Stockholm University and Copenhagen University.

1.1 Introduction

Fathers' uptake of parental leave continues to be low in most countries (OECD, 2016). Two reasons are often advanced for the existence and persistence of the unequal division of parental leave: monetary incentives (Becker, 1991) and norms (Akerlof and Kranton, 2000).[1] The uneven division of caretaking for children has been shown to explain much of the gender earnings gap (e.g., Bertrand et al., 2010; Angelov et al., 2016; Kleven et al., 2019). To encourage a more equal division of caring responsibilities between parents, the European Parliament mandated all member states in 2019 to reserve at least two months of paid parental leave for each parent.[2]

In addition to the direct effect of parental leave quotas, Dahl et al. (2014) have shown that the Norwegian quotas triggered peer effects among brothers and coworkers, further affecting the extensive margin of parental leave uptake. When few men take parental leave, increased uptake of a peer can provide novel information and encourage marginal fathers to take more parental leave than they otherwise would. But what happens when most fathers already take some days of parental leave? Do quotas still trigger peer effects at the workplace level? And how do these potential spillovers vary with characteristics of the workplace? Are spillovers stronger in workplaces where parental leave is relatively costly (and uptake is previously low): where fathers are more difficult to replace or where coworkers are less likely to share parental leave themselves?

In this paper, I investigate whether the introduction and expansion of parental leave quotas in Sweden each triggered spillovers at the workplace level, thereby increasing the number of days of parental leave taken by male coworkers. I also analyze whether these spillovers differed by

[1]Monetary incentives relate to the theory of intra-household specialization as proposed by Becker (1991). This is consistent with more recent findings of fathers' parental leave being sensitive to the replacement rate (Skyt Nielsen, 2009; Jørgensen and Søgaard, 2021), relative opportunity costs within the household (Angelov et al., 2016), different marginal costs (Bygren and Duvander, 2006), and different margins where parental leave can be bargained (Bekkengen 2002). Akerlof and Kranton (2000) instead focus on gender identity and the importance of conforming to the group norm. This is consistent with research comparing division of parental leave (Moberg and Van der Vleuten, 2021) and the child penalty (Kleven et al. 2021) for biological parents relative to adoptive parents.

[2]Directive 2019/1158.

workplace characteristics.

Previous studies have found that the direct response to the Swedish quotas was strong (e.g., Ekberg et al., 2013; Avdic and Karimi, 2018), but indirect reform effects between peers have not been estimated before. Introduced in 1995, the first quota reserved one month for each parent from the existing parental leave days. In 2002, when the second quota month was implemented, the total parental leave length was increased correspondingly. Already before the introduction of the first quota, a majority of fathers took some parental leave, but the reform increased the uptake further. Thus, another potentially important difference between the first and second reform is that the norms of gender and parenthood were different at the time of implementation, encouraging more active parenting of fathers in 2002 than in 1995 (as measured by a higher uptake of parental leave among fathers, and attitudes in the World Values Survey). Analyzing spillover effects at the workplace level of both the introduction and expansion of parental leave quotas in Sweden allows for comparisons of policy design and context, with possible policy implications.

Following the identification strategy first proposed by Dahl et al. (2014), I estimate spillovers using a Regression Discontinuity (RD) design. I focus on the reduced form estimate, which captures the average additional reform effect on the number of days of parental leave taken by *fathers*, who are all covered by the quota, but randomly exposed to a *peer*(coworker) whose child was born earlier, and also covered by the quota. I add to the findings of Dahl at al. (2014) by evaluating the effects on the number of days of parental leave rather than focusing on the share of fathers taking any leave. Because it is increasingly common that fathers take at least some parental leave (Eurofond, 2019), the intensive margin is the relevant margin of quota spillovers for fathers in many countries. Yet, the intensive margin is previously unexplored. Furthermore, in line with the literature on household incentives, I examine new dimensions of heterogeneous responses at the workplace: monetary and normative costs of parental leave.[3]

In a flexible system such as Sweden, it is important to also consider

[3] Monetary costs of parental leave at the workplace refers to the cost of substitution facing the employer, which can be internalized by the worker (e.g., Hensvik and Rosenqvist, 2019; Ginja et al., 2020). Normative costs capture the attitudes towards male parental leave among coworkers based on their (or their potential partner's) parental leave uptake as predicted by their individual characteristics.

when the parental leave is taken in addition to the number of days. Contrary to most countries where parental leave is used during infancy, parental leave in Sweden can be used to extend summer vacations, reduce working hours, and supplement household income until the child is eight years old. Early parental leave is favorable for long-run effects on household responsibilities and subsequent labor market attachment (Duvander and Johansson, 2019). Therefore, my main outcome is parental leave in the first two years following birth, but I construct several other measures of parental leave to characterize the quality of leave in response to the reforms.

I find no indication of spillovers from the first reform, the reduced form estimate is consistently small and insignificant. Meanwhile for the second reform, I find weak evidence of spillovers at the workplace; the reduced form estimate for the second reform is significant and substantial at seven days in the first two years.[4] However, the standard errors are large and the estimate is somewhat sensitive to details of the specification. The total uptake of parental leave (by age eight) shows no corresponding increase for the second reform, meaning that the potential spillovers in the first two years are capturing a reallocation of parental leave to be used earlier. In fact, the effect largely appears in the first year of life. Thus comparing the two reforms, the analysis suggests that the second reform was more successful at triggering a positive trend in fathers' involvement at a relatively young age of the child.[5]

The fact that there are (weak) spillovers only for the second reform is possibly explained by differences in the perception regarding the reforms in particular and parental leave of fathers more generally. The design of the second reform was less restrictive; for instance it did not reduce the days of parental leave available to mothers. In addition, parental leave was more socially accepted at the time of implementation of the second reform, compared to the first reform. Consequently, the expansion was presumably perceived more positively than the introduction of parental leave quotas, which could affect the potential for spillovers. The difference in restrictiveness is also put forward by Avdic and Karimi (2018) to explain

[4]The corresponding 2SLS-estimate is 0.534 (0.247). However, this should be interpreted with caution as the exclusionary restriction is not convincingly satisfied.

[5]Parental leave in the first two years is also the relevant time window for implications to countries with less flexible parental leave schemes (e.g., Iceland, Finland, and Norway) (OECD, 2021).

why they found effects on marital stability following the first but not the second reform. Given the flexibility of the Swedish system, it could also be that the duration of cohesive parental leave by peers was sufficiently long to trigger spillovers only after the extension in 2002.

I find that there is no significant heterogeneity in the reform response with respect to monetary or normative costs of parental leave at the workplace. Although parental leave uptake before the reforms was lower in workplaces characterized by high costs, the response among peers and fathers is not statistically different. Yet, the accumulated effect on fathers from the second reform almost entirely closed the initial gap in parental leave with respect to monetary costs. The gap with respect to predicted (male) parental leave at the workplace is largely unaffected, which is consistent with normative costs being more persistent. This finding contributes with a new perspective to the literature on peer effects that has so far established a stronger peer effect in workplaces characterized by high job security (Dahl et al., 2014; Welteke and Wrohlich, 2019; Lassen, 2021). My analysis, which uses new measures to capture costs of parental leave, is consistent with previous findings of initial differences–i.e., men internalize costs at the workplace and adjust the uptake of parental leave accordingly. However, these costs do not seem to be very important for the quota response.

This paper contributes to the literature on peer effects on parental leave of fathers by estimating quota spillovers on the days of parental leave taken by male coworkers, when the uptake was relatively high already before implementation. Previously, extensive margin spillovers from parental leave quotas has been studied by Dahl et al. (2014). They estimate a fuzzy RD design and find substantial peer effects, increasing the fraction of coworkers and brothers who take at least some parental leave. Dahl et al.'s (2014) identification strategy has previously been applied to reform induced peer effects on the intensive margin for mothers. Lassen (2021) found a strong peer effect on the duration of leave for sisters in Denmark, and Welteke and Wrohlich (2019), adopting a similar instrumental approach, found increased probabilities of staying home the first year among female coworkers in Germany. Because both reforms and outcomes are different across studies, the findings are difficult to compare. The peer effect I find for the second reform is large relative to previous findings, but it is also imprecisely estimated and should be interpreted with caution. Yet, my paper suggests that how and when quotas are implemented affects the scope for peer effects. Another related paper is the parallel and independent work of Carlsson and Reshid (2022). Although they do not capture reform spillovers, they estimate peer effects in parental leave at Swedish workplaces using the variation in parental leave uptake of the family members of peers, often referred to as a "peer of peer" instrumental approach. They also find significant peer effects on the number of days taken by fathers (and mothers). While their estimated peer effect is less local than reform induced variation, allowing for a greater variation in the first stage, it is also less exogenous. One more related paper is that of Johnsen et al. (2020), who find that the Norwegian quota triggered also spillovers in terms of improved career trajectories of coworkers, consistent with competition effects.

My paper also contributes to the literature evaluating effects of the parental leave quotas, especially the Swedish reforms. The first stage of both the introduction and the extension of parental leave quotas has been evaluated previously and the direct response is consistently strong (e.g., Eriksson, 2005; Duvander and Johansson, 2012; *Försäkringskassan*, 2019b). The consequences of increased parental leave uptake of fathers have been estimated for a wide range of outcomes: employer responses (Ginja et al., 2020), human capital formation of children (Avdic et al., 2022), marital stability (Avdic and Karimi, 2018), fertility (Duvander et al., 2020), and mothers' sickness absence (*Försäkringskassan*, 2015). Regarding gender equality, Ekberg et al. (2013) estimated effects for household work (measured as care for sick children) and labor market outcomes, but found no robust effect from the first reform. Ekberg et al.'s (2013) findings are consistent with the insignificant effect on earnings found by Johansson (2010), Duvander and Johansson (2013), and Karimi et al. (2012), evaluating both reforms.[6] The international literature has typically found quotas to have a positive impact on gender equality,[7]

[6]Duvander and Johansson (2019) found a positive impact on fathers' share of care for sick children from the first reform (but not the second), although this is driven by a reduction of mothers' uptake rather than fathers' changed behavior. The different findings relative to Ekberg et al. (2013) are explained by differences in specification and time horizon for which outcomes are measured.

[7]For example, see Kotsdam and Finseraas (2011) for Norway, Druedahl et al. (2019) for Denmark, and Patnaik (2019) for Quebec. See Canaan et al. (2022) for an excellent review of the most recent literature.

which makes Swedish quotas, and spillovers in particular, especially relevant for further study. These types of spillovers between peers have not been estimated in the context of Sweden. Yet, the extent to which quotas trigger an increasing trend of fathers' involvement during infancy is presumably critical for the impact on gender equality in parenting. As many countries have or are in the process of implementing similar quotas, lessons from Sweden are relevant also internationally.

This paper proceeds as follows. Section 2 describes the institutional context. Section 3 presents the empirical strategy. Section 4 presents data and descriptives. Section 5 presents the results. Section 6 describes the robustness of the findings. Section 7 provides some concluding remarks.

1.2 The institutional context

In 1974, the maternity leave insurance was replaced by the parental leave insurance, which gave both parents equal rights to share the six months of paid leave. At that time, fathers took 0.5 percent of all parental leave days. Since the reform, the number of days with parental leave benefits available to both parents, as well as the fraction of days used by fathers, have increased gradually (*Försäkringskassan*, 2014). The total amount of parental leave benefits in 1995 (2002) was 450 (480) days.[8] At the time of birth, fathers are also entitled to ten days of temporary paternity leave, which is in addition to the 450 (480) days, and take-up rates of these have been at a constant high rate (own calculations).[9] In addition to the benefits paid by the social insurance office, many employees are

[8]Benefits come in two forms; 90 days are a low flat rate and the remaining days are income based. The income replacement is based on capped income, which was in 1995 binding for 12 percent of fathers and 4 percent of mothers (Ekberg et al., 2013). The first 180 days of income replacement are subjected to a working condition of employment for 240 days before the due date. If this requirement is not met, the replacement is instead flat. The low flat rate was 60 SEK/day in 1995 and 180 SEK/days since 2002. Also, parents without a sufficiently high income receive the flat rate (*Försäkringskassan*, 2022). Parents are also covered by job protection 18 months after birth and the sickness benefit qualifying income (SGI) is maintained for 12 months irrespective of their use of parental leave benefits. Consequently, parents who are willing to accept a lower replacement can disperse days to last longer. Unpaid days of parental leave before childcare enrollment are used especially by mothers (*Försäkringskassan*, 2020).

[9]Because days of paternity leave are reported separately, they are not included in the data on parental leave used in the main analysis.

covered by collective insurances that increase the replacement during parental leave (Sjögren Lindquist and Wadensjö, 2005).[10] Each parent is entitled to three periods of parental leave every year, if applied for at least two months in advance.[11] It is possible to save days of parental leave benefits up to eight years, and a substantial fraction of the paid parental leave days is used after childcare enrollment to extend vacations or reduce working hours (Hall and Lindahl, 2018).

In 1994, the year before the first parental leave quota, 54.5 percent of all fathers took some parental leave, and the average number of days was 42.5 (by age eight, own calculations). In May 1994, the government bill targeting gender equality was passed and changes to the parental leave insurance were implemented on January 1, 1995.[12] The new law stipulated that 30 days of the 360 days of parental leave with income replacement should be reserved for each parent. As this was more often binding to fathers, this reform has been referred to as a ”daddy-month” reform. The replacement rate was also lowered to 80 percent, except for the quota days, which remained at 90 percent.[13] Since 1998, the quota days were replaced at the same level as the remaining income based parental leave benefits at 80 percent until 2001, when the effective replacement rate was slightly reduced to 77.6 percent. The first quota month introduced in 1995 was followed by a second month in 2002.[14] Before the second reform, 88.8 percent of all fathers took some parental leave, and the average number of days was 68.9 (by age eight, own calculations). The second parental leave quota was accompanied by a corresponding increase of total days and the replacement rate was unchanged (*Försäkringskassan*, 2014).

[10] There was great heterogeneity in collective agreements until the 2000s when especially the municipal and private sector agreements became increasingly generous and caught up with the state sector (Duvander et al. 2020).

[11] Parents are also entitled to reduce working hours by 25 percent (irrespective of use of parental leave benefits).

[12] Prop. 1993/94:147.

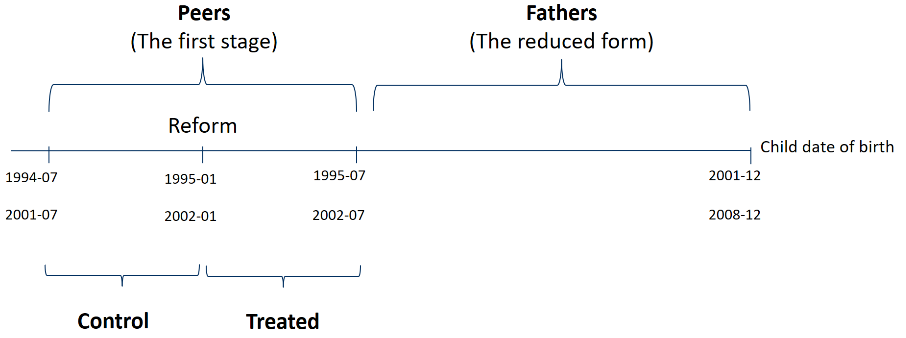
[13] Children born before the reform received the higher rate of parental leave benefits for days taken within two years (Ekberg et al. 2013). The new law of 1995 also reinstated the 90 days of minimum pay at 60 SEK/day, which was removed in 1993 (*Försäkringskassan*, 2019a). Because the oldest children of this analysis are born in 1994, they were too young to receive the alternative allowance, which was temporarily in place (*vårdnadsbidrag*). Instead, all children included had access to the flat minimum pay.

[14] Prop. 2000/01:44, agreed upon on March 22.

1.3 Empirical strategy

I follow Dahl et al. (2014) and make use of pre-existing networks at the workplace to capture reform spillovers in a Regression Discontinuity (RD) design. Exogenous assignment of treatment to members in pre-existing peer groups efficiently deals with the problems related to estimation of peer effects raised by Manski (1993).[15] Averages and functional form of pre-characteristics may differ on each side of the cutoff due to strategic planning of conception, but close to the threshold date of birth should be random such that the RD estimates capture the causal effect of randomly assigned quotas. I apply a reform window of six months on each side of the date of implementation, January 1, 1995 and January 1, 2002, respectively (see Figure 1.1).[16]

Figure 1.1: Timeline



Notes: Timeline of when children of peers and fathers are born for the first and second reform. The treatment status of both peers and fathers is determined by the birthdate of peers.

Males whose child is born within the reform window are referred to as *peers* and they are directly affected by the reform. The treatment status is based on the date when their child is born relative to the date of reform implementation. Treated peers are assigned 30 days of parental leave benefits that cannot be transferred

[15]These include the reflection problem, endogenous group membership, and correlated unobservables.

[16]The size of the reform window affects the composition of fathers at the included workplaces. To match the previous study of reform spillovers for fathers by Dahl et al. (2014), I use the same six-month reform window in my main specification.

to the mother and consequently are forfeited if not used by him. Spillovers are measured for male coworkers whose child is born after the reform window (June the year of implementation) and within six years. These are referred to as *fathers* and they differ only by the treatment status of their peers. Thus, the reduced form effects are driven by reform induced changes in the parental leave uptake of peers.

I estimate the regression discontinuity models:

$$PL_{j,g} = \alpha_j + 1(t_j \geq c)\Big(f_l(t_j - c) + \lambda\Big) + 1(t_j < c)\Big(f_r(c - t_j)\Big) + \beta X_g + e_j \quad (1.1)$$

$$PL_{i,g} = \gamma_i + 1(t_j \geq c)\Big(h_l(t_j - c) + \pi\Big) + 1(t_j < c)\Big(h_r(c - t_j)\Big) + \beta X_g + e_i, \quad (1.2)$$

where peers are denoted j and fathers are denoted i, employed at the same workplace g. The outcome of interest is parental leave uptake, denoted PL in the specification. For the main analysis, this is parental leave in the first two years following birth. The cut-off—January 1, 1995 for the first reform and January 1, 2002 for the second—is indicated by c. t_j is the date of birth of the peer's child. The sign of the difference $t_j - c$ indicates treatment status, such that positive (negative) implies treated (control). f_l, f_r, h_l, and h_r are the unknown functions on each side of the cut-off. In my main specification, I use separate quadratic trends. X_g captures fixed effects for the birth year of the father's child, as well as group specific covariates: share of males, number of employees, and municipality fixed effects. Standard errors are clustered at the running variable: the date of birth of the peer's child.

The coefficient λ is the first stage estimate, and captures the direct reform response in parental leave among peers, following the implementation of the reform. The reduced form estimate, π, captures the corresponding indirect effect on fathers with a treated peer, compared to those with a pre-reform peer. The reduced form estimate captures the average effect of working with a peer whose child is born after the reform. The identifying assumption for the reduced form estimate is the independence assumption, which requires that the assignment variable (the date of birth of the peer's child) is non-manipulable and as good as random.

The reduced form estimate (equation 1.2) divided by the first stage (equation 1.1) gives the fuzzy RD (2SLS) estimation of the peer effect, as reported by Dahl et al. (2014). The estimate captures the *marginal* effect of increased peer parental leave–i.e., the father's response to a one-day increase in the uptake of the peer. However, this estimate relies on additional assumptions that are not credibly fulfilled in the Swedish setting. In particular, the exclusion restriction, which in this setting requires that fathers are affected only via the increased parental leave uptake of the peer. As Ekberg et al. (2013) have shown, the reforms not only affected the amount of parental leave but also when parental leave is taken.[17] It is possible that also the monotonicity assumption is violated, meaning that some fathers may respond to the increased peer uptake by instead reducing their own days of parental leave.[18]

1.4 Data and descriptives

The empirical analysis is based on data from several Swedish registries and individuals are linked by unique identifiers. The population of interest is children born between 1994 and 2008 and their fathers.

Fathers (and mothers) are linked to children in the Multi-Generation Register where all biological and adoptive links are mapped. To this dataset, I add parental characteristics from the Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA) by Statistics Sweden, which covers everyone above the age of 15 registered in Sweden. From LISA I retrieve the information about characteristics such as immigrant status, education, employment, and income both before and after birth of the child. The workplace identifiers enable the matching of peers and fathers. The workplace identifiers are also used to construct the workplace controls—i.e., the share of men in the workplace in the year before the reform and the total number of employees—as well as the costs of parental leave.

The outcome of interest is the parental leave uptake of fathers. The

[17]In addition, the first reform affected also the replacement rate, while the second reform increased the total days of parental leave available. Both features can affect the potential spillovers via, e.g., age at childcare enrollment.

[18]Fathers would reduce their uptake if for instance quotas induced parental leave of peers that revealed high costs of absence or if increased uptake of peers also increased the gains from reduced parental leave (in line with the competition effects found by Johnsen et al., 2020).

measures of parental leave are based on data from the parental leave registry from the database MiDas provided by the Social Insurance Office (*Försäkringskassan*). The dataset contains date of birth and information about parental leave by child and beneficiary (most often the parents). The information includes days of paid leave, benefit amounts, and replacement rates by constructed episodes. The episodes consist of paid and unpaid days that are assessed to constitute a cohesive period of parental leave and the registry also contains exact start and end dates for these episodes.[19]

1.4.1 Sample restrictions

I estimate the spillovers for the first father at the workplace to have a child born after the reform window.[20] Sampled fathers have a single eligible peer working at the same workplace the year before the reform. Eligible peers refer to male coworkers with the same or higher educational level,[21] whose child is born within the reform window of one year centered around the reform date. I require the educational level of peers to be at least as high as the educational level of the father to target pairs more likely to interact and peers more likely to be influential. I make no restriction on the parity of the child of either peer or father. However, I want to focus on spillovers on workers and therefore remove pairs where the father is coded as a business owner. Although the sampling restrictions imply that few large workplaces are included, I also impose a restriction of at most 150 employees in each workplace to target workplaces where peers and fathers are more likely to interact.[22]

Given the width chosen for the reform window, children of peers are born within 6 months of the reform. There is no implied restriction on the year of birth for children of the fathers. In the analysis, I include births in the six years following the reform. That is, for the first reform

[19] A episode of parental leave allows for at most 6 days of unpaid leave between paid days of parental leave. See Duvander (2013) for a discussion of the measure.

[20] Given the possible dynamic effects in terms of competition as proposed by Johnsen et al. (2020), inclusion of subsequent fathers is potentially complex and not the main specification.

[21] This is measured in three levels the year before the relevant reform. The levels are compulsory school (up to grade 9), upper secondary school (including some university), and university (at least 2 years).

[22] The main results are also presented for workplaces with fewer than 30 employees.

children are born between 1995 and 2001 and for the second reform the corresponding range is between 2002 and 2008.[23]

The sample restrictions yield an analysis sample of workplaces that are non-representative of the full population, but constant over the two reforms. There are a variety of sectors included with a fair representation of male workplaces (construction, consultancy/business, and wholesale being in the top). Appendix Table A2 shows that the percentage of males in sampled workplaces is about 9 percentage points higher than the average, 72 percent compared to 63.5 percent, which is reasonable given that all female workplaces are removed. Similarly, sampling affects the average number of employees as few small workplaces are included, and single-employee workplaces are removed entirely. In both samples, the average number of employees is about 30 compared to the total average of 9. Differences in the median are even larger in relative terms, 20 compared to 2.[24] Furthermore, the norms regarding male parental leave is higher in the sampled workplaces relative to the full population.

Similarly, sampled peers and fathers differ from the full population of men who have a child in the same time frame. Appendix Tables A3 and A4 show that sampled men have a higher income and are less likely to have an immigrant background. Sampled peers are also higher educated, while fathers are slightly older and more likely to be married. Parental leave use is higher, both on the extensive margin and in number of days, and the average response to the reform was greater in the sample. The observed differences are not surprising as the sample is restricted to working fathers. Comparing the first and second reform, there are significant differences between the two samples for both peers and fathers. However, the differences are consistently small and do not indicate that either was better off overall.

Measures of parental leave

Using the parental leave registry, I create several measures for parental leave. The main outcome, parental leave during the first two years

[23] See Appendix Table A1 for frequencies by cohort. There is a strong correlation between the number of employees at the workplace and birth year of the first father; fewer employees imply a longer average gap between the births.

[24] The median of 20 is smaller than the median of 27 for the sampled Norwegian workplaces in Dahl et al. (2014).

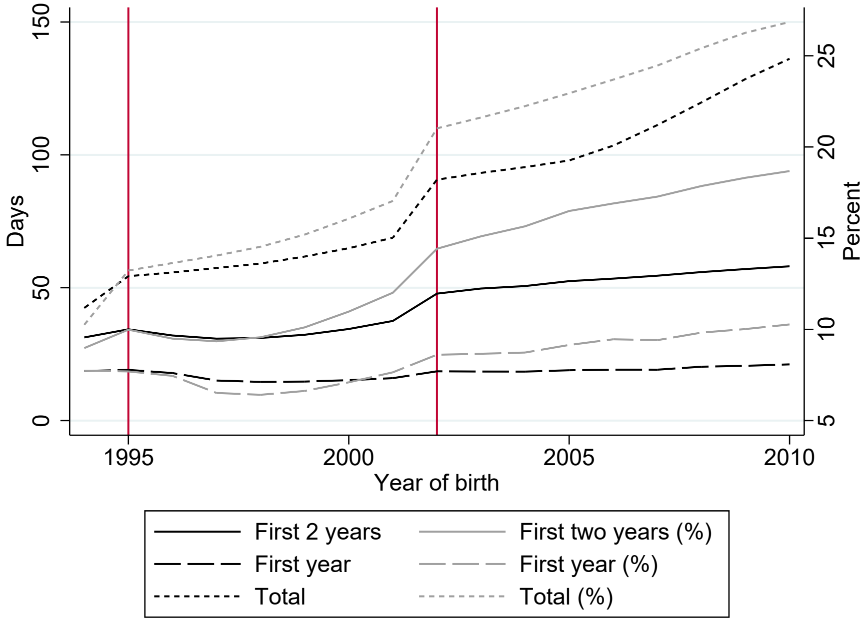
following birth, is constructed using the exact date of birth. By restricting the uptake to the first two years, I target parental leave before childcare enrollment. Compared to parental leave after childcare enrollment, this is more likely to reflect a cohesive period of being the main caregiver which is favorable for parent-child attachment, and where one could expect effects on long-run outcomes in terms of household responsibilities and subsequent labor market attachment (Duvander and Johansson, 2019). I also measure the parental leave uptake during the first year of birth. Parental leave in the first year is potentially more favorable in terms of long-run outcomes, but it is a too narrow time span to fully capture fathers' parental leave before childcare enrollment.[25] I also report the total number of paid days by age eight. Furthermore, I construct two measures to identify parental leave likely used to reduce working hours, supplement income, and extend vacations: parental leave taken during weekends, and dispersion of parental leave.[26]

The constructed measures of parental leave are not constant across time. For the full population of children born between 1994 and 2010, there is a positive trend in the uptake of fathers for all measures (see Appendix Figures A1–A3). Figure 1.2 shows that this is true for both the number of days and as the percent of the household total.

[25]The average age of enrollment is 18 months (Duvander, 2006) and fathers typically take parental leave last (Boye and Evertsson, 2018).

[26]Weekends are defined as 1–3 days of benefits, with a majority of days allocated to the weekend. Dispersion is measured as the number cohesive episodes of parental leave. To construct the number of episodes, I collapse episodes (including unpaid gaps of at most 6 days) that are reported separately, but between which the unpaid gap is fewer than 5 days.

Figure 1.2: Parental leave of the full population of fathers



Notes: The uptake of paid parental leave of fathers in the first two years, the first one year, and in total (by age eight). Percent is relative to the total household uptake. Red vertical lines indicate the first and the second reform.

For parental leave at a relatively young age, there is an increasing gap where the percentage of leave taken by males is increasing more than the absolute number of days. The number of days taken by men in the first year is in fact almost constant although the percentage is increasing. The divergence between the two measures is driven by fewer days of parental leave taken by mothers, reducing the uptake of both parents combined.[27] For total parental leave, the difference between relative and total increase is fairly constant since the second reform.[28]

[27]The lower uptake of mothers reflects both earlier childcare enrollment and a higher uptake of unpaid leave before childcare enrollment.

[28]Parental leave of inferior quality: during weekends and dispersed across time, is increasing similarly for both mothers and fathers (see Appendix Figure A3). There is a discontinuity in 2006 after which the increase is steeper. This is coinciding with an increase of the amount paid for the 90 minimum-pay days, from 60 to 180, which can be used more flexibly than the income-based days.

Measures of costs

In the heterogeneity analysis, I estimate workplace-specific reform responses. Survey-based studies have found features of the workplace to influence parental leave of fathers (Haas and Hwang, 1995; Haas et al., 2002), but how this affects the response to parental leave quotas has received little attention. Differences in peer effects with respect to job security was estimated by Dahl et al. (2014) and later for mothers by Welteke and Wrohlich (2019) and Lassen (2021). In this paper, I characterize how the reform response differs with respect to workplace-specific costs related to parental leave. Costs can be monetary and refer to earnings and the career trajectory, but they can also have a non-monetary component related to preferences and norms. Bygren and Duvander (2006) found suggestive evidence of fathers internalizing costs of parental leave as they are less likely to take parental leave if uptake among coworkers is low or when the workplace is either small, private, or male dominated. Quotas have the potential to target both monetary and normative costs of parental leave by increasing the reference point of fathers' parental leave; therefore, I expect the spillovers to increase with costs.

The measure of monetary costs of parental leave at the workplace refers to the costs of absence incurred by the employer. The implied cost can be internalized by the worker who wants to accommodate the employer and avoid possible reprisals. If there is a sufficient pool of qualified coworkers, the tasks of the worker on parental leave are likely covered internally. If there is instead a shortage of qualified coworkers, parental leave may require hiring a temporary employee, who is often more costly to the employer. The definition of monetary costs is consistent with the findings for parental leave in Sweden (Ginja et al., 2020), Norway (Brenøe et al., 2020), and Denmark (Gallen, 2019). Ginja et al. (2020) also found that workers are less likely to extend parental leave when internal substitutes are few, suggesting that they internalize the cost of absence, which is consistent with the findings for sickness absence by Hensvik and Rosenqvist (2019).[29] Previous studies have defined substitutability using size of the workplace (Gallen, 2019), the occupational code (3-digit (Hensvik and Rosenqvist, 2019) and 1-digit (Brenøe et al., 2020)) and a

[29] Hensvik and Rosenqvist (2019) found that in addition to the changed behavior of the worker, the lower absence of workers who are difficult to replace is also driven by employers sorting of workers (according to their absence relative to substitutability).

composite measure of educational level and field (Ginja et al., 2020). For my main analysis of heterogeneous effects, I define substitutability using three levels of education. I choose a wide definition to keep as much of the sample as possible because I also impose the restriction of peers and fathers facing the same cost of absence at the workplace. I consider costs to be low where the pool of potential replacement, as indicated by the number of coworkers with the same education in addition to the peer and the father, exceeds two coworkers.[30]

Normative costs of parental leave at the workplace refer to the attitudes towards parental leave of men, among the coworkers. If coworkers consider mothers to be the primary caregiver, fathers who take parental leave may encounter reprisals and social exclusion for not conforming to the gender norm. To construct the measure of normative costs, I predict parental leave uptake in the first two years. Specifically, I use all births two years before each reform and regress the parental leave of those fathers. The outcome of interest is an indicator variable, taking the value one for parental leave above the reserved number of days (corresponding to any leave before the first reform, above 30 days before the second reform). For the mothers, I use the parental leave of their male partner. The analysis is based entirely on individual characteristics: birth year, municipality, education, earnings, household disposable income, employment status, occupational rank, industry, sector, marital status, and immigrant status.[31] From this, I obtain predictions of male parental leave, which is assigned to everyone at the workplace, both men and women. Workplaces are ranked according to their average prediction (excluding the peer and the father) and the sample is divided into two based on the sample median, with low average prediction indicating high normative costs. An advantage of this measure relative to the more common measure of average parental leave (Bygren and Duvander, 2006; Lappegård, 2012) is that observed behavior also captures other internalized costs, whereas predicted attitudes only capture individual characteristics.

[30] In the robustness section, I relax the restriction of peers and fathers having the same level of education, allowing for workplaces without any internal substitution. I also test different definitions of substitutability.

[31] R^2 of about 0.1.

Table 1.1: Workplace characteristics by costs

	(1) Male share	(2) Size	(3) Private	(4) N
First reform			*Monetary*	
Low	0.705	34.611	0.777	7,955
High	0.851	6.635	0.960	1,207
Reform 2002				
Low	0.709	35.566	0.836	8,689
High	0.866	6.498	0.972	1,091
Second reform			*Monetary*	
Low	0.700	34.202	0.697	4,585
High	0.749	27.644	0.904	4,577
Reform 2002				
Low	0.743	33.085	0.841	4,891
High	0.710	31.562	0.860	4,889

Notes: Monetary cost is the number of coworkers with the same education grouped. Normative cost is the average predicted parental leave of coworkers based on pre-reform characteristics split into two. Male share is the share of males at the workplace. Size is the number of employees at the workplace and private indicates domestic or foreign private ownership.

Table 1.1 presents features of the workplaces according to the constructed costs. High monetary costs constitutes 13.2 percent and 11.2 percent for the first and second reform, respectively. The measure of monetary costs is correlated with other features of the workplace; high monetary cost workplaces are on average smaller, have a higher fraction of male employees, and are to a larger extent privately owned. However, there is no clear distinction in terms of sector across monetary costs. Normative costs divide the sample of workplaces into two equally-sized groups. As the prediction of male parental leave is assigned to both male and female coworkers, there is no correlation between normative costs and the percentage of male coworkers by construction. Table 1.1 shows that the fraction of males is relatively constant across normative costs, but workplaces with high normative cost are slightly smaller and more likely to be private. Although construction and retail are dominating sectors across normative costs, low-cost workplaces also include computer science, business, and education sectors, whereas maintenance/repairs, transportation, and industry are characterized by high normative costs.

1.4.2 The independence assumption

For the RD design to consistently estimate the reduced form, the independence assumption must be satisfied; that is, the treatment status should be as good as randomly assigned.

First, there can be no manipulation of treatment status such that parents have planned the birth or conception relative to the reform. For the first reform, the days were reallocated from the shared leave and there was a coinciding decrease in replacement rate for days not covered by the quota. Therefore, there was an incentive to give birth before the reform. Because the government bill for the first reform was passed less than 9 months before implementation, parents were unable to time conception accordingly and both C-sections and induced labor were rare at that time (Ekberg et al., 2013). For the second reform, the post period was advantageous; the total number of days available increased by 30 days and the flat rate minimum pay was increased. The second reform was agreed upon almost exactly 9 months before implementation, leaving a small window of merely a few days where forward looking couples could respond and delay conception. The densities in peer births reveal an imbalance: more children were born before the reform than after the reform (see McCrary test in Appendix Figure A4). Although this is in line with incentives from the first reform, the opposite applies to the second reform. Therefore, there is no reason to believe that the imbalance reflects manipulation of the treatment status. The corresponding densities of the placebo years suggest that the imbalance reflects a repeated seasonal pattern in timing of birth as the cutoff coincides with the turn of the year (see Appendix Figure A5). Therefore, the main specification is carried out using all observations (but I also test varying donut sizes).

Second, there can be no other discontinuity at the cutoff. The date of implementation does not coincide with any nation-wide reforms affecting the parental leave uptake of the studied fathers. However, exposure to treated peers could be non-random if the timing of birth among peers correlates with workplace and father characteristics.[32] Columns 1–3 in

[32]The control group consists of children born in the fall while treated children are born in the spring. Previous research has shown that timing of birth correlates with household characteristics; particularly, mothers of higher socioeconomic status tend to give birth in the spring (Buckles and Hungerman, 2013). This speaks in favor of the RD specification compared to the alternative Difference-in-differences specification.

Table 1.2 show that there is no significant discontinuity in features of the workplace for either reform, suggesting that workplaces are comparable across treatment status.[33]

Table 1.2: Balance of workplace characteristics

	(1) Spacing	(2) Male share	(3) Size	(4) PL peer	(5) PL father
First reform	0.027	-0.023	1.214	0.033***	0.015**
	(0.105)	(0.015)	(1.940)	(0.008)	(0.006)
Observations	11,597	11,597	11,597	11,016	11,031
Second reform	-0.135	0.016	-2.129	0.003	0.004
	(0.102)	(0.015)	(1.870)	(0.006)	(0.005)
Observations	12,309	12,309	12,309	12,279	12,297

Notes: Estimates from separate RD regressions on workplace characteristics. Spacing refers to the time between the birth of the peer's child and the father's child. Male share is the share of males at the workplace. Size is the number of employees at the workplace. PL peer and PL father refer to parental leave above the prescribed amount as predicted by their individual characteristics. All estimations include separate quadratic trends and triangular weights. Robust standard errors are in parentheses and are clustered at the running variable, peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1

Although workplace characteristics are well balanced, there is imbalance in the individual characteristics for the first reform. Using the full set of individual covariates to predict parental leave uptake of peers and fathers, columns 4 and 5 in Table 1.2 show a discontinuity for the first reform, suggesting that treated peers and fathers are more in favor of male parental leave. Appendix Table A6, showing the individual characteristics separately, reveals that the discontinuity is driven by a higher socioeconomic status of treated peers and fathers, especially higher earnings.[34] Reassuringly, the imbalance in individual characteristics is similar for the placebo reform, and the discontinuities for the first reform disappear in the Difference-in-Discontinuity specification (Appendix Table A7).[35]

[33] Also the constructed costs of parental leave measured at the workplace are well balanced. See Appendix Table A5.

[34] The greater imbalance in earnings for the first reform is consistent with a positive trend in earnings following the financial crisis in Sweden that ended in 1994.

[35] In the difference-in-discontinuity design, the placebo years (1996 and 2003) are introduced as a first difference to remove effects from seasonality. Although the

1.5 Results

The reduced form estimate captures the reform spillovers at the workplace. Although the amount of parental leave is not the only potential mechanism, it is the main channel that the reform is expected to affect fathers in addition to the direct effect. Previous literature has shown that the overall response to the quotas was strong (e.g., Avdic and Karimi, 2018), but to ensure that the sampled fathers are in fact exposed to increased parental leave uptake of peers, I first estimate the first stage of both reforms. Second, I estimate the reduced form regressions. The main outcome is parental leave in the first two years, but I also estimate spillovers on the parental leave uptake at other times. Main results are followed by an analysis of workplace specific reform effects, allowing for heterogeneous effects with respect to monetary and normative costs of parental leave at the workplace.

1.5.1 The first stage

Table 1.3 shows that there is a robust first stage for each reform; the number of days with parental leave taken by peers is significantly higher in the post-reform period.[36] Almost half of the total increase is taken in the first two years and the response is somewhat stronger for the first reform, 16.5 days compared to 14.5 days.[37] A greater impact of the first reform is consistent with previous findings; however, my estimates are

estimates from the Difference-in-Discontinuity become more complex, it is a useful exercise to show that imbalance is driven by the seasonality of birth rather than manipulation relative to the reform.

[36]The second reform affected essentially the intensive margin, whereas the first reform also affected the extensive margin of parental leave among males. About 60 percent of all males in my sample took some parental leave already before the first reform. In addition, the first stage was strong, increasing the fraction by about 35 percentage points, which leaves little variation remaining for potential spillovers on the extensive margin. If one also considers the 10 days of paternity leave by the time of birth, the pre-reform extensive margin was even higher, above 80 percent. The near full adoption of at least some parental leave makes the extensive margin in practice irrelevant for studying peer effects in Sweden. See Appendix 1.B for extensive margin figures and estimations, including a replication of Dahl et al. (2014) and an exercise where I target sectors with relatively low incidence among treated peers to allow for an additional effect on fathers.

[37]F-statistic of 69.54 and 99.02, respectively. See corresponding graphical results in Appendix Figure C1.

much larger, which is explained by the selected sample in my analysis.[38]

Table 1.3: First stage regression estimates

	(1)	(2)	(3)	(4)
	First reform		Second reform	
	First two years	Total	First two years	Total
First stage	16.273***	27.261***	14.536***	25.941***
	(3.225)	(3.886)	(4.137)	(4.226)
Observations	11,597	11,597	12,309	12,309

Notes: Estimates of the first stage from separate RD regressions for the first reform (left panel) and second reform (right panel). Columns 1 and 3 report the main results, parental leave in the first two years. Columns 2 and 4 report the parental leave in total (by age eight). All estimations include separate quadratic trends, triangular weights, workplace covariates, and cohort- and muni-fe. Robust standard errors are in parentheses, and are clustered at the running variable, peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1

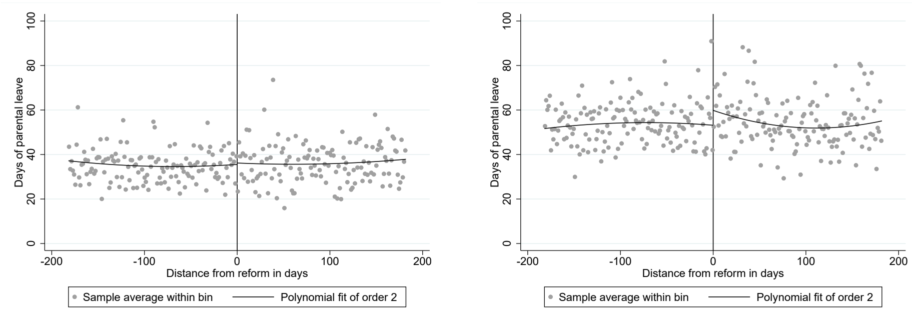
[38]Previous literature has shown that for all fathers, the first stage increase in parental leave is 4.9 and 3.4 days, respectively, in the first 17 months (Eriksson, 2005), 9.9 and 4.4 days, respectively, in the first two years (*Försäkringskassan*, 2019b) and about 15 days in the first eight years (Ekberg et al., 2014; Avdic and Karimi, 2018).

1.5.2 Main results

Having established a robust first stage, we now turn to the spillovers of interest. Figure 1.3 shows the days of parental leave taken by fathers in the first two years by the date of birth of their peer's child.

Figure 1.3: Reduced form

(a) First reform **(b)** Second reform



Notes: Each observation is the average days of parental leave taken by fathers in the first two years, by date of birth of the peer child (normalized to the date of implementation). The vertical line indicates the date of the reform, January 1, 1995 (left) and January 1, 2002 (right). The fitted line is a second order polynomial.

For the first reform (Panel A of Figure 1.3), there is no visible discontinuity; however, for the second reform (Panel B of Figure 1.3), there is a jump at the cutoff. The discontinuity suggests that the second reform triggered spillovers at the workplace, further increasing the average parental leave uptake of fathers who are working with a peer covered by the quota.

Table 1.4 presents the corresponding reduced form RD estimates in the first two years (Column 1) and by other measures of parental leave with respect to timing.

Table 1.4: Reduced form regression estimates

	(1) First two years	(2) First year	(3) Total	(4) Weekend	(5) Episodes
First reform					
Reduced form	1.301	-0.089	1.583	-0.052	0.103
	(3.127)	(2.217)	(3.561)	(0.241)	(0.273)
Control mean	30.061	17.710	39.920	0.898	1.913
Observations	11,597	11,597	11,597	11,597	11,597
Second reform					
Reduced form	7.777**	4.570**	2.541	-0.195	-0.569
	(3.740)	(2.284)	(4.420)	(0.426)	(0.404)
Control mean	40.931	16.832	71.605	1.614	5.325
Observations	12,309	12,309	12,309	12,309	12,309

Notes: Estimates of the reduced form from separate RD regressions. Column 1 reports the main results, parental leave in the first two years. Column 2 reports the parental leave in the first year and column 3 reports the total number of days of parental leave (by age eight). Column 4 reports the number of days allocated to weekends and Column 5 the number of cohesive periods of parental leave. All estimations include separate quadratic trends, triangular weights, workplace covariates, and cohort- and muni-fe. Robust standard errors are in parentheses, and are clustered at the running variable, peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.4 confirms that there are no significant spillovers on the parental leave of fathers in the first two years following the first reform. Although standard errors are large, the estimate is consistently small and insignificant throughout the different measures of parental leave with respect to timing.

For the second reform, the increase observed in Panel B of Figure 1.3 is significant at the 5 percent level. The point estimate is large at 7.78, but imprecise as the standard errors are relatively large (more than half, at 3.7). Nevertheless, the estimate suggests that fathers take more days of parental leave in the first two years if working with a peer whose child is born after the second reform. The estimate is substantial also in relative terms—a 19 percent increase of the control mean and half of the direct reform response among peers. Also the estimate for parental leave in the first year is significant at the 5 percent level following the second reform. At 4.57, it is similar to the main estimate, suggesting that a substantial part of the increase in the first two years is taken during the very first year after birth. Relative to the control mean, the increase in the first year is more than 27 percent. There is no increase in the total amount of parental leave, so the spillover of the second reform is a reallocation of parental leave from later to earlier.[39] That is, fathers working together with a peer who is also covered by the quota use more parental leave when the child is relatively young. There is no significant increase in parental leave during weekend nor the number of episodes, which would have indicated parental leave of inferior quality. Instead, the analysis suggests that spillovers increase the uptake of parental leave from which we can expect effects on continued childrearing responsibilities.[40]

The significant jump in the average uptake for the second reform is driven by a higher fraction of treated fathers taking about 60 days of parental leave in the first two years; there is no difference by treatment status for parental leave exceeding the quota.[41] Nor is there a reform effect on when parental leave of fathers is first taken,[42] or for which months *any* parental leave is taken across the full eight-year period (Appendix Figure C3). This suggests that the second quota triggered spillovers at the workplace *extending* the time fathers spend with their child relatively early.

The corresponding 2SLS-estimates capturing the marginal peer effect show a significant estimate of 0.534 for the second reform, corresponding to an increase of 7.76 days in the first two years after birth (Appendix Table C4). Although outcomes are difficult to compare across studies, the 2SLS-estimate is large relative to previous literature.[43]

[39] The estimate for parental leave *after* the first two years is significant and negative at -5.309 (2.592) for the second reform.

[40] The positive impact of the second reform is reflected also in the ratio of parental leave taken by fathers in the first two years, while there is no effect for the first reform (see Appendix Table C1).

[41] See the distribution of parental leave days in Appendix Figure C2 and regression estimates for parental leave grouped in bins of 10 in Appendix Table C2.

[42] RD regressions on the timing of parental leave of peers and fathers relative to birth and relative to each other confirm that there is no treatment effect in timing of parental leave for fathers (see column 2 in Table C3.

[43] Recall that the identifying assumptions are not credibly satisfied (see discussion in Section 1.3), so my estimate is merely suggestive. Dahl et al. (2014) found extensive margin effects for coworkers and brothers of 11 and 15 percentage points respectively. Lassen (2021) found an intensive margin effect for mothers of 17 percent and Welteke and Wrohlich (2019) found the probability of staying home the first year to increase by 30 percentage points. Carlsson and Reshid (2022) found peer effects on the number of days taken by fathers and mothers to be 15 percent and 9 percent, respectively.

1.5.3 Workplace specific reform effects

In this section, I allow the reform response to differ with respect to costs of parental leave at the workplace. As discussed in Section 1.4.1, I consider two costs of parental leave: monetary and normative costs. Both costs are identified by the literature as highly relevant for the parental leave uptake of fathers (e.g., Angelov et al., 2016; Moberg and Van der Vleuten, 2021). Monetary costs refer to the cost of replacing a worker during absence, which is potentially internalized by workers (Hensvik and Rosenqvist, 2019; Ginja et al., 2020). As the monetary costs are specific to the educational level of the peer and the father, I restrict this analysis to pairs with the same education. Normative costs are measured as the predicted parental leave uptake among coworkers (or their hypothetical partner), excluding the peer and the father. For each type of cost, workplaces are categorized as high or low. The discrete measure of monetary costs is grouped, and high costs refer to workplaces with at most four coworkers with the same level of education (including the peer and the father). For the continuous measure of norms, high costs refer to the bottom half of the distribution of workplace averages in terms of predicted parental leave of males. The peer effect is interacted with the indicator of high costs to obtain a peer effect specific to the type of workplace.

Table 1.5: Control mean heterogeneity by cost

	(1)	(2)	(3)	(4)
	Monetary		Normative	
	First two years	Total	First two years	Total
First reform				
Low	30.321	40.109	33.264	43.825
High	24.868	34.321	27.183	36.356
Fraction	0.820	0.856	0.817	0.830
Second reform				
Low	41.555	72.165	45.993	77.153
High	30.042	59.220	36.430	66.758
Fraction	0.723	0.821	0.792	0.865

Notes: Parental leave of control peers in different subsamples. For monetary costs, the workplaces are ranked by the number of coworkers with the same education (3 levels) as the peer. High costs refer to four or fewer coworkers (including the peer and the father). Normative cost is the average predicted parental leave of coworkers based on pre-reform characteristics, split in two. Fraction is the uptake in low cost workplaces, divided by the uptake in high cost workplaces.

Table 1.5 shows that the constructed costs are relevant for the uptake of parental leave before each reform; the average number of days with parental leave taken by peers is lower in workplaces with high costs of parental leave. For monetary costs, this implies that peers who were more difficult to replace internally also took less parental leave. For normative costs, it implies that peers took less parental leave if their colleagues were less in favor of male parental leave (as predicted by their individual characteristics). The difference in uptake between workplaces is relatively similar across costs and measures of parental leave before the first reform, ranging between 82 percent and 86 percent. By the second reform, this difference increased, especially for the monetary costs in the first two years. Differences across measures of parental leave before the second reform suggest that the timing of parental leave was adjusted to workplace related costs (see Appendix Table D1 for all measures of parental leave).

Table 1.6 presents heterogeneity with respect to monetary and normative costs for parental leave in the first two years and in total (by age eight).[44] The indicator of high costs is interacted with treatment, allowing

[44] Appendix Tables D2 and D3 present the corresponding estimations for all measures of parental leave.

for heterogeneous response in both the first stage and the reduced form for the first (top panel) and second (bottom panel) reform, respectively.

Table 1.6: Heterogeneity in the first stage, by costs

	(1)	(2)	(3)	(4)
	Monetary		Normative	
	First two years	Total	First two years	Total
First stage		*First reform*		
Treated (Low)	19.581***	31.228***	14.609***	26.593***
	(4.192)	(5.336)	(4.647)	(5.930)
TreatedXhigh	-1.037	-2.936	3.122	0.769
	(11.517)	(13.989)	(6.059)	(7.871)
Reduced form				
Treated (Low)	-0.310	-0.434	0.380	3.009
	(3.346)	(4.135)	(4.683)	(5.647)
TreatedXhigh	-3.693	-7.392	1.437	-3.093
	(9.536)	(13.094)	(6.725)	(8.400)
Observations	9,185	9,185	11,455	11,455
First stage		*Second reform*		
Treated (Low)	13.953***	26.475***	16.235**	21.910***
	(5.284)	(5.829)	(6.369)	(6.331)
TreatedXhigh	6.656	6.556	-4.818	5.340
	(12.670)	(14.436)	(8.010)	(8.504)
Reduced form				
Treated (Low)	9.081**	5.270	3.458	1.864
	(4.146)	(4.668)	(5.341)	(5.859)
TreatedXhigh	-2.206	0.318	7.122	-0.750
	(12.117)	(16.301)	(8.123)	(9.579)
Observations	9,803	9,803	12,158	12,158

Notes: Estimates from separate RD regressions on parental leave the first two years an in total (by age eight). Monetary cost is the number of coworkers with the same education (3 levels). High costs refers to four or fewer coworkers (including the peer and the father). Normative cost is the average predicted parental leave of coworkers based on pre-reform characteristics, split into two by the median. Costs are interacted with the treatment variable and the control function. All estimations include separate quadratic trends, triangular weights, workplace covariates, and cohort- and muni-fe. Robust standard errors are in parentheses and clustered at the running variable, peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table 1.6 shows that the reform response was similar across workplaces. The similarities apply to both the peers and the fathers: the difference for workplaces characterized by high costs is consistently imprecise and insignificant.

Table 1.7: Uptake of fathers treated by the second reform

	(1)	(2)	(3)	(4)
	Monetary		Normative	
	First two years	Total	First two years	Total
Low	55.219	100.373	59.279	104.416
High	50.293	99.673	48.436	92.087
Fraction	0.911	0.993	0.817	0.882

Notes: Parental leave of treated fathers in different subsamples. For monetary costs, the workplaces are ranked by the number of coworkers with the same education (3 levels) as the peer. High costs refers to four or fewer coworkers (including the peer and the father). Normative cost is the average predicted parental leave of coworkers based on pre-reform characteristics, split into two. Fraction is the uptake in low cost workplaces, divided by the uptake in high cost workplaces. Columns 1 and 3 report the main results, parental leave in the first two years. Columns 2 and 4 report the total number of days of parental leave (by age eight).

However, Table 1.7 shows that after the second reform the initial gap in uptake across workplaces with respect to monetary costs (as observed in Table 1.5) is almost closed. That is, although the reform response is not significantly different, the total reform effect for fathers increased parental leave more in workplaces characterized by high monetary costs such that uptake is almost the same across workplaces characterized by high and low monetary costs.[45] This could indicate that fathers no longer internalize monetary costs of being replaced while on leave. However, it is also consistent with lowered costs of replacement if, for example, employers learn to anticipate and easily replace fathers while on parental leave. There is no similar reduction by normative costs; the gap in parental leave uptake in the first two years is in fact at the initial level (82%), suggesting that normative costs are more difficult to target or that the measure is less successful at targeting the intended cost.

[45]See Appendix Table D4 for all measures of parental leave.

1.6 Robustness

In this section, I evaluate the robustness of the results. I also evaluate the first stage in terms of timing and dispersion of the direct reform response among peers.

1.6.1 Identification

To further assess the independence assumption, I examine how sensitive the estimates are to inclusion of covariates. Table 1.8 shows the first stage and the reduced form estimates for parental leave in the first two years. All estimates are relatively stable as more covariates are added. The imbalance detected in Table 1.2 for the first reform does not appear problematic; there is only a small decrease in the first stage when peer characteristics are controlled for and the estimates are larger when the full set of covariates is included. The reduced form estimates for the first reform are consistently small while the second reform estimates are relatively stable at seven days. There is no gain in precision for the reduced form when adding more covariates.

Table 1.8: Sensitivity to covariates

	(1)	(2)	(3)	(4)	(5)
First reform					
First stage	16.154***	16.273***	15.656***	15.703***	18.041***
	(3.228)	(3.225)	(3.282)	(3.271)	(3.185)
Reduced form	0.368	1.301	0.669	1.009	0.780
	(3.042)	(3.127)	(3.145)	(3.129)	(3.266)
Observations	11,597	11,597	11,597	11,597	11,597
Second reform					
First stage	13.502***	14.536***	13.975***	14.259***	15.137***
	(4.186)	(4.137)	(4.164)	(4.149)	(4.060)
Reduced form	7.098*	7.777**	8.020**	8.027**	7.721**
	(3.644)	(3.740)	(3.809)	(3.783)	(3.583)
Observations	12,309	12,309	12,309	12,309	12,309
Year & Muni fe	Yes	Yes	Yes	Yes	Yes
Workplace cov		Yes	Yes	Yes	Yes
Peer cov			Yes	Yes	Yes
Father cov				Yes	Yes
Mother cov					Yes

Notes: Estimates from separate RD regressions. Year fixed effects refers to the child of the father, municipality refers to that of the workplace. Workplace covariates include share of males and number of employees. Peer covariates include age, age squared, marital status, education, earnings, household income, business owner indicator, and immigrant status. Father covariates include age, age squared, marital status, education, earnings, household income, and immigrant status. Mother covariates refer to both mothers and peer mothers and include age, age squared, education, and immigrant status. All estimations include separate quadratic trends and triangular weights. Robust standard errors are in parentheses and are clustered at the running variable, peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

For the findings to be credible, they should be robust to alternative specifications. In Appendix Table E1, I report the first stage and the reduced form for parental leave in the first two years, with varying donuts, weights, slopes, and bandwidths. Throughout, the estimates are relatively stable and never statistically different from one another. The fact that the estimates are similar using the uniform kernel is comforting; if the estimates were driven by covariate imbalance, they should increase as observations further away become more influential.[46] The first stage is

[46]All estimates are largely unchanged also when estimated in a Local Linear Regression (see Appendix Table E2), a method possibly more robust to trends further away from the cutoff (Dahl et al., 2014).

consistently significant and ranges between 13.1 and 16.3 for the first reform and 10.8 and 17.3 for the second reform.[47] The reduced form estimates are robust to inclusion of donuts and similar in magnitude when using a smaller bandwidth.[48] For the optimal bandwidth at approximately 4.5 months, the estimate for the second reform remains large at 6 days but imprecisely estimated and no longer statistically significant.[49] The significant reduced form estimates of parental leave in the first two years and the first year are both significant also when adjusted for multiple hypothesis testing (see Appendix Table E3).

Another alternative specification is to extend and analyze spillovers for more fathers at the workplace. Dahl et al. (2014) found substantial snowball effects such that the initial peer effect is amplified as the father interacts with the subsequent father, who in turn interacts with the next, and so on. My main analysis is restricted to the father whose child is first to be born after the reform window at each workplace, but my findings are robust to extending the analysis to subsequent fathers (Appendix Table E4).[50]

Furthermore, the analysis of workplace specific reform response is robust to alternative definitions of high monetary and normative costs. High monetary costs is defined as no more than two coworkers with the same educational level, in addition to the peer-father pair. Estimating the same analysis for a maximum of zero, one, and three additional coworkers respectively, the findings of a similar reform response across monetary

[47]The first stage is not statistically different if estimated in a difference-in-discontinuity design using the placebo years (13.782 (3.036) and 17.620 (1.978) for the first and second reform respectively). For the reduced form estimates, the placebo is less informative since father's children are born at any time of the year. Furthermore, they are potentially treated if the timing of birth among coworkers is correlated across years such that the treatment status of the placebo correlates with actual treatment status and the father in the placebo corresponds to the second father in the reform analysis.

[48]For smaller bandwidths, the curvature towards the end is removed and therefore I also adjust the trend to be linear.

[49]The restriction of a single peer within the reform window implies that the sample of workplaces changes with the bandwidth. Consequently, optimal bandwidth is not entirely applicable in this analysis. The suggested bandwidth of 4.5 months was found using the sample of 6 months, and this is the sample for which the estimates are calculated. With the sample of 4.5 months, the optimal bandwidth is instead 4 months.

[50]There is no discontinuity in the number of fathers at the workplace nor in the age difference between the children of peers and fathers (see Appendix Table E5).

costs remain (Appendix Table E6).[51] The findings for monetary costs are also robust for the alternative definition of substitutability, using instead educational level (four categories) and field (nine categories), as suggested by Ginja et al. (2020) (see Appendix Table E8). For the second reform, the occupational codes are available and I find similar estimates also using (3-digit) occupational code as suggested by Hensvik and Rosenqvist (2019) (see Appendix Table E9). High normative costs were defined as the top half of the continuous measure of average predicted parental leave among coworkers. When estimating the analysis for the top third, fourth, and fifth, denoting high costs to a smaller fraction of workplaces, the results are largely the same with a similar response across normative costs (Appendix Table E10).

A potential concern in the Swedish setting is that the parental leave system allows continuous applications of parental leave. That is, the order of birth does not determine the timing of application nor parental leave. The flexibility implies that the father can apply for and spend his leave before his peer does, which violates the order of causality to be estimated. However, given that treatment is random, fathers exposed to treated/control peers are comparable and therefore they are equally likely to respond to the reform in absence of peer influence. Similarly, the tendency to postpone parental leave is comparable among treated and control peers in absence of the reform. The timing of parental leave is itself a potential outcome, but the reversed causality can potentially bias the first stage. The first column of Table 1.9 shows that the first stage estimate is largely unaffected by removal of workplaces where the father takes parental leave before the peer, suggesting that reversed causality does not have a great impact on the direct reform response among peers.[52] By restricting the analysis to peer-father pairs whose children are born at least two years apart, the peer parental leave by definition precedes the leave of the father. As seen in columns 2 and 4, this drastically reduces the sample size and therefore affects the precision of the estimates, but the magnitude is relatively robust both for the first stage and the reduced form.

[51] The findings are similar also when peers and fathers are allowed to differ with respect to educational level (peers still need at least the same level as the father) and consequently there can be zero coworkers to substitute (see Appendix Table E7).

[52] There is no significant discontinuity in peer parental leave to be taken after fathers (Appendix Table E11).

Table 1.9: Timing of peer parental leave relative to that of fathers

	(1)	(2)	(3)	(4)
	First reform		Second reform	
	Excl. reversed	Min. 2y gap	Excl. reversed	Min. 2y gap
First stage	16.718***	15.964***	14.063***	10.636*
	(3.498)	(4.588)	(4.707)	(6.208)
Reduced form	2.576	-3.801	7.443*	5.860
	(3.547)	(5.339)	(3.955)	(5.973)
Observations	10,546	5,081	10,567	5,457

Notes: Estimates from separate regressions on parental leave taken during the first two years following the second reform (except for the first stage (columns 1 and 3) with total days). In Excl. reversed, I removed peer-father pairs where the peer PL is taken after the father PL. In Min 2 y gap, I removed fathers whose child was born less than two years after the peer child and used only peer parental leave for the first 2 years. All estimations include separate quadratic trends, triangular weights, workplace covariates, and cohort- and muni-fe. Robust standard errors are in parentheses and are clustered at the running variable, peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

A possible reason for the imprecise reduced form estimates is that I fail to capture actual peer-father interaction. Since transition to another workplace is a potential outcome, both peers and fathers are allowed to switch after the reform and take parental leave while not being in the same workplace. In my sample, more than 80 percent of treated fathers observe parental leave of their peer, and this is stable across reforms (see Appendix Table E12).[53] In Appendix Table E13, I allow the effect to be different where peer effects are expected to be stronger. These are workplaces where interaction between peer and father is more likely, or where interaction is expected to have a greater impact.[54] However,

[53]Although working together at the time of peer parental leave makes the potential consequences more visible to the father, this is not a necessity for peer effects. Treated peers (and fathers for the first reform) are less likely to switch workplace before taking parental leave. Although this is a possible effect of the reform, it is partially driven by the difference in timing of parental leave, where the first parental leave of treated peers is taken when the child is younger compared to control peers.

[54]Peer-father interaction is more likely in smaller workplaces (fewer than 30 employees) and the information to be more relevant when coworkers have the same education (3 levels) and when the gap between births is short (no more than 3 years). Also, the value of information is expected to be higher when the peer is a manager, which is indicated by the highest or second highest income rank at the workplace. Furthermore,

the differences in the reduced form estimates between subsamples are not statistically significant. In column 7 of Appendix Table E13, I also validate that the results are not driven by the smallest workplaces (5 or fewer) where the peer-father dynamics are presumably different.

1.6.2 Timing of the direct peer response

The analysis suggests that the extension of parental leave quotas triggered spillovers at the workplace, but the initial introduction did not. The two reforms are different in terms of design and implemented seven years apart. These differences possibly led to differences in the direct peer response, affecting the spillovers at the workplace.[55] Table 1.3 showed that the average increase in parental leave of peers due to the reform was slightly higher for the first compared to the second reform. A closer examination shows that this difference is driven by a higher percentage of treated peers who increased their uptake in the first two years by more than stipulated by the reform (Appendix Figure E1). Given the flexibility of the parental leave insurance, it is possible that also the dispersion of peer parental leave, across the possible eight-year period, differs across the two reforms. The spillovers are presumably higher for parental leave taken as a cohesive period during infancy compared to short and dispersed spells often allocated to holiday season or weekends.

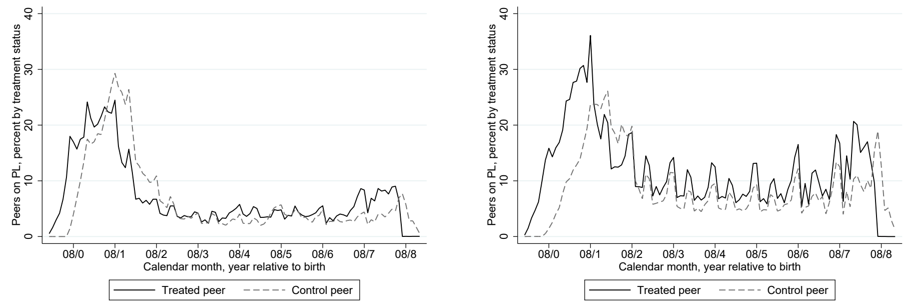
Figure 1.4 includes only peers who take at least some parental leave and shows the percentage who take parental leave at a given time, expressed as calendar months by year relative to birth.

the peer signal is likely stronger when there are no additional confounding (lower educated) peers in the reform window. Lastly, first parity fathers are expected to be more susceptible to peer influence.

[55]An alternative explanation is that features of men responding to the first and second reform differ, as suggested by previous findings (e.g., *Försäkringskassan*, 2019b), which possibly translates into different potential for spillovers. However, heterogeneity by educational level of peers and fathers does not indicate that this is driving the observed differences (see Appendix Table E14).

Figure 1.4: Fraction peers on PL, by calendar months

(a) First reform **(b)** Second reform



Notes: The figures show the percentage of peers who take any parental leave in a given month by treatment status. The years are normalized to the birth year of the child.

Figure 1.4 suggests that the additional quota days in 2002 were more dispersed across the full eight-year period than the quota days introduced in 1995. The seasonal variation in the percent of peers who take parental leave is more pronounced for the second reform, and the difference by treatment status is also more distinct. Although it is common for peers to take parental leave in the first two years, the second reform induced even more peers to do so, especially during the child's second year of life and in particular the second summer.[56] The second pronounced mass point observed in Figure 1.4 is before the age limit is met at eight years, after which no days can be saved. Here, especially treated peers bunch, suggesting that not all quota days are spent early in life but rather that a significant share of peers still have days remaining after the child starts childcare. Bunching at eight years is particularly common for the second reform for which the incidence in summer and winter holidays are also consistently steeper.

Also considering the *amount* of parental leave by timing, the fraction of parental leave taken early is steadily decreasing with the two reforms. Before the first reform, 75 percent of all days were taken before the child was two years old and a substantial part even in the first year. For the additional days induced by the quotas, the corresponding division was 60

[56]The staggered peaks relative to the birth year are partially mechanical due to the six month wide reform window at the turn of the year. But, treated peers also take parental leave for the first time at a substantially lower age than control peers (Appendix Table C3).

percent for the first and 50 percent for the second reform (see Appendix Table E15).

Taken together, the first reform appears to have increased peer parental leave of higher quality than the second reform. Thus, the quality of the direct reform response does not explain the lack of peer effects from the first reform.

1.7 Conclusion

A parental leave quota lowers the financial cost of shifting parental leave from the mother to the father. It can also normalize parental leave of fathers such that norms are affected and the signaling value of parental leave uptake decreases. Consequently, parental leave quotas can increase parental leave of fathers over and above the reserved amount. As fathers become more involved during infancy, they develop their childcare skills and possibly a preference for it, contributing to improved gender equality in parenting (Duvander and Johansson, 2019).

In this study I find that the first reform did not trigger significant spillovers, but there appears to be spillovers following the second reform. Although standard errors are large, the estimate is substantial at about seven days in the first two years. There is no corresponding effect on total days; that is, fathers working with a peer covered by the quota take a larger share of their parental leave relatively early.

For Sweden, an important takeaway from this paper is that timing of parental leave must be considered as this appears to be a dimension of adjustment, in addition to the number of days. Consequently, by focusing on parental leave in total or within a certain time span, some of the dynamics are missed. In terms of policy evaluation, neglecting the timing of parental leave can lead to misguided expectations regarding reform effects. Furthermore, timing of parental leave benefits is a possible dimension to target by policies, restricting when benefits can be used in addition to the number of days with benefits.

The difference between the first and the second reform does not appear to be driven by differences in the first stage. Welteke and Wrohlich (2019) discuss three channels where the duration of (mothers') parental leave can be affected by the uptake of their peers: leisure complementarities, conformity to social norms and transmission of information.[57] Given that the reduced form estimations are similar when the analysis is restricted to peer-father pairs with children born at least two years apart (as seen in Table 1.9), leisure complementarities are unlikely to be driving the results. Instead, the fact that there appears to be spillovers from the second reform but not the first is consistent with conforming to gender norms. Although the first quota increased the uptake of parental leave, presumably affecting the norms of parenthood, attitudes were more favorable towards parental leave of fathers by the second reform,[58] possibly making fathers more responsive to the increased uptake of their peers. The findings are also consistent with transmission of information. However, contrary to previous studies (Dahl et al., 2014; Welteke and Wrohlich, 2019; Lassen, 2021), the information appears to regard benefits of parental leave rather than costs. Otherwise, we would expect stronger spillovers in workplaces characterized by high costs. Transmission of benefits would also explain why the second reform, which was less restrictive on the choices of parents[59] (in addition to the differences in gender norms of parenting at the time of implementation), was more successful at triggering spillovers. Comparing the first and the second reform, it appears that when the extensive margin is high already before implementation, the perception of the reform is important for the potential for spillovers.

The findings of this paper suggest that it is important to consider also spillovers when evaluating the effect of parental leave quotas; while the direct response to the first reform was stronger to the first compared to the second reform (both in my sample and as found by others e.g., Eriksson (2005) and *Försäkringskassan* (2019b)), the second reform was more successful at triggering peer effects, suggesting a higher net impact on the parental leave uptake of fathers in the first two years. Given the

[57]Dahl et al. (2014) also discuss the channel of sharing practical knowledge about the childcare system but this is unlikely in the context of Sweden. For Swedish fathers, in particular by the second reform, the extensive margin was high and consequently most fathers were aware of the system.

[58]More reported a belief that working mothers can establish just as warm and secure a relationship with her children as a mother who does not work: 70.8 percent compared to 83.7 percent, as measured by the World Values Survey Wave 2 and 4.

[59]The first reform reduced the parental leave available to mothers. Also, by the second reform, the coverage of collective agreements was higher, implying a lower earnings loss of parental leave relative to working, compared to the first reform and the low flat rate was increased (tripled).

possibility for fathers to take out parental leave during many years in Sweden, it might also be that changes in peer behavior were difficult to observe after the implementation. Only when colleagues started to have a higher concentrated parental leave uptake, did it trigger peer effects. Thus, extending the amount of days reserved to each parent can further improve the equality in childrearing responsibilities during infancy.

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1.A Descriptives

Table A1: Year of birth of the father's child

	(1)	(2)	(3)	(4)
	First reform		Second reform	
	Freq	Percent	Freq	Percent
Year of reform	3,300	27.13	3,378	26.51
Year+1	3,623	29.79	3,796	29.79
Year+2	1,890	15.54	2,109	16.55
Year+3	1,198	9.85	1,324	10.39
Year+4	842	6.92	892	7.00
Year+5	718	5.90	709	5.56
Year+6	591	4.86	534	4.19

Notes: Frequencies and fractions of fathers by child year of birth relative to the reform.

Table A2: T-test for workplace characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	First reform			Second reform		
	Total	Sample	Difference	Total	Sample	Difference
Male share	0.635	0.720	-0.086***	0.635	0.722	-0.087***
	(0.001)	(0.002)	(0.004)	(0.001)	(0.002)	(0.004)
Size	9.072	30.772	-21.670***	9.001	32.304	-23.303***
	(0.097)	(0.277)	(0.578)	(0.090)	(0.273)	(0.548)
Predicted PL	0.390	0.480	-0.090***	0.604	0.685	-0.082***
	(0.000)	(0.001)	(0.002)	(0.020)	(0.071)	(0.119)

Note:T-tests of plant characteristics, comparing the full population to the sample. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A3: T-test for Peer characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	First reform			Second reform		
	Total	Sample	Difference	Total	Sample	Difference
Income	5.922	7.248	-1.326***	6.631	7.559	-0.928***
	(0.010)	(0.010)	(0.027)	(0.009)	(0.009)	(0.024)
Age	31.593	31.263	0.329***	32.726	32.155	0-0.571***
	(0.020)	(0.052)	(0.059)	(0.022)	(0.050)	(0.058)
Married	0.466	0.455	0.011**	0.425	0.412	0.012***
	(0.002)	(0.005)	(0.005)	(0.002)	(0.004)	(0.005)
University	0.182	0.202	-0.019***	0.246	0.269	-0.023***
	(0.001)	(0.004)	(0.004)	(0.002)	(0.004)	(0.004)
Immigrant	0.195	0.079	0.116***	0.221	0.107	0.114***
	(0.001)	(0.002)	(0.004)	(0.001)	(0.003)	(0.004)
Control PL dummy	0.526	0.571	-0.045***	0.880	0.937	-0.057***
	(0.002)	(0.006)	(0.007)	(0.002)	(0.003)	(0.004)
Treated PL dummy	0.855	0.931	-0.076***	0.894	0.955	-0.060***
	(0.002)	(0.004)	(0.005)	(0.001)	(0.003)	(-0.060)
Control PL days	40.463	39.926	0.537	69.743	71.605	-1.862*
	(0.350)	(0.839)	(0.964)	(0.376)	(0.833)	(0.958)
Treated PL days	54.156	57.526	-3.369***	89.117	93.355	-4.238***
	(0.291)	(0.836)	(0.942)	(0.343)	(0.878)	(1.006)

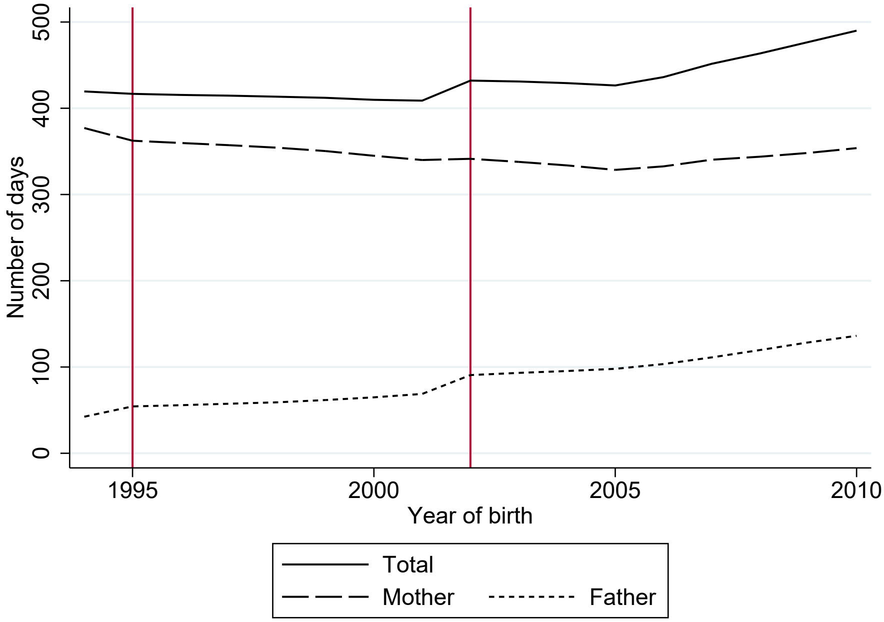
Note: T-tests of peer characteristics, comparing the full population to the sample. *** p<0.01, ** p<0.05, * p<0.1.

Table A4: T-test for Father characteristics

	(1)	(2)	(3)	(4)	(5)	(6)
	First reform			Second reform		
	Total	Sample	Difference	Total	Sample	Difference
Income	5.793	7.135	-1.342***	6.592	7.522	-0.930***
	(0.004)	(0.009)	(0.026)	(0.003)	(0.008)	(0.022)
Age	28.628	29.435	-0.808***	29.522	30.371	-0.849***
	(0.008)	(0.055)	(0.057)	(0.008)	(0.054)	(0.056)
Married	0.235	0.284	-0.049***	0.201	0.253	-0.053***
	(0.001)	(0.004)	(0.004)	(0.000)	(0.004)	(0.004)
University	0.168	0.111	0.057***	0.263	0.160	0.102***
	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)	(0.004)
Immigrant	0.203	0.077	0.126***	0.236	0.102	0.134***
	(0.001)	(0.002)	(0.004)	(0.001)	(0.003)	(0.004)
PL dummy	0.873	0.926	-0.053***	0.870	0.935	-0.065***
	(0.000)	(0.002)	(0.003)	(0.000)	(0.002)	(0.003)
PL dummy	60.530	58.784	1.746**	89.441	94.763	-5.322***
	(0.088)	(0.562)	(0.629)	(0.091)	(0.618)	(0.677)

Note: T-tests of father characteristics, comparing the full population to the sample. *** p<0.01, ** p<0.05, * p<0.1.

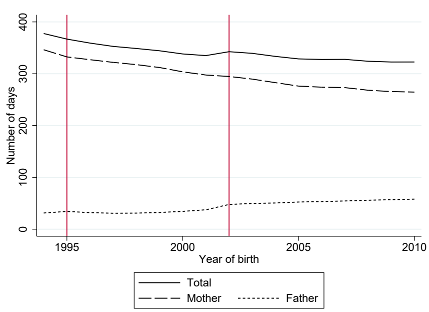
Figure A1: Parental leave in total



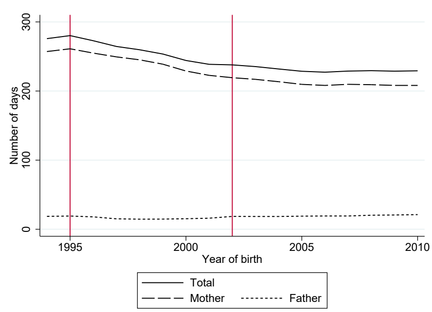
Notes: Average days for all children born 1994-2010.

Figure A2: Parental leave, high quality

(a) Days in first 2 years



(b) Days in first year



Notes: Average days for all children born 1994-2010.

Figure A3: Parental leave, low quality

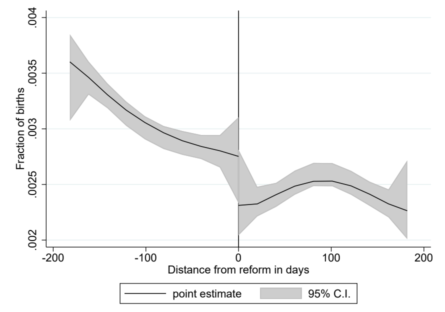
(a) Days during weekend

(b) Number of episodes

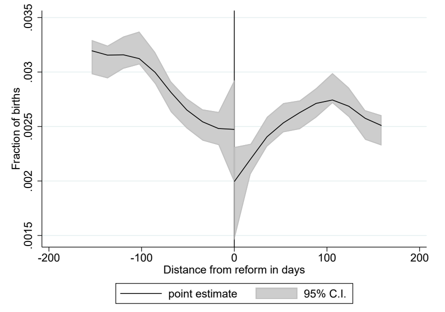
Notes: Average days for all children born 1994-2010.

Figure A4: McCrary test of density

(a) First reform



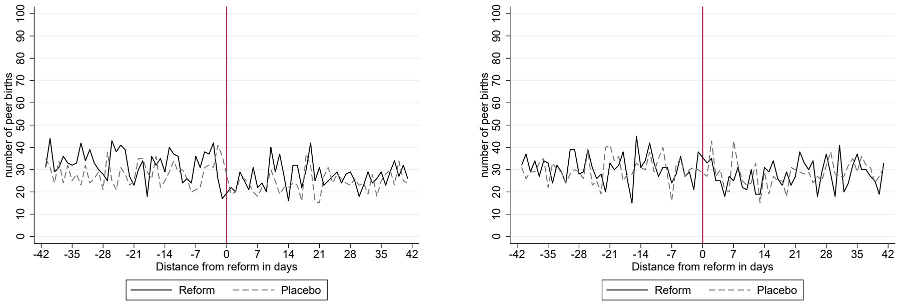
(b) Second reform



Notes: McCrary test of manipulation of density for the first (left) and second (right) reform.

Figure A5: Number of births

(a) First reform **(b)** Second reform



Notes: Number of births, binned by week, for the first (left) and the second (right) reform and corresponding placebo.

Table A5: Balance of workplace characteristics

	(1)	(2)	(3)	(4)
	First reform		Second reform	
	Monetary	Normative	Monetary	Normative
Treated	0.047	0.591	-0.005	0.477
	(0.036)	(1.386)	(0.035)	(1.330)
Observations	9,085	9,185	9,686	9,804

Notes: Estimates from separate regressions on workplace characteristics. Normative costs is measured as the predicted PL is the average prediction of parental leave use among coworkers. Monetary cost is measured ans the number of coworkers with the same education. All estimations include separate quadratic trends and triangular weights. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table A6: Balance of individual covariates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Father					Mother			
	Earnings	Age	Uni.	Immig.	Married	Earnings	Age	Uni.	Immig.
Peer									
First reform	267.234***	0.560	-0.036	-0.051***	0.006	23.935	0.323	0.047	-0.027
	(69.925)	(0.360)	(0.027)	(0.017)	(0.031)	(45.294)	(0.309)	(0.031)	(0.018)
Observations	11,597	11,597	11,597	11,597	11,597	11,571	11,597	11,597	11,597
Second reform	211.380**	0.571	-0.031	-0.019	0.059*	-19.445	0.698**	0.020	-0.019
	(96.961)	(0.356)	(0.028)	(0.020)	(0.030)	(61.787)	(0.321)	(0.031)	(0.021)
Observations	12,309	12,309	12,309	12,309	12,309	12,255	12,309	12,309	12,309
Fathers									
First reform	117.718*	0.427	-0.013	-0.007	-0.013	31.618	0.791*	0.014	0.005
	(60.952)	(0.381)	(0.020)	(0.017)	(0.028)	(46.306)	(0.412)	(0.026)	(0.018)
Observations	11,597	11,597	11,550	11,597	11,597	11,305	11,597	11,242	11,597
Second reform	-9.130	-0.144	-0.038	-0.027	0.016	44.307	0.363	-0.010	-0.042**
	(76.235)	(0.366)	(0.024)	(0.019)	(0.027)	(63.393)	(0.461)	(0.030)	(0.021)
Observations	11,918	11,918	11,894	11,918	11,914	11,490	11,490	11,485	11,918

Notes: Estimates from separate regressions on individual characteristics. Earnings are measures in 100 SEK. All estimations include separate quadratic trends and triangular weights. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table A7: Balance of plant characteristics, Diff-in-Disc

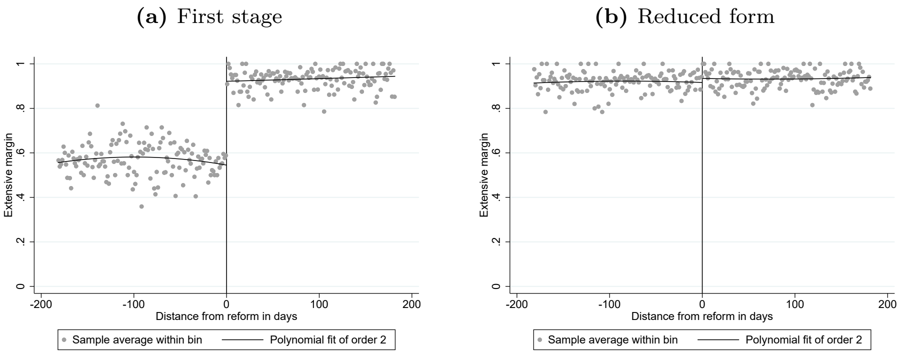
	(1) Spacing birth	(2) Male share	(3) Plant size	(4) PL father	(5) PL peer	(6) Monetary	(7) Normative
				First reform			
Treated	0.034	-0.004	0.505	0.002	0.006	0.019	0.331
	(0.049)	(0.007)	(0.936)	(0.003)	(0.004)	(0.018)	(0.648)
Observations	22,515	22,515	22,515	21,277	21,247	17,631	17,810
				Second reform			
Treated	0.049	0.011	-1.208	-0.002	-0.002	-0.015	-0.403
	(0.046)	(0.007)	(0.913)	(0.002)	(0.003)	(0.017)	(0.636)
Observations	24,335	24,335	24,335	24,312	24,273	19,159	19,369

Notes: Estimates from separate regressions on workplace characteristics using placebo years. Spacing birth refers to the time between the birth of the peer's child and the father's child. Male share is the fraction of males at the workplace. Plant size is the number of employees at the workplace. PL peer and PL father refers to parental leave above the prescribed amount, as predicted by their individual characteristics. Normative costs is measured as the predicted PL is the average prediction of parental leave use among coworkers. Monetary cost is measured ans the number of coworkers with the same education. All estimations include separate quadratic trends and triangular weights. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

1.B Extensive margin and replication

In this section I replicate the main results of "Peer effects in Program Participation", by Dahl, Løken and Mogstad (2014). For the coworker network, all first parity fathers with a single reform peer, are included and I restrict the analysis to workplaces with at most 500 employees. I also replicate the exercise for family networks, where all brothers of first parity are potentially influenced by their peer (brother). Similar to Dahl et al. (2014), I use a wider reform window of one year on each side of the cutoff for brothers.[60] The family network is included for replication purpose only and will not be further investigated in this paper. The pattern for the family networks are very similar to the coworker network, suggesting that the results are not specific to the coworker network in Sweden.

Figure B1: Extensive margin



Notes: Each observation is the fraction of peers (left) and fathers (right) who take any parental leave, by date of birth of the peer child. The vertical line indicates the date of the reform, January 1 st 1995. The fitted line is a second order polynomial.

My preferred specification deviates from Dahl et al. (2014) in several

[60]In Norway, parental leave eligibility for men was contingent on both his and his spouse's employment prior to birth and for full replacement, earnings should be above the substantial gainful activity level for the 10 months prior to birth. Dahl et al. (2014) restrict the analysis to peers whose earnings, and the earnings of the spouse, exceeded 37 820 NOK in 1994 (corresponding to 41 384 SEK). Although the Norwegian income restriction is not meaningful for the Swedish parental leave benefits, it restricts the sample of households included and is therefore an interesting exercise. The Norwegian income restriction removes households where either parent has a very low pre-birth income e.g., students.

aspects. Most notably, restricting the sample to the first succeeding father. Because the incidence of parental leave among men was high already before the reform, and close to universal in the post-reform period, there is little variation left that can be subjected to peer effects. Especially when succeeding fathers are included. To accommodate the Swedish context, I therefore focus on the first father only. By doing so, I have only one observation per workplace and I cluster instead at the running variable, date of birth of peer child. I use quadratic slopes on each side, include father year of birth fixed effects and make no restriction on father parity. However, I remove workplaces larger than 150 employees and fathers coded as business owners.

Table B1: Extensive margin replication

	(1)	(2)	(3)	(4)	(5)
	Dahl et al.	Replication		Own specification	
	Main	All	Inc rest.	All	Inc. rest
			Coworkers		
First stage	0.317***	0.327***	0.332***	0.377***	0.372***
	(0.026)	(0.021)	(0.027)	(0.023)	(0.035)
Reduced form	0.035***	0.012	0.015	0.014	0.019
	(0.013)	(0.008)	(0.010)	(0.019)	(0.024)
2SLS	0.110***	0.037	0.044	0.038	0.052
	(0.043)	(0.024)	(0.028)	(0.049)	(0.062)
Control mean	0.03	0.570	0.599	0.571	0.604
Observations	26,851	26,280	16,433	11,199	7,262
			Brothers		
First stage	0.304***	0.349***	0.361***	0.365***	0.374***
	(0.026)	(0.014)	(0.018)	(0.016)	(0.021)
Reduced form	0.047**	-0.008	-0.013	-0.015	-0.012
	(0.020)	(0.010)	(0.011)	(0.013)	(0.017)
2SLS	0.153**	-0.022	-0.035	-0.040	-0.031
	(0.065)	(0.028)	(0.031)	(0.037)	(0.046)
Control mean	0.026	0.591	0.655	0.592	0.654
Observations	12,495	18,935	10,329	23,330	12,904

Notes: Estimates from separate regressions. Column 1 presents the results of Dahl et al. (2014). Column 3 and 5 impose an income restriction of 41 000 SEK (measured in 1994) on both peer and peer mother in line with Norwegian eligibility criteria. All estimations include a one-week donut and triangular weights. The coworker network also include controls for workplace size and the reform window is 6 months while the family network is 12 months. Dahl et al. (2014) specification use all fathers, standard errors are clustered at plant level and trends are linear. Dahl et al. (2014) controls for age, age squared, marital status, education and municipality of peer and peer mother, as well as the gender of the peer child. Own specification include only the first father by plant/family and consider only peer-father links where the peer has the same or higher education in the coworker network. The standard errors are clustered at the running variable; peer child date of birth and trends are quadratic. Own specification include cohort and municipality fe as well as workplace covariates for the coworker network. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

The level of parental leave among fathers is higher in Sweden compared to Norway, both before and after the reform. Although the variance of father take-up is comparable, it has implications for the scope of the peer effect. In Sweden, the fathers whose extensive margin can be affected by parental leave of their peers are the 10 percent of fathers who do not respond to the reform irrespectively. These are more rare, and likely different, from the corresponding Norwegian fathers. To approach the levels of Norway and make the scope of the reform more comparable, I identify sectors where the incidence among treated peers is relatively low. Absent the peer effect, the post reform peers and fathers face comparable incentives. Thus, I identify sectors where there is potential for a detectable peer effect since not all fathers are expected to respond to the reform alone. That is, I average the extensive margin among treated peers by 2-digit sni code and focus on workplaces in sectors with a comparably low response because this is where the extra nudge of a peer are expected to have a measurable effect on fathers. However, in this sample of workplaces, the extensive margin is only marginally affected even when restricting to the bottom percentile.

Table B2: Extensive margin, high scope

	(1) 50%	(2) 33%	(3) 20%
Treated peer mean	0.918	0.904	0.887
Treated	0.397***	0.445***	0.376***
	(0.032)	(0.040)	(0.075)
Reduced form	0.000	-0.022	-0.009
	(0.025)	(0.039)	(0.052)
RD	0.000	-0.048	-0.024
	(0.062)	(0.085)	(0.129)
Observations	7,242	3,527	1,742

Notes: Estimates from separate regressions using subsets based on the average incidence among treated peers in the sector. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table C1: Ratio of parental leave in the first 2 years

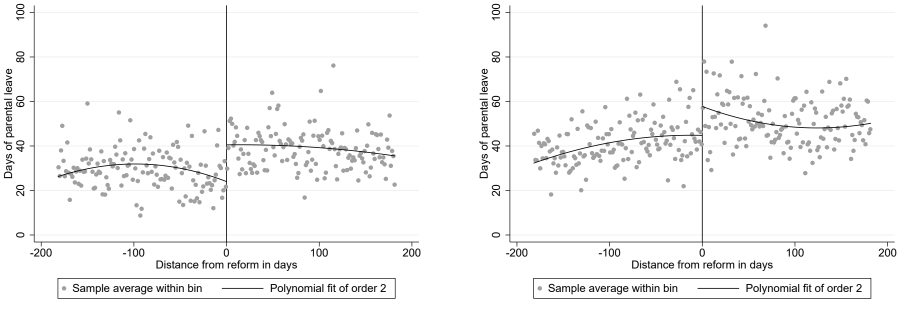
	(1) First reform	(2) Second reform
First stage	0.046***	0.029**
	(0.009)	(0.012)
Reduced form	0.000	0.020*
	(0.010)	(0.012)
Control mean	0.083	0.123
Observations	11,596	12,309

Notes: Estimates from separate RD regressions with fathers' days of parental leave relative to the household total, as outcome variables. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth.Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

1.C Results

Figure C1: First stage reform effects

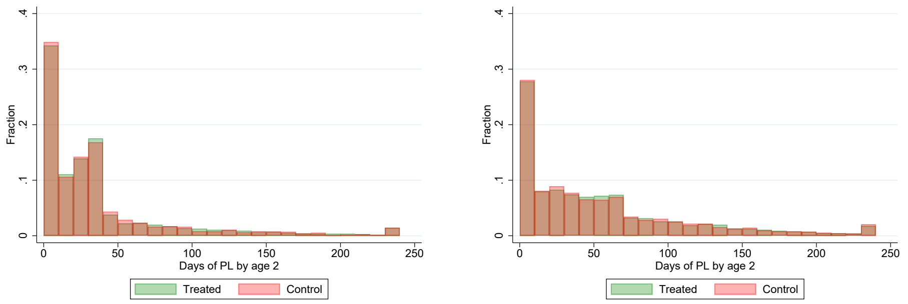
(a) Reform 1995 **(b)** Reform 2002



Notes: Each observation is the average days of parental leave taken by peers in the first two years, by date of birth of the peer child (normalized to the date of implementation). The vertical line indicates the date of the reform, January 1 st 1995 (left) and January 1st 2002 (right). The fitted line is a second order polynomial.

Figure C2: Days of PL, fathers

(a) 1995 **(b)** 2002



Notes: The figures shows the days of parental leave taken by peers in the first two years, by treatment status, in a histogram. Bin size is 10 days. The last bar contains peers who take 240 days or more.

Table C2: Main results, days of parental leave grouped

	(1) 0-10	(2) 11-20	(3) 21-30	(4) 31-40	(5) 41-50	(6) 51-60	(7) 61-70	(8) 71-80	(9) 81-90	(10) 91-100	(11) 100+
First reform											
Reduced form	0.023	0.026	0.015	-0.007	-0.022*	0.003	-0.007	-0.000	0.001	-0.014*	0.025*
Observations	11,596	11,596	11,596	11,596	11,596	11,596	11,596	11,596	11,596	11,596	11,596
Second reform											
Reduced form	-0.017	-0.009	0.002	0.003	-0.004	0.041***	-0.010	-0.004	-0.008	0.005	0.024
	(0.013)	(0.018)	(0.018)	(0.013)	(0.013)	(0.015)	(0.016)	(0.010)	(0.011)	(0.010)	(0.028)
Observations	12,309	12,309	12,309	12,309	12,309	12,309	12,309	12,309	12,309	12,309	12,309

Notes: Reduced form estimates from separate RD regressions on indicator variables, capturing reform effects on the days of parental leave in the first two years in bins of 10 days. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

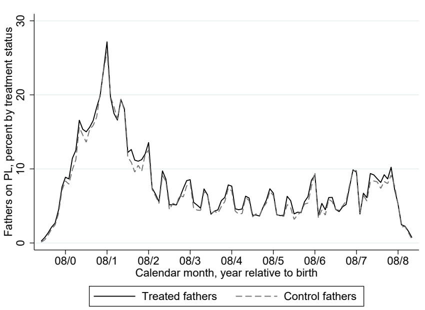
Table C3: Timing of birth and PL

	(1)	(2)	(3)	(4)
	Peer	Father	Pair	
	Age PL	Age PL	Distance PL	Distance birth
Treated	-96.011*	6.105	99.955	3.512
	(50.636)	(33.290)	(76.025)	(6.614)
Control mean	485.008	487.688	905.788	925.949
Observations	6,785	8,586	6,371	9,196
Treated	-119.552***	-42.019	88.979*	-6.917
	(43.051)	(32.732)	(50.006)	(7.615)
Control mean	576.343	463.071	788.998	910.349
Observations	9,295	9,280	8,829	9,806

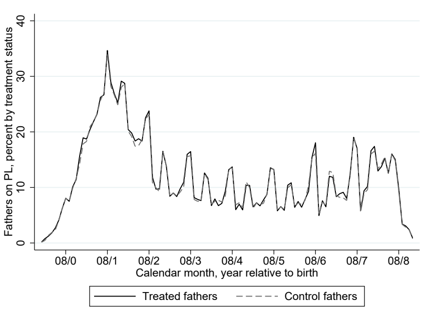
Notes: Estimates from separate RD regressions. Column 1-2 report the estimate for the age at which parental leave was first taken by peers and fathers respectively. Column 3 reports the time elapsed between parental leave of the peer and the child, and column 4 the time elapsed between birth of peer child and father child. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Figure C3: Fraction fathers on PL, by calendar months

(a) 1995



(b) 2002



Notes: The figures shows the fraction of fathers who take any parental leave in a given month, by treatment status. The years is normalized to the year of birth.

Table C4: Peer effect (2SLS) with respect to timing

	(1) First two years	(2) First year	(3) Total	(4) Weekend	(5) Episodes
			First reform		
Peer effect	0.080	-0.006	0.097	-0.003	-1.270
	(0.191)	(0.134)	(0.217)	(0.015)	(3.752)
Observations	11,596	11,596	11,596	11,596	8,510
			Second reform		
Peer effect	0.534**	0.314**	0.168	-0.013	0.123***
	(0.247)	(0.147)	(0.287)	(0.030)	(0.043)
Observations	12,309	12,309	12,309	12,309	11,637

Notes: Estimates of the first stage and reduced form from separate RD regressions. Column 1 reports the main results, parental leave in the first two years. Column 2 reports the parental leave in the first year and column 3 in the total number of days of parental leave (by age 8). Column 4 report the number of days allocated to weekends and column 5 the number of cohesive periods of parental leave. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

1.D Heterogeneity

Table D1: Control mean heterogeneity wrt cost

	(1) First two years	(2) First year	(3) Total	(4) Weekend	(5) Episodes
		Monetary			
First reform					
Low	30.321	17.907	40.109	0.982	2.034
High	24.868	14.806	34.321	0.649	1.500
	0.820	0.827	0.856	0.661	0.737
Second reform					
Low	41.555	16.771	72.165	1.747	5.577
High	30.042	13.372	59.220	1.311	4.368
	0.723	0.797	0.821	0.750	0.783
		Normative			
First reform					
Low	33.264	18.650	43.825	1.078	2.270
High	27.183	16.964	36.356	0.728	1.581
	0.817	0.910	0.830	0.675	0.696
Second reform					
Low	45.993	18.045	77.153	1.515	5.354
High	36.430	15.879	66.758	1.733	5.354
	0.792	0.880	0.865	1.144	1

Notes: Parental leave of control peers in different subsamples. For monetary costs, the workplaces are ranked by the number of coworkers with the same education as the peer. High costs refers to 3 or less coworkers. Normative cost is the average predicted parental leave of coworkers based on pre-reform characteristics, split into two. Costs are interacted with the treatment variable.

Table D2: Heterogeneity wrt monetary cost of employer

	(1) First two years	(2) First year	(3) Total	(4) Weekend	(5) Episodes
First stage			*First reform*		
Treated (Low)	19.581***	11.609***	31.228***	0.597	2.897***
	(4.192)	(3.044)	(5.336)	(0.369)	(0.334)
TreatedXhigh	-1.037	-3.378	-2.936	-0.238	-0.306
	(11.517)	(7.552)	(13.989)	(0.580)	(0.613)
Reduced form					
Constant (Low)	-0.310	0.654	-0.434	0.085	-0.037
	(3.346)	(2.132)	(4.135)	(0.274)	(0.397)
TreatedXhigh	-3.693	-16.533**	-7.392	-0.817	0.064
	(9.536)	(7.115)	(13.094)	(1.417)	(1.004)
Observations	9,185	9,185	9,185	9,185	9,185
First stage			*Second reform*		
Treated (Low)	13.953***	4.915	26.475***	0.446	1.838***
	(5.284)	(3.314)	(5.829)	(0.300)	(0.409)
TreatedXhigh	6.656	4.492	6.556	-0.497	-1.182
	(12.670)	(7.996)	(14.436)	(0.997)	(0.964)
Rduced form					
Treated (Low)	9.081**	5.011*	5.270	0.245	-0.013
	(4.146)	(2.765)	(4.668)	(0.544)	(0.463)
TreatedXhigh	-2.206	2.547	0.318	-0.746	-1.108
	(12.117)	(7.840)	(16.301)	(2.087)	(1.380)
Observations	9,803	9,803	9,803	9,803	9,803

Notes: Estimates from separate RD regressions. The workplaces are grouped by the number of coworkers with the same education and interacted with the treatment variable and the control function. High costs refers to 3 or less coworkers. Column 1 reports the main results, parental leave in the first two years. Column 2 reports the parental leave in the first year and column 3 in the total number of days of parental leave (by age 8). Column 4 report the number of days allocated to weekends and column 5 the number of cohesive periods of parental leave. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table D3: Heterogeneity wrt normative cost of employee

	(1) First two years	(2) First year	(3) Total	(4) Weekend	(5) Episodes
First stage			*First reform*		
Treated (Low)	14.609***	8.190***	26.593***	0.301	2.678***
	(4.647)	(2.974)	(5.930)	(0.376)	(0.343)
TreatedXhigh	3.122	1.400	0.769	0.577	0.200
	(6.059)	(4.536)	(7.871)	(0.537)	(0.533)
Reduced form					
Treated (Low)	0.380	-1.412	3.009	0.010	0.528
	(4.683)	(3.033)	(5.647)	(0.455)	(0.434)
TreatedXhigh	1.437	2.791	-3.093	-0.180	-0.813
	(6.725)	(4.586)	(8.400)	(0.552)	(0.563)
Observations	11,455	11,455	11,455	11,455	11,455
First stage			*Second reform*		
Treated (Low)	16.235**	4.509	21.910***	0.245	0.958*
	(6.369)	(3.892)	(6.331)	(0.318)	(0.501)
TreatedXhigh	-4.818	0.150	5.340	0.567	1.430**
	(8.010)	(4.677)	(8.504)	(0.574)	(0.654)
Reduced form					
Treated (Low)	3.458	3.097	1.864	-0.235	-0.166
	(5.341)	(3.296)	(5.859)	(0.569)	(0.645)
TreatedXhigh	7.122	2.651	-0.750	0.219	-0.849
	(8.123)	(5.044)	(9.579)	(0.885)	(0.994)
Observations	12,158	12,158	12,158	12,158	12,158

Notes: Estimates from separate RD regressions. The workplaces are ranked by the average predicted parental leave of coworkers based on pre-reform characteristics, split into two by the median and this is interacted with the treatment indicator and the control function. Column 1 reports the main results, parental leave in the first two years. Column 2 reports the parental leave in the first year and column 3 in the total number of days of parental leave (by age 8). Column 4 report the number of days allocated to weekends and column 5 the number of cohesive periods of parental leave. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table D4: Uptake of treated fathers, heterogeneity wrt cost

	(1) First two years	(2) First year	(3) Total	(4) Weekend	(5) Episodes
		Monetary			
Control peers (1995)	0.820	0.827	0.856	0.661	0.737
Control peers (2002)	0.723	0.797	0.821	0.750	0.783
Treated fathers (2002)					
Low	55.219	20.258	100.373	2.985	8.137
High	50.293	20.650	99.673	3.191	7.299
Fraction	0.911	1.019	0.993	1.069	0.897
		Normative			
Control peers (1995)	0.817	0.910	0.830	0.675	0.696
Control peers (2002)	0.792	0.880	0.865	1.144	1
Treated fathers (2002)					
Low	59.279	21.341	104.416	2.832	7.737
High	48.436	19.016	92.087	2.999	7.869
Fraction	0.817	0.891	0.882	1.059	1.017

Notes: Parental leave of treated fathers in different subsamples. For monetary costs, the workplaces are ranked by the number of coworkers with the same education as the peer. High costs refers to 3 or less coworkers. Normative cost is the average predicted parental leave of coworkers based on pre-reform characteristics, split into two. Column 1 reports the main results, parental leave in the first two years. Column 2 reports the parental leave in the first year and column 3 in the total number of days of parental leave (by age 8). Column 4 report the number of days allocated to weekends and column 5 the number of cohesive periods of parental leave.

1.E Robustness

Table E1: Model specification

	(1) 1-week donut	(2) 2-week donut	(3) Uniform weight	(4) Linear trend	(5) Cubic trend	(6) 3 months	(7) Optimal
First reform							
First stage	13.407***	13.064***	14.975***	14.050***	16.273***	15.540***	14.469***
	(4.019)	(4.866)	(3.242)	(2.349)	(3.225)	(2.964)	(2.562)
Reduced form	1.875	-0.083	0.746	1.020	1.301	1.773	0.996
	(3.943)	(4.477)	(2.933)	(2.183)	(3.127)	(2.924)	(2.434)
Observations	11,200	10,791	11,722	11,597	11,597	5,605	8,391
Second reform							
First stage	10.974**	17.308***	13.249***	10.824***	14.536***	14.154***	11.851 ***
	(4.892)	(6.026)	(3.807)	(2.691)	(4.137)	(3.755)	(2.940)
Reduced form	7.174*	9.411*	7.689**	4.930*	7.777**	7.122**	6.034
	(4.137)	(5.138)	(3.513)	(2.525)	(3.740)	(3.439)	(2.731)
Observations	11,918	11,516	12,425	12,309	12,309	5,712	9,372

Notes: Estimates from separate RD regressions on parental leave in the first two years using the baseline specification, adjusting the dimension as stated in the column title. For column 7 and 8, I adjust the polynomial of the trend (from second to first) in addition to the bandwidth. Optimal bandwidth (in days, relative to cutoff) is (-134,135) and (-134,134) for the first reform, (-140,140) and (-141,140) for the first reform. Baseline estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E2: Local linear regression

	(1) 6 months	(2) 4.5 months	(3) 3 months
First reform			
Reduced form	0.512	1.058	0.504
	(2.998)	(2.300)	(2.809)
Second reform			
Reduced form	6.162*	3.958	7,507**
	(3.419)	(2.758)	(3,201)

Notes: Reduced form estimates on parental leave in the first two years, estimated using local linear regression model with 2000 repetitions. Column 1 use a quadratic trends, column 2-3 use linear trends. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table E3: Multiple hypothesis testing

	(1) First two years	(2) Firt year	(3) Total	(4) Weekend	(5) Episodes
First reform					
Parent Model p-value	0.679	0.966	0.658	0.826	0.705
Resample p-value	0.582	0.948	0.570	0.781	0.669
Romano-wolf p-value	0.960	0.964	0.960	0.964	0.964
Second reform					
Parent Model p-value	0.039	0.047	0.580	0.649	0.161
Resample p-value	0.020	0.016	0.454	0.594	0.072
Romano-wolf p-value	0.036	0.036	0.753	0.753	0.179

Notes: Reduced form p-values adjusting for multiple hypothesis using the Romano-Wolf (rwolf stata command, 250 replications). Estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table E4: Main results including higher order fathers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First reform				Second reform			
	First	Second	Third	All	First	Second	Third	All
First stage								
Order rest.	14.540***	14.965***	16.053***		16.296***	15.809***	15.182***	
	(3.453)	(3.473)	(3.545)		(3.925)	(4.118)	(4.279)	
Observations	18,399	22,534	25,177		19,767	24,375	27,272	
Year rest.	13.137***	14.094***	15.071***	16.400***	16.020***	15.135***	14.847***	16.810***
	(3.576)	(3.467)	(3.632)	(3.835)	(4.484)	(4.972)	(4.925)	(5.483)
Observations	17,302	20,998	24,109	30,517	18,141	22,517	26,091	33,412
Reduced form								
Order rest.	-1.853	-2.549	-2.949		8.892***	8.715***	7.506**	
	(2.700)	(2.626)	(2.631)		(3.168)	(2.982)	(2.939)	
	18,399	22,534	25,177		19,767	24,375	27,272	
Year rest.	1.260	0.279	-0.622	-1.955	7.356**	5.829*	6.685**	6.793**
	(3.047)	(2.831)	(2.737)	(2.404)	(3.368)	(3.098)	(2.950)	(2.829)
Observations	17,302	20,998	24,109	30,517	18,141	22,517	26,091	33,412

Notes: Estimates from separate RD regressions. Order restriction refers to the rank of the father (after the initial father) at the workplace, in terms of birthdate. Year restriction refers to the allowed gap between fathers included and the initial father. All fathers include all subsequent fathers ath the workplace, within the 6 year post-period. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E5: Treatment effects on composition of fathers

	(1)	(2)	(3)	(4)
	First reform		Second reform	
	Age gap	Number of fathers	Age gap	Number of fathers
treated	-0.006	-0.023	0.005	0.203
	(0.011)	(0.146)	(0.010)	(0.152)
Observations	33,162	33,162	35,652	35,652

Notes: Estimates from separate RD regressions. Both measures refers to workplace averages regarding fathers in the 6 year post-period. Age gap refers to the the average gap between peers and fathers (in years), number of fathers refers to the average number of fathers. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table E6: The number of coworkers with same education

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First two years				In total (age 8)			
	None	One	Two	Three	None	One	Two	Three
First stage				*First reform*				
Treated (Low)	19.108***	19.166***	19.581***	19.967***	30.943***	31.090***	31.228***	31.745***
	(3.762)	(3.748)	(4.192)	(3.751)	(4.627)	(5.336)	(4.633)	(4.619)
TreatedXhigh	19.283	4.037	-1.037	-2.200	1.186	-3.139	-2.936	-3.680
	(28.401)	(6.046)	(11.517)	(3.656)	(32.303)	(6.546)	(13.989)	(4.195)
Reduced form								
Treated (Low)	-0.741	-1.204	-0.310	-0.534	-1.404	-1.887	-0.434	-1.233
	(3.235)	(3.260)	(3.346)	(3.366)	(3.695)	(3.683)	(4.135)	(3.824)
TreatedXhigh	-2.568	5.238	-3.693	-1.106	-9.414	4.085	-7.392	-1.527
	(21.993)	(5.570)	(9.536)	(3.354)	(24.549)	(6.159)	(13.094)	(3.847)
Number of high cost	303	789	1,212	1,855	303	789	1,212	1,855
Observations	9,185	9,185	9,185	9,185	9,185	9,185	9,185	9,185
First stage				*Second reform*				
Treated (Low)	14.079***	14.212***	13.953***	13.885***	26.603***	26.933***	26.475***	26.257***
	(5.133)	(5.104)	(5.284)	(5.039)	(5.303)	(5.343)	(5.829)	(5.302)
TreatedXhigh	24.026	6.412	6.656	5.407	21.240	1.552	6.556	5.626
	(31.813)	(6.053)	(12.670)	(3.915)	(34.863)	(7.018)	(14.436)	(4.423)
Reduced form								
Treated (Low)	7.501*	8.366**	9.081**	9.078**	3.650	4.119	5.270	4.593
	(4.325)	(4.233)	(4.146)	(4.204)	(4.755)	(4.669)	(4.668)	(4.640)
TreatedXhigh	43.088	5.370	-2.206	-1.211	58.497*	15.160*	0.318	3.854
	(30.721)	(6.165)	(12.117)	(4.154)	(34.908)	(8.324)	(16.301)	(4.439)
Number of high cost	280	702	1,098	1,710	280	702	1,211	1,710
Observations	9,803	9,803	9,803	9,803	9,803	9,803	9,803	9,803

Notes: Estimates from separate RD regressions on parental leave in the first two years (left) and in total(right). The number of coworkers with the same education is in addition to the peer and father. Costs are interacted with the treatment variable and the control function. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table E7: The number of coworkers with same education, full sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First two years				In total (age 8)			
	None	One	Two	Three	None	One	Two	Three
First stage				*First reform*				
Treated (Low)	16.292***	16.796***	16.695***	16.635***	27.719***	29.011***	29.412***	28.967***
	(3.349)	(3.359)	(3.421)	(3.719)	(4.035)	(4.129)	(4.284)	(4.754)
TreatedXhigh	-0.342	-8.572	-3.610	-2.225	-16.518	-26.627*	-16.951	-9.623
	(19.677)	(13.039)	(10.466)	(8.836)	(23.133)	(14.799)	(11.842)	(11.111)
Observations	11,596	11,596	11,596	11,596	11,596	11,596	11,596	11,596
Reduced form								
Treated (Low)	1.225	1.045	0.027	0.718	1.320	1.052	-0.411	0.642
	(3.133)	(3.141)	(3.179)	(3.192)	(3.566)	(3.655)	(3.713)	(3.926)
TreatedXhigh	10.073	4.005	13.438	4.039	35.259	8.399	21.324**	6.535
	(21.746)	(11.995)	(9.432)	(8.440)	(32.098)	(12.906)	(9.824)	(10.900)
Number of high cost	249	779	1,453	2,138	249	779	1,453	2,138
Observations	11,596	11,596	11,596	11,596	11,596	11,596	11,596	11,596
First stage				*Second reform*				
Treated (Low)	14.607***	13.780***	14.721***	14.385***	25.690***	25.547***	26.562***	26.080***
	(4.152)	(4.256)	(4.450)	(4.523)	(4.305)	(4.303)	(4.559)	(4.935)
TreatedXhigh	-0.757	12.722	-2.896	0.553	14.265	7.771	-6.995	-1.047
	(31.944)	(16.891)	(13.942)	(11.804)	(30.425)	(19.310)	(15.165)	(12.923)
Observations	12,309	12,309	12,309	12,309	12,309	12,309	12,309	12,309
Reduced form								
Treated (Low)	7.790**	7.515*	8.704**	9.925***	2.595	2.147	4.274	4.575
	(3.769)	(3.919)	(3.691)	(3.578)	(4.472)	(4.557)	(4.331)	(4.302)
TreatedXhigh	-2.826	4.676	-10.252	-14.950	-9.506	6.458	-18.933	-14.675
	(22.615)	(17.634)	(13.890)	(10.912)	(21.627)	(20.551)	(18.197)	(14.342)
Number of high cost	115	615	1,280	1,971	115	615	1,280	1,971
Observations	12,309	12,309	12,309	12,309	12,309	12,309	12,309	12,309

Notes: Estimates from separate RD regressions on parental leave in the first two years (left) and in total(right). The number of coworkers with the same education for the first stage (reduced form) estimate is in addition to the peer (father). Workplaces coded as high costs are not necessarily the same in the first stage and for the reduced form. Costs are interacted with the treatment variable and the control function. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E8: The number of coworkers with same education (level and field)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First two years				In total (age 8)			
	None	One	Two	Three	None	One	Two	Three
First stage				*First reform*				
Treated (Low)	15.914***	14.035**	13.342**	14.289**	26.253***	25.421***	24.634***	25.213***
	(5.828)	(5.453)	(5.610)	(5.776)	(6.966)	(6.717)	(6.884)	(7.026)
TreatedXhigh	-7.694	7.619	7.792	3.085	0.234	6.294	7.164	3.402
	(26.266)	(7.112)	(4.861)	(4.448)	(30.888)	(7.594)	(5.391)	(4.876)
Reduced form								
Treated (Low)	1.403	0.043	0.748	1.366	-0.666	-2.476	-1.508	-0.859
	(4.748)	(4.973)	(5.096)	(5.154)	(5.486)	(5.710)	(5.826)	(5.899)
TreatedXhigh	-14.354	2.705	-0.893	-2.821	-24.797	1.680	-2.654	-4.534
	(20.104)	(5.055)	(3.911)	(3.812)	(24.338)	(6.209)	(4.765)	(4.417)
Number of high cost	412	885	1,356	1,763	412	885	1,356	1,763
Observations	4,772	4,772	4,772	4,772	4,772	4,772	4,772	4,772
First stage				*Second reform*				
Treated (Low)	15.436*	15.800**	16.352**	17.083**	27.431***	28.558***	29.130***	30.070***
	(8.114)	(7.783)	(8.003)	(7.843)	(8.104)	(7.984)	(8.187)	(8.057)
TreatedXhigh	20.391	8.761	4.260	1.434	32.398	8.356	3.274	-0.115
	(23.227)	(6.580)	(5.725)	(4.913)	(26.720)	(7.760)	(6.592)	(5.726)
Reduced form								
Treated (Low)	14.418*	13.626*	13.667*	12.438*	6.943	6.306	5.549	4.033
	(7.521)	(6.976)	(6.993)	(7.171)	(9.115)	(8.603)	(8.704)	(8.856)
TreatedXhigh	-24.727	-0.989	-0.658	1.635	-10.935	1.403	3.123	5.458
	(16.430)	(7.378)	(5.513)	(5.109)	(25.789)	(8.244)	(6.117)	(5.508)
Number of high cost	396	847	1,258	1,609	396	847	1,258	1,609
Observations	4,772	4,772	4,772	4,772	4,772	4,772	4,772	4,772

Notes: Estimates from separate RD regressions on parental leave in the first two years (left) and in total(right). The number of coworkers with the same education (level and field) is in addition to the peer and father. Level of education (4 categories; compulsory schooling, high school, some college, and college degree) and field (9 categories as defined by statistics Sweden). Costs are interacted with the treatment variable and the control function. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Table E9: The number of coworkers with same occupation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First two years				In total (age 8)			
	None	One	Two	Three	None	One	Two	Three
First stage				*First reform*				
Treated (Low)	8.278	8.139	7.379	6.219	20.614***	21.271***	20.954***	19.694***
	(6.316)	(6.229)	(6.273)	(6.372)	(6.747)	(6.560)	(6.645)	(6.715)
TreatedXhigh	-10.722	-0.411	4.680	7.022	13.815	0.273	2.931	5.850
	(19.330)	(6.228)	(5.507)	(4.673)	(24.344)	(7.134)	(6.110)	(5.102)
Reduced form								
Treated (Low)	3.343	1.144	2.294	2.598	-3.833	-4.737	-3.344	-3.359
	(4.791)	(4.730)	(4.829)	(4.911)	(6.781)	(6.600)	(6.645)	(6.684)
TreatedXhigh	-44.275*	2.580	-4.358	-4.673	-29.173	-1.663	-7.879	-6.180
	(25.439)	(8.946)	(5.965)	(4.801)	(31.877)	(11.206)	(7.333)	(5.851)
Number of high cost	278	604	987	1,374	278	604	987	1,374
Observations	5,759	5,759	5,759	5,759	5,759	5,759	5,759	5,759

Notes: Estimates from separate RD regressions on parental leave in the first two years (left) and in total(right). The number of coworkers with the same occupation is in addition to the peer and father. Occupation is defined using SSYK 3-digit provided by statistics Sweden. Costs are interacted with the treatment variable and the control function. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table E10: Different cuts for high normative costs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	First two years				In total (age 8)			
	50%	33%	25%	20%	50%	33%	25%	20%
				First reform				
First stage								
Treated (Low)	14.609***	15.398***	15.499***	16.152***	26.593***	26.455***	26.453***	27.061***
	(4.647)	(3.491)	(3.294)	(3.239)	(5.930)	(4.263)	(4.017)	(3.960)
TreatedXhigh	3.122	1.754	2.413	0.197	0.769	1.006	2.242	0.329
	(6.059)	(3.417)	(3.331)	(3.752)	(7.871)	(3.889)	(3.774)	(4.080)
Reduced form								
Treated (Low)	0.380	3.824	3.066	2.441	3.009	3.641	3.077	2.176
	(4.683)	(3.210)	(3.177)	(3.136)	(5.647)	(3.667)	(3.666)	(3.605)
TreatedXhigh	1.437	0.999	-7.398**	-5.171	-3.093	-6.617*	-6.165	-2.968
	(6.725)	(3.026)	(3.222)	(3.294)	(8.400)	(3.759)	(3.937)	(4.056)
Number of high cost	5,789	3,860	2,895	2,316	5,789	3,860	2,895	2,316
Observations	11,455	11,455	11,455	11,455	11,455	11,455	11,455	11,455
				Second reform				
First stage								
Treated (Low)	16.235**	15.835***	15.112***	14.868***	21.910***	26.090***	26.124***	26.123***
	(6.369)	(4.304)	(4.190)	(4.231)	(6.331)	(4.359)	(4.309)	(4.294)
TreatedXhigh	-4.818	-3.838	-2.976	-1.947	5.340	-0.467	-1.359	-1.228
	(8.010)	(3.396)	(3.323)	(3.257)	(8.504)	(4.039)	(3.841)	(3.842)
Rduced form								
Treated (Low)	3.458	7.483*	7.138*	7.411**	1.864	2.212	2.143	3.086
	(5.341)	(3.948)	(3.730)	(3.670)	(5.859)	(4.504)	(4.368)	(4.282)
TreatedXhigh	7.122	0.794	2.123	1.444	-0.750	0.673	0.724	-3.316
	(8.123)	(3.632)	(3.442)	(3.475)	(9.579)	(4.086)	(4.037)	(4.174)
Number of high cost	5,789	3,860	2,895	2,316	5,789	3,860	2,895	2,316
Observations	12,158	12,158	12,158	12,158	12,158	12,158	12,158	12,158

Notes: Estimates from separate RD regressions on parental leave in the first two years (left) and in total(right). Defining high normative costs using different cuts on the distribution of workplaces, with respect to the average predicted parental leave uptake of men. Costs are interacted with the treatment variable and the control function. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table E11: Peer's parental leave before father's

	(1) First reform	(2) Second reform
Treated	-0.011	-0.032
	(0.016)	(0.021)
Control mean	0.067***	0.132***
Observations	6,550	6,688
R-squared	0.000	0.000

Notes: Estimates from separate RD regressions on the indicator variable, taking 1 if fathers took parental leave before the peer first did so. Baseline estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table E12: Timing of switching firms

	(1) Move before PL	(2) Move before PL	(3) Move before peer PL
	Peer	Father	Father
Treated	-0.209***	-0.053**	-0.180***
	(0.029)	(0.025)	(0.032)
Control mean	0.252	0.484	0.312
Observations	8,510	10,739	8,510
Treated	-0.190***	-0.033	-0.184***
	(0.028)	(0.030)	(0.029)
Control mean	0.283	0.442	0.347
Observations	11,637	11,551	11,637

Notes: First stage estimates from separate regressions. Column 1 is an indicator of the peer switching workplace before taking parental leave. Column 2 is an indicator of the father switching workplace before taking parental leave. Column 3 is an indicator of the father switching workplace before observing parental leave of the peer. All estimations include a one-week donut, quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Table E13: High scope for spillovers, interaction terms

	(1) Small	(2) Same educ.	(3) Close birth	(4) Manager	(5) Single peer	(6) 1st parity	(7) >5 employees
First reform							
X First stage	-0.458	5.902**	-0.901	-1.040	-0.921	-0.622	-3.549
	(2.622)	(2.861)	(2.619)	(3.132)	(4.273)	(2.528)	(4.252)
X Reduced form	-0.576	-0.053	0.701	1.831	5.256	-2.274	1.249
	(2.199)	(2.685)	(2.665)	(2.777)	(3.856)	(2.157)	(3.593)
Observations	11,597	11,597	11,597	11,597	11,597	11,597	11,597
Second reform							
X First stage	0.697	2.449	-5.412*	1.132	-4.544	1.883	-5.346
	(2.524)	(3.358)	(3.040)	(3.299)	(4.333)	(2.726)	(4.709)
X Reduced form	2.838	0.156	-0.836	5.846	0.559	-2.107	-2.742
	(2.913)	(3.371)	(2.990)	(3.597)	(4.710)	(2.540)	(4.778)
Observations	12,309	12,309	12,309	23	12,309	12,309	12,309

Notes: Interaction terms from separate RD regressions on parental leave in the first two years. For the interaction terms I use indicator variables for workplaces with less than 30 employees (column 1); if the peer and father have the same education (column 2); if the birth gap is no more than 3 years (column 3); if the peer is a manager, measured as the top income rank (column 4); if there is a single peer in the reform window meaning no additional peers with lower education (column 5); if the father has his first child (column 6); if the workplace has more than 5 workers. High scope indicators are interacted with the treatment variable and the control function. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

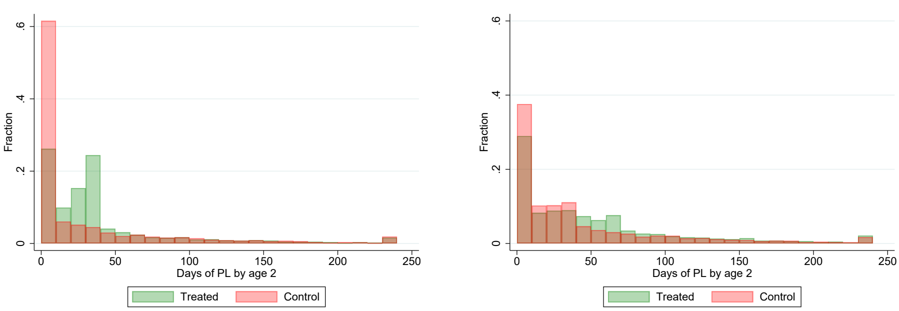
Table E14: Heterogeneity by educational level

	(1)	(2)	(3)	(4)	(5)	(6)
	Peer education			Father education		
	Low	Medium	High	Low	Medium	High
First reform						
First stage	24.236	14.131***	20.685**	-0.253	18.665***	27.036**
	(21.735)	(3.430)	(8.805)	(7.042)	(3.778)	(11.166)
Reduced form	-14.512	1.297	-7.081	12.375	-1.370	-11.084
	(20.102)	(3.240)	(8.445)	(7.996)	(3.443)	(13.869)
Observations	606	8,654	2,336	2,079	8,226	1,291
First stage	47.868	16.192***	5.436	16.245	18.518***	-0.844
	(35.291)	(4.734)	(8.417)	(10.642)	(4.858)	(11.305)
Reduced form	4.601	3.693	16.562	3.067	7.539*	18.378
	(40.763)	(4.247)	(10.757)	(11.965)	(4.028)	(15.662)
Observations	341	8,657	3,311	1,645	8,693	1,971

Notes: Estimates from separate RD regressions on parental leave in the first two years. The sample is divided by peers and fathers level of education the year before the reform, low (0-9 years), medium (10-13 years), high (at least 14 years). All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Figure E1: Days of PL, peers

(a) 1995 **(b)** 2002



Notes: The figures shows the days of parental leave taken by peers in the first two years, by treatment status, in a histogram. Bin size is 10 days. The last bar contains peers who take 240 days or more.

Table E15: First stage with respect to timing

	(1) First two years	(2) First year	(3) Total	(4) Weekend	(5) Episodes
Reform 1995					
First stage	16.273***	8.907***	27.261***	0.590**	2.790***
	(3.225)	(2.252)	(3.886)	(0.297)	(0.284)
Observations	11,597	11,597	11,597	11,597	11,597
Reform 2002					
First stage	14.536***	5.193*	25.941***	0.556**	1.761***
	(4.137)	(2.671)	(4.226)	(0.237)	(0.371)
Observations	12,309	12,309	12,309	12,309	12,309

Notes: Estimates of the first stage from separate regressions. Column 1 reports the main results, parental leave in the first two years. Column 2 reports the parental leave in the first year and column 3 in the total number of days of parental leave (by age 8). Column 4 report the number of days allocated to weekends and column 5 the number of cohesive periods of parental leave. All estimations include separate quadratic trends, triangular weights, workplace covariates and cohort- and muni-fe. Robust standard errors in parentheses, clustered at the running variable; peer child date of birth. *** p<0.01, ** p<0.05, * p<0.1.

Chapter 2

Human Capital Effects of Opportunities for One-on-one Time with Parents

Evidence from a Swedish Childcare Access Reform[*]

[*]A previous version of this paper was circulated under the title "Human capital effects of one-on-one time with parents: evidence from a Swedish childcare access reform" (IFAU WP 2022:2). We thank Lisa Laun for invaluable contributions at the initial stages of the project. We are grateful to Christina Felfe, Erica Lindahl and Caroline Hall for detailed comments and to seminar and workshop participants at IFAU, SOFI, the Social Gradient Workshop Uppsala, 2019, and AASLE, Singapore 2019, and SOLE EALE 2020. The paper uses data from the IFAU Children Database which has been financed by grants from VR 2014-10165 and RJ P15-0812:1.

2.1 Introduction

There is growing evidence that early childhood conditions are important determinants of children's human capital development. Francesconi and Heckman (2016) summarize the recent literature on early life conditions and conclude that observed socioeconomic skill gaps are associated with gaps in child-related investments, such as language exposure and supportive and human capital enhancing parenting styles. The quantity and quality of parental time investments in early childhood are important for secure attachment (Cox et al., 1992; Bureau et al., 2017) and have been shown to be beneficial for human capital development (Fiorini and Keane, 2014; Hsin and Felfe, 2014; Del Bono et al., 2016; Fort et al., 2020, Ginja et al., 2020).[1] Differential and lower parental time investments in younger siblings have also been proposed as an explanation for why younger siblings fare worse than older siblings in many different dimensions (Black et al., 2005; Björkegren and Svaleryd, 2017; Black et al., 2018; Lehmann et al., 2018).

In light of this evidence, it is of interest to investigate effects on children's human capital formation of policies that potentially affect parental time investments during early childhood. In this paper, we exploit a Swedish childcare access reform implemented in 2002 that likely increased infants' one-on-one time with parents by guaranteeing older siblings at least 15 hours of highly subsidized, high quality childcare per week while their parent was on parental leave with the infant. Before the reform, municipalities were not required to provide childcare for the older siblings so many municipalities did not. In these municipalities, the reform thus created an exogenous change in childcare access for older siblings while also improving the opportunity of parents to spend time exclusively with their infant child by increasing the adult-to-child ratio in home care from 1:2 to 1:1 for at least a few hours per day.[2] This reduced care load may also have reduced parental stress and improved opportunities for parental leisure or work, potentially affecting the quality of parental investments. Moreover, the reform reduced the exposure of siblings to one another during the infant's first year of life and it also

[1] See Moullin et al. (2018) for a discussion of the importance of attachment for the transmission of socioeconomic disadvantage.

[2] See Fort et al. (2020) for a discussion of the importance of one-on-one communication for child development.

implied that families maintained a connection to their childcare provider, possibly facilitating the enrollment of the younger child, mother's return to the labor market, and fertility choices. It is, however, not obvious if and how fertility decisions should be affected by the reform. On the one hand, a lessened care load might make the idea of a larger family more attractive and therefore increase fertility. On the other hand, caring for only one child at the time and maintaining contacts with the childcare environment might reduce the returns to specializing in childbearing during some intense years by lowering transaction costs of returning to work between children. As a result, child spacing might increase.

We identify causal effects on the younger child's human capital development of increased flexibility in childcare arrangement for the older sibling using a Difference-in-differences (DD) approach. To this end, we apply rich linked Swedish administrative data on parental education, health, income, and earnings as well as health and educational outcomes for the children born around the time of the reform. We exploit the fact that the childcare access reform targeted families with infants and siblings of childcare age. Specifically, we compare standardized core subject test scores at age 13 of children, born in affected municipalities before and after the reform, who at birth did or did not have an older sibling of childcare age. As the core subjects include Mathematics, Swedish, and English, we were able to capture different aspects of children's language and cognitive development. The same comparison, in municipalities that were not affected by the reform, is used as a placebo test to verify that we are not merely picking up trends in educational outcomes of children with and without siblings of childcare age. Because individual level childcare enrollment data are unavailable, our strategy captures intention-to-treat (ITT) effects. We rely on survey data to verify that the reform significantly affected the childcare enrollment of older siblings and hence that families faced increased opportunities for more one-on-one time with the younger child.

We analyze whether school results differ by the gender of the child as evidence suggests that there are gender differences in sensitivity to childhood environment (see, e.g., Bertrand and Pan, 2013; Autor et al., 2019; Fort et al., 2020). We also explore the extent that effects on test scores depend on maternal education, as mothers are typically the primary care giver of infants.[3] This is of particular interest because parental stress can differ with education or socioeconomic status (SES) of parents (Parkes et al., 2015) and consequently families of different SES may, in practice, have been more or less constrained by the lack of childcare before the reform. Furthermore, SES has implications for the quality of the home environment, as discussed extensively when analyzing effects of parental and alternative modes of care (see e.g., Drange and Havnes, 2019; Ginja et al., 2020) and the conditions for parent child attachment (Moullin et al., 2018).

For the infant child in focus, increased childcare access for the older sibling affects several potentially important margins, but perhaps the most important and direct consequence is that the child is likely to gain more undisturbed one-on-one time with the parent on parental leave. Childcare access for the older sibling reduces the competition for parental time, which might reduce parental stress and improve quality of parent-infant interactions. This could potentially also affect the parent's willingness to stay on parental leave or change parental time allocation while on leave. The direction of the net effects is not clear. On the one hand, more undisturbed one-on-one interaction with a parent may be beneficial for a child's socioemotional development (NICHD-ECCRN, 2003) as it allows for closer attachment (Cox et al., 1992) and more stimulus and direct exposure to spoken language, which has been found to be important for language development (Fernald et al., 2013). On the other hand, absence of the sibling may also reduce the indirect exposure to spoken language.

In addition to studying child schooling outcomes, we explore several possible pathways through which the increased access to childcare of older siblings may affect children's school performance, including health, family environment, parental time allocation, and sibling spillovers. Although childcare access implies less exposure to the older sibling, the infant child is more likely to get exposed to the older sibling's childcare environment at delivery and pick up time and via the sibling. Childcare attendance has been connected to short run increases in infections and viruses and long run decreases in asthma and allergies (see e.g., Lu et al., 2004; de Hoog et al., 2014; Ball et al., 2002; Ball et al., 2000 and Aalto et al., 2019).[4] The

[3]At the time of the studied reform, mothers accounted for around 90 percent of total parental leave take-up and almost all parental leave during the first year of life.

[4]Effects of childcare are typically stronger for the oldest sibling as younger siblings

reform potentially also affected the mental health of the child as increased one-on-one time with a parent during infancy facilitates attachment and socioemotional development. Moreover, childcare access for the older sibling can affect the quality of the home environment, which is important for the cognitive development of the child (Francesconi and Heckman, 2016). The reform may directly affect the parent, which in most cases will be the mother, by reducing stress and the need to juggle the care of both the infant and the toddler. Better access to support following birth has been found important for the post-partum health among mothers (Persson and Rossin-Slater, 2021). Beyond the possible health effects, increased flexibility and less stress during parental leave can affect the marital stability, timing of younger siblings, the duration of parental leave, and subsequent labor market attachment of mothers. Furthermore, the maintained contact with the childcare environment implied by the reform, may ease the transition back to work and affect the timing of childcare enrollment. We also explore alternative mechanisms unrelated to more one-on-one time per se, such as the division of parental leave, as suggested by Cools et al. (2015), and spillovers from potentially increased human capital of the sibling who gains access to childcare, as is found in Hallberg (2019).

Using survey evidence, we verify that the reform increased childcare enrollment of older siblings substantially: in the country as a whole, enrollment of children with a parent on parental leave almost doubled from 25 percent in 1999 to 47 percent in 2002. Formal analysis shows a mean reform effect around 30 percentage points and that effects were somewhat higher for children of university educated mothers. Although the first stage shows that families took advantage of the better opportunities for one-on-one time, our results show that this did not have a significant effect on child test scores on average: mean effects, while positive, are not significantly different from zero at conventional levels. Estimating effects by child sex shows that the test scores of boys improved by 0.043 standard deviation (SD). This effect is entirely driven by sons of less than college educated mothers, who gain 0.063 SD). There is no average

are exposed to microorganisms through their older siblings. See Scudellari (2017) for an updated discussion of the so-called hygiene hypotheses, according to which early life exposure to microorganisms stimulates the immune system and therefore reduces the risk of developing autoimmune diseases such as asthma and allergies.

effect on girls, but we find that test scores improved by 0.086 SD for daughters of university educated mothers. For this sub-analysis, however, the parallel trends assumption is not satisfied, so the findings are merely suggestive.

We explore several mechanisms through which the increased opportunities for more one-on-one time may have affected the human capital development. The pattern of the results of this analysis indicates that less behavioral and psychiatric problems in school age may have contributed to the better school performance of sons of less than college educated mothers. We find no evidence that the improved opportunities for one-on-one time affected mother's mental health, family separations, mother's return to work, the child's age at childcare enrollment, parental leave division between parents, or human capital spillovers from the older sibling. Hence, human capital improvements were not the result of drastic changes of the home environment or changes in the quantity of time spent with a parent. Rather, the effect on low SES boys is consistent with more subtle improvements in the quality of parent-child interaction and reduced competition for parental time due to more one-on-one time during the first year of life, facilitating a better attachment and socioemotional development. A reduction in competition for parental time, allowing for more intellectual stimulus, may be the reason also for the suggested improvement in test scores of daughters of university educated mothers, in line with Fort et al. (2020). For this group, we also find indications of a further reduction in competition for parental time due to a reduced likelihood of having a younger sibling before age three.

This paper relates to several strands of literature with the common overarching objective to better understand the process of human capital formation in early childhood. This includes work on the role of parental time investments and the role of siblings. A central theme in all these literatures is the role of time allocation—or exposure—of infants and children to parents, siblings, and childcare.

Specifically, this paper relates to Francesconi and Heckman (2016). They survey the literature on early childhood human capital development and argue that financial investments and constraints have received too much focus compared to exposure to parenting and mentoring relationships in forming the human capital of children. A reason for strong correlations between child outcomes and family income or financial resources is that these often are good proxies for the quality of a child's early environment, such as the amount of parental time, the quality of parental time investments, and the quality of childcare services. It is further argued that the socioeconomic gap in human capital development, which emerges early and persists or grows through childhood, has counterparts in the quantity and quality of child related investments, such as language exposure and supportive and human capital enhancing parenting styles. We contribute to the understanding of parental investment by estimating the effects of exogenously increased opportunities to undisturbed one-on-one time with a parent during infancy, potentially improving both quality and quantity of child related investments.

We also add to the understanding of parental time investments per se. In particular, we estimate effects of parental time investments before childcare enrollment and on the importance of the adult-to-child ratio in home care. Parental investments have previously been explored by e.g., Fiorini and Keane (2014), Hsin and Felfe (2014), Del Bono et al. (2016), Liu and Nordstrom Skans (2010), and Fort et al. (2020). A takeaway from this literature is that parental time, in particular if spent in early childhood and with an educated parent, is beneficial for human capital development. However, most of the evidence relates to time investments beyond age one, moreover it mostly compares parental time to other forms of childcare, where, as stressed in Fort et al. (2020), the adult-to-child ratio is typically lower than when children are cared for at home. We extend this literature by providing evidence on the importance of one-on-one time, as opposed to shared time, with a parent during infancy, a period likely to be sensitive for the child's socioemotional development. We show that at this early age more undisturbed time for attachment and communication may be beneficial, especially for low SES boys, but potentially also for girls of highly educated mothers.

Furthermore, since differential parental time investments and time allocation during childhood are central in understanding birth order effects, we also contribute to this literature. The economic literature on birth order effects shows important differences in a wide range of relevant outcomes such as educational attainment and labor earnings (Black et al., 2005), personality traits and social ability (Black et al., 2018), as well as IQ (Barclay, 2015). There are several possible channels.[5] Siblings

[5]See Black et al. (2018) for an excellent review of the evidence.

may, for good and bad, influence each other as caregivers, teachers, and role models (Lei, 2019; Karbownik and Özek (2019). Another strand of the literature emphasizes competition between siblings (Joensen and Skyt Nielsen, 2018) and competition for family resources (Björkegren and Svaleryd, 2017; Black et al., 2020; Black et al., 2018; Lehmann et al., 2018). Although we do not model sibling differences, we relate to this literature since we capture the effects of increased access to childcare of the older sibling, which potentially creates a home environment more like that of a first born also for higher parity children, implying less sibling interactions and a possible reallocation of parental time from the older sibling to the younger child, both key drivers of birth order effects. We also explore the potential mechanism of sibling spillovers in educational achievement.

In Section 2, we present the background on childcare arrangements in Sweden and the exploited childcare reform. In Section 3, we present the empirical strategy and in Section 4, we present the data, define the reform and control municipalities, discuss sampling and measurement, and provide evidence on how the reform affected childcare enrollment. A graphical analysis is presented and threats to identification are discussed in Section 5. Results are presented in Section 6. Section 7 presents our conclusions.

2.2 Childcare arrangements and the reform

Most Swedish infants are cared for at home by their mother during the first year of life. Parental leave legislation was implemented in 1974, giving equal rights to paid, job protected, leave for both parents, and although fathers' share of parental leave has increased over time it was only 30 percent in 2019. During the period studied in this paper, Swedish parents were entitled to 15 months of parental leave to be used flexibly by either parent during the child's first eight years of life, although mothers took 80–90 percent of the leave days. Of these months, 12 months were paid at a wage replacement of 80–90 percent up to a cap, and three months were paid at a low flat base level. Since 1995, one of the wage-replaced months was not transferrable between parents, a so called "daddy month." In January 2002, another non-transferrable wage-replaced month was added, extending total paid leave to 16 months (see e.g., Duvander and

Johansson, 2012; Ekberg et al., 2013; Avdic and Karimi, 2018).

The use of parental leave is close to universal during the first year of the child's life. One reason is that municipalities are obliged to offer subsidized childcare within 4 months of application, but only from the child's first birthday. Children are typically enrolled in childcare during their second year of life. For children born in 1999, the mean enrollment age was 18 months (Duvander, 2006) and almost 80 percent of 1–5-year-olds were enrolled in formal childcare in 1999.

Between 2001 and 2003, the Swedish government implemented a comprehensive childcare reform. The purpose of the reform was to make childcare affordable and available to all children from their first birthday. Since 1995, the Swedish municipalities have been obliged to offer childcare to all children whose parents were working or studying. The reform expanded this obligation to cover all children, guaranteeing 15 hours/week to children of parents who were unemployed from July 2001 and on parental leave from 2002.[6] Moreover, the reform imposed a uniform fee schedule with a low cap in all municipalities in 2002 and granted free childcare for all 4–5-year-olds 525 hours/year staring in 2003. The various parts of the reform have been extensively studied. Effects of lower childcare costs are studied in Lundin et al., 2008 (Maternal labor supply), Mörk et al., 2013 (fertility), and Van den Berg and Siflinger, 2020 (child health). Effects of granting access to children of unemployed parents are studied in Vikman, 2010 (maternal job finding rates) and Aalto et al., 2019 (child health). Norén, 2015 (parental leave uptake) and Hallberg, 2019 (human capital effects on older sibling) study the same aspect of the reform as we do, i.e., access to childcare for older siblings.[7]

For children of parents on parental leave with a younger sibling, the

[6]Note that the children of unemployed parents on parental leave only gained access with the 2002 reform.

[7]Lundin et al. (2008) found no effects on female labor supply of reduced childcare fees, Mörk et al. (2013) found heterogeneous effects on fertility of the same reform, and Van den Berg and Siflinger (2020) found positive effects on child health as more children were enrolled in childcare. Vikman (2010) found a substantial effect on maternal job finding rates and Aalto et al. (2019) found limited overall effects on child health of childcare access for children of the unemployed, but they also found that medication for respiratory conditions in school age children was reduced. Studying access to childcare for children of parents on parental leave, Norén (2015) found no effects on parental leave division among parents. Hallberg (2019) found positive effects on ninth grade mathematics test scores of the older siblings who gained childcare access.

reform hence, implied that the municipalities were obliged to offer a childcare slot of at least 15 hours per week starting January 1, 2002. This part of the reform was motivated by the importance of maintaining a stable childcare environment for older siblings (Government Proposition 1999/2000:129). Moreover, childcare became cheaper.

The studied reform implied greater freedom for families to decide how to care for their toddler(s) (and preschoolers) during the parental leave period, both because there was now access and because fees were low. Before the reform, very few municipalities offered childcare for children whose parents were on parental leave. Exceptions were made in case of excess supply and for children with special needs, but for most children of childcare age this meant that they could no longer attend childcare when they got a sibling. Hence parents on parental leave needed to care both for older siblings and the newborn infant, making the adult-to-child ratio at most 1:2 rather than 1:1. The reform substantially increased the access to childcare for this group of families. This increase meant that the infant's exclusive one-on-one time with the parent on leave during the first year of life increased, that the older sibling could remain enrolled in childcare, and that the family maintained a contact with the childcare environment. Aggregate figures show that childcare enrollment rates of children ages 1–5 with parents on parental leave increased from 25 to 47 percent between 1999 and 2002, while the corresponding overall enrollment rates for all 1–5-year-olds were 77 percent in 1999 and 87 percent in 2002 (NAE, 2004). The effect of the reform on households on parental leave has previously been studied by Norén (2015), who found no effect on the parental division of parental leave, and in a master thesis by Hallberg (2019), who found a sizeable increase in ninth grade mathematics test scores for the sibling who gained access to childcare. To our knowledge, effects of the reform on the younger child possibly gaining more one-on-one time with a parent, have previously not been studied.

2.3 Empirical strategy

Our aim is to estimate the effects on child educational outcomes of granting families more flexibility in choosing childcare arrangements for their older children and allowing them more one-on-one time with their infant. We also aim to study the potential mechanisms–i.e., effects through child and maternal health, time allocation and family environment. To this end, we use a difference-in-differences (DD) framework, exploiting the exogenous variation in one-on-one time induced by the 2002 childcare reform, which mandated municipalities to grant childcare access to older siblings whose parents were on parental leave with an infant. Before the reform, some municipalities provided access to all children, but most municipalities prioritized access for children of working parents. Hence, the reform created more opportunities for one-on-one time with a parent for infants with older siblings of childcare age in affected municipalities, but it did not change the situation for children without siblings of childcare age living in these municipalities.

The way the reform affected families allows us to compare the outcomes of children born before and after the reform in January 2002 (first difference) who have and do not have an older sibling of childcare age (second difference) in reform municipalities. The same comparison in municipalities that were not affected by this access reform serves as a placebo experiment. This serves to verify that any detected differential outcomes of children with and without siblings of childcare age are not driven by trends unrelated to the increase in potential for one-on-one time induced by the childcare reform. We classify municipalities into treated reform municipalities and untreated control municipalities based on the variation in childcare access for older siblings before the reform as reflected in the pre-reform enrollment difference between parents working and on parental leave. That is, a small difference is assumed to reflect few restrictions in access for parents on parental leave and a large difference is assumed to imply restricted access and that the municipality was affected by the reform. We discuss the classification in Section 2.4.2. We estimate the following model:

$$Y_{imcd} = \alpha + \delta post_c \times sibling_i + \gamma sibling_i + \theta_{mc} + \lambda_d + X_i \beta' + \varepsilon_{imcd} \quad (2.1)$$

where Y_{imcd} is the outcome of child i born in municipality m of birth cohort c in calendar month d.[8] The variable $post_c$ is an indicator variable taking the value 1 for children born after the reform, from 2002, and 0 for

[8]The municipality of the mother's residence at the year of birth is a proxy for the municipality of the child.

pre-reform cohorts.[9] The variable $sibling_i$ is an indicator variable taking the value 1 if the child has an older sibling in childcare age (1–5) and 0 otherwise. The parameter of interest is δ which captures the interaction, comparing children with and without siblings of childcare age, born before and after the reform. Consequently, δ captures the intention-to-treat (ITT) effect on the child (or on the parent or family) of the opportunity to have more one-on-one time between infant and parent, induced by the reform granting access to childcare for the older sibling, of children born in the post reform period, in reform municipalities.

The coefficient γ for *sibling* captures any time-invariant difference in the outcome between children with and without older siblings. We include municipality-specific cohort fixed effects, θ_{mc}, to remove any potential remaining confounders at the municipality level common to children (with and without siblings) of the same cohorts, such as changing quality of childcare and education, local grade inflation, or general time trends due to national policy changes such as the introduction of the second paternity leave quota in 2002.

The model also includes birth month fixed effects, λ_d, which account for differences in outcomes, e.g., between children born early and late in the year. Finally, the model includes a set of predetermined family and parental controls, X_i, as listed in Table 2.3. In order to analyze heterogeneous effects of the opportunity to have more one-on-one time, we split the sample by maternal education and child gender. We also explore mechanisms by estimating the same model, but with child health, maternal- and family outcomes. For sibling spillovers in educational achievement, we include also cohort fixed effects of the older sibling.

The identifying assumptions of the model are first that there are common trends in outcomes of children with and without siblings in the reform municipalities. Second, we need treatment assignment to be exogenous. That is, predetermined municipal and individual characteristics should not predict treatment, i.e. being born in a reform municipality in the post period and having a sibling of childcare age. Because it is possible that changes in childcare access could affect fertility choices, we carefully examine the composition of families of children with and without siblings (Section 2.5).

[9] Pre-reform differences between reform and control municipalities are displayed in Table 2.2.

Alternative empirical strategies would be to estimate a difference-in-differences model comparing children with siblings of childcare age in reform and control municipalities before and after the reform or to estimate the full triple-differences model comparing children with and without childcare age siblings born in reform and control municipalities before and after the reform. Although different pre-reform trends between reform and control municipalities challenge these alternative strategies (See Figure 2.1), we provide the estimates in the Appendix.

2.4 Data and definitions

In this section, we present our data sources, discuss sampling, variable definitions, and measurements, define reform and control municipalities, and assess reform effects on childcare enrollment. Then we describe the data.

2.4.1 Sampling and data

We use linked administrative data from the Multi-Generation Register and from education, health, tax, and social insurance registries, which include all Swedish children and their families. These administrative records contain family links and demographics, such as age, sex, and birth order, annual records of parental leave uptake, parental education, earnings, income, and health, as well as child test scores and health outcomes. Administrative data are complemented with survey data from the National Education Agency on childcare enrollment from 1999 and 2002, the years surrounding the reform.

We restrict the analysis to children born in Sweden between 1999 and 2003. Infant children with older siblings of childcare age are defined as potentially treated by the childcare access reform, which was implemented in January 2002. Infants without siblings of childcare age were not affected by the reform and therefore serve as our control group. However, we exclude infants with an age difference to their older sibling of less than one year since they are at most partially treated because the older sibling was eligible for childcare only since the first birthday. We also remove children born in the spring of their older sibling's sixth year–i.e., the year when they leave childcare for school.[10] Therefore, the maximum age difference for treated children is 5 years and 11 months. Infants with a larger age gap to the older sibling,[11] or who do not have older siblings, are included as controls.

We link parents and children using the Multi-Generation Register compiled by Statistics Sweden. From this dataset, we also retrieve information about siblings, birth month and birth year of the child, mother's country of birth as well as the mother's age at first birth. From the Medical Birth Register we create an indicator of low birthweight. Children who are not present in the birth registry are removed from the analysis.[12] Children whose mother is not present in tax registries and hence not a Swedish resident in the child's birth year or when the child is in school (ages 6–12), are also removed from the sample.[13]

We use data from tax and education registries to capture parental characteristics in the year of the child's birth, such as education and municipality of residence, as well as income and earnings history in the years before birth. Because the period of interest coincides with a large-scale adult education program known as the Knowledge Lift (see for instance Albrecht et al., 2008), which had large impact on the upper-secondary school margin of the adult educational attainment distribution over a short time period, we construct and control for a measure of the parents' educational rank rather than for parental educational attainment directly.[14] For the heterogeneity analysis, low SES is captured by the educational level of the mother, where high education implies at least 14 years of education, well beyond the upper-secondary school margin.[15]

The interest of this paper is to estimate the impact of better oppor-

[10] Both restrictions regarding the sibling age difference removes 2.6 percent of the analysis sample.

[11] For this group, the minimum age difference is 5 years and 9 months.

[12] 3.3 percent of the initial sample.

[13] 0.2 percent and 3.2 percent of the analysis sample, respectively. In the analysis of preschool age outcomes, we require that the mother is present in the tax registries for the relevant years; 0.25 percent of children have mothers who are not.

[14] This measure is constructed using detailed information about parental education (3-digit Sun code) to predict children's sixth grade average test score. Quintiles of the prediction are included as factor variables to control for parental human capital in all regressions.

[15] This corresponds to 2–3 years of tertiary education/university depending on the length of elementary education.

tunities for one-on-one time with a parent during infancy,[16] on human capital development. We measure human capital development using test scores from national tests in the core subjects–Mathematics, Swedish, and English–from sixth grade, when children are 13 years of age. The data are available from the National Agency for Education.[17] Sixth grade test scores are good predictors of later test scores and GPA upon leaving compulsory school, which are in turn good predictors of long run educational attainment and labor market outcomes (Holmlund et al., 2019). We also construct indicators of grade for age and test participation to capture possible changes to the age composition or selection into the test score sample, triggered by the reform. We measure human capital spillovers from siblings using the sibling's ninth grade test scores.

To explore potential mechanisms through which the reform may have affected the human capital development of children, we estimate effects on measures of child health in preschool and school age. We also estimate effects on measures capturing changes in parental time use and quality of the family environment during the first three years of life. We do not have direct measures of parental time use and child-related time investments. Instead, we use parental leave uptake, a measure of child age at preschool enrollment and maternal earnings on the labor market to detect changes to time allocation during the child's first years of life. We also study the effects on the birth of a younger sibling, as a new sibling further affects the adult-to-child ratio at home. We capture changes in the quality of the home environment by an indicator of family separations and by a measure of maternal mental health.

Health outcomes are constructed based on the National Patient Register, which contains information on diagnoses for all inpatient care visits since 1987 and outpatient care visits since 2005.[18] In addition, we make

[16]Throughout the paper, we alternately refer to the treatment period as the first year. This is the youngest age at which children can be enrolled in childcare. However, actual duration of treatment corresponds to the length of parental leave, which is 18 months on average and differs, possibly endogenously, across households.

[17]The test scores are standardized within each school cohort. There are national tests also in grades 3 and 9. The latter are available only for cohorts born before the reform (1987–2000) and the latter are graded on a simple pass/fail scale detecting only the very weakest students, which leaves very little variation to be explored.

[18]The National Patient Register contains outpatient care visits from 2001, but the coverage is increasing over time and it has full coverage only since 2005. Therefore, we only use outpatient care visits since 2005. Outpatient care includes specialized health

use of the Prescribed Drug Register, which contains all drugs prescribed since 2006. Based on the inpatient care registry, we construct individual indicators of any hospitalization for children during preschool age and hospitalizations relating to mental health for mothers during the first three years after birth.[19] Based on inpatient and outpatient registries and medical drug prescriptions registry, we construct indicators of any health care use by the child in school age (age 7–13).[20] For the school age, we also construct two diagnosis-specific indicators.[21] The first captures care for infections/respiratory diseases common in childhood and plausibly affected by exposure to the childcare environment or to older siblings.[22] The second captures care and prescriptions for conditions relating to mental health–i.e., psychiatric, behavioral, and neuropsychiatric conditions such as depression, anxiety, and ADHD, which may relate to the quality of the child's attachment, socioemotional development influenced by the quality of the early home environment (NICHD-ECCRN, 2003; Moullin et al., 2018).

Because the reform gave parents more flexibility during parental leave and allowed the family to maintain contacts with childcare, the value to parents of being on parental leave may have been affected and therefore the timing of preschool enrollment and mothers' return to the labor market. Increased flexibility may also have reduced parental stress and conflict. Therefore, we estimate the effects on the child's age at preschool enrollment[23] and maternal labor earnings during the child's first years of

care by medical doctor but not primary care visits.

[19]Hospitalization related to conditions and complications at birth (perinatal) are excluded.

[20]Medical drug prescription data are available from 2005 and therefore can be measured when the studied cohorts are in school age.

[21]Diagnoses in inpatient and outpatient care are based on the main diagnosis and up to 20 auxiliary diagnoses. See Appendix Table A1 for a detailed description of the ICD10 and the ATC codes that correspond to the diagnosis groups used.

[22]According to the hygiene hypothesis, early exposure to microorganisms might have long-term effects influencing the incidence of, e.g., asthma and allergies in school age. See e.g., Scudellari, 2013; Ball et al., 2000; Ball et al., 2002.

[23]We follow Duvander and Viklund's (2017) approach. That is, we use detailed data on parental leave benefits from the database MiDAS, administered the by Social Insurance Office, as a proxy preschool enrollment. The date of enrollment is set to the date when no more than 2 days of benefits per week have been used for 6 consecutive weeks. When estimating these effects, we followed Duvander and Viklund (2017) and restrict the sample by excluding 1) children with a sibling within 18 months, and 2) children with parental leave benefits of less than 104 days for the first year as these

life. We capture family separations by an indicator that takes the value one if the biological parents reside in the same household at the age of three, and we construct an indicator for the birth of a younger sibling on the mother's side by the age of three. In Appendix I, we also explore within family allocation of parental leave uptake during infancy from the MiDas database as an additional indicator of parental time use and the mother's use of parental leave before birth as a measure of pre-birth health.

2.4.2 Defining reform and control municipalities and assessing reform effects on childcare enrollment

The extent that the childcare access reform affected childcare arrangements of families across municipalities depends on the bite of the restrictions imposed on families before the reform. This, in turn, depends both on the supply of and demand for childcare for older siblings.[24] We capture the bite of the reform using measures of actual enrollment before the reform. Unfortunately, nationwide administrative registries on childcare enrollment are not available for the period studied in this paper. However, the National Agency for Education (NAE) conducted childcare arrangement surveys in the fall of 1999, (i.e., before the reform) and in the fall of 2002 (i.e., just after the reform). These surveys were addressed to parents of children 1–12 years old and contained questions regarding the family's childcare arrangements for the first two weeks of September and parental employment status. Children aged 1–5 were drawn from a stratified sample representative at the municipality level. Parents of 141,000[25] preschool children were surveyed and the response rate was very high in both waves–92 percent in 1999 and 90.4 percent in 2002 (NAE, 2000, 2004).

The surveys allow us to rank municipalities according to the pre-reform enrollment difference between children whose parents work and children with a parent on parental leave with a younger sibling. Enrollment in the former group is assumed to capture the local demand (and supply) for childcare in the municipality. Municipalities with a large enrollment

are possibly difficult to assign the correct date.

[24]See Appendix Section 2.B for a discussion about formal restrictions as reported by the municipalities.

[25]69,000 children in 1999 and 72,000 children in 2002.

difference between these groups likely imposed stronger restrictions on childcare enrollment for families on parental leave than municipalities with a small enrollment difference.[26]

Table 2.1: The enrollment of children as reported in the parental surveys in 1999 and 2002 by municipality quintile of pre-reform difference between working parents and parents on parental leave.

Quintile	Pre-reform 1999	Post reform 2002	Enrollment increase 2002–1999
Either parent on parental leave			
5	0.10	0.58	0.48
1	0.63	0.83	0.20
Both parents working			
5	0.92	0.95	0.03
1	0.93	0.97	0.04

Source: NAE Parental Surveys 1999, 2002.

Table 2.1 shows the childcare enrollment rates of children aged 1–5 based on the parental surveys in 1999 and 2002, for municipalities in the top and bottom quintiles of the distribution of the pre-reform enrollment difference between these groups. Averages for children of parents on parental leave (top panel)[27] and working parents (bottom panel), weighted by the number of children in our sample, are reported. While there is very little variation in pre-reform enrollment for children of working parents, there is a substantial difference for children of parents on parental leave, findings that reflect differences in access. In the top quintile group (group 5), which we define as reform municipalities, the pre-reform average enrollment of children with parents on parental leave was only 10 percent, implying a gap of 82 percentage points to working parents and suggesting that access was very restrictive. Also in restrictive municipalities, children could get access either because the supply of slots was excessive or because they had special needs.[28] In the bottom quintile (group 1), defined as

[26] Appendix Figure C1 displays the distribution of the pre-reform difference in enrollment between these groups.

[27] This group consists of 6,773 observations in 1999 and 7,568 observations in 2002.

[28] If children with special needs are those whose sibling is likely to gain the most from more one-on-one time, we will underestimate any positive effects of one-on-one

control municipalities, 63 percent of the children of parents on parental leave were enrolled in 1999, which represents a gap to working parents of 30 percentage points, suggesting that childcare access was much less restricted. Our definition of reform municipalities as the top quintile of the pre-reform difference in enrollment between employed parents and parents on parental leave, thus implies that we compare children with and without childcare age siblings in municipalities where restrictions were most likely severe and where the reform consequently implied an exogenous change in access to childcare. The control municipalities, for which we perform a placebo analysis, are defined as the bottom quintile of the pre-reform difference in which we can be quite certain that there were only limited or no restrictions in childcare access.

In Appendix Figure C2, the pre-enrollment difference is plotted against the 1999–2002 *change* in the enrollment difference between children of working parents and children of parents on parental leave (i.e., the difference-in-differences in childcare enrollment or “first stage” reform effects at the municipal level), which demonstrates a clear positive correlation.

Using the top and bottom quintiles to define reform and control municipalities based on the enrollment rates presented in Table 2.1, the change in enrollment due to the reform is 45 percentage points in reform municipalities. Net of the enrollment increase in the control municipalities, which likely reflects the general trend, the change is 29 percentage points. The change is similar regardless of gender of the preschool child (Appendix Table D1). The first stage is estimated formally in a difference-in-differences model (Appendix equation 2.2), and results are presented in Appendix Table D3. The formal first stage estimate confirms the results in Table 2.1. The reform increased childcare enrollment in the reform municipalities by 44.2 percentage points. The corresponding estimate in the control municipalities was 12.3 percentage points. The net increase in enrollment due to the reform is thus some 32 percentage points. Heterogeneity by maternal education shows a stronger first stage for children of university educated mothers, but the difference compared to less than university educated mothers is not statistically

time. Therefore, 10 percent is the upper bound for the fraction of households with especially high gains from childcare access not captured in the estimations.

different than zero.[29]

Defining reform and control municipalities by the pre-difference in enrollment rates, we are unfortunately unable to differentiate restricted childcare access from low childcare demand specific to parents on parental leave as compared to working parents. Municipalities with large pre-reform demand differences will be classified as reform municipalities. They will contribute to attenuation of our estimated effects of increased one-on one time if the reform, due to lack of childcare demand, actually does not lead to a change in enrollment behavior.[30] Low demand due to high fees before the reform does not, however, pose a threat to our identification. To the extent that the studied reform, in combination with the nationally imposed fee cap from 2002, led to an increase in enrollment of children of parents on parental leave, this in fact also implies increased opportunities for one-on-one time between the parent and the infant child.

2.4.3 Data description

Table 2.2 presents pre-reform descriptive statistics for all Swedish municipalities (column 1) for the sample of studied control and reform municipalities (column 2) and for the control (column 3) and reform (column 4) municipalities separately.[31] Reform municipalities are somewhat disadvantaged compared to the whole country in terms of mean labor earnings and parental education level. Population size and density are smaller and so is the fraction of private childcare providers. A reason is that none of the largest cities are part of the reform group. Instead, the largest cities, Stockholm and Gothenburg, are in the control group of municipalities. The reform municipalities are representative of the whole country when comparing childcare quality as measured by the child-teacher ratio, cost per child, and the mean age at enrollment. Also, the unemployment and welfare dependency rates as well as health outcomes are similar in reform municipalities compared to the average municipality. The main outcome of interest—i.e., the standardized test score in grade

[29] See also averages by maternal education in Appendix Table D2. Estimating heterogeneity with respect to maternal education with an interaction term results in an insignificant coefficient for the interaction.

[30] In this case, we would have a weak first stage.

[31] Appendix Figure C3 shows the geographical distribution of the quintile groups, indicating that both reform and control municipalities are well dispersed across Sweden.

6—reveals that test scores based on individual data are somewhat below the country average in reform municipalities. In the empirical specification, we include municipality-specific cohort fixed effects accounting for both level and trend differences in outcomes between municipalities.

Table 2.2: Municipality, child, and family characteristics at municipal level before the reform

	(1) Total	(2) Sample	(3) Control	(4) Reform
Municipality characteristics				
Real labor earnings (SEK)	176,103	180,311	188,686	171,789
Real disposable income (SEK)	172,462	177,559	186,699	168,259
Unemployed, percent	17.3	17.0	16.1	18.0
Welfare recipients, percent	5.2	5.2	5.5	5.0
Compulsory educated, percent	33.8	33.1	30.6	35.6
University educated, percent	16.0	16.8	20.0	13.5
Mean age	40.7	40.4	39.6	41.2
Population size	30,630	36,381	54,539	17,585
Population density	122	200	338	57
Conservative votes, percent*	31.5	32.0	34.3	29.6
Cost of childcare per child (SEK)	83,243	83,011	81,637	84,386
Private childcare, percent	10.0	10.1	14.8	5.3
Child teacher ratio	5.4	5.4	5.5	5.4
Number of municipalities	290	116	59	57
Child and family characteristics				
Test score average, std, grade 6	0.04	0.10	0.14	-0.03
Inpatient care, child (preschool age)**	326	313	307	331
Any health care use, child (school age)**	931	936	938	931
Age at preschool enrollment (days)	547	547	547	545
Inpatient care mental, mother**	9.5	9.2	9.4	8.6
Parents separated	0.16	0.18	0.19	0.15
Younger sibling	0.20	0.20	0.21	0.19
Number of children	82,651	40,868	31,986	8,882

Notes: Measured in 2000, except for *measured in 1998. ** measured per 1000 individuals.

Table 2.3 shows the averages of *predetermined* background characteristics for children with and without an older sibling in reform municipalities for the pre- and post-reform (i.e., cohorts 1999–2001 and 2002–2003, respectively). First, there are very small differences in the characteristics of children with or without childcare age siblings, except that the latter group are of course more likely to be firstborns. This likely also drives some of the difference in birthweight (Björkegren and Svaleryd, 2017). It is also the case that children without siblings of childcare-age are slightly less likely to have university educated parents. To examine whether there are changes in the composition of children who have and do not have siblings of childcare age that could be driven by the reform, we report the DD estimates of the reform impact on each covariate (column 5) using the specification in equation 2.1 without the vector of controls. Overall, the sample appears to be well balanced, but there are some significant changes in the composition of parental education for children with siblings relative to the composition of children without childcare age siblings. The predicted educational rank is declining for control children (i.e., those without siblings) while the opposite holds for children with siblings of childcare age. Also, there is a larger decline in the fraction of mothers with low (compulsory) education among the treated children. Appendix Table E1 provides the corresponding table for the control municipalities. Control municipalities display a similar trend in the DD estimate in the sibling-no sibling difference in parental educational controls, but the changes in composition are somewhat more pronounced. Given the similarity in development, it is unlikely that this change in composition is due to the reform. Therefore, we control for predetermined characteristics in the empirical analysis.

Table 2.3: Summary statistics of predetermined characteristics and outcomes for pre- and post-reform cohorts born 1999–2003 in the reform municipalities

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	All	No sibling		Sibling		Difference	
		Pre	Post	Pre	Post	DD	P-value
Female	0.49	0.49	0.49	0.49	0.49	0.00	0.81
Multiple-birth	0.03	0.03	0.03	0.02	0.03	0.01	0.13
Low birth weight	0.04	0.05	0.05	0.03	0.03	0.00	0.54
First born	0.45	0.77	0.77	0.00	0.00	0.00	0.61
Second born	0.36	0.10	0.10	0.68	0.69	0.01	0.14
Third/higher parity	0.19	0.13	0.13	0.32	0.31	-0.01	0.08
Mother age at first birth	27.13	26.37	26.63	25.63	26.07	0.17	0.07
Mother foreign born	0.16	0.12	0.13	0.12	0.12	-0.01	0.13
Mom disp. income, mean rank	50.86	48.06	47.06	48.64	47.81	0.13	0.77
Dad disp. income, mean rank	50.76	47.57	46.62	53.47	52.65	0.24	0.69
Predicted education, mean rank	52.87	47.15	45.14	47.76	48.78	2.95	0.00
Mom compulsory education	0.15	0.17	0.16	0.17	0.14	-0.02	0.03
Mom university education	0.32	0.22	0.27	0.24	0.27	-0.01	0.12
Dad compulsory education	0.16	0.17	0.17	0.17	0.16	-0.00	0.69
Dad university education	0.24	0.14	0.17	0.15	0.18	0.00	0.54
Observations	416,029	14,115	10,126	11,930	8,210	44,381	44,381

Note: Results from separate estimations of full DD model including municipality by cohort fixed effects and birth month fixed effects.

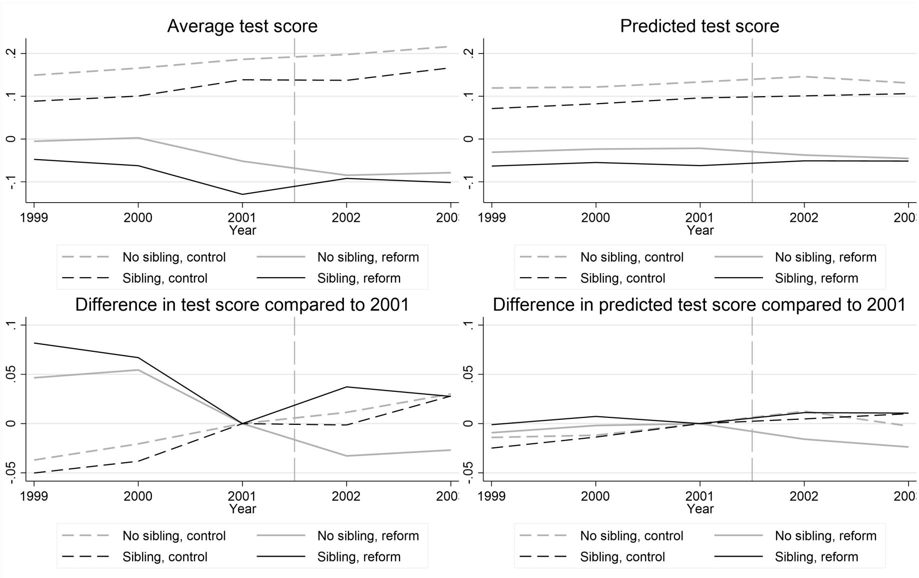
2.5 Graphical analysis and threats

Before turning to the formal analysis in Section 2.6, we present graphical event study evidence of how test scores evolve for the (treated) children who have siblings of childcare age compared to other children in reform and control municipalities. This comparison allows us to assess the validity of the parallel trends assumption underlying our identification strategy. We also investigate the balance of covariates pre- and post-reform by presenting event study graphs for predicted test scores from a regression model of the pre-reform relationship between test scores and covariates.[32] This analysis aims to detect if decisions regarding fertility, child spacing, or relocation patterns correlate with the reform such that a changing composition of families may confound any effects of the reform. In addition, we investigate whether children's grade for age (or school starting age) or participation in national testing might have been affected by the reform. If there are strong positive (negative) effects of the reform, children might be less (more) likely to be retained and more (less) likely to participate in national testing.

The event study graphs presented in the top panel of Figure 2.1 show the average and predicted test scores of children in birth cohorts between 1999 and 2003 in reform (full drawn line) and control (dashed line) municipalities for treated children (black) who at birth had a sibling of childcare age and children in the control group (gray) who did not have an older sibling of childcare age. The vertical line at 2001 indicates the last pre-reform data points. Because of the reform, siblings of children born since 2002 have access to childcare regardless of where the family lives.

[32]The predicted test scores are obtained from a regression model relating average test scores to child and parental controls for the years 1999–2001. See equations 2.3 and 2.4 in Appendix.

Figure 2.1: Average and predicted test scores at age 13 of children with and without siblings of childcare age in reform and control municipalities



Notes: The empirical specification is found in Appendix, equations 2.3 and 2.4.

The development of average test scores suggests that the parallel trends assumption holds satisfactorily for a comparison of children with and without childcare age siblings within reform municipalities or within control municipalities: children with and without siblings in reform and control municipalities respectively, follow roughly the same pre-reform time trends. However, pre-reform trends differ significantly between reform and control municipalities. While control municipalities show a steady increase, reform municipalities display a negative pre-trend. Hence, we chose as our main specification a difference-in-differences analysis within reform municipalities, comparing children with and without preschool age siblings born before and after the reform, and let the control municipalities serve as a placebo experiment. In the robustness section of the Appendix, we also provide DDD estimates, resting on the assumption that the relative outcomes (i.e., difference between children with and children without siblings) would have followed a parallel trend in the absence of the reform (Olden and Moen, 2020).

The right-hand side panel of Figure 2.1, which shows predicted test scores, also indicates that there is indeed a change in the covariate composition, as children without siblings in reform municipalities display a negative trend following the introduction of the reform. Therefore, it is important to include covariates that can account for this change in family composition in the model specification so as not to overstate the effects of the reform.

In the lower panel, we present average and predicted test scores net of the 2001 level, aligning all groups in the year just before the reform, revealing more clearly any impacts of the reform. On the left-hand side graph, it becomes more apparent that the pre-reform trends between children with (treated) and without (control) siblings within each group of municipalities are very similar. There is also a clear difference in the development of test scores between treated and untreated children in reform municipalities, induced by the reform, while the same does not apply to children in the control municipalities. In the graph on the right-hand side, however, it becomes obvious that this development is to some degree driven by changes in predetermined characteristics–i.e., by compositional changes correlated with the reform. In Appendix Figure G1, we present event study graphs for residualized test scores–i.e., where test scores are purged of compositional changes. These show that, conditional on predetermined characteristics, the pre-reform development of test scores is very similar for children with and without siblings of childcare age in both the reform and control municipalities, and that that development diverges after the reform in reform municipalities.

2.5.1 Test of covariate balance

Table 2.4 shows a formal test of covariate balance, as displayed in the right panel of Figure 2.1. This test of covariate balance was performed by examining whether changes in the composition of children's and parents' characteristics are related to treatment status by running our DD model on the predicted average test score. The first column contains the DD estimate of the reform effect on the average test score in sixth grade without any controls. This is 0.047 for reform municipalities, compared to 0.003 for control municipalities, where there was no change in childcare access. The second column shows the same model on the predicted test score. This model reveals a significant increase also in predicted test scores, suggesting that the reform is correlated with a changing difference in background characteristics between children with and without siblings of childcare age. There is a positive trend in the difference in background characteristics of children with childcare age siblings relative to other children also in control municipalities, but this trend is insignificantly different from zero and much smaller. In the third column we test whether our measure of parental education, based on a regression predicting test scores with detailed parental education indicators, is affected by the reform. The magnitude of the reform estimate is even larger than the estimate on our measure of predicted test scores, which includes all family background measures. This suggests that the imbalance in our included covariates is largely driven by differences in parental human capital.

Table 2.4: DD model of effects on average test scores, predicted test scores, and parental educational background

	(1) Average test score	(2) Predicted test score	(3) Parental educ. background
	Reform		
Sibling*post	0.047**	0.022***	0.030***
	(0.019)	(0.007)	(0.007)
Post	-0.024	-0.017***	-0.012*
	(0.024)	(0.006)	(0.006)
Sibling	-0.057***	-0.031***	0.013***
	(0.013)	(0.005)	(0.004)
Observations	43,566	43,566	43,566
	Placebo: Control municipalities		
Sibling*post	0.003	0.007*	0.017***
	(0.007)	(0.004)	(0.005)
Post	0.029***	-0.000	0.003
	(0.011)	(0.006)	(0.006)
Sibling	-0.047***	-0.036***	0.006
	(0.013)	(0.008)	(0.007)
Observations	157,483	157,483	157,483

Notes: Robust standard errors clustered at the municipality level in parenthesis. ***p<0.01, **p<0.05, *p<0.1. DD model includes municipality by cohort fixed effects.

2.5.2 Test taking

Because the main outcome of interest is the test score average in sixth grade, we need to be concerned with potential effects also on the extensive margin, which would require us to take selection into account when interpreting our main results. That is, the possibility that children's school starting age, grade for age, or test participation was affected by the reform if the reform has strong positive or negative effects on human capital development. The results when estimating the effect of the reform on test participation and on test taking age are presented in Table 2.5. As the results show precisely estimated zero effects, there is no problematic selection into the test taking sample nor is there an effect on the age at which the test was taken, indicating that school starting age, or the probability of repeating or skipping a grade, are unaffected.

Table 2.5: Test taking and school starting age

	(1) Test participation	(2) Age at test
	Reform	
Sibling*post	0.001	-0.002
	(0.001)	(0.003)
Observations	43,819	43,566
	Placebo	
Sibling*post	0.001	-0.002*
	(0.001)	(0.001)
Observations	158,499	157,483

Note: Robust standard errors are clustered at the municipality level in parentheses, ***p<0.01, **p<0.05, *p<0.1. Results from separate estimations of full DD model include sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

To conclude this section dealing with various threats to the identification strategy, we find there is reasonable support for the parallel trends assumption of the DD strategy. There are, however, some concerns about selection into treatment. The observed imbalance in covariates is largely driven by parental education, which may be due to the coinciding expansion of adult education. Given the heterogeneous fertility response to the fee reduction with respect to household income found in Mörk et al. (2013), it is also possible that the improved childcare availability and lower childcare costs in the reform municipalities influenced the fertility decisions of families differentially depending on their education level. In our estimations, we control for the imbalance in terms of parental background through the inclusion of family background characteristics, particularly parental educational rank. Reassuringly, there is no evidence that this imbalance is reflected in—or that the reform directly affected—the likelihood of participating in national tests, school starting age, or grade repetition.

2.6 Results

We investigate effects on child human capital, as measured by sixth grade test scores, of a reform that increased the opportunities for one-on-one time between parents and infants by granting childcare access for older siblings during the first year of a younger sibling's life. We also investigate differential effects over the test score distribution, by gender and maternal education. This analysis is motivated by, for example, Francesconi et al. (2016), Bertrand and Pan (2013), and Autor et al. (2019). We study several potential mechanisms–i.e., the effects on child- and maternal health, and family environment. As we are particularly interested in the extent that the reform can be tied to changes in quality and quantity of parental time investments, we investigate reform effects on maternal mental health, parental separation, and the probability of getting a younger sibling as well as the age at preschool enrollment and maternal labor earnings during the first years of life.

2.6.1 Human capital and more one-on-one time with a parent: main results

Table 2.6 shows the effect of increased opportunities for one-on-one time on the child's standardized test score average for different specifications when we estimate the DD model presented in equation 2.1. In the model, we compare children with and without siblings of childcare age before and after the reform in reform municipalities–i.e., municipalities with a large pre-enrollment difference in childcare enrollment between working parents and parents on parental leave. Column 1 shows the estimate with municipality fixed effects and cohort fixed effects without controls. In the second column, the municipality fixed effects are replaced with municipality-specific cohort fixed effects. The estimate is largely unchanged, and it corresponds to the first column of Table 2.4. The estimate of 0.048 standard deviations (SD) is sizeable and remains unchanged when child characteristics are controlled for. When the educational rank of parents is included, the estimate is reduced to 0.28 and is no longer significantly different from zero. Inclusion of additional parental controls affects the estimate only marginally. This estimate is about half of the test score gap between children with and without siblings of childcare age, which was 0.057 SD (*sibling* in Table 2.4). To draw any firm conclusions about the effect of increased access, we would need more precise estimates. Unfortunately, we gain no precision from inclusion of controls. When estimating the model for the control municipalities presented in the lower panel, point estimates are reassuringly close to zero.

Table 2.6: Main results: Effects of better opportunities for one-on-one time on average test scores

	(1) Test score average std	(2) Test score average std	(3) Test score average std	(4) Test score average std	(5) Test score average std
			Reform		
Sibling*post	0.047**	0.048**	0.053***	0.028	0.029
	(0.019)	(0.019)	(0.020)	(0.019)	(0.019)
Observations	43,566	43,566	43,566	43,566	43,566
			Placebo		
Sibling*post	0.003	0.007	0.008	-0.003	-0.003
	(0.007)	(0.007)	(0.006)	(0.008)	(0.007)
Observations	157,483	157,483	157,483	157,483	157,483
Municipal fe	Yes	No	No	No	No
Year fe	Yes	No	No	No	No
Municipal*Year fe	No	Yes	Yes	Yes	Yes
Child controls	No	No	Yes	Yes	Yes
Education controls	No	No	No	Yes	Yes
Parent controls	No	No	No	No	Yes

Notes: Robust standard errors are clustered at the municipality level in parenthesis, *** p<0.01, **p<0.05, *p<0.1. Results are from separate estimations of DD model. The child and parental controls are listed in Table 2.3.

Motivated by previous research which suggest gender differences in sensitivity to childhood circumstances (see, e.g., Bertrand and Pan, 2013; Autor et al., 2019) and differential effects of parental investments (Ginja et al., 2020) and quality of attachment depending on parental human capital (Moullin et al., 2018), Table 2.7 provides results of split sample regressions for boys and girls and by maternal education. We have verified that the covariate imbalance detected in Section 2.5 is not aggravated by splitting samples[33] and Figure G2 and Figure G3 in Appendix shows the parallel trends for the split samples. For boys overall and sons of mothers with low education in particular, the pre-trends are satisfactorily parallel. For girls, however, there is a deviating negative trend for girls with siblings before the reform. This graphical analysis indicates that girls with and without older siblings are not entirely comparable, so the results for this group should be interpreted with caution and are merely suggestive, although the difference in pre-trends would suggest even lager effects.

The first panel shows that the test scores of boys whose older sibling gained childcare access improved by 0.043 SD. The estimated effect for girls is also positive but not significantly different from zero. The second and third panel show results by maternal education. The improvement in boys' test scores is present only for sons of low educated (less than college) mothers, who gain 0.063 SD,[34] and there is a sizable positive effect of 0.086 SD for daughters of university educated mothers. These estimates are robust to inclusion of municipality-specific sibling fixed effects. Again, the corresponding estimates for the control municipalities, displayed in Appendix Table H1, shows small and insignificant estimates for the studied subgroups.

[33] In absolute terms, the covariate imbalance is larger for low educated mothers but consistently smaller in relative terms for significant values. Results are available from the authors.

[34] Using three educational groups to separate mothers with low education, we see that this effect is in fact driven by mothers with less than 12 years of education rather than by high school educated mothers. Since the knowledge-lift affected this margin, moving people from the lowest to the middle education group, this suggests that the positive estimate is not driven by spillovers from Knowledge Lift.

Table 2.7: Main results: Effects of better opportunities for one-on-one time on average test scores by gender and maternal education

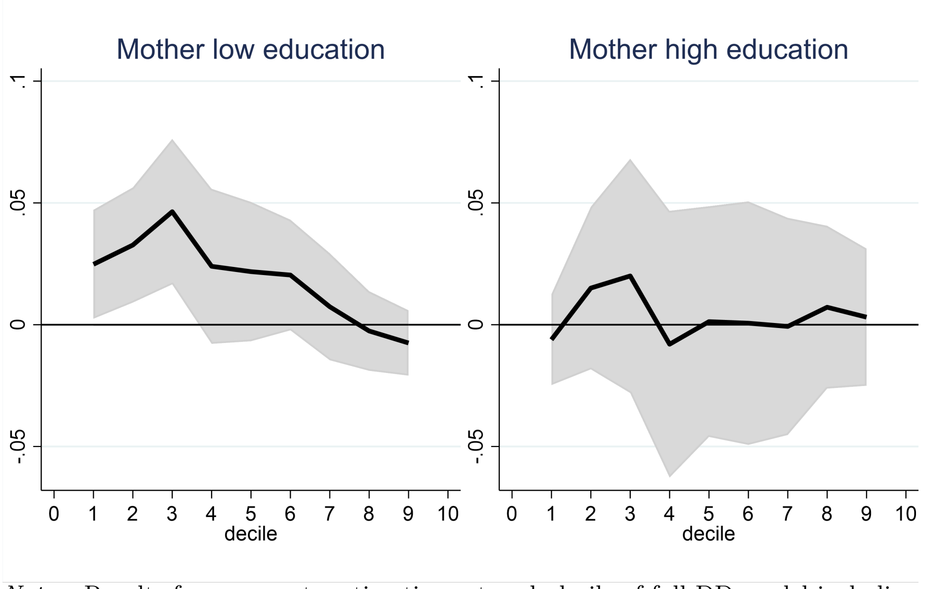
	(1) All	(2) boys	(3) girls
		All	
One-on-one time	0.029	0.043**	0.017
	(0.019)	(0.021)	(0.025)
Observations	43,566	22,145	21,421
Control mean	-0.0790	-0.199	0.0467
		Mother low education	
One-on-one time	0.034	0.063**	0.007
	(0.024)	(0.028)	(0.034)
Observations	32,173	16,400	15,773
Control mean	-0.215	-0.337	-0.0843
		Mother high education	
One-on-one time	0.041	0.003	0.086**
	(0.029)	(0.040)	(0.041)
Observations	10,874	5,498	5,376
Control mean	0.364	0.256	0.475

Notes: Robust standard errors are clustered at the municipality level in parentheses, ***p<0.01, **p<0.05, *p<0.1. Results are from separate estimations of full DD model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Before we explore possible mechanisms for the positive effects on sixth grade test scores, Figure 2.2 and Figure 2.3 present the results when estimating our DD model on an indicator for having a test score above a particular decile of the test score distribution for boys and girls separately. The figures confirm the positive reform effects on sons of less than college educated mothers and on daughters of university educated mothers. Moreover, the figures show that boys' test scores improve in the bottom of the distribution and that the positively affected girls are found in the middle of the distribution.[35]

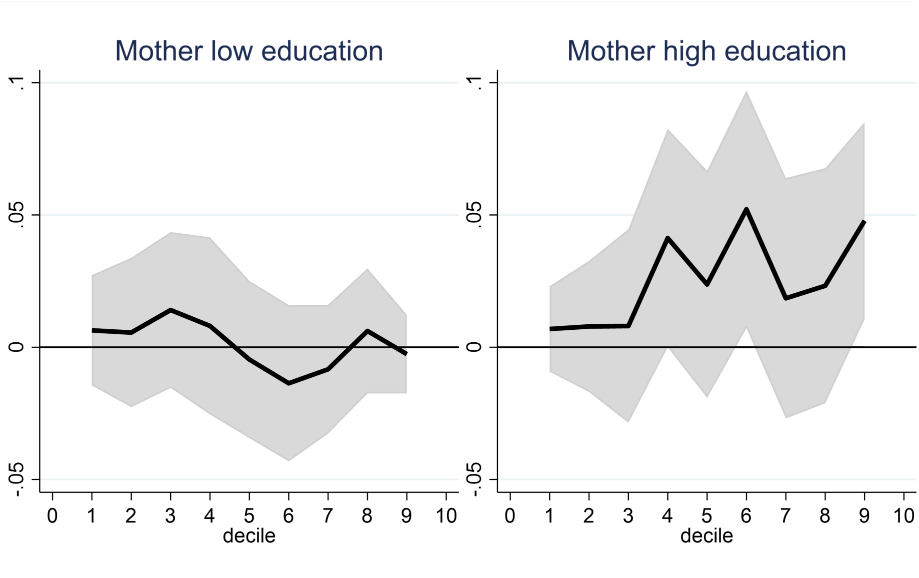
[35] There is no corresponding pattern in the control municipalities, see Appendix Figures H1 and H2.

Figure 2.2: Effects of better opportunities for one-on-one time over the test score distribution, boys



Notes: Results from separate estimations at each decile of full DD model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Grey area shows 95-percent confidence interval, with standard errors clustered at the municipality level.

Figure 2.3: Effects of better opportunities for one-on-one time over the test score distribution, girls.



Notes: Results from separate estimations at each decile of full DD model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Grey area shows 95-percent confidence interval, with standard errors clustered at the municipality level.

Robustness of main results

Table 2.7 shows no average effects of the reform, although there is evidence of heterogeneous effects by gender and maternal education. We test the robustness of these results in several ways. First, we present the difference-in-differences estimates for each quintile of the pre-reform distribution of the enrollment gap between working parents and parents on parental leave. The results are presented in Table G1-Table G3. The results show that the reform impact is only present in the municipalities that were most restrictive before the reform.

Second, as previously shown, a placebo analysis in control municipalities supports the interpretation that the found test score improvements are indeed effects of increased childcare access for older siblings. Third, in the analysis, we defined treatment status based on the birth year and therefore included children born in 2001 in the group of untreated children. However, depending on the birth month, their first year also covers 2002–i.e., when the older sibling gained access to childcare. Therefore, they are partially treated if parents used the opportunity to (re)enroll the older sibling. We present i) an analysis excluding the 2001 cohort entirely and ii) an analysis where the 2001 cohort is included, but treatment is defined as the share of the first year of life that the older sibling had access to childcare.[36] The results and the corresponding placebo analyses show patterns very similar to the main results and are presented in Appendix Table G4 and Table G5.

Fourth, because first born children typically perform better in school compared to higher parity children, they may be a poor control (Black et al., 2005). Therefore, we test whether our results are robust to excluding firstborns from the sample of control children, restricting the control group to children who do have older siblings but whose siblings are already of school age. This reduces the number of observations in the control group significantly, as is clear from the results presented in Table G6 and Table G7. The estimated positive effects of the reform on boys and in particular on sons of low educated mothers are larger in magnitude. Also, the estimate of girls of high educated mothers is higher for the restricted sample, but standard errors are large.

Fifth, we estimate an alternative difference-in-differences (equation 2.5 in Appendix) model and we also estimate a triple difference model (equation 2.6 in Appendix). In the alternative DD model presented in Table G8, treated children, defined as those having an older sibling in childcare age, are compared between reform and control municipalities pre and post reform. Note, however, that Figure 2.1 showed that the parallel trends assumption before the reform was far from satisfied when comparing reform and control municipalities. The specification suggests a larger overall effect of the reform, but the heterogeneity analysis does not support our findings for boys. The placebo analysis, presented in Appendix Table G9, did not perform well and there is reason to suspect that part of the large estimate on overall effect is driven by pre-reform trend differences. The triple-difference model presented in Appendix Table G10 effectively removes confounding effects, but the many dimensions make the estimate less transparent. The results are, however, very similar

[36] We assign 0 to children born January 2001 and 11/12 for children born in December 2001.

to the results of the main analysis.

2.6.2 Mechanisms

There are several pathways through which the increased access to childcare of older siblings may affect children's school performance. We first investigate effects on health, which may have been affected through contact with the older sibling's childcare environment, but also if parents had more undivided time for the younger child. We also investigate effects on outcomes relating to parental time allocation to assess whether there is any evidence that parents spent more or less time caring for the child during the first year and effects on the quality of the home environment, particularly effects on maternal mental health and family separations.

Effects on child health

Childcare access for older siblings implies that infants could have more undivided parental attention and possibly better conditions for attachment and socioemotional development. The reform, however, also implied that infants were more exposed to viruses and infections in the older sibling's childcare environment during the first year of life. The first column of Table 2.8 presents estimates of the risk of being hospitalized at some point during the preschool years (i.e., 0–5 years of age), excluding conditions related to complications at birth. The top panel shows the results for all children, and the bottom two panels present the corresponding results for boys and girls respectively. The point estimates for preschool health show small increases in the number of children ever hospitalized: at most 4.5 percent compared to the mean for boys, but estimates are not significantly different from zero. We conclude that increased childcare access for older siblings had no effect on early childhood hospitalizations of the younger sibling.[37]

Columns 2–4 explore effects on health outcomes in early school age (ages 7–13). We have constructed indicator measures of utilization of care based on presence in registries of in- and outpatient care and prescription drugs, overall and due to specific conditions relating to (i) mental health and (ii) respiratory conditions and infections. Overall, estimates are

[37]We have explored hospitalizations due to cause specific diagnoses, also yielding close to zero effects.

negative, indicating reductions in care use. For all children, there is a weakly significant reduction in care for respiratory conditions and infections, although the effect size is small relative to the control mean. For boys there is a reduction in care relating to mental health, by 11 children per 1,000 or 10 percent relative to the pre-reform mean. The estimate is, however, significant only at the 10 percent level, and the estimate is no longer significant at conventional levels when corrected for multiple hypothesis testing. Nonetheless, the consistently negative estimates suggest that improved health during school age cannot be discarded as a possible mechanism through which school results were affected.

Table 2.8: Effects of better opportunities for one-on-one time on health in preschool and primary school age

	(1)	(2)	(3)	(4)
	Preschool	School		
	Inpatient Any	Any	Mental Any	Infec/Resp Any
		All children		
One-on-one time	10.447	-1.763	-9.302	-13.432*
	(9.688)	(4.583)	(5.810)	(6.749)
Observations	43,743	43,819	43,819	43,819
Pre-reform mean	313.2	914.0	93.09	729.4
		Boys		
One-on-one time	14.442	-4.867	-10.960*	-12.133
	(9.764)	(5.465)	(6.183)	(8.660)
Observations	32,367	32,399	32,399	32,399
Pre-reform mean	320.4	915.6	100.6	729.2
		Girls		
One-on-one time	0.906	7.748	-9.102	-3.355
	(20.503)	(9.227)	(11.688)	(16.659)
Observations	10,857	10,898	10,898	10,898
Pre-reform mean	287.0	909.2	70.18	730.1

Notes: Robust standard errors are clustered at the municipality level in parentheses, ***p<0.01, **p<0.05, *p<0.1. Results are from separate estimations of full DD model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Health outcomes are measured per 1,000 individuals.

Table 2.9 presents the results by maternal education. There is a significant increase in inpatient care in preschool age for sons of low educated mothers. A similar positive effect can be detected for girls of highly educated mothers, but the estimate is imprecisely estimated. Beyond this, we again see the tendency for overall improvements in health in school age. In particular, there are reductions in care use related to mental health and behavioral problems for boys. Only the estimate for sons of low educated mothers is significant, albeit weakly.

The results so far show that the increased opportunity for one-on-one parental time during infancy did not have strong effects on child heath. There is a tendency for improved health overall in school age. However, for the groups where test scores were affected (i.e., boys with low educated mothers and girls with high educated mothers), evidence suggests worse preschool health. For school age boys, there also appears to be an improvement in mental health, which could have contributed to the improved school results for this group. The placebo estimates, presented in Appendix Tables H2 and H3, generally show smaller and insignificant estimates.

Table 2.9: Effects of better opportunities for one-on-one time on health in preschool and primary school age, by maternal education

	(1)	(2)	(3)	(4)
	Preschool	School		
	Inpatient Any	Any	Mental Any	Infec/Resp Any
		Boys		
		Mother low education		
One-on-one time	37.538**	-3.920	-15.350*	-10.753
	(16.451)	(8.762)	(8.417)	(13.464)
Observations	16,543	16,558	16,558	16,558
Pre-reform mean	346.9	920.6	127.5	725.8
		Mother high education		
One-on-one time	-2.432	4.145	-16.445	-22.229
	(25.454)	(15.111)	(19.168)	(25.466)
Observations	5,502	5,521	5,521	5,521
Pre-reform mean	320.0	918.1	92.22	729.5
		Girls		
		Mother low education		
One-on-one time	-8.881	-6.906	-8.851	-15.917
	(12.071)	(9.305)	(7.632)	(10.576)
Observations	15,824	15,841	15,841	15,841
Pre-reform mean	291.9	910.3	71.81	732.7
		Mother high education		
One-on-one time	14.915	6.888	-3.017	26.175
	(31.520)	(13.434)	(10.902)	(24.813)
Observations	5,355	5,377	5,377	5,377
Pre-reform mean	252.9	900.1	47.48	730.7

Notes: Robust standard errors are clustered at the municipality level in parentheses, ***p<0.01, **p<0.05, *p<0.1. Results are from separate estimations of full DD model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Health outcomes are measured per 1000 individuals.

Family and childhood environment

We further explore possible mechanisms for the findings on test scores for boys with low educated mothers and girls with highly educated mothers by studying reform effects on the family environment during the first three years of life. We are interested in the extent that the reform affected the quality and quantity of parental time investments during early childhood. To capture the effects on quality of investments, we investigate effects on maternal stress and mental health as measured by hospital care for psychiatric diagnosis and parental separations during the child's first three years of life. Needless to say, these measures would capture rather severe shocks to the family environment. To capture effects on parental time allocation, we explore if the reform affected the propensity to have another child which would likely introduce competition for parental time, age at which the child was enrolled in childcare, and maternal labor earnings. Results are presented in Table 2.10.

Table 2.10: Effects of better opportunities for one-on-one time on family and childhood environment

	(1) Mother mental health	(2) Parents separated	(3) Younger sibling	(4) Mother earnings	(5) Age at preschool enrollment
			Boys		
			Mother low education		
One-on-one time	0.528	-0.002	0.001	-0.004	-4.655
	(3.598)	(0.015)	(0.012)	(0.037)	(4.056)
Observations	16,543	16,543	16,543	15,178	14,463
Pre-reform mean	10.47	0.135	0.0900	12.03	537.2
			Mother high education		
One-on-one time	1.845	0.005	-0.012	0.019	5.343
	(3.254)	(0.013)	(0.025)	(0.055)	(8.107)
Observations	5,502	5,502	5,502	5,338	4,688
Pre-reform mean	4.838	0.0401	0.0822	12.54	575.9
			Girls		
			Mother low education		
One-on-one time	-1.091	0.004	-0.009	0.041	-7.890
	(4.364)	(0.013)	(0.012)	(0.039)	(4.730)
Observations	15,824	15,823	15,824	14,496	13,739
Pre-reform mean	8.957	0.125	0.0896	12.04	533.8
			Mother high education		
One-on-one time	1.095	-0.001	-0.038*	0.041	0.811
	(6.301)	(0.012)	(0.020)	(0.035)	(7.894)
Observations	5,355	5,354	5,355	5,190	4,573
Pre-reform mean	5.714	0.0464	0.0657	12.59	577.7

Notes: Robust standard errors are clustered at the municipality level in parentheses, ***p<0.01, **p<0.05, *p<0.1. Results are from separate estimations of full DD model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Maternal mental health is measured per 1000 individuals.

Overall, we see no signs of drastic changes in the quality of the home environment as measured by maternal mental health and family separations during the first three years following birth.[38] In addition, there is no evidence that maternal return to the labor market or age at childcare enrollment was affected by the reform. Hence, quantity of time with parents during infancy does not seem to have changed. Column 3 in Table 2.10 indicates that children of highly educated mothers were less likely to have a younger sibling within three years when the older sibling gained access to childcare. The estimate is not significantly different from zero for boys, but for girls the estimate is large, 50 percent relative to the mean, and significantly different from zero at the 10 percent level. The corresponding placebo estimates are presented in Appendix Table H4 do not show a similar decrease in fertility for this group. Although highly suggestive, it is possible that further reduced competition for parental time during early childhood, because of reduced fertility or increased child spacing, is a mechanism for improved test scores for daughters of highly educated mothers.

We have also explored the possibility that the improvement in test scores is driven by changes in the division of parental leave between parents, measured by the first-year allocation of parental leave benefits, as suggested in e.g. Cools et al., 2015. We test directly whether parental leave uptake was affected, both in terms of division and intensity, and in line with Norén (2015), we find no evidence of an effect on parental leave uptake (see Appendix Tables I1 and I2). In addition, we did not find any indication of changed pre-birth health as parental leave use before giving birth remained unaffected. Another possible mechanism is that the increased access to childcare affected the human capital of the older sibling, and that this spills over to the younger sibling (as found in e.g., Karbownik and Özek (2019) and Lei (2019)). Evaluating the effect on the older sibling using a sample restricted to sibling-pairs where the older sibling is in childcare (treated) or in school (control), we find overall negative effects on the performance of older siblings (see Appendix Tables I3 and I4). We find it unlikely that these negative effects would drive the positive effects we find. Our findings thus contradict Hallberg (2020) who found a positive reform effect from gaining access to childcare on the

[38] Maternal mental health is unaffected also when evaluating each of the 3 years separately.

ninth grade test scores in mathematics for older siblings.[39]

2.7 Conclusions

This paper studies human capital effects of better opportunities for one-on-one time with a parent during infancy. To this end, we exploit a reform that mandated municipalities to grant childcare access to the older siblings of infants while parents were on parental leave. A first stage analysis using survey data establishes that the reform increased childcare enrollment of older siblings by about 30 percentage points. We identify causal effects on human capital formation using a DD approach, which compares sixth grade test scores in core subjects of infants with and without a sibling of childcare age in municipalities that were affected by the reform. Although we find no significant average effect on test scores of increased opportunities for one-on-one time, analysis by child sex shows that the test scores of boys whose older sibling gained childcare access improved by 0.043 SD. Splitting the sample by maternal education shows that the improvement in boys' test scores is driven entirely by sons of less than university educated mothers, who gain 0.063 SD. There is no average effect on girls, but we find a positive effect of 0.086 SD for daughters of university educated mothers. When we analyze the effects along the test score distribution, we find improved test scores for boys in the lower end of the distribution, while the gains for girls come in the third quartile of the test score distribution. Examination of pre-reform trends, accounting for detected imbalances in predetermined characteristics, and a placebo analysis using municipalities that were unaffected by the reform support a causal interpretation of the results for boys. For girls, however, the pre-trends are somewhat deviating, so these results are more suggestive.

Although we are unable to estimate the first stage by gender of the infant child, there is no statistical difference in the estimated first stage with respect to maternal education. Similar reform response suggests that the heterogeneous effects captured are more likely to reflect differences in gains from the improved opportunities for one-on-one time created by increased enrollment of the older sibling, rather than differences in changes in enrollment *per se*. We explore a number of mechanisms

[39]We find a negative effect also for mathematics. Contradicting result may be explained by differences in both samples and empirical specification.

through which the increased opportunities for more one-on-one time may have affected the human capital development of children. Although we find little support for overall effects on child health, it is possible that less behavioral and psychiatric problems in school age contribute to better school performance of sons of less than university educated mothers. Improved mental health could be a result of more one-on-one time, leading to more secure attachment and better socioemotional development. In line with Bertrand and Pan (2013) and Moullin et al. (2018), our results suggest that boys in low SES families are particularly sensitive to these parental inputs. Furthermore, because less educated mothers are often found to experience more parental stress (Parkes et al., 2015), it is possible that reducing their care burden from two children to one may sufficiently raise the quality of parent-child interactions in these families.

Reduced competition for parental time and improved quality of parent-child interaction allowed by increasing the adult-to-child ratio from 1:2 to 1:1, may also be the mechanism behind the tentative findings of improved test scores of daughters of college educated mothers. This is consistent with Fort et al. (2020), who argue that girls are more likely to benefit from the cognitive stimulus of this interaction than boys, especially in high SES families. We also find some suggestive evidence that a reduced likelihood of having an additional child within three years may have further contributed to reducing competition for parental time for this particular group.

We find no evidence that the improved opportunities for one-on-one time had drastic effects on the quality of the early childhood environment as measured by mothers' mental health hospitalizations and family separations. In addition, we did not find any evidence that a mother's return to work or the child's age at childcare enrollment were affected by the reform such that the quantity of time spent with a parent would have changed. Hence it is likely that the effects we find stem from more subtle improvements in the quality of parent-child interactions resulting from the reduction in competition for parental time, afforded by the improved childcare access of the older sibling. These results have implications for the literature on sibling differences, suggesting that competition for parental time in early childhood is an important mechanism.

The explicit aim of the studied childcare access reform was to ensure a stable environment for older siblings at a time when their home environment changed due to the birth of a new sibling (Proposition 1999/2000:129). In this paper, we establish positive spillovers on some infant siblings who gained increased opportunities for one-on-one time with their parent on leave, pointing to the importance of one-on-one adult-child interaction for child development. Positive effects on test scores of boys at the lower end of the test score distribution are of particular interest. This evidence points to a potential for family policy to strengthen the home environment in disadvantaged families. Flexibility in choosing childcare arrangements, allowing for more one-on-one time during infancy, has the potential to improve child development and reduce inequalities in educational outcomes among boys.

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2.A Definitions of diagnoses

Table A1: Effects of better opportunities for one-on-one time on family and childhood environment

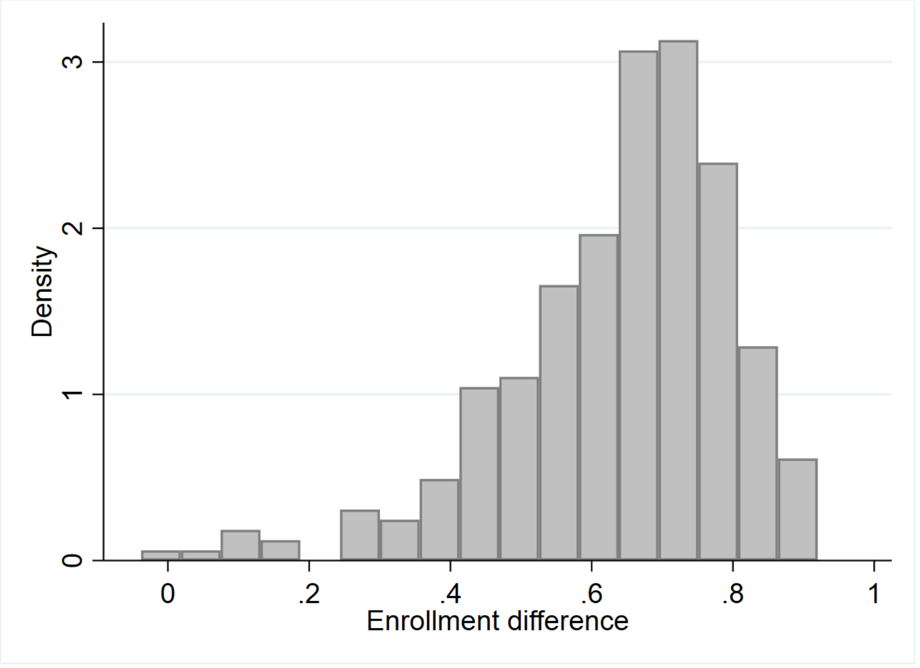
Variable	Definition	Example
Hospitalizations and outpatient care		
Hospitalization	=1 if admitted to hospital for any reason	
Infection	=1 if admitted to hospital with the ICD10 diagnosis code A00-A99, B10-B84, B90-B99, B00-B09, B85-B89	Infectious diarrhea, mononucleosis, chicken pox.
Respiratory	=1 if admitted to hospital with the ICD10 diagnosis code J07-J08; J19-J39; J48-J99	Upper and lower respiratory infections.
Mental/psychiatric	=1 if admitted to hospital with diagnosis codes F00–F99	Insomnia, behavioral disorder, anxiety, depression.
Medical drug prescriptions		
Infection	=1 if prescribed a medication with ATC code J01	Ear infection, urinary infection.
Respiratory	=1 if prescribed a medication with ATC codes R01-R06	Asthma-related, cough.
Mental/psychiatric	=1 if prescribed a medication with ATC codes N06B, N06A, N05	ADHD, depression, insomnia

2.B Alternative categorization of reform and control municipalities

An alternative to using pre-reform enrollment to categorize municipalities into reform and control municipalities would be to use information on stated formal local restrictions. This information is provided by the NAE and based on municipality survey responses from two waves; 1998 and 2001. We have explored the possibility to use this, but it turns out that stated policy is poorly aligned with evidence from actual arrangements: Most municipalities report that they had restricted access to childcare for children with parents on parental leave prior to the reform. Yet the NAE municipality survey conducted in 1998 suggests that formal restrictions did not bind if there was an excess supply of slots, or for children with special needs. Additional information obtained via e-mail and telephone interviews confirms that provision of slots in many municipalities was more generous than stipulated by the formal local policy. In addition, other municipalities, while having no formal restrictions, may have imposed pricing policies that effectively restricted access. Hence, basing the categorization of municipalities into reform and control municipalities based on pre-reform differences is more likely to capture actual limitations in access.

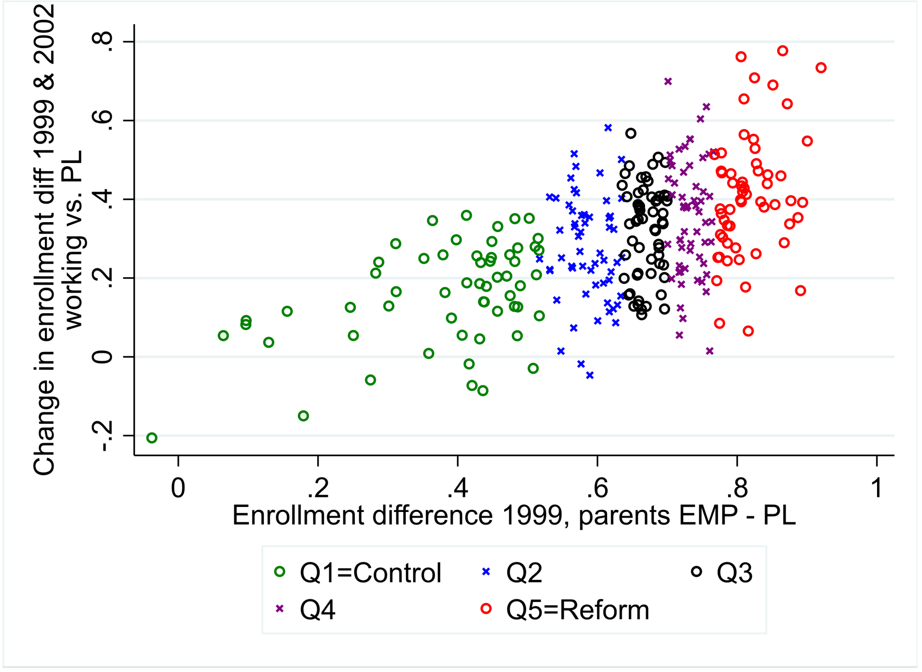
2.C Variation in pre reform childcare enrollment

Figure C1: Distribution of pre-diff, municipality level



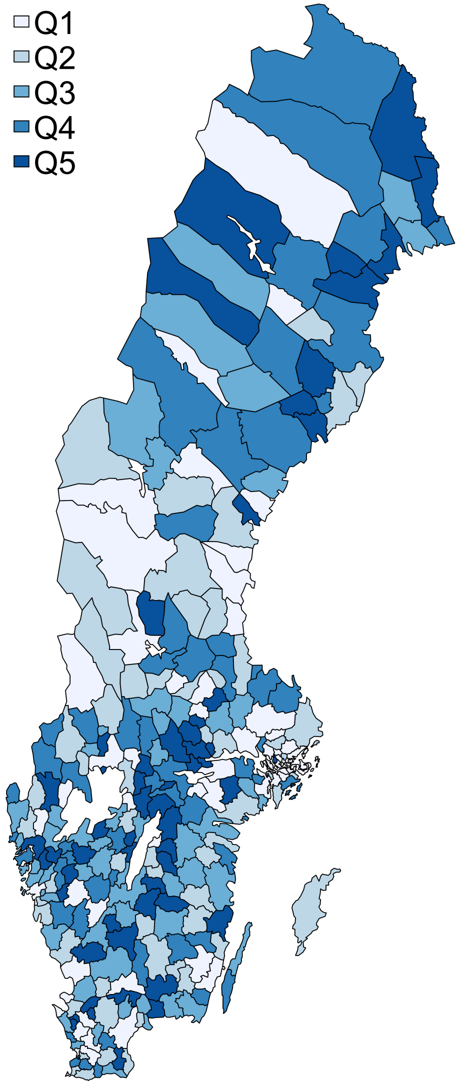
Source: NAE Parental Surveys

Figure C2: Difference in enrollment between 1999 and 2002, working parents compared to parents on parental leave, plotted against the pre-enrollment difference



Source: NAE Parental Surveys

Figure C3: Map of municipalities by pre-diff quintiles



Source: NAE Parental Surveys

2.D Changes in enrollment: First stage

Table D1: The enrollment of children as reported in the parental surveys in 1999 and 2002, by municipality treatment status, year and child gender

		Pre-reform 1999	Post reform 2002	Enrollment increase 2002-1999
Parent on parental leave				
Reform	Boy	0.13	0.59	0.46
	Girl	0.07	0.57	0.49
Control	Boy	0.59	0.85	0.25
	Girl	0.67	0.81	0.14
Both parents working				
Reform	Boy	0.92	0.95	0.03
	Girl	0.92	0.95	0.03
Control	Boy	0.93	0.98	0.05
	Girl	0.93	0.96	0.03

Source: Source: NAE Parental Surveys

Table D2: The enrollment of children as reported in the parental surveys in 1999 and 2002, by municipality treatment status, year and education

		Pre-reform 1999	Post reform 2002	Enrollment increase 2002-1999
Parent on parental leave				
Reform	Low	0.10	0.57	0.47
	High	0.08	0.64	0.56
Control	Low	0.60	0.78	0.18
	High	0.74	0.94	0.2
Both parents working				
Reform	Low	0.91	0.95	0.04
	High	0.96	0.97	0.01
Control	Low	0.92	0.97	0.05
	High	0.95	0.98	0.03

Source: Source: NAE Parental Surveys

$$Y_{imcd} = \alpha + \delta post_c \times reform_m \times work_i + \gamma work_i \times reform_m$$
$$+ \eta post_c \times reform_m + \phi work_i \times post_c + work_i + post_c + \theta_m + X_i\beta' + \varepsilon_{imc} \tag{2.2}$$

The controls included are age, gender, parity grouped, indicator of twin/multiple birth, indicator for mother and father education 3 levels, mother immigration status and an indicator of parents cohabiting. The child controls refer to the older sibling. The model also includes municipality fixed effects and the standard errors are clustered at the municipality level. Observations are weighted by the number of children in each municipality (based on our sample).

Table D3: DD of first stage using parental survey

	Reform municipalities	Control municipalities
All		
PL*post	0.442***	0.123**
	(0.037)	(0.047)
Observations	15,435	16,679
Mother low education		
PL*post	0.423***	0.104**
	(0.030)	(0.040)
Observations	13,664	13,519
Mother high education		
PL*post	0.532***	0.141***
	(0.088)	(0.050)
Observations	1,771	3,160

Note: Robust standard errors clustered at the municipality level in parenthesis, , *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including municipality fixed effects and child and parental controls.

2.E Control municipalities: Descriptives

Table E1: Summary statistics of predetermined characteristics and outcomes for pre and post reform cohorts born 1999-2003 in reform municipalities

	(1)	(2)	(3)	(4)	(5)	(6)
	No sibling		Sibling		Difference	
	Pre	Post	Pre	Post	DD	P-value
Female	0.48	0.49	0.49	0.49	0.00	0.78
Multiple-births	0.04	0.03	0.03	0.03	0.00	0.86
Low birth weight	0.05	0.05	0.03	0.03	0.00	0.87
First born, mom	0.82	0.82	0.00	0.00	0.00	0.31
Second born, mom	0.09	0.09	0.70	0.72	0.02	0.00
Third/higher parity, mom	0.09	0.09	0.30	0.28	-0.01	0.00
Mother age at birth	28.12	28.61	27.17	27.73	0.08	0.09
Mother foreign born	0.20	0.20	0.22	0.22	0.00	0.61
Mom disp. income percentile	53.08	54.44	53.59	54.12	-0.78	0.11
Dad disp. income percentile	49.08	50.60	55.17	56.56	0.02	0.96
Pred. education percentile	57.18	57.37	56.97	58.41	1.38	0.00
Mom compulsory education	0.15	0.13	0.16	0.14	0.00	0.31
Mom university education	0.35	0.42	0.35	0.39	-0.02	0.00
Dad compulsory education	0.15	0.14	0.16	0.15	0.01	0.06
Dad university education	0.28	0.34	0.29	0.34	-0.02	0.05
Observations	52,983	40,422	38,523	28,192	160,120	160,120

Note: Results from separate estimations of full DD-model including municipality by cohort fixed effects, birth month fixed effects.

2.F Predicting test scores

Equations to predict average test score and identify changes to the composition:

$$Y_{im} = X_i\beta' + \theta_m + \varepsilon_{im} \tag{2.3}$$

$$\begin{aligned}\hat{Y_{imcd}} = \alpha + \delta post_c \times reform_m \times sibling_i + \gamma sibling_i \times reform_m \\ + \phi sibling_i \times post_c + \mu sibling_i + \theta_{mc} + \varepsilon_{imc}\end{aligned} \tag{2.4}$$

2.G Graphical analysis and threats to identification

Figure G1: Residualized test scores for children with and without childcare age siblings in reform and control municipalities.

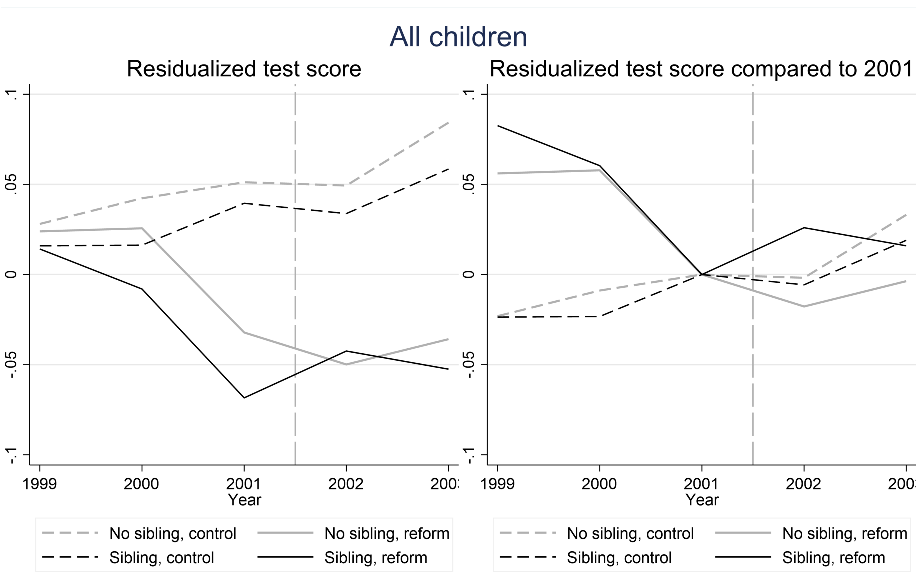


Figure G2: Residualized test scores for children with and without childcare age siblings in reform and control municipalities, boys

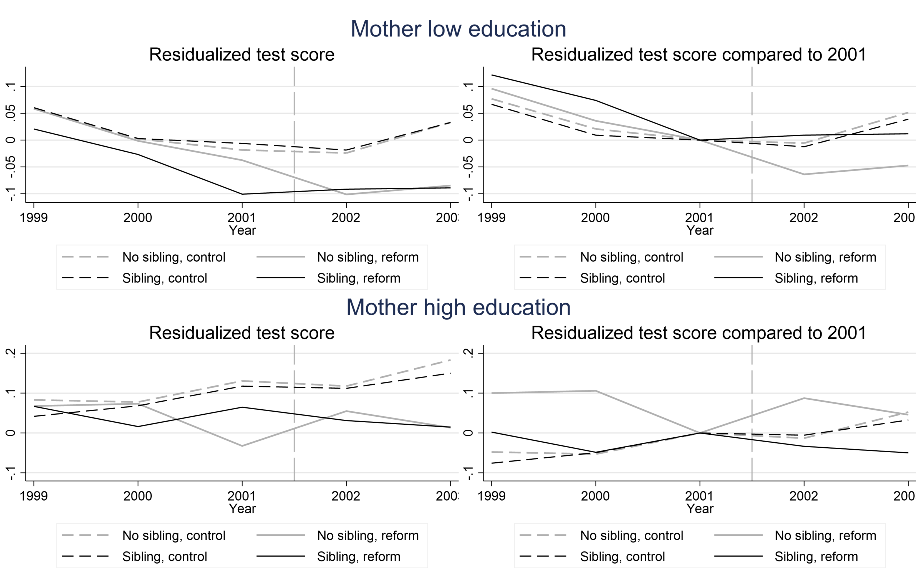
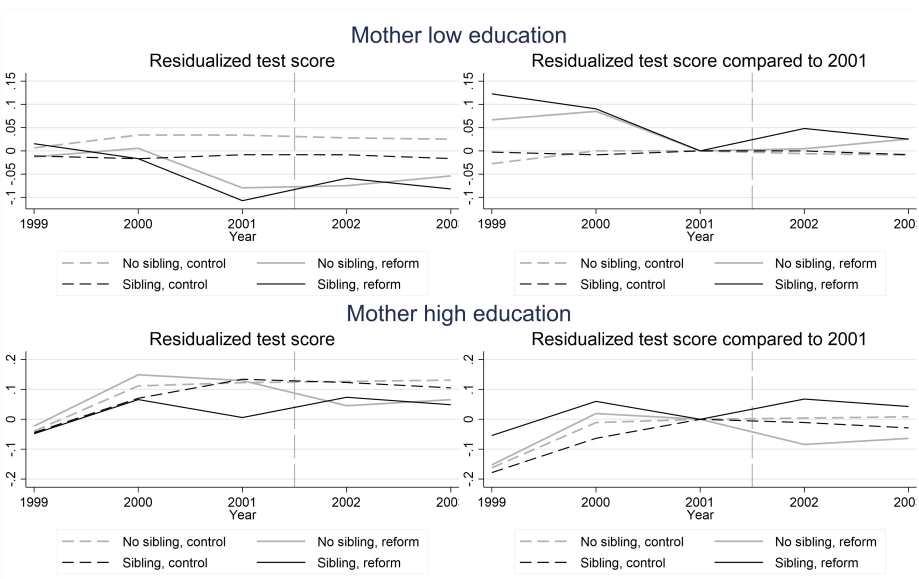


Figure G3: Residualized test scores for children with and without childcare age siblings in reform and control municipalities, girls



Differences in differences estimation by quintile of the pre-reform enrollment difference

In order to assess the division of municipalities into reform and control municipalities, we present in Table G1-Table G3 the within municipality group Difference-in-Differences model, by quintile of the pre-reform enrollment difference between children with working parents and parents on parental leave for all children and by maternal education.

Table G1: Within municipality DD by quintile of the pre-reform child preschool enrollment gap between working parents and parents on parental leave

	(1) All	(2) Boys	(3) Girls
Quintile 5: most restrictive pre reform			
Sibling*post	0.029	0.043**	0.017
	(0.019)	(0.021)	(0.025)
Observations	43,566	22,145	21,421
control mean	-0.0790	-0.199	0.0467
Quintile 4			
Sibling*post	0.006	-0.008	-0.004
	(0.014)	(0.019)	(0.023)
Observations	64,200	32,673	31,527
control mean	-0.0986	-0.223	0.0305
Quintile 3			
Sibling*post	-0.021	-0.019	-0.025
	(0.014)	(0.018)	(0.017)
Observations	70,326	36,006	34,320
control mean	-0.0123	-0.134	0.114
Quintile 2			
Sibling*post	-0.008	0.001	-0.019
	(0.012)	(0.018)	(0.016)
Observations	72,895	37,380	35,515
control mean	-0.0115	-0.144	0.128
Quintile 1: least restrictive			
Sibling*post	-0.003	-0.004	-0.002
	(0.007)	(0.012)	(0.008)
Observations	157,483	80,650	76,833
control mean	0.110	0.00653	0.218

Notes: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Table G2: Within municipality DD by quintile of the pre-reform child preschool enrollment gap between working parents and parents on parental leave, mother low education

	(1) All	(2) Boys	(3) Girls
Quintile 5: most restrictive pre reform			
Sibling*post	0.034	0.063**	0.007
	(0.024)	(0.028)	(0.034)
Observations	32,173	16,400	15,773
control mean	-0.215	-0.337	-0.0843
Quintile 4			
Sibling*post	-0.007	-0.018	0.005
	(0.015)	(0.021)	(0.019)
Observations	45,295	22,966	22,329
control mean	-0.217	-0.344	-0.0872
Quintile 3			
Sibling*post	-0.028*	-0.024	-0.031
	(0.015)	(0.020)	(0.021)
Observations	48,657	24,971	23,686
control mean	-0.160	-0.279	-0.0349
Quintile 2			
Sibling*post	-0.010	-0.008	-0.014
	(0.015)	(0.023)	(0.022)
Observations	49,688	25,460	24,228
control mean	-0.162	-0.299	-0.0191
Quintile 1: least restrictive			
Sibling*post	-0.003	-0.006	-0.000
	(0.009)	(0.014)	(0.014)
Observations	96,236	49,294	46,942
control mean	-0.0800	-0.182	0.0274

Notes: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Table G3: Within municipality DD by quintile of the pre-reform child preschool enrollment gap between working parents and parents on parental leave, mother high education

	All	Boys	Girls
Quintile 5: most restrictive pre reform			
Sibling*post	0.041	0.003	0.086**
	(0.029)	(0.040)	(0.041)
Observations	10,874	5,498	5,376
control mean	0.364	0.256	0.475
Quintile 4			
Sibling*post	-0.006	0.042	-0.055
	(0.026)	(0.035)	(0.040)
Observations	17,569	9,028	8,541
control mean	0.329	0.200	0.466
Quintile 3			
Sibling*post	-0.002	-0.001	-0.008
	(0.022)	(0.029)	(0.030)
Observations	20,898	10,655	10,243
control mean	0.388	0.267	0.510
Quintile 2			
Sibling*post	0.005	0.028	-0.018
	(0.018)	(0.037)	(0.027)
Observations	22,490	11,572	10,918
control mean	0.367	0.245	0.495
Quintile 1: least restrictive			
Sibling*post	0.006	0.008	0.005
	(0.014)	(0.021)	(0.013)
Observations	57,940	29,714	28,226
control mean	0.504	0.394	0.619

Notes: Robust standard errors clustered at the municipality level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Table G4: Within municipality DD by quintile of the pre-reform child preschool enrollment gap between working parents and parents on parental leave, mother high education

	(1) All	(2) Boys	(3) Girls
		2001 cohorts dropped	
All SES			
One-on-one time	0.025	0.050*	0.000
	(0.021)	(0.027)	(0.028)
Observations	34,958	17,813	17,145
Control mean	-0.0548	-0.175	0.0747
Mother high education			
One-on-one time	0.026	0.060*	-0.004
	(0.028)	(0.033)	(0.036)
Observations	25,798	13,158	12,640
Control mean	-0.179	-0.304	-0.0437
Mother low education			
One-on-one time	0.047	0.035	0.061
	(0.034)	(0.049)	(0.043)
Observations	8,749	4,463	4,286
Control mean	0.367	0.258	0.481
		Dose treatment	
All SES			
One-on-one time	0.023	0.041	0.006
	(0.020)	(0.026)	(0.024)
Observations	43,566	22,145	21,421
Control mean	-0.0491	-0.169	0.0800
Mother high education			
One-on-one time	0.021	0.057*	-0.009
	(0.026)	(0.032)	(0.033)
Observations	32,173	16,400	15,773
Control mean	-0.176	-0.300	-0.0406
Mother low education			
One-on-one time	0.050	0.010	0.102***
	(0.032)	(0.048)	(0.037)
Observations	10,874	5,498	5,376
Control mean	0.378	0.270	0.491

Notes: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Table G5: PLACEBO: Within municipality DD by quintile of the pre-reform child preschool enrollment gap between working parents and parents on parental leave, mother high education

	(1) All	(2) Boys	(3) Girls
		2001 cohorts dropped	
All SES			
One-on-one time	-0.000	-0.001	-0.000
	(0.007)	(0.014)	(0.011)
Observations	126,182	64,556	61,626
Control mean	0.0947	-0.00116	0.195
Mother high education			
One-on-one time	-0.002	-0.002	-0.002
	(0.009)	(0.015)	(0.017)
Observations	77,264	39,540	37,724
Control mean	-0.0739	-0.169	0.0265
Mother low education			
One-on-one time	0.016	0.013	0.021
	(0.018)	(0.025)	(0.016)
Observations	46,325	23,716	22,609
Control mean	0.470	0.373	0.571
		Dose treatment	
All SES			
One-on-one time	-0.001	0.000	-0.002
	(0.007)	(0.013)	(0.010)
Observations	157,483	80,650	76,833
Control mean	0.101	0.00389	0.202
Mother high education			
One-on-one time	-0.001	0.001	-0.003
	(0.010)	(0.015)	(0.016)
Observations	96,236	49,294	46,942
Control mean	-0.0712	-0.167	0.0297
Mother low education			
One-on-one time	0.013	0.010	0.017
	(0.019)	(0.026)	(0.017)
Observations	57,940	29,714	28,226
Control mean	0.480	0.381	0.583

Notes: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Table G6: Sensitivity of estimated effects on test scores to restricting the sample of controls to children with siblings

	(1) All	(2) Boys	(3) Girls
All SES			
One-on-one time	0.052*	0.086**	0.029
	(0.027)	(0.038)	(0.039)
Observations	24,780	12,556	12,224
Control mean	-0.0790	-0.199	0.0467
Mother high education			
One-on-one time	0.049	0.107**	-0.002
	(0.034)	(0.048)	(0.047)
Observations	18,582	9,457	9,125
Control mean	-0.215	-0.337	-0.0843
Mother low education			
One-on-one time	0.050	-0.018	0.144
	(0.067)	(0.083)	(0.102)
Observations	5,927	2,974	2,953
Control mean	0.364	0.256	0.475

Notes: Robust standard errors clustered at the municipality level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Table G7: PLACEBO: Sensitivity of estimated effects on test scores to restricting the sample of controls to children with siblings

	(1) All	(2) Boys	(3) Girls
All			
One-on-one time	0.016	0.020	0.014
	(0.013)	(0.022)	(0.026)
Observations	80,386	41,071	39,315
Control mean	0.110	0.00653	0.218
Mother high education			
One-on-one time	0.011	-0.004	0.028
	(0.019)	(0.025)	(0.029)
Observations	51,256	26,221	25,035
Control mean	-0.0800	-0.182	0.0274
Mother low education			
One-on-one time	0.004	0.063	-0.065**
	(0.022)	(0.043)	(0.032)
Observations	27,406	14,011	13,395
Control mean	0.504	0.394	0.619

Notes: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Differences in differences estimation by sibling status

An alternative variation possible to explore is the difference between reform and control municipalities for children with and without siblings in childcare age, respectively. We estimate the following equation:

$$Y_{imcd} = \alpha + \eta post_c \times reform_m + post_c + \theta_{reform,c} + \lambda_d + \rho_m \psi_c + X_i \beta' + \varepsilon_{imcd} \quad (2.5)$$

The variable $reform_m$ is an indicator variable taking the value 1 for reform municipalities, and 0 for control municipalities. The difference in difference estimate is captured by the term η and the model includes separate time trends for control and reform municipalities, $\theta_{reform,c}$, and municipality and cohort fixed effects, denoted ρ_m and ψ_c, respectively. Similar to the main specification, the model includes covariates as specified in Table 3, and month of birth fixed effects.

Table G8: DD-model Average test scores, comparing children with siblings in reform and control municipalities.

	(1) All	(2) Boys	(3) Girls
All			
Reform * post	0.072**	0.039	0.102**
	(0.035)	(0.050)	(0.041)
Observations	84,702	43,225	41,477
Control mean	-0.0790	-0.199	0.0467
Mother high education			
Reform * post	0.061*	0.029	0.091*
	(0.036)	(0.053)	(0.051)
Observations	54,472	27,900	26,572
Control mean	-0.215	-0.337	-0.0843
Mother low education			
Reform * post	0.076	0.045	0.113
	(0.062)	(0.093)	(0.080)
Observations	28,560	14,532	14,028
Control mean	0.364	0.256	0.475

Note: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Table G9: PLACEBO: DD-model Average test scores, comparing children with siblings in reform and control municipalities.

	(1) All	(2) Boys	(3) Girls
All			
Reform * post	0.017	0.018	0.020
	(0.031)	(0.038)	(0.041)
Observations	116,347	59,570	56,777
Control mean	-0.0185	-0.136	0.103
Mother high education			
Reform * post	-0.004	-0.043	0.041
	(0.033)	(0.045)	(0.046)
Observations	73,937	37,794	36,143
Control mean	-0.143	-0.249	-0.0319
Mother low education			
Reform * post	0.026	0.140**	-0.067
	(0.050)	(0.064)	(0.073)
Observations	40,254	20,680	19,574
Control mean	0.426	0.274	0.576

Note: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Differences in differences in differences estimation by sibling status

$$\begin{aligned} Y_{imcd} = \alpha + \delta post_c \times reform_m \times sibling_i + \gamma sibling_i \times reform_m \\ + \phi sibling_i \times post_c + \mu sibling_i + \theta_{m,c} + \lambda_d + X_i\beta' + \varepsilon_{imcd} \quad (2.6) \end{aligned}$$

The parameter of interest is δ which captures the triple interaction, comparing children with and without siblings of childcare age, born in the same year in the same municipality. This estimate is net of the time-invariant difference between reform and control municipalities in the outcome gap between children with and without sibling (γ) , as well as changes, post reform, in the country-level outcome gap between children with and without siblings (ϕ) and the overall level of which is captured by

μ. Similar to the main specification, it includes also municipality-specific cohort fixed effects, birth month fixed effects and covariates as specified in Table 2.3.

Table G10: DDD-model Average test scores, comparing children with and without siblings in reform and control municipalities.

	(1) All	(2) Boys	(3) Girls
All			
Post*reform*sibling	0.029	0.042*	0.018
	(0.019)	(0.024)	(0.026)
Observations	201,049	102,795	98,254
Control mean	-0.0790	-0.199	0.0467
Mother high education			
Post*reform*sibling	0.033	0.065**	0.005
	(0.025)	(0.030)	(0.036)
Observations	128,409	65,694	62,715
Control mean	-0.215	-0.337	-0.0843
Mother low education			
Post*reform*sibling	0.031	-0.013	0.076*
	(0.032)	(0.044)	(0.039)
Observations	68,814	35,212	33,602
Control mean	0.364	0.256	0.475

Note: Robust standard errors clustered at the municipality level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

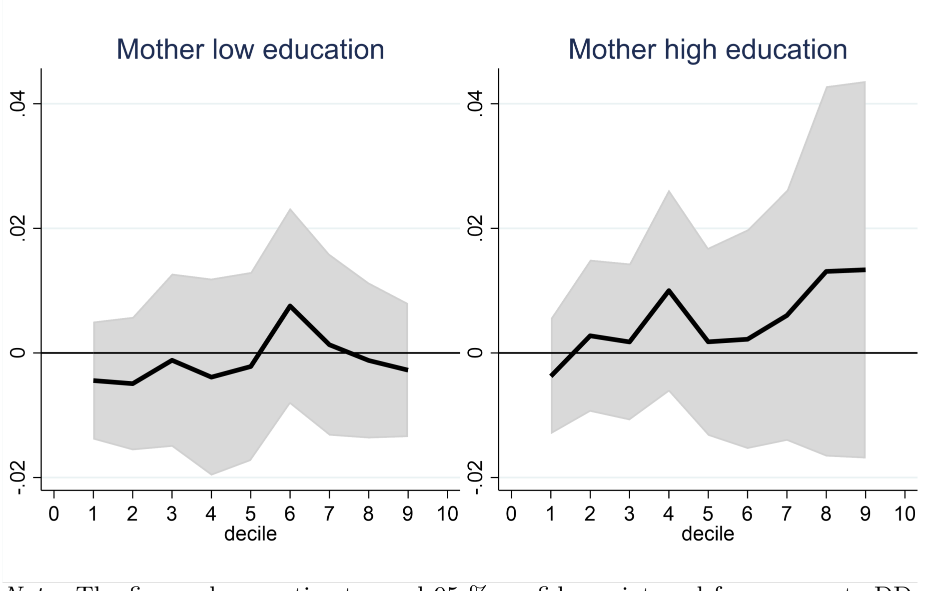
2.H Control municipality PLACEBO analysis

Table H1: DDD-model Average test scores, comparing children with and without siblings in reform and control municipalities.

	(1) All	(2) Boys	(3) Girls
		All SES	
One-on-one time	-0.003	-0.004	-0.002
	(0.007)	(0.012)	(0.008)
Observations	157,483	80,650	76,833
Control mean	0.110	0.00653	0.218
		Mother high education	
One-on-one time	-0.003	-0.006	-0.000
	(0.009)	(0.014)	(0.014)
Observations	96,236	49,294	46,942
Control mean	-0.0800	-0.182	0.0274
		Mother low education	
One-on-one time	0.006	0.008	0.005
	(0.014)	(0.021)	(0.013)
Observations	57,940	29,714	28,226
Control mean	0.504	0.394	0.619

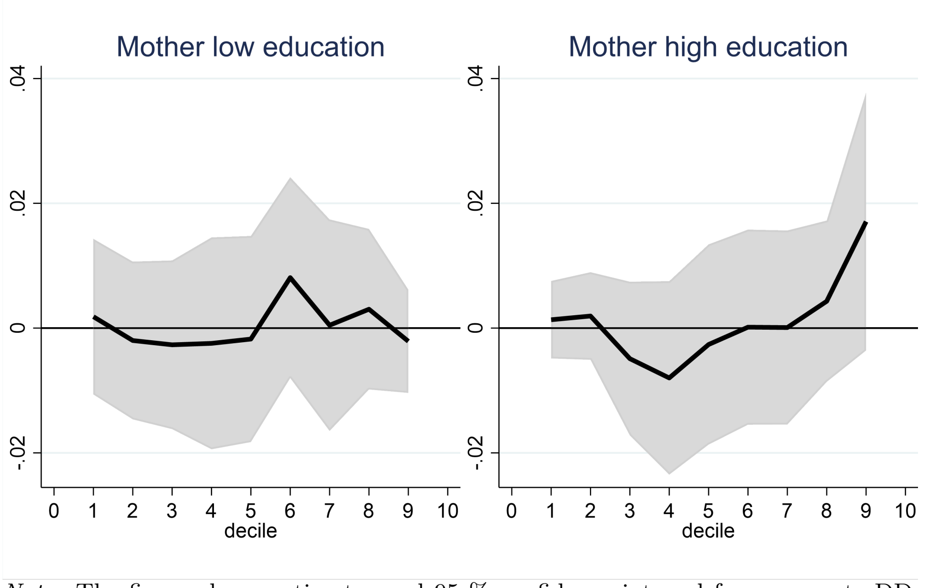
Notes: Robust standard errors clustered at the municipality level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3.

Figure H1: PLACEBO: Effects of child care access for older siblings over the test scores distribution, boys



Note: The figure shows estimates and 95 % confidence interval from separate DD-estimations of scoring above the i-th decile including municipality by cohort fixed effects, birth month fixed effects and controls for the list of predetermined characteristics in Table 3. Standard errors are robust and clustered at the municipal level.

Figure H2: PLACEBO: Effects of child care access for older siblings over the test scores distribution, girls



Note: The figure shows estimates and 95 % confidence interval from separate DD-estimations of scoring above the i-th decile including municipality by cohort fixed effects, birth month fixed effects and controls for the list of predetermined characteristics in Table 3. Standard errors are robust and clustered at the municipal level.

Table H2: PLACEBO: Effects on health in primary school age for the younger sibling

	(1)	(2)	(3)	(4)
	Preschool	School		
	Inpatient		Mental	Infec/Resp
	Any	Any	Any	Any
		All children		
One-on-one time	3.321	1.412	-3.406	2.815
	(4.125)	(2.552)	(3.430)	(3.821)
Observations	157,740	158,499	158,499	158,499
Pre-reform mean	300.6	928.0	96.44	749.5
		Boys		
One-on-one time	5.753	-0.228	-6.409	3.085
	(5.907)	(2.358)	(4.488)	(4.690)
Observations	80,911	81,327	81,327	81,327
Pre-reform mean	333.0	934.9	125.1	750.1
		Girls		
One-on-one time	0.750	2.829	-0.416	2.381
	(5.310)	(4.471)	(3.578)	(8.262)
Observations	76,829	77,172	77,172	77,172
Pre-reform mean	266.5	920.8	66.23	748.9

Notes: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Note that health outcomes are measured per 1000 individuals.

Table H3: PLACEBO: Effects on health in primary school age for the younger sibling, by maternal education

	(1)	(2)	(3)	(4)
	Preschool	School		
	Inpatient		Mental	Infec/Resp
	Any	Any	Any	Any
	Boys			
	Mother low education			
One-on-one time	1.856	-5.683	-4.313	-3.368
	(10.050)	(3.877)	(6.454)	(7.326)
Observations	49,675	49,835	49,835	49,835
Pre-reform mean	346.1	935.4	138.6	754.7
	Mother high education			
One-on-one time	8.027	7.158	-11.193*	14.206
	(9.963)	(4.525)	(6.036)	(9.161)
Observations	29,585	29,829	29,829	29,829
Pre-reform mean	302.4	933.7	96.85	738.6
	Girls			
	Mother low education			
One-on-one time	10.660	1.657	0.267	5.726
	(6.458)	(4.922)	(4.820)	(7.504)
Observations	47,054	47,185	47,185	47,185
Pre-reform mean	274.2	923.7	75.43	756.3
	Mother high education			
One-on-one time	-14.911	3.724	-2.143	-1.834
	(11.240)	(7.743)	(5.553)	(13.294)
Observations	28,104	28,305	28,305	28,305
Pre-reform mean	248.8	915.1	48.65	733.0

Notes:Robust standard errors clustered at the municipality level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Note that health outcomes are measured per 1000 individuals.

Table H4: PLACEBO:Effects of better opportunities for one-on-one time on family and childhood environment

	(1) Mother mental health	(2) Parents separated	(3) Younger sibling	(4) Mother earnings	(5) Age at preschool enrollment
			Boys		
			Mother low education		
One-on-one time	-0.873	-0.006	-0.009	0.017	-7.612***
	(1.739)	(0.006)	(0.009)	(0.024)	(2.248)
Observations	49,675	49,668	49,675	44,597	42,670
Control mean	9.275	0.167	0.0933	12.06	537.0
			Mother high education		
One-on-one time	1.336	-0.008	-0.018*	-0.028	-2.688
	(2.446)	(0.005)	(0.009)	(0.026)	(4.800)
Observations	29,585	29,585	29,585	28,261	25,286
Control mean	4.917	0.0687	0.0699	12.71	566.9
			Girls		
			Mother low education		
One-on-one time	-1.008	-0.014**	-0.010*	-0.022	-3.698
	(1.699)	(0.005)	(0.005)	(0.020)	(2.299)
Observations	47,054	47,038	47,054	41,989	40,234
Control mean	10.75	0.173	0.0920	12.05	535.1
			Mother high education		
One-on-one time	1.508	-0.010	-0.008	0.007	-5.468**
	(1.605)	(0.010)	(0.008)	(0.034)	(2.699)
Observations	28,104	28,101	28,104	26,815	23,991
Control mean	5.934	0.0592	0.0670	12.70	567.9

Notes: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Note that maternal mental health is measured per 1000 individuals.

2.I Additional analyses

Table I1: Parental leave benefits

	(1)	(2)	(3)
	PL first year		PL bef. birth
	Father	Mother	
		Boys	
		Mother low education	
One-on-one time	-1.820	2.853	-0.058
	(1.103)	(2.748)	(0.413)
Observations	16,543	16,543	16,543
Pre-reform mean	14.54	244.4	6.229
		Mother high education	
One-on-one time	-0.051	6.462*	-0.962*
	(2.065)	(3.714)	(0.511)
Observations	5,502	5,502	5,502
Pre-reform mean	12.07	210.2	5.127
		Girls	
		Mother low education	
One-on-one time	1.191	0.843	0.098
	(1.232)	(2.590)	(0.432)
Observations	15,824	15,824	15,824
Pre-reform mean	12.87	247.1	6.340
		Mother high education	
One-on-one time	0.195	0.823	-0.193
	(1.822)	(5.162)	(0.579)
Observations	5,355	5,355	5,355
Pre-reform mean	12.26	211.8	4.596

Note: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Note that maternal mental health is measured per 1000 individuals.

Table I2: PLACEBO: Parental leave benefits

	(1)	(2)	(3)
	PL first year		PL bef. birth
	Father	Mother	
	Boys		
	Mother low education		
One-on-one time	0.472	1.679	0.171
	(0.702)	(1.514)	(0.217)
Observations	49,675	49,675	49,675
Pre-reform mean	14.08	244.3	5.828
	Mother high education		
One-on-one time	-0.178	-0.768	0.062
	(0.866)	(1.354)	(0.229)
Observations	29,585	29,585	29,585
Pre-reform mean	14.85	216.2	4.149
	Girls		
	Mother low education		
One-on-one time	1.027	-1.297	0.551**
	(0.856)	(1.289)	(0.275)
Observations	47,054	47,054	47,054
Pre-reform mean	13.68	246.3	5.640
	Mother high education		
One-on-one time	0.224	-0.875	0.397
	(1.053)	(1.476)	(0.316)
Observations	28,104	28,104	28,104
Pre-reform mean	14.19	214.9	4.123

Note: Robust standard errors clustered at the municipality level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Note that maternal mental health is measured per 1000 individuals.

Table I3: Sibling spillovers: DD-model of more one-on-one time on Average test scores for younger and older sibling respectively

	(1)	(2)	(3)	(5)	(6)	(7)
	Younger sibling			Older sibling		
	All	Boys	Girls	All	Boys	Girls
			Mother low education			
One-on-one time	0.031	0.081	-0.011	-0.066**	-0.007	-0.125**
	(0.042)	(0.055)	(0.060)	(0.031)	(0.050)	(0.054)
Observations	15,785	8,028	7,757	15,785	8,028	7,757
Control mean	-0.190	-0.308	-0.0663	-0.175	-0.158	-0.193
			Mother high education			
One-on-one time	0.021	0.014	0.067	-0.064	-0.036	-0.073
	(0.091)	(0.117)	(0.146)	(0.053)	(0.104)	(0.084)
Observations	5,052	2,536	2,516	5,052	2,536	2,516
Control mean	0.366	0.260	0.477	0.451	0.477	0.424

Note: Robust standard errors clustered at the municipality level in parentheses, *** $p<0.01$, ** $p<0.05$, * $p<0.1$. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Note that maternal mental health is measured per 1000 individuals.

Table I4: PLACEBO: Sibling spillovers: DD-model of more one-on-one time on Average test scores for younger and older sibling respectively

	(1)	(2)	(3)	(5)	(6)	(7)
	Younger sibling			Older sibling		
	All	Boys	Girls	All	Boys	Girls
	Mother low education					
One-on-one time	0.000	-0.028	0.035	-0.042*	-0.062*	-0.022
	(0.024)	(0.031)	(0.032)	(0.022)	(0.031)	(0.042)
Observations	42,710	21,945	20,765	42,710	21,945	20,765
Control mean	-0.0607	-0.163	0.0470	0.0170	0.0137	0.0204
	Mother high education					
One-on-one time	0.012	0.067	-0.047	-0.015	0.017	-0.045
	(0.024)	(0.045)	(0.038)	(0.036)	(0.059)	(0.041)
Observations	22,695	11,625	11,070	22,695	11,625	11,070
Control mean	0.515	0.405	0.632	0.729	0.738	0.720

Note: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 2.3. Note that maternal mental health is measured per 1000 individuals.

Chapter 3

Seasonality of Childcare Enrollment*

*I am grateful to Anne Boschini, Erik Lindqvist, Lisa Laun and Johanna Rickne, as well as seminar participants at Stockholm University for helpful comments and suggestions.

3.1 Introduction

In Sweden, childcare is heavily subsidized and available from the child's first birthday (SFS 2010:800).[1] Although the quality is generally high, childcare providers can differ in pedagogy, distance from home, and hours open. In principle, the flexible parental leave system enables the age at childcare enrollment to vary with preferences and constraints of the household. In reality, however, the institutional structure of the childcare system in Sweden implies a non-constant supply of childcare with two distinct peaks in supply: in August when a bulk of slots opens up as an entire cohort start school, and in January when children tend to be re-allocated between age groups. As the match between households and childcare providers is presumably worse when supply is scarce (SOU, 2013), the age when childcare enrollment is especially favorable differs according to the child's date of birth. The seasonal variation in childcare supply can therefore nudge households to enroll either earlier or later than otherwise preferred. The financial cost of delayed enrollment is born by the parents and they are ultimately faced with a trade-off between childcare access and childcare quality.

The purpose of this paper is to study how seasonal variation in childcare enrollment shapes seasonality in the enrollment age, by month of birth and socioeconomic status of the parents. The analysis is based on children born between 2002 and 2015. As data on actual childcare enrollment are unavailable, the date when parents stop using parental leave benefits is used as a proxy for the enrollment date, an approach suggested by Duvander and Viklund (2017).[2] Detailed data on parental leave benefits allow the date of childcare enrollment to be convincingly proxied for 84 percent of all children, yielding a sample of more than 1 million observations.

While there is an extensive literature evaluating the timing of birth to capture effects of school starting age (e.g., Fredriksson et al., 2021; Black et al., 2011), I focus on an earlier, less explored stage–i.e., how the timing of birth affects the age at childcare enrollment. This variation, although less salient, is potentially important. Previous literature suggests that the

[1] More than 85 percent of all children aged 1–5 are enrolled in childcare (*Skolverket*, 2021), ensuring a high female labor force participation.

[2] By evaluating enrollments, I capture the realized outcome given childcare supply and childcare demand.

age at childcare enrollment can affect the human capital accumulation of children (e.g., Drange and Havnes, 2019; Fort et al., 2020). The age at childcare enrollment, and consequently the duration of parental leave, can also have dynamic effects on the household which is explored in a tentative analysis.

I find that that there is seasonal variation in the timing of childcare enrollment with bunching in late August and January; more than 50 percent of all children are enrolled in either August or January, and this pattern cannot be explained by variations in birthrates or household characteristics. The seasonality in childcare enrollment is mirrored in the average age at childcare enrollment by birth month. However, the relation between date of birth and age at childcare enrollment is non-monotone; the counterfactual date of childcare enrollment, corresponding to the perceived ideal age if supply were constant, is below the actual allocation for some while above for others. Non-monotone effects impact the distribution of age and the variation in average age across birth month is relatively small, 16 days. Evaluating instead seasonality in enrollment at certain ages (grouped), I find that few children are enrolled above 12 months of age unless it corresponds to enrollment in either August or January. The seasonal variation in enrollments above 12 months suggests that for a substantial fraction of households, the age at childcare enrollment is elastic and the birth month relative to August or January can alter the age at childcare enrollment by several months. Meanwhile, childcare enrollments at the youngest age possible (12 months) is consistently high across month of birth.

The heterogeneity analysis shows that the seasonal variation in age at childcare enrollment increases with socioeconomic status (SES), measured as the pre-birth earnings quintile of the mother. The observed SES differences in seasonality of childcare enrollment are driven by enrollment at ages above 12 months. For enrollments at 12 months, the seasonality is similar across SES, suggesting that SES differences for higher ages are at least partially driven by differences in financial constraints. Consequently, a smoother supply of childcare across the year would mitigate the financial impact and improve equality in childcare access and quality.

Examining potential household implications, I find that the seasonal variation in fathers' share of parental leave is similar to the seasonality of the age at childcare enrollment. This similarity suggests that parental leave of fathers is more sensitive, compared to mothers, to exogenous variation in the age at childcare enrollment. The finding is consistent with fathers' parental leave uptake being a function of the total length of parental leave. There is no indication of consequences for other outcomes explored, i.e., fertility, marital stability or earnings trajectory.

This paper contributes to the literature on the determinants of age at childcare enrollment. To my knowledge, the seasonality of age at childcare enrollment is largely unexplored, with the exception of Duvander (2006), who found differences in the average age at childcare enrollment by quarter of birth using survey data. Qualitative studies indicate that parents consider financial constraints, labor market attachment, social and cultural expectations (Meyers and Jordan, 2006), and institutional constraints (Olson, 2002), when deciding the age to enroll their child in childcare. Furthermore, the age at childcare enrollment is found to differ with SES of the parents, as indicated by parental income (Duvander and Viklund, 2017) and educational level (Hall and Lindahl, 2018). While these studies control for birth month, I explicitly explore the impact of birth month on the age at childcare enrollment. Thus, this paper corroborates and complements the existing findings and suggests additional discrepancies in childcare enrollment behavior associated with SES.

Examining a novel source of variation in the age at childcare enrollment, this paper is motivated by the literature on consequences of age at childcare enrollment. In particular, there is an extensive literature focusing on human capital effects of children, evaluating exogenous variations in the age at childcare enrollment due to childcare or parental leave policies (e.g., Drange and Havnes, 2019; Fort et al., 2020). The results from the previous literature are somewhat mixed, suggesting that the quality of alternative modes of care are important for the value of childcare relative to parental care. Consequently, the seasonal variation in age at childcare enrollment may impact children differently, depending on the SES of the household.[3] Furthermore, the existing literature suggests that the age at childcare enrollment can affect the household via the duration of parental leave, which is also examined in the exploratory analysis.[4] Differences in the age at childcare enrollment can affect fertility

[3]This is in addition to the potential long-term gains from enrollment during high supply when childcare providers are better aligned with preferences of the household.

[4]Although there are no established effects on earnings, fertility, divorce, or health

via the implied timing of labor market return, since the age gap between siblings is susceptible to nudges (Lalive and Zweimuller, 2009; Ginja et al., 2020). It also impacts the income of the household, a stressor that can potentially affect the marital stability (Dew et al., 2012). Variations in the length of parental leave that does not apply similarly to all, can also affect the career trajectory of parents (Duvander and Evertsson, 2011). Finally, total length of parental leave can affect the parental leave uptake of fathers (Duvander and Viklund, 2017), and any effects on this may further feed into all of the above-mentioned outcomes (Cools et al., 2015; Farré and González, 2019; Avdic and Karimi, 2018; Ekberg et al., 2013).

The paper is structured in the following way. In Section 2, I summarize the institutional context, accounting for relevant details of the Swedish childcare system and the parental leave insurance. In Section 3, I describe the data, how childcare enrollment is estimated, and characterize the sample. Section 4 presents the seasonality in childcare enrollment, and Section 5 describes the seasonality in age at childcare enrollment. In Section 6, I present the heterogeneity analysis by socioeconomic status. In Section 7, I examine possible household implications and, in Section 8, I make some concluding remarks.

3.2 Institutional context

Since 2002, formal childcare has been offered to all children at a highly subsidized cost.[5] The purpose of subsidized childcare is to equalize social inequality by enabling mothers to participate in the labor force and integrating and preparing children for school (Lundin et al., 2008). All children above one year old living in Sweden are eligible and exceptions for younger children is rare (Duvander and Viklund, 2017).[6]

Although there are variations in the childcare queuing-system across municipalities and time, all children are guaranteed childcare within four months of application. In most municipalities, it is also possible to apply for childcare enrollment at a specific date, more than four months

from parental leave expansion (Liu and Nordström Skans, 2010), seasonal variation in age at childcare enrollment may still have consequences.

[5]Although access was extended, children whose parent is either unemployed or on parental leave often have restricted access (at least 15 hours per week).

[6]Exceptions can be made if there are special needs such as physical or mental limitations (SFS 2010:800).

into the future. For applications with long notice, childcare should be offered by the date they applied for.[7] Typically, parents rank their preferred childcare providers. If none of their preferences are available or if ranking is not applied, they are offered childcare by the nearest provider available.[8] When a slot opens, children who applied for that month (or earlier) are ranked by the time in the queue and ties are settled by age. It is common that children with special needs, who need to be covered by the guarantee or who have older siblings enrolled with the provider, are prioritized. Parents can accept an offer and delay actual enrollment while paying for at most two months (SFS 2010:800; Stockholms stad, 2019). Childcare enrollment can follow different strategies, and the required parental presence typically varies between a few days and two weeks (Arnesson Eriksson, 2010).

Although parents have a legal right to childcare once the child is 12 months old, childcare supply varies across the year. The seasonal variation in supply is not formally documented but is conveyed to parents during the application process.[9] During the typical vacation periods of summer and Christmas, the supply is highly restricted. Because both parents and teachers are on vacation during these periods, childcare providers downscale and this creates an environment far from ideal for new enrollment. Instead, childcare supply is particularly high in the second half of August, when an entire cohort starts school, and in January, when age groups are often re-arranged, freeing up slots for younger children. Therefore, by applying in August or January, the probability of receiving a childcare provider in line with preferences is maximized.

During the period studied in this paper, all parents are entitled to a total of 480 days of paid parental leave.[10] Often, parents use also unpaid

[7]Survey evidence suggests that about 2 percent of all children are not offered childcare within this time frame, but the delay is usually less than a month. Each year since 1995, on average, 20 municipalities report being unable to meet the time requirement (SOU 2013:41).

[8]This offer does not remove children from the queue for higher ranked alternatives.

[9]See, for example, Stockholms stad, 2022; Göteborgs stad, 2022.

[10]Since 2002, parents are entitled to 390 days of income-based replacement and 90 days of a low flat benefit. Parents with a low or no income receive the low flat benefit for all days. The income-based benefit replaces almost 80 percent of the labor earnings, capped at 10 times the base amount (*Försäkringskassan*, 2022). In addition to parental leave, many employees are covered by collective insurances that top up the replacement during parental leave (Sjögren Lindquist and Wadensjö 2005, Duvander et al. 2020). Also, the sickness benefit qualifying income (SGI) is protected for 12 months and all

parental leave before childcare enrollment. With a higher dispersion of paid days (and consequently a lower replacement rate), the duration of parental leave can be extended or days with parental leave benefits can be saved for after childcare enrollment.[11] To encourage fathers to take more parental leave, one month was reserved for each parent in 1995 and this was followed by a second month in 2002 and an equal division bonus in 2008 (*Försäkringskassan*, 2014).

3.3 Data

3.3.1 Data sources

The empirical analysis is based on data from several Swedish administrative registries where individuals can be linked by unique identifiers. The main dataset is the Parental Leave Registry from the Social Insurance Office (*Försäkringskassan*), which contains information about parental leave by child and parent (or other beneficiary). All information is reported by estimated episodes and sub-episodes. An episode consists of paid and unpaid days that are assessed to constitute a cohesive period of parental leave.[12] The variables include start and end dates for these periods, amount of benefits, and net days (adjusted for partial replacement). The dataset also includes the date of birth of the child. The population of interest are children born between 2002 and 2015, and their parents. To this dataset, I link parental characteristics from the Longitudinal Integration Database for Health Insurance and Labor Market Studies (LISA) from Statistics Sweden, which covers everyone above the age of 15 registered in Sweden as of last December that year. From this dataset, I can retrieve information about immigrant status, education, employment, and earnings both before and after birth. To determine SES, I use quintiles of the annual labor earnings of the mother before birth. Specifically, I select the highest earnings in the two preceding years and rank within

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parents are covered by job protection for 18 months after birth.

[11] Benefits can be saved until the child is eight years old. Hall and Lindahl (2019) found that households save on average 30 percent of all paid days until after childcare enrollment. This is used primarily to extend vacations and cover for reduced working hours. Parental leave can also be used outside regular working hours (e.g., weekends) if the episode also covers a regular working day.

[12] See Duvander (2013) for a discussion of the measure.

cohort.[13] To construct a measure of separation between parents, I use a variable indicating the position in the household.[14] Parents are further linked to the Multi-Generation Register, which allows me to map all of the parents' biological and adoptive children.

3.3.2 Estimating childcare enrollment

To estimate the date of childcare enrollment, I follow Duvander and Viklund (2017). The idea is to determine the date when neither parent is on parental leave, and equate this with childcare enrollment. Specifically, the estimated date of childcare enrollment corresponds to the last day of paid parental benefits, after which the uptake of benefits in the following six weeks does not exceed an average of two days per week.[15] See Appendix section 3.A for technical details.

This approximation method has both advantages and disadvantages relative to the (hypothetical) data of actual childcare enrollments. The advantages of this approach include targeting exact enrollment date rather than the date the slot is assigned to the child (which could differ by up to two months). In addition, this approach equates childcare enrollment with informal care not financed by parental leave benefits, for example, by older relatives or a nanny.[16] For household implications, it is desirable to treat informal care (i.e., people other than the parents) this way, as informal care, like formal care, enables both parents to return to work. Also, informal care is presumably costly to the household in terms of money or effort, and therefore better considered an emergency solution (similar to a poor match with the childcare provider) rather than an extended period of parental leave. However, both these aspects are expected to attenuate the observed seasonality in childcare enrollments.

[13]I use the maximum in the two preceding years to avoid systematic differences by birth month due to parental leave with the child in question or older siblings.

[14]This variable also captures cohabiting (with a child) unlike the alternative measure of civil status.

[15]I set the date of enrollment six weeks earlier than Duvander and Viklund (2017) as I use the end date of the episode before the conditions are met rather than the date once they are met. Furthermore, I identify parental leave during weekends and allow also for them while working. The distinction of weekends turns out to be unimportant for the date of childcare enrollment but leads to an additional 2,550 children being removed as the parental leave uptake for them is inconsistent with their assigned date of childcare enrollment.

[16]In 1999, this constituted about 5 percent of all children (Duvander 2006).

A disadvantage of the approximation is that the date of childcare enrollment involves some degree of measurement error. When vacation precedes childcare enrollment, the date is set too early. In particular, observed drops in December, July, and late June (when childcare access is highly restricted) are often followed by paid vacation to cover the gap until enrollment can take place.[17] Consequently, childcare enrollment frequencies during these months are misleadingly high (and frequencies in August and January misleadingly low). Furthermore, when parental leave benefits are used during parent active childcare enrollment, the date is set too late and some enrollments toward the end of a month are instead observed in the following month.

In addition to the imprecision of the approximation, the date of childcare enrollment is difficult to identify for some households. The parental leave system allows a flexible out-take with a possibility to combine the leave of different siblings and a high dispersion of paid days. For 16 percent of the children, the date of childcare enrollment cannot be determined with sufficient certainty and these are consequently removed from the analysis.[18]

As can be seen from Appendix Table B1, the households removed differ in terms of household characteristics compared to the remaining sample of 1,175,560 children. Discrepancies are to be expected since exclusion is non-random. In fact, due to the imposed restriction regarding dispersion of benefits, enrollments at high ages are more likely to be excluded. Appendix Table B1 shows that both parents in the sample are slightly younger, less educated, and with lower earnings. Importantly, the excluded households should not affect the seasonality observed since they are not systematically allocated to birth month or month of enrollment (see Panel A and B in Appendix Figure B1).

[17] Since vacation days are collected also during parental leave, the use of vacation to extend parental leave is common (Duvander and Viklund, 2017).

[18] These are children with a high dispersion of days with benefits (less than 3 days per week) during the first year (13.8%), too many days in total (0.6%) or whose assigned date is not consistent with their behavior (i.e., long gaps before enrollment or high uptake after enrollment) (10.9%), some behavior overlaps. The estimated timing of enrollment is validated using care for sick children. Care for sick children is usually used after childcare enrollment, but it is also used when the parent on parental leave is too sick to care for the child. When I examine the uptake of care for sick children, I find no difference in uptake before the age of one (when no one is enrolled in childcare) and before the estimated date childcare enrollment.

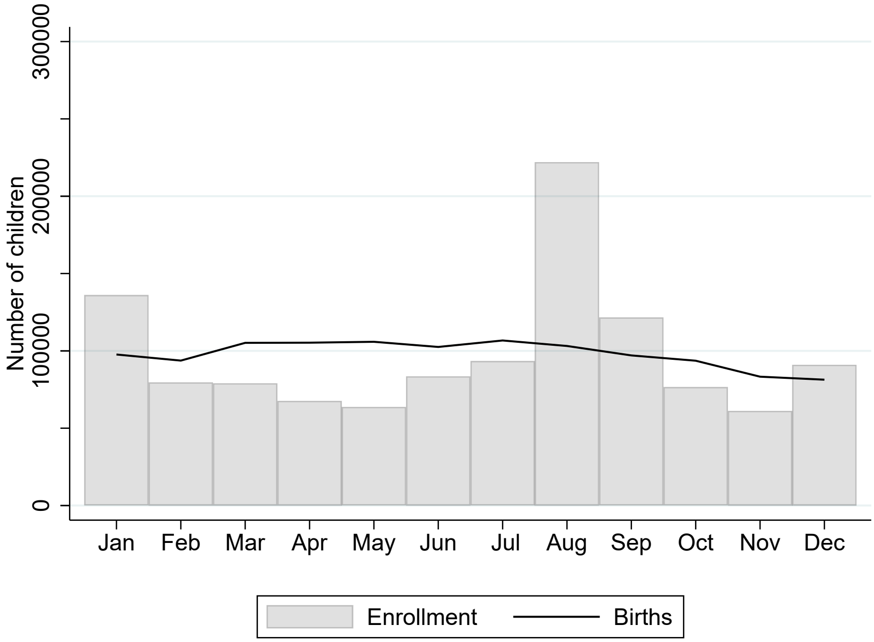
The exclusion of childcare enrollments at relatively high ages implies an earlier childcare enrollment than suggested by previous literature.[19] Although children enrolled at comparably high ages are influential to the mean age at childcare enrollment, they are not crucial to characterize the behavior of the typical household. My sample covers more than 80 percent of all children born during the study period. To uncover implications and economic relevance, the priority in this paper is to present a fair picture of the seasonality in childcare enrollments for households mostly affected. Parents who enroll children at unusually high ages have preferences or resources that differ from the typical household. Older children are also less constrained by the seasonality in supply since they are prioritized at childcare enrollment, and the adjustment in age implied by enrollment during high supply is smaller relative to the counterfactual age. Consequently, adjusting the date of childcare enrollment to accommodate variations in the supply is more optional and less decisive for these households.

3.4 Seasonality of childcare enrollment

In this section, I document the seasonal variation in childcare enrollment in relation to the potential seasonality of births. The bars in Figure 3.1 show the distribution of childcare enrollments by month. The black horizontal line indicates the number of births in each month.

[19]The mean age at childcare enrollment in my sample is 489 days which corresponds to 16 months, and 66 percent are enrolled by this age (Appendix Table B2). Meanwhile, Duvander (2006) reports an average age of 18 months and Hall and Lindahl (2018) report that 55 percent of all children are enrolled in childcare by 18 months. The difference is explained by sample differences; I exclude children for which the date of enrollment cannot be assigned, and Hall and Lindahl (2018) exclude children who move to another municipality by the age of three (21%). The two previous studies use earlier cohorts; Duvander (2006) uses survey data, and Hall and Lindahl (2018) restrict data to Gothenburg municipality, which might not be nationally representative. In addition, I allow for enrollments during the holiday season when I calculate age at enrollment, although these are assumed to be followed by vacation until enrollment is more likely. An alternative age can be computed using instead enrollment on August 16 or January 16 for these children. This has only a modest impact on the average age (Appendix Table B2) and no effect on the seasonality (Panel B in Appendix Figure E4).

Figure 3.1: Timing of childcare enrollment



Note: The gray bars show the number of children enrolled in childcare each month. The black horizontal line shows the number of children born each month.

Figure 3.1 shows that the variation in enrollments is not explained by birth rates. On average, almost 100,000 children are born each month. The horizontal line in Figure 3.1 depicts a declining trend of births towards the end of the year, which picks up again in the spring. Meanwhile, the gray bars for childcare enrollment indicate a spike in enrollments in August, when almost 20 percent of all enrollments take place. There is also comparably high enrollment in September, but this is confined to early September (see Appendix Figure C1 for daily frequencies) and consequently credibly captures enrollments initiated in late August. Another peak in childcare enrollments is observed in January.

The months of high enrollments observed in Figure 3.1 coincide with the expected peaks in childcare supply in August and January. The high enrollment rates are matched with enrollments below the birth frequencies for the remaining months, especially in November and May.[20]

[20]Since children are typically not enrolled at 12 (or 24) months, trends in enrollment driven by birth rates are not expected to coincide, but rather to lag by a couple of

A substantial amount of enrollments also occur during the typical months for vacation (i.e., June, July and December); after which paid vacation is likely used to cover the gap until childcare enrollment is less restricted.[21] When I adjust the date of childcare enrollment for observations during restricted supply (June 21-September 7; December 21-February 7), I find that half of all children are enrolled in August or January.[22]

The relatively constant low levels of childcare enrollments for other months suggest that children who enroll in August and January are born all across the year. In Figure 3.2, I explore directly how enrollment in August or January varies by birth month.[23]

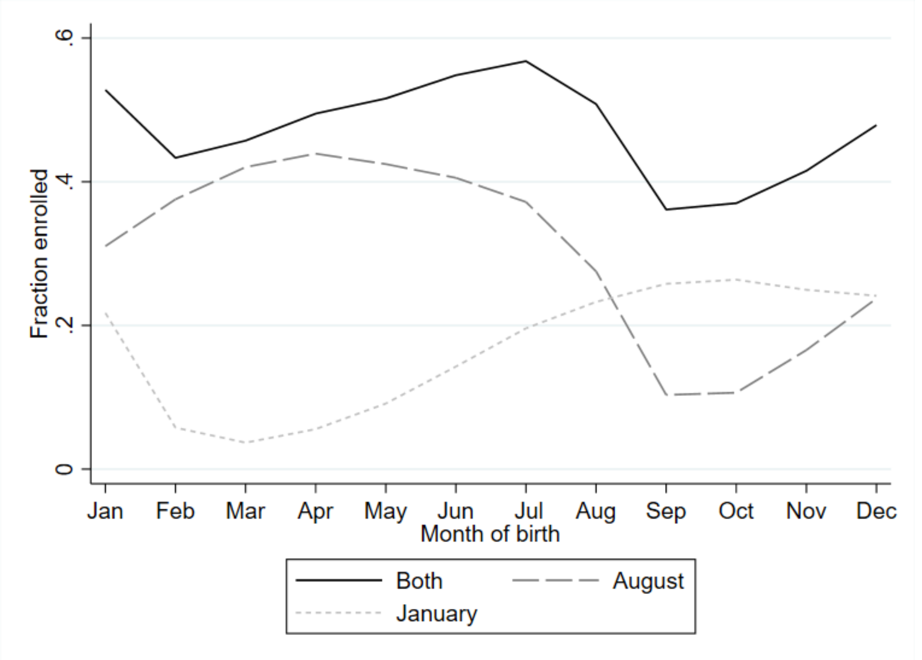
months.

[21]A comparatively large fraction of the enrollments in June and December are taken towards the end of the month (Appendix Figure C1), which is consistent with drops that are followed by paid vacation until enrollment.

[22]When I examined the seasonality of enrollments for different cohorts separately, I found that the fraction of households who enroll in either August or January is relatively constant throughout the period studied but that enrollments in January has become increasingly common (see Appendix Figure C2). Enrollment in August and January is also common across municipalities (see Appendix Figure C3).

[23]In this analysis, I adjust the observed date of enrollment and assign August or January to enrollments close in time (June 21–September 7 and December 21–February 7, respectively). I also restrict the sample to enrollments below two years of age to focus on the first opportunity to enroll in either August or January. In my sample, most children are enrolled before two years of age (97.5%), so the patterns are similar if including enrollments above two years of age.

Figure 3.2: Month of enrollment



Note: Share of children enrolled in August and/or January before age 2 by birth month. August includes observed enrollments between June 21 and September 7; January includes observed enrollments between December 21 and February 7.

Figure 3.2 confirms that there is a consistently high share of children enrolled when childcare supply is high (black solid line). Among the children born in July, almost 60 percent are enrolled in childcare in either August or January. The implied age difference at childcare enrollment between the two peaks is five months. August is more common (gray dashed line), but a substantial share are enrolled in childcare in January (gray dotted line). Most children born in September are eligible for childcare just *after* the largest peak in supply, which is in August, and they are also the least likely to enroll during high supply, especially in August.[24] Instead, the share of enrollments in August is highest among children born in April. The share of enrollments in January is highest for children born in October. However, the high enrollments in August

[24]Enrollments between September 1 and 7 are considered enrollments in August. Consequently, the share of births in September (February) includes both enrollments at 12 months and enrollments in the next August (January), which is at 23 months.

and January are relatively constant for adjacent birth months. At least 40 percent of all children born in March–June enroll in August, which implies a difference of 3 months in age at childcare enrollment. Similarly, about 25 percent of all children born between September and December enroll in childcare in January.

The observed differences in enrollment rates across the calendar year suggest bunching. As seen in Figure 3.1, the seasonal variation in childcare enrollment does not correspond to the seasonal variation in birth rates. Nor is the seasonal variation in childcare enrollments explained by the seasonal variation in household characteristics across birth month (see predicted enrollment frequencies using household covariates and birth rates in Appendix Figure D1).[25] Instead, bunching implies that there is a general preference to enroll at a certain time and that households behave accordingly. There are several reasons that could lead to bunching over the year. The foremost reason is the peak in childcare supply following from children leaving childcare to start school in August. Another reason is the smaller peak in childcare supply in January. In addition to the seasonality in childcare supply, preferences may be affected by seasonal aspects *per se*, such as variations in weather and infections. The registry data do not distinguish the motives for preferences, but the correspondence between enrollments and the institutional structure of childcare supply suggests that the childcare supply is the main mechanism.

[25] Consistent with the findings for the United States (Buckles and Hungerman, 2013), births of children from low socioeconomic families are slightly overrepresented at the turn of the year (see Appendix Figure D2). However, the variation is modest and cannot predict the seasonal variation observed for childcare enrollments. The timing of enrollment is predicted using covariates of the household, consistent with the existing literature (but one should keep in mind that the timing of childcare enrollment is the outcome of complex considerations of both observables and non-observables (Meyers and Jordan, 2006), so it is challenging to predict using registry data). The literature has identified several parental characteristics correlated with parental leave length of both mothers and fathers; in particular income (Hobson et al., 2006) and education (Haas, 1992; Sundström and Duvander, 2002), as well as the sector of employment, gender composition at the workplace and employer top-ups (Hobson et al., 2006, Almqvist and Duvander, 2014). To predict the age at childcare enrollment I include parity of the child and for each parent: labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self-employment. In addition, indicators of the mother being the top earner or the highest educated, parental cohabitation and marriage. The predictions also include cohort by municipality fixed effects.

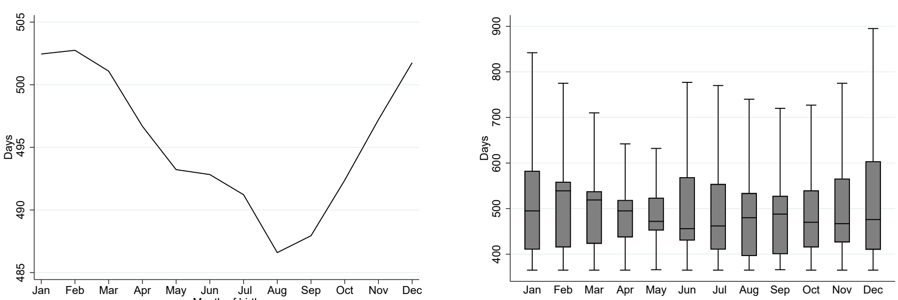
3.5 Seasonality in age at childcare enrollment

Depending on the date of birth, the age corresponding to childcare enrollment during high supply differs. The previous section establishes that a substantial fraction of children enrolls in childcare during high supply irrespective of date of birth. In this section, I document how the seasonality in childcare enrollment translates into differences in age at childcare enrollment by birth month. The age at childcare enrollment is potentially important for child outcomes in line with the literature on alternative modes of care (e.g., Drange and Havnes, 2019), but it may also affect parents and the household via the length of parental leave before childcare enrollment. I describe the seasonal variation in average age at childcare enrollment and how the distribution of age varies by birth month and assess the seasonality of childcare enrollment at certain ages.

Figure 3.3 shows the seasonal variation in age at childcare enrollment measured in days; the average age (Panel A) and the distribution of age (Panel B), across month of birth.

Figure 3.3: Age at childcare enrollment

(a) Mean age **(b)** Distribution of age



Note: Age at childcare enrollment, in days. Mean (left) and box plots (right), by birth month. The box contains the observations between the 25th and the 75th percentile; the horizontal line shows the median. I exclude outside values and the end points of whiskers represent lower/upper adjacent values.

Panel A of Figure 3.3 shows that there is seasonal variation in the average age at childcare enrollment, consistent with the observed seasonality in childcare enrollment. The average age at childcare enrollment is highest for children born in January, and there is a relatively steady decline throughout the spring, reaching its minimum for children born in August. For children born in the fall, the age increases linearly to the level of January. The variation in average age at childcare enrollment across birth months is only 16 days,[26] which implies that children born in August on average enroll two weeks earlier than children born in January. The observed pattern is consistent with the survey-based findings in Duvander (2006), but the variation is about half as large.

Panel B of Figure 3.3 shows that also the distribution of age at childcare enrollment varies by birth month.[27] The distribution is increasingly compressed throughout the spring; that is, the relatively high ages are lower and relatively low ages are higher for births closer to the summer. For the second half of the year, the distribution is more stable.

Differences in the distribution of age at childcare enrollment is expected when the age can be adjusted both upwards and downwards and when there are multiple bunching points (i.e., August and January). The decline in average age at childcare enrollment is consistent with the high enrollments in August among children born in the spring. High enrollments in August are also reflected in the compressed distribution of age throughout spring. The relatively high number of enrollments in months other than August, especially in January, explains why there is no jump in the age at childcare enrollment for children born after August. Instead, there is a positive trend, which is in line with the increasing number of children enrolled in August the next year. Children born in December display the largest variation, and this is consistent with the substantial enrollments in both January and August, corresponding to ages of 13 months and 20 months, respectively. The seasonal variation in average age is stronger in subgroups where demand for childcare enrollment during high supply is presumably high, suggesting that average age is a function of supply (see Appendix Figure E1).[28]

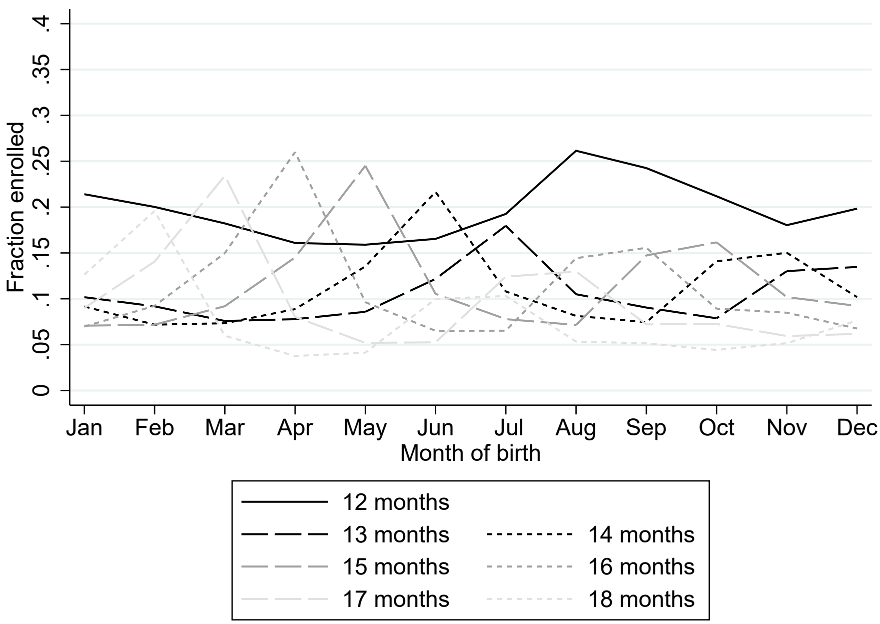
[26] 16 days corresponds to 3.3 percent relative to the lowest age at childcare enrollment.

[27] The end points of the whiskers represent lower/upper adjacent values and the box contains the observations between the 25th and the 75th percentile. The median is indicated by the horizontal line inside the box.

[28] The variation of preferences for age at childcare enrollment across the year should be comparable across subgroups and therefore observed differences in seasonal variation in childcare enrollment suggest supply affects age and not the reverse. Panel A in Appendix Figure E1 shows the urban municipalities relative to the rest. Although getting any slot is constant (guaranteed), preferences are presumably stronger when there are many childcare providers to choose from that differ in terms of, e.g., quality

Figure 3.4 instead shows the seasonality in age group, which allows for counteracting effects to be explored, potentially making the seasonal variation in enrollment more pronounced. Specifically, it shows the share of children enrolled in childcare at ages 12–18 months by birth month. Every line represents age groups of one month. The black solid line indicates enrollments at 12 months, which is the earliest age possible. Higher ages are indicated by a brighter shade and ages above 18 months are presented in Appendix Figure E3.

Figure 3.4: Age at childcare enrollment grouped



Note: Share of children enrolled in childcare at ages 12–18 months, by month of birth.

Figure 3.4 shows how the age at childcare enrollment corresponds to

and pedagogy. This implies higher expected gains from enrollment in August or January. Panel B shows the households with older siblings or where at least one parent is born in Sweden, relative to the rest. Parents experienced with the Swedish childcare system are presumably better informed about the expected gains of enrolling during high supply and therefore are more likely to enroll in August or January. I also confirmed that the seasonal variation in childcare enrollments is in fact more pronounced in subgroups where demand for enrollment during high supply is presumably high (Appendix Figure E2).

the peaks in childcare supply.[29] For all age groups between 12 and 18 months, there are two sets of visible peaks; the steeper peaks correspond to enrollments in August and the flatter peaks correspond to January.[30] Because age at childcare enrollment is grouped by month, the peaks of adjacent groups are one month apart. There are no signs of the restricted supply during summer causing bunching *before* summer. As seen by the relatively low levels of enrollments at 12 months among children born in May and June, this applies even to constrained households.

The strength of the seasonality differs across age groups. Figure 3.4 shows that about 5 percent of all children are enrolled at an age irrespective of the implied timing of enrollment, with the exception of enrollments at 12 months (black solid line). Across the year, at least 15 percent of all children are enrolled in childcare at 12 months. Meanwhile, the share of children enrolled at 16 months (dark gray dotted) is especially variable by birth month. More than 25 percent of all children born in April enroll in childcare at 16 months, which corresponds to enrollment in August. Among children born one month later, merely 10 percent are enrolled at the same age (in September). For children born in April, enrollment at 16 months is five times more common than enrollment at 18 months (corresponding to October) and this relative difference is reversed for children born in February. The seasonal variation for higher ages, presented in Appendix Figure E3, shows that also enrollment at high ages is largely coinciding with childcare enrollment in August or January. In relative terms, seasonal variation is actually the strongest for childcare enrollment at 19 months, ranging from 2.5 percent for children born in March to 13 percent for children born in January. However, this should be interpreted with caution due to the poor coverage of enrollment at higher ages.

In my sample, it is especially common to enroll in childcare in August

[29] Allowing for enrollments during the holiday season when supply is extremely restricted, implies an underestimation of the actual seasonality. Actual peaks are steeper and the ages that corresponds to enrollment in the summer or during Christmas should be close to zero, which affects the enrollments at 12 months in June and July, in particular. Panel A of Appendix Figure E4 shows the corresponding figure when no one is allowed to enroll June 21–August 1 and December 21–January 15, which can serve as an upper bound for seasonality.

[30] The latter is lower but less pointy; that is, the drop at the turn of the year is equally likely to be observed in December as in January, whereas the peak for August is more distinct.

when the corresponding age is either 12 or 16 months. The high percentage at 12 months is particularly informative as this is the youngest age when childcare is provided. Therefore, given constant preferences, bunching at this age is entirely driven by children enrolling earlier than what they would have chosen if supply was constant (i.e., below the counterfactual age at childcare enrollment). Meanwhile, high enrollments at 16 months can be driven by both upwards and downwards adjustment relative to the counterfactual age. Because the preferred age at childcare enrollment may be subjected to some critical points beyond which enrollment is perceived as unacceptable, 16 months is not necessarily indicative of the average age preference in my sample. Rather, it is the average of the range of ages, 14–18 months, for which seasonality is especially pronounced. Thus, 14–18 months is the range that many parents consider childcare enrollment to be acceptable, while also subjected to a relatively vague preference (as indicated by the low minimum percentages).

Taken together, the seasonal variation observed in Figure 3.4 suggests that there is a substantial percentage of children for whom the age at childcare enrollment can vary by several months, depending on the date of birth relative to August or January.[31] However, the high childcare enrollments at 12 months suggest that these enrollments are comparably less sensitive to any seasonal variation in childcare supply. Weaker seasonal variation for enrollments at 12 months is not surprising since childcare enrollment as early as possible is likely subjected to a stronger preference or necessity than enrollments at higher ages. Consequently, these households may be less flexible in terms of timing of childcare enrollment in exchange for a potentially better match with the childcare provider.

3.6 Socioeconomic status

In this section, I explore the potential heterogeneity in the seasonality of age at childcare enrollment with respect to socioeconomic status (SES) of the household. SES is indicated by quintiles of the mother's labor earnings before birth of the child. There are at least two explanations for

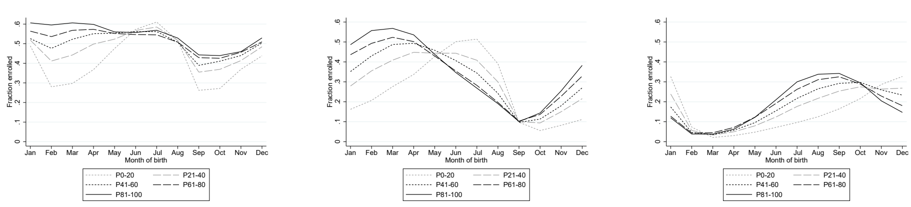
[31] Patterns for age at childcare enrollment presented in this section are similar for regression estimates (Appendix Figures E5 and E6); that is, seasonality is not sensitive to household covariates.

why households of different SES may exhibit different seasonal variation in the age at childcare enrollment. First, because earnings affect the financial capacity to cover the cost of extended parental leave duration, I would expect differences by SES when childcare enrollment in August or January implies a delay relative to the counterfactual timing of childcare enrollment. Given that the match to childcare provider is presumably worse when supply is low, this would reinforce inequalities with respect to SES. Second, SES can potentially capture differences in alternative costs or preferences, leading to differences in the costs and benefits of childcare enrollment in August or January.

Figure 3.5 shows the fraction of enrollments in August and January, corresponding to Figure 3.2, by earnings quintile of the mother.

Figure 3.5: Fraction children enrolled during high supply

(a) Both **(b)** August **(c)** January



Note: Share of children enrolled before age 2 in August or January (left), August (middle), January (right), by birth month. August includes observed enrollments between June 21 and September 7; January includes observed enrollments between December 21 and February 7. Quintiles refer to the pre-birth earnings of the mother, measured as the highest earnings the two years before birth of child.

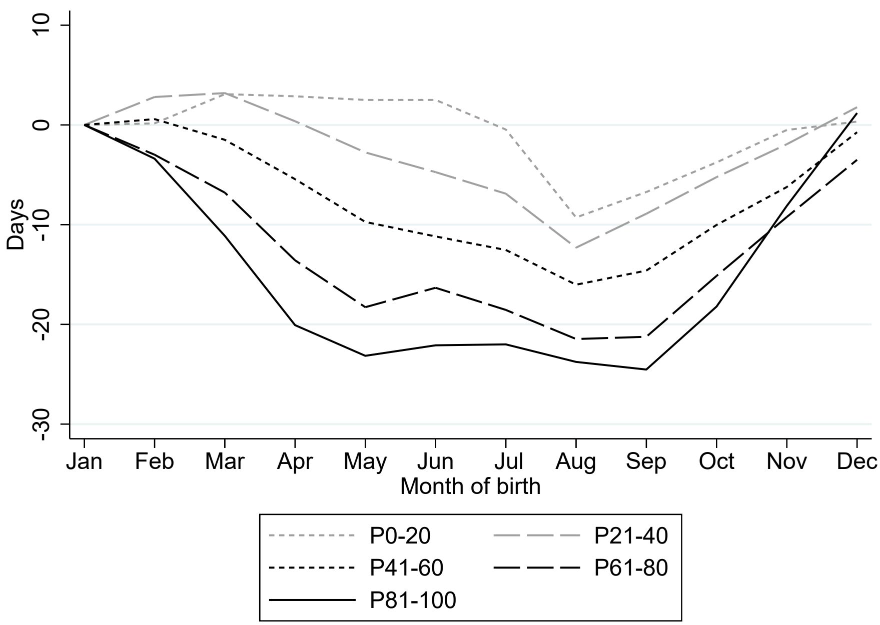
Panel A of Figure 3.5 shows a different response to the seasonality in childcare supply by SES of the household. In particular, the share of enrollments in August or January increases with SES.[32] For high SES children (black solid line), a majority are enrolled during high supply and this is relatively constant across birth month. For low SES (gray dotted line), the corresponding fraction is more variable, ranging between 30 and 60 percent. Panel B and C of Figure 3.5 shows that the highest share of childcare enrollment in August and January respectively, is comparable across SES. However, this refers to children born at different

[32]The same pattern is observed for the number of children enrolled, by SES (Appendix figure F2). See also births and enrollment by quintiles of mother's earnings in Appendix Figure F2a.

times of the year, suggesting different margins at which parents are willing to adjust childcare enrollment to peaks in supply. For high SES children, the share of enrollments in August is highest among children born in March while the corresponding birth month for low SES is July, a difference of four months. Similarly, for high SES children the share of enrollments in January is highest among children born in September while the corresponding birth month for low SES is January. The peaks for high SES are also wider, reflecting the higher average percentage of enrollments in August and January (55.4% compared to 41.7%).[33]

Figure 3.6 shows heterogeneity in the seasonality of average age at childcare enrollment, corresponding to Panel A of Figure 3.3. The age is expressed in days, normalized to January for each quintile.

Figure 3.6: Age at enrollment, heterogeneous effects



Note: Mean age at childcare enrollment, in days (normalized to January). Quintiles refer to the pre-birth earnings of the mother, measured as the highest earnings the two years before birth of child.

Figure 3.6 shows that the seasonal variation in average age at childcare

[33] See Appendix Table F1.

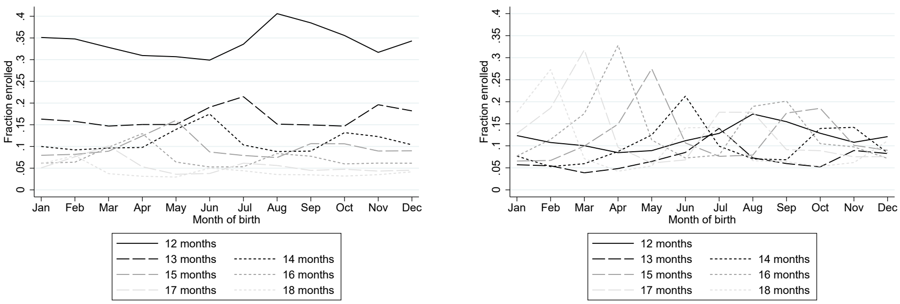
enrollment differs substantially with household SES, measured as the earnings of the mother. There are differences in the range of average age at childcare enrollment across birth month as well as the range of months when the age is low (relative to January). For low SES, the age at childcare enrollment is fairly constant for the first 6 months and drops in August. For these children, the greatest difference in age at childcare enrollment, about 10 days, is observed comparing births in August to those in June. There is a drop observed also for higher SES children, but this is both earlier in the year and increasingly wide and steep for higher quintiles. For the top SES, the average age at childcare enrollment drops already for children born in the spring. High SES children born in February are about 20 days older at childcare enrollment than high SES children born in May; however, for children of low earning mothers, there is no difference in average age at childcare enrollment for these months.

The small but distinct drop in the age at childcare enrollment for low SES children is consistent with the high enrollments in August for low SES children born in the summer. Similarly, the drop for high SES children is in line with the high enrollments in August for these children, born in the spring. The persistence of the drop in average age at childcare enrollment for high SES children is explained by declining enrollments in August being counteracted by an increasing number of children enrolling later, especially in January.

Figure 3.7 shows the seasonality in age group, corresponding to Figure 3.4, for the bottom 20 percent (Panel A) and the top 20 percent (Panel B) of the distribution of maternal earnings.

Figure 3.7: Enrollment by age group

(a) Low SES **(b)** High SES



Note: Share of children enrolled in childcare at certain ages over birth month. Low SES (left) refers to bottom 20 percent, High SES (right) refers to top 20 percent. Quintiles refer to the pre-birth earnings of the mother, measured as the highest earnings the two years before birth.

Comparing the age at childcare enrollment in Panel A and B of Figure 3.7, there is more variation across birth month for the latter (i.e., children of high SES).[34] This is true also for enrollment at ages 19–21 months, but the highest ages remain relatively flat across month of birth irrespective of SES (see Appendix Figure F4).

Panel A of Figure 3.7 shows that many of the low SES children are enrolled in childcare irrespective of the implied month of childcare enrollment. The age at childcare enrollment is relatively constant; the most variable is the percentage of children enrolled at 12 months (black solid line), although this is small in relative terms. The minimum share of childcare enrollment is steadily decreasing with age group, suggesting an increased, albeit modest, seasonality for higher ages. While Figure 3.4 showed that enrollment in August was especially common among children born in April, a majority of low SES children born in April enroll before the summer, at ages 12–14 months. Another discrepancy to Figure 3.4 is that for low SES households, the importance of childcare enrollments in January and August are similar, while childcare enrollments in August was dominating for the average child.

Panel B of Figure 3.7 shows that children from high SES households are typically enrolled during high supply. The seasonality is similar to the

[34]Appendix Figure F3 shows that there is no correspondence between predicted and actual values for either low or high SES, suggesting that observed seasonality is not driven by household covariates.

total presented in Figure 3.4, but with even greater variation in enrollment percentage across birth month, up to 25 percentage points. The highest peak, for children born in April enrolling in childcare at 16 months, is 33 percent. Thus, every third high SES child born in April, is enrolled in childcare in August. For children born in April, the share enrolled in childcare at 16 months is also 6.5 times higher than the share who enroll at 13 months (in May) or at 18 months (in October). Although high SES children are often enrolled in childcare at older ages, a non-negligible percentage, about 10 percent, are enrolled at 12 months irrespective of the birth month. Enrollment at 12 months peaks for births in August (17%), but it is more common for children born this month to enroll instead in January (as indicated by the ages 16–17 months).

The heterogeneity analysis shows that seasonality in childcare enrollment differs with respect to household SES. The pattern is nearly identical for other measures of SES. See Appendix for different measures of income (Figure F5), education (Figure F6), immigrant status and cohabitation (Figure F7).[35] Although all measures are strongly correlated, the somewhat higher heterogeneity by SES indicated by income suggests that financial resources are important. To a large extent, children from high SES households enroll in childcare in August or January, when childcare supply is presumably high. Children from low SES households predominantly enroll in August or January when this corresponds to relatively low ages.[36] The behavior of low SES households reveal a relatively low and inflexible counterfactual age at childcare enrollment. The differences in propensity to enroll during high supply can be explained by differences in preferences or financial constraints. For children born in August or January, there are no financial costs associated with adjusting the date of childcare enrollment to the sharp peak in supply at 12 months. Therefore, the similar absolute increase in the share of enrollments at 12 months suggests that absent financial constraints, also low SES households choose to enroll during high supply. Thus, it seems that the observed differences with respect to SES are at least partially capturing differences in the *constraint* to adjust childcare enrollment to accommodate the seasonality in

[35] See also Appendix Tables F2-F4 for heterogeneity with respect to the average share of enrollments in August or January, by different measures.

[36] In addition to the heterogeneity in seasonality of age at childcare enrollment, there are level differences in the average age at childcare enrollment with respect to SES in my sample. See Appendix Section 3.G for more on this.

childcare supply. Differences by financial constraints are important from a welfare perspective since enrollment during high supply is expected to improve the match between household preferences and childcare provider. Consequently, low SES households are likely offered childcare that is less aligned with preferences regarding, for example, quality, location, and pedagogy.

3.7 Household implications

Although enrollment during high supply coincides with the optimal age at childcare enrollment for some households, the majority of households enrolled in August or January have presumably adjusted the age at childcare enrollment relative to their counterfactual age. This nudge–i.e., shifting the age at childcare enrollment by a couple of months–may have consequences. Therefore, in this section, I characterize the seasonal variation for short- and medium-term household outcomes. If the seasonality in household outcomes is consistent with the observed seasonality in age at childcare enrollment, this indicates that households are being affected by the non-constant supply of childcare.[37] However given that this is purely correlational, any findings are merely exploratory, stressing that further analysis is warranted.

Because the average age effect is a mix of older and younger ages at childcare enrollment, household implications are expected to be modest. It is not clear what heterogeneity to expect with respect to maternal earnings beforehand; while the seasonality in age at childcare enrollment is more pronounced for high SES, it is possible that the response is different depending on SES due to either preferences or constraints.

[37] As household outcomes are correlated with household covariates, I present regression estimates in the main text to control for measurable characteristics. The included covariates are parity of the child and for each parent's labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self-employment as well as indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level.

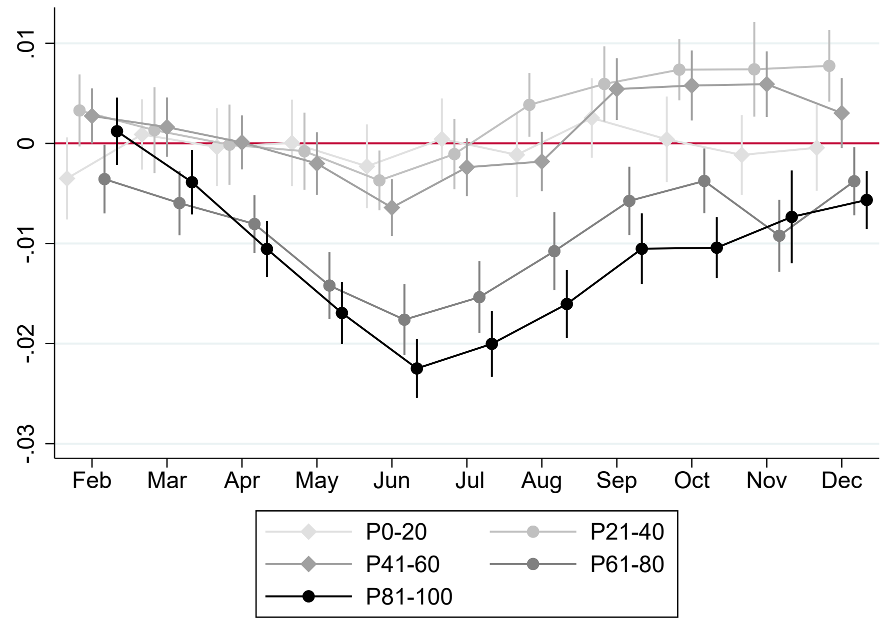
3.7.1 Division of parental leave

The previous literature has shown that there is a positive correlation between age at childcare enrollment and parental leave of fathers (e.g., Duvander and Viklund, 2017). However, since both age at enrollment and division of parental leave are endogenous decisions by the households, it is difficult to disentangle causality. Here, I examine how the parental leave uptake of fathers differs with the seasonal variation in the age at childcare enrollment.

Figure 3.8 presents the seasonal variation in parental leave division by quintiles of maternal earnings. Division of parental leave is expressed as the father's share of all days before childcare enrollment, including unpaid days of leave.[38]

[38]To determine the division of total parental leave, I extend the episodes of paid benefits so that they together cover the entire period until childcare enrollment. Unpaid leave is allocated according to the relative division of paid leave for adjacent episodes.

Figure 3.8: Father's ratio of parental leave before childcare enrollment



Note: Regression estimates for the fathers' share of the total parental leave uptake before childcare enrollment, including unpaid days, by birth month. The covariates include parity of the child and for each parent: labor earnings, age, education (4 levels), sector of employment and indicators for immigration and self-employment. In addition, indicators of parental cohabitation and marriage, cohort by municipality fixed effects and standard errors clustered at the municipal level. Quintiles refer to the pre-birth earnings of the mother.

Figure 3.8 shows that the seasonal variation in the division of parental leave before childcare enrollment differs by SES of the household. For low SES households, in particular the bottom quintile, the ratio is relatively constant. For the top two quintiles, the division varies across birth month; there is a declining percentage of parental leave by fathers throughout the spring, reaching the minimum in June. For top SES fathers, the share of the combined parental leave varies by 2.3 percentage points across birth month. In relative terms, this means that fathers whose child is born in January take on average 10.8 percent more of the shared parental leave before childcare enrollment than fathers whose child is born in June (see the division of parental leave in levels in Appendix Figure H1). The variation in high SES fathers' ratio corresponds to 11 days of parental leave, which is half of the variation in parental leave of both parents combined (i.e., age at childcare enrollment).

The seasonality for high SES in Figure 3.8 is similar to the seasonality in age at childcare enrollment for high SES children observed in Figure 3.6. However, age drops earlier in the spring and remains at constant low levels before increasing again after the summer. Thus, although the division of parental leave is possibly affected by the age at childcare enrollment for high SES households, the relationship is not linear with respect to age. Nonlinearity means that the response of fathers, relative to mothers, differs with the age implied by enrollment during high supply.[39]

The seasonality in Figure 3.8 appears to be driven by variation in parental leave uptake of fathers, which is relatively high; the seasonal variation in the probability of fathers' parental leave exceeding the quota of 60 days is nearly identical.[40] There is seasonality also in the probability of fathers taking *any* leave before childcare enrollment, but this is weaker and small relative to the high extensive margin.[41] The seasonality is also similar when restricted to paid days of parental leave before childcare enrollment (Panel A of Appendix Figure H4), suggesting that the seasonality affects division of paid and unpaid leave similarly. Further, it appears that some of the seasonal variation in the division of parental leave before childcare enrollment is due to changes to the timing of the parental leave, as indicated by a weaker seasonality for the division of total days of benefit (by age eight).[42]

3.7.2 Medium-term household outcomes

It is possible that the age at childcare enrollment affects the household also in a longer time perspective. Previous literature has shown that parents

[39]When I examine the seasonal variation in the days of parental leave by each parent, I find that the seasonality observed for the division is driven by a stronger response among fathers (see Appendix Figure H2). The uptake of fathers appears to be mostly affected for medium-high ages at childcare enrollment; for children born in the spring, the variation appears to be driven by enrollments in August (as seen in Panel B of Figure 3.5) but less affected by the counteracting effect from enrollments in January.

[40]This variation is also substantial at 10 percentage points relative to the minimum of 51.5 percent (see Panel A of Appendix Figure H3).

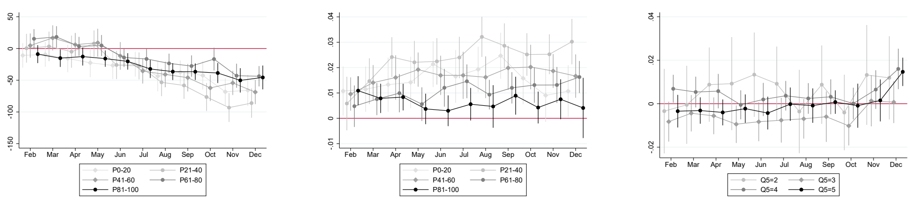
[41]The extensive margin is at its minimum of 80 percent for high SES children born in June (Panel B of Appendix Figure H3).

[42]The variation in parental leave by age eight is 1.5 percentage points, relative to a minimum value of 25.1 percent. See Panel B of Appendix Figure H4.

adjust the age difference between siblings to financial circumstances (Lalive and Zweimuller, 2009; Ginja et al., 2020). Therefore, the seasonal variation in the age at childcare enrollment can affect the age difference to a younger sibling, via the implied timing of the labor market return and the amount of time parents work between the siblings. Another potential implication relates to the financial costs due to delayed childcare enrollment. Although higher age at childcare enrollment requires parental leave to be longer, the number of paid days of parental leave benefits is fixed. Thus, enrollment at higher ages implies a potentially problematic reduction of the household income during parental leave, and financial distress may lead to conflict and marital instability (Dew et al., 2012). Delayed enrollment also extends the absence from work with potential consequences in terms of career trajectory. While causal effects of parental leave duration are typically measured using implementation of a reform that affects everyone (e.g., Ekberg et al., 2012), the variation due to seasonality of childcare enrollment is more likely to have a signaling value as the average parental leave uptake is unaffected (Duvander and Evertsson, 2011). The effects on the career trajectory is evaluated as labor earnings after birth relative to before birth of the child. Because the extra time on parental leave can be used by either parent, I assess the total parental earnings.

Figure 3.9: Medium term household implications

(a) Age difference to younger sibling **(b)** Cohabiting relative to the child's birth year **(c)** Household income in 3 years



Note: Regression estimates by birth month. Age difference to the younger sibling in days (left) and cohabiting three years after birth (middle). Cohabiting is proxied using the status of the household provided by Statistics Sweden. Total parental earnings in the third year following birth (right). Parental earnings are normalized with the highest (total) labor earnings two years before birth. Bottom quintile is presented on left y-axis. The covariates include parity of the child and for each parent: labor earnings, age, education (4 levels), sector of employment and indicators for immigration and self-employment as well as indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level. Quintiles refer to the pre-birth earnings of the mother.

Figure 3.9 shows that there is no correspondence between the seasonality of childcare supply and the medium-term household outcomes. The age difference to the younger sibling is steadily decreasing across month of birth (Panel A of Figure 3.9).[43] Panel B of Figure 3.9 shows that cohabitation three years after birth is relatively stable.[44] Similarly, trends for total earnings three years after birth are relatively stable (Panel C of Figure 3.9).[45]

Although averages combine both increased and decreased ages, separation and career trajectory is expected to be affected primarily by delays in childcare enrollment. The restriction in childcare supply is especially binding before the summer as few children are enrolled during the typical vacation period. Therefore, any negative effects from delayed enrollment, in particular financial distress and damages to the career, should be especially visible here, but there is no discontinuity observed in Figure 3.9. Thus, the tentative analysis shows no indication of implications for the household in the medium-term.

3.8 Conclusion

In this paper, I characterize the seasonal variation in childcare enrollments for children born in Sweden between 2002 and 2015. Because enrollment data is unavailable, I estimate the date of childcare enrollment using detailed data of paid benefits of parental leave. The analysis shows that there is seasonal variation in childcare enrollments that is consistent with expected variations in the childcare supply implied by the institutional structure of the childcare system in Sweden.

I find that the seasonality in childcare enrollment at certain ages is pronounced, suggesting that many households are willing to adjust the age at childcare enrollment by several months to enroll during high supply. In fact, few children are enrolled in childcare above 12 months unless it corresponds to enrollment in either August or January. Households with children enrolled at 12 months are less susceptible to nudges, suggesting that the gains from enrollment during high supply does not outweigh the

[43]There is a linear negative trend also for the probability of having a younger sibling (see Panel A in Appendix Figure H5.)

[44]Cohabitation after eight years is also stable (see Panel B of Appendix Figure H5).

[45]Also, earnings after eight years is stable (see Panel C of Appendix Figure H5).

cost of delayed enrollment for these households.

The heterogeneity analysis reveals differences in the enrollment behavior with respect to socioeconomic status. A majority of children from high SES households enroll in either August or January, which is reflected by a strong seasonal variation in the age at childcare enrollment. Also children from low SES households enroll during high supply but foremost when this corresponds to a relatively young age. The observed heterogeneity appears to be at least partially driven by differences in the financial capacity to afford delayed childcare enrollment. Given that the match between childcare providers and households is presumably worse when supply is scarce, these differences may reinforce the unequal conditions with respect to SES overall. Therefore, a policy recommendation from this paper is to improve on the continuity in childcare supply, ensuring that all children have the same opportunity to be matched with a suitable childcare provider.

For children who are enrolled in childcare during high supply, the implied age at enrollment varies by date of birth. Consequently, there is variation in the age at enrollment for households that are otherwise comparable, and this could have substantial effects on child outcomes and the household at large. However, because effects on the age at childcare enrollment due to seasonality in childcare supply are not monotone, consequences are difficult to capture. I find no effects on fertility, career trajectory, or marital stability, but there appears to be an effect on the parental leave uptake of high SES fathers. Although suggestive, these findings are consistent with an interpretation of fathers taking more parental leave when the child is enrolled in childcare at an older age. The previous literature suggests that increased involvement of fathers during infancy is potentially important as there can be effects on child outcomes (Cools et al., 2015), fertility (Farré and González, 2019), marital stability (Avdic and Karimi, 2018), and labor market attachment (Patnaik, 2019). Therefore, the consequences of seasonality in childcare supply should be analyzed further to identify possible causal effects.

A future extension of this paper would be to include child outcomes. The literature suggests that shifting the age at childcare enrollment by a single month can substantially affect the human capital formation of children. Drange and Havnes (2019) found that starting childcare one month earlier in Norway improves test scores for under-performing children by 0.05 and 0.03 SD in language and mathematics, respectively. Meanwhile, Fort et al. (2020) found that one month earlier childcare enrollment in a relatively affluent population in Italy decreases the IQ of girls by 4.7 percent of a SD at ages 8–14, and this negative impact increases with household income. The opposite effects of alternative modes of care by SES implies that there is possible seasonality also in terms of differences in children's human capital by SES. If so, the difference should be minimized for children born in the summer as they on average enroll at younger ages.

Although this paper is purely descriptive, it characterizes a household behavior that is largely neglected in the literature, yet affects most households and therefore is potentially important. Future studies should capture causal effects using actual enrollment data.

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3.A Technical details

The raw data is reported as sub-episodes and episodes by recipient and child, estimated by the Social insurance office. Some episodes are very close in time, they can even be parallel or overlapping when benefits are paid to different recipients. I collapse all episodes during the first year, before childcare enrollment is possible, as well as the episodes that are no more than 4 days apart. When the gap between episodes is greater, I condition on the number of days in the episode relative to the length of the episode, as well as the gap to the previous episode:

$$\frac{paid\,days\,in\,episode}{length\,episode + gap\,to\,previous} \tag{3.1}$$

Paid days in the episode refers to net days. A ratio of 4/7, corresponding to an average of 4 paid days per week, is collapsed with the previous episode. A ratio of 3/7 is supplemented with the condition of the days not being allocated to the weekend. This procedure brings the data from parent-child to child-level-data, and is repeated to deal with the newly created episodes. It is possible to repeat further, but episodes become more complex each time. Instead, I do one last collapse where I deal with the remaining overlapping/parallel episodes only.

To determine when parental leave ends, I make use of the ratio again to indicate childcare enrollment when parents together use no more than two days, net of weekends, for at least 6 weeks. A weekend consists of 1-3 days of benefits, with a majority of days allocated to the weekend. For the first episode, which by construction covers the entire first year, I also calculate the ratio of the time exceeding the first year and use the larger ratio of paid benefits.

3.B Sample descriptives

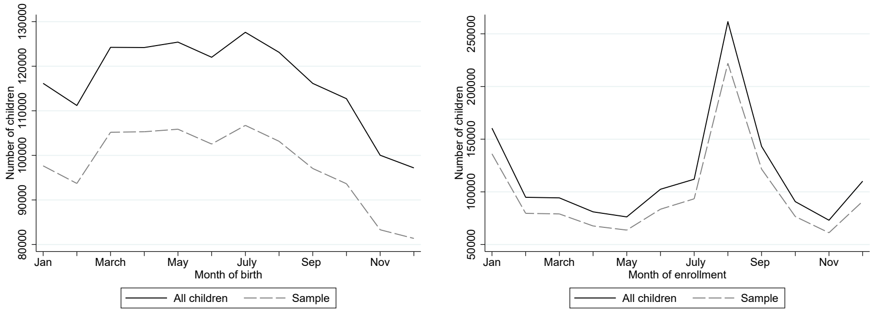
Table B1: Mean differences of sample and excluded households

	(1) Sample	(2) Removed	(3) Difference
First parity	0.391	0.397	0.006***
	(0.488)	(0.489)	(0.001)
Cohabiting	0.917	0.975	0.058***
	(0.329)	(0.610)	(0.001)
Age father	33.491	34.751	1.260***
	(6.200)	(5.982)	(0.014)
Age mother	30.618	31.838	1.220***
	(5.139)	(4.899)	(0.011)
Immigrant father	0.212	0.194	-0.018***
	(0.408)	(0.395)	(0.001)
Immigrant mother	0.206	0.211	0.005***
	(0.405)	(0.408)	(0.001)
High education father	0.306	0.434	0.128***
	(0.461)	(0.496)	(0.001)
High education mother	0.411	0.559	0.148***
	(0.492)	(0.497)	(0.001)
Private sector father	0.702	0.678	-0.024***
	(0.457)	(0.467)	(0.001)
Private sector mother	0.449	0.397	-0.052***
	(0.497)	(0.489)	(0.001)
High earnings father	0.308	0.420	0.112***
	(0.461)	(0.493)	(0.001)
High earnings mother	0.291	0.321	0.030***
	(0.454)	(0.467)	(0.001)
Low earnings father	0.087	0.081	-0.006***
	(0.281)	(0.273)	(0.001)
Low earnings mother	0.115	0.153	0.038***
	(0.319)	(0.360)	(0.001)
HH earnings pre birth	8.120	8.225	0.105***
	(0.868)	(0.832)	(0.002)
Low PL benefit mother	0.158	0.166	0.008***
	(0.365)	(0.372)	(0.001)
Age at enrollment	488.862	588.364	99.503***
	(98.021)	(271.592)	(0.580)
Enrollment Aug/Jan	0.503	0.492	-0.010***
	(0.500)	(0.500)	(0.001)
Observations	1,175,560	224,486	1,400,046

Notes:T-test for the sample (column 1) and the removed observations (column) for which the date at childcare enrollment cannot be determined. The difference (column 2-column 1) is presented in column 3, *** p<0.01, ** p<0.05, * p<0.1

Figure B1: Comparison full and robust sample

(a) Month of birth **(b)** Month of enrollment



Notes:The total number of children born in Sweden 2002-2015 (black solid line) and in the sample (gray dashed) for which the date at childcare enrollment can be determined. The left figure shows the frequencies by month of birth, the right figure shows the frequencies by month of enrollment.

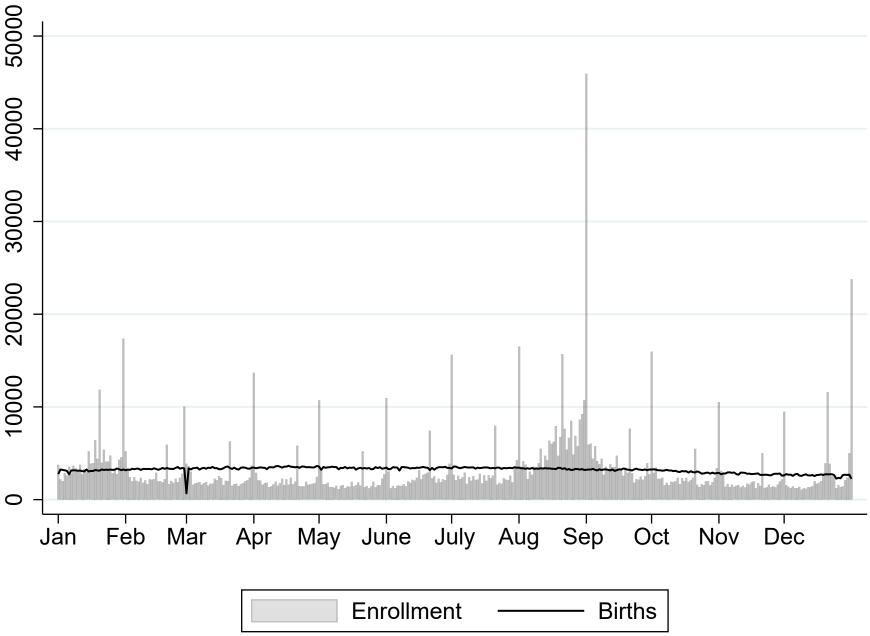
Table B2: Averages of adjusted age measures

	(1) Mean	(2) Mean
	Raw	Adjusted
Average age (days)	494	
Age (months)		
12	0.194	0.175
13-14	0.223	0.223
15-16	0.238	0.237
17-18	0.185	0.191
19-20	0.091	0.097
21-22	0.039	0.043
23-24	0.017	0.019
25+	0.013	0.015

Notes:Adjusted fractions in Column 2 use the estimated age at enrollment, when enrollment during holiday season is shifted to August or January 16th.

3.C Seasonality of childcare enrollment

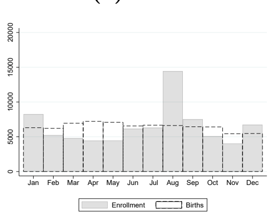
Figure C1: Timing of childcare enrollment



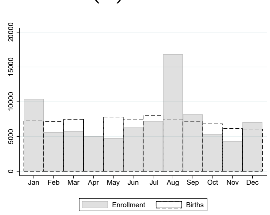
Note: The bars show the number of children by date of childcare enrollment. Each bar corresponds to one day. The horizontal line shows the number of children born each date. There is a spike at the 20th and the last of each month, caused by natural breaking points in the application process.

Figure C2: Timing of childcare enrollment

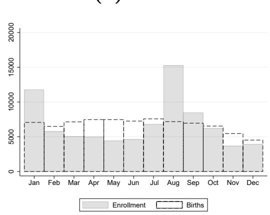
(a) 2002



(b) 2008



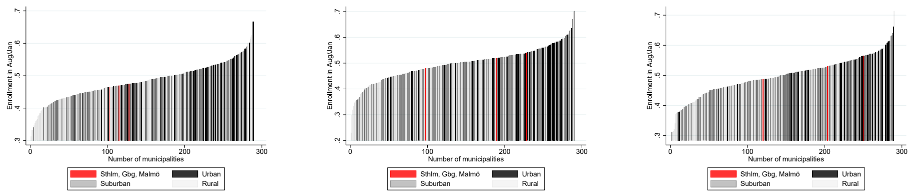
(c) 2015



Note: The gray bars show the number of children enrolled in childcare each month. The white bars show the number of children born each month. For cohorts 2002 (left), 2008 (middle) and 2015(right).

Figure C3: Enrollment in August of January

(a) 2002 **(b)** 2008 **(c)** 2015

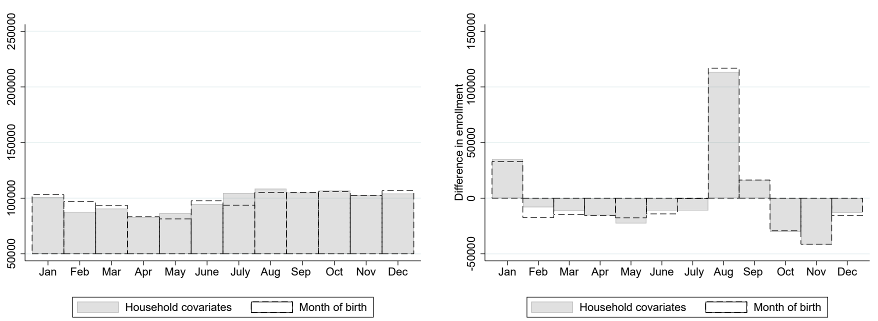


Note: The bars indicate the fraction of households whose child is enrolled in August or January, by municipality. The categories are based on the definition of Statistics Sweden. For cohorts 2002 (left), 2008 (middle) and 2015(right).

3.D Seasonality of birth

Figure D1: Predicted timing of childcare enrollment

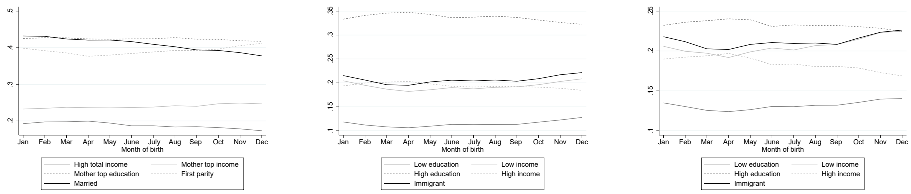
(a) Frequencies **(b)** Difference relative to actual enrollment



Note: The left panel show frequencies as predicted by the full set of household covariates (gray bars) and birthrate in month m-5 (white bars). The right panel shows the corresponding difference in estimated and predicted number of children. Each bar corresponds to one month.

Figure D2: Seasonality in characteristics

(a) Household **(b)** Mothers **(c)** Fathers

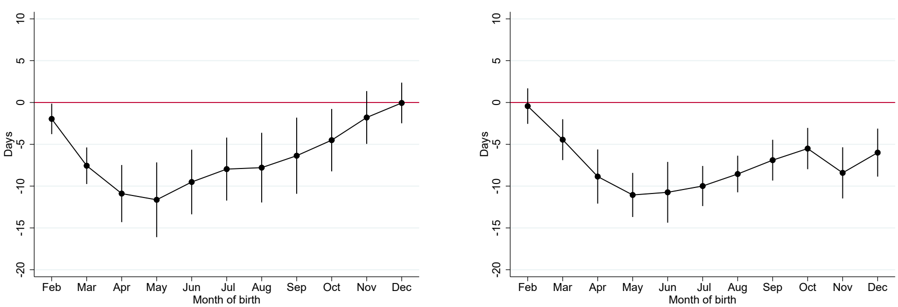


Note: Fraction of households (left), mothers (middle) and fathers (right) with characteristics, by month of birth. High/low income refers to the top/bottom 20% of pre-birth earnings, measured as the highest earnings the 2 years before birth. Low education is a maximum of 9 years of schooling, high education is 3 years of university or more. Immigrant refers to being born outside Sweden, married is the marital status.

3.E Age

Figure E1: Age at childcare enrollment in subgroups

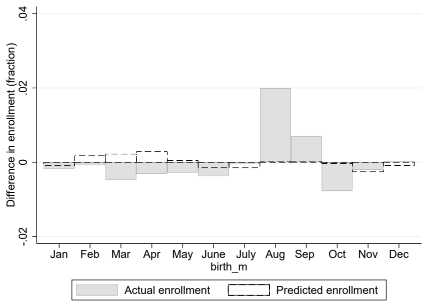
(a) Urban municipalities **(b)** Swedish born or higher parity parents



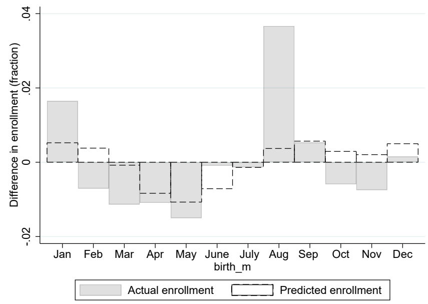
Note: The figures display interaction terms for different subgroups. Urban municipalities (left) are distinguished using the definition provided by Statistics Sweden. Swedish born or higher parity (right) requires at least one parent to be born in Sweden, or the child to have an older sibling. All regressions include the full set of covariates: Parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level.

Figure E2: Childcare enrollment in subgroups

(a) Urban municipalities



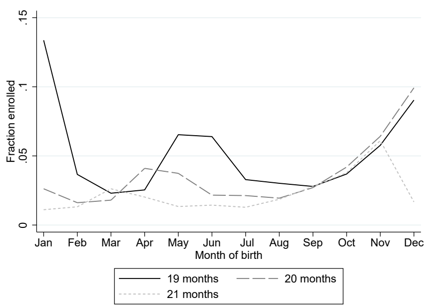
(b) Well informed parents



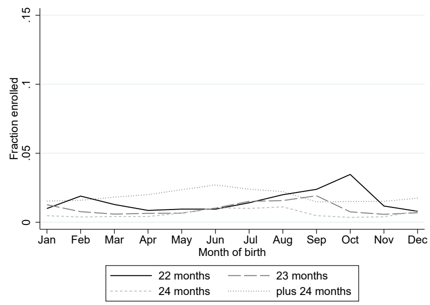
Note: Urban municipalities (left) are distinguished using the definition provided by Statistics Sweden. Well informed parents (right) implies that either a parent is born in Sweden, or that the child have an older sibling. The bars capture number of actual enrollments (gray) and as predicted using the full set of household covariates (white), relative to the total number of children in the subgroup.

Figure E3: Grouped age at childcare enrollment, 20 months and older

(a) Medium ages



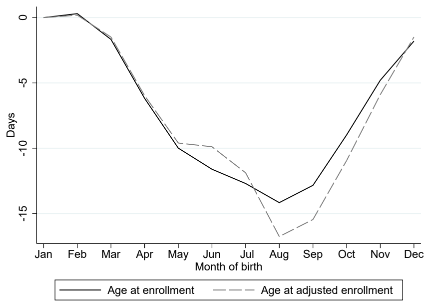
(b) High ages



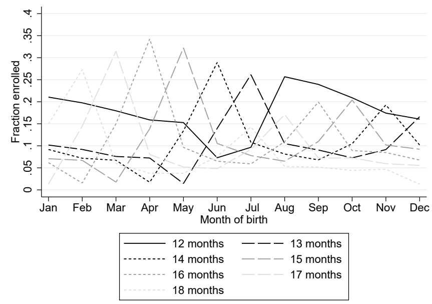
Note: Fraction of children enrolled in childcare at certain ages, over month of birth. 19-21 months (left) and 22 months and older (right), raw averages.

Figure E4: Age at adjusted childcare enrollment

(a) Average age



(b) Age grouped

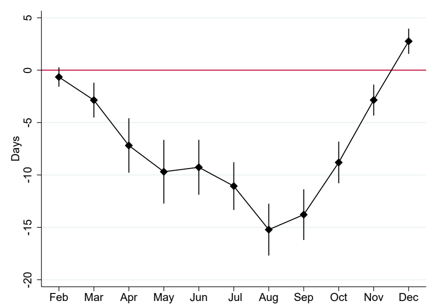


Notes: Age at childcare enrollment. In the left figure, black solid line use the estimated date of childcare enrollment. All other ages are based on the adjusted date of childcare enrollment, using August 16th for observed enrollments between June 21st and September 7th; January 16th for observed enrollments between December 21st and February 7th.

Figure E5: Age at childcare enrollment, normalized to January

(a) Raw average

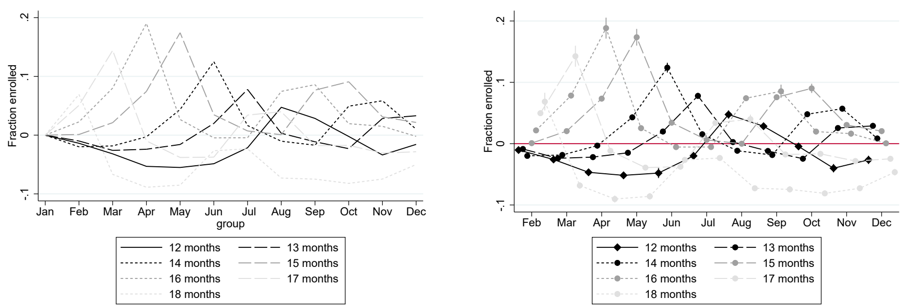
(b) Regression estimates



Note:The average age at childcare enrollment. Days of parental leave, including unpaid leave. Raw average (left) normalized to January and regression estimates (left). The covariates include parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level.

Figure E6: Grouped age at childcare enrollment

(a) Normalized means **(b)** Regression estimates

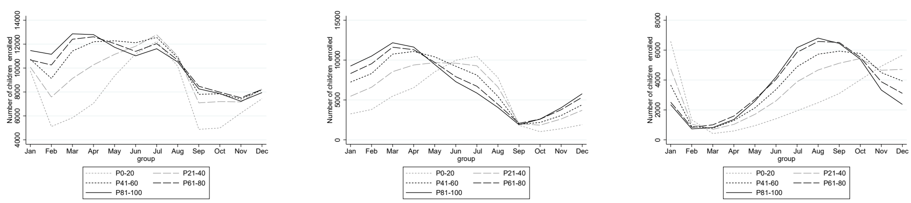


Note: Fraction of children enrolled in childcare at certain ages, over month of birth. Raw average (left) normalized to January and regression estimates (left). The covariates include parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level.

3.F Heterogeneity

Figure F1: Number of children enrolled during high supply

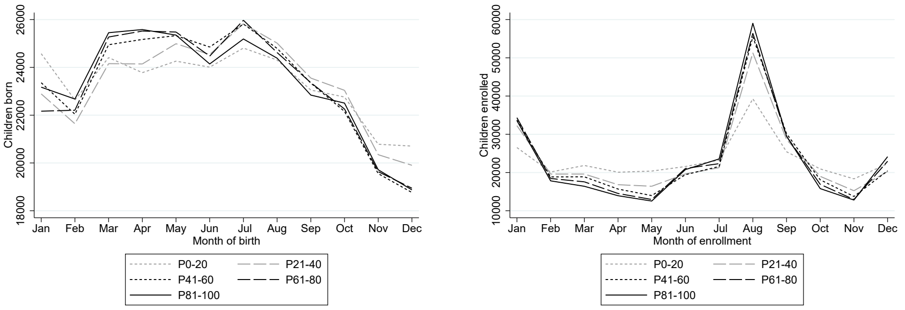
(a) Both **(b)** August **(c)** January



Note: The number of children enrolled before age 2 in August or January (left), August (middle), January (right), by birth month. August includes observed enrollments between June 21 and September 7; January includes observed enrollments between December 21 and February 7. Quintiles refer to the pre-birth earnings of the mother, measured as the highest earnings the two years before birth of child.

Figure F2: Children by SES

(a) Children born **(b)** Children enrolled



Note: The number of children born (left) and enrolled before age 2 (right), by month. Quintiles refer to the pre-birth earnings of the mother, measured as the highest earnings the two years before birth of child.

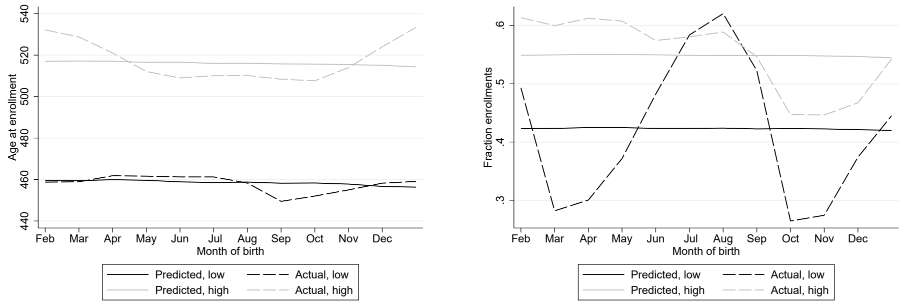
Table F1: Averages of central measures

	(1) P0–20	(2) P21–40	(3) P41–60	(4) P61–80	(5) P81–100
Age at enrollment (days)	456.120	484.761	497.025	510.444	514.629
Enrollment in Aug/Jan	0.417	0.480	0.504	0.526	0.554
12 months	0.343	0.236	0.184	0.140	0.119
13 months	0.170	0.116	0.102	0.088	0.073
14 months	0.111	0.116	0.117	0.111	0.104
15 months	0.097	0.112	0.119	0.122	0.125
16 months	0.071	0.102	0.114	0.126	0.139
17 months	0.053	0.083	0.098	0.112	0.130
18 months	0.041	0.066	0.077	0.088	0.103
19 months	0.029	0.044	0.052	0.058	0.063
20 months	0.019	0.031	0.036	0.041	0.042
21 months	0.013	0.021	0.023	0.027	0.025
21+ months	0.027	0.043	0.048*	0.056	0.047

Notes: Estimates from separate regressions. Quintiles refer to the pre-birth earnings of the mother, measured as the highest earnings the 2 years before birth.

Figure F3: Robustness: estimates relative to predictions

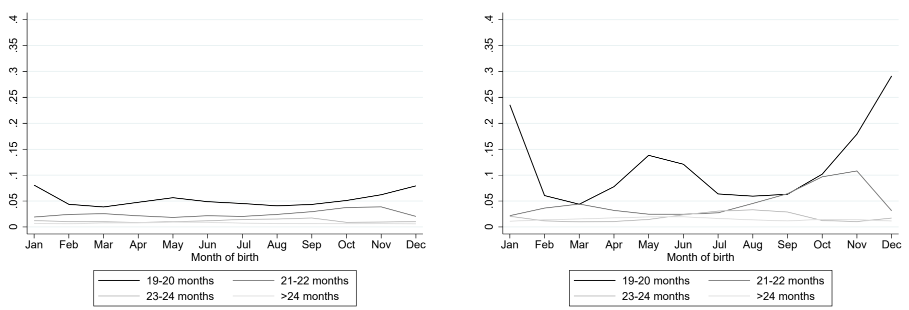
(a) Age at childcare enrollment **(b)** Fraction enrolled in August or January



Note: The left figure shows the average age at childcare enrollment in days. This is using the exact date of childcare enrollment. The right figure show the fraction of enrollments in August or January, before age 2. This is using the adjusted date of enrollment. August include observed enrollments between June 21st and September 7th; January include observed enrollments between December 21st and February 7th. Low/high refers to the bottom/top quintile of the pre-birth earnings of the mother. Predictions are based on the following: parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level.

Figure F4: Enrollment by age group

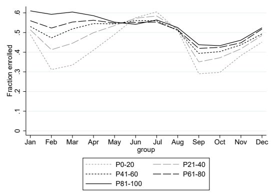
(a) Low SES **(b)** High SES



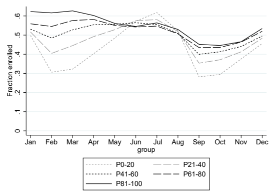
Note: Fraction of childcare enrollments by grouped age. Low SES (left) refers to bottom quintile of the pre-birth earnings of mothers, high SES (right) refers to the top quintile.

Figure F5: Fraction enrolled in August/January, by different income measures

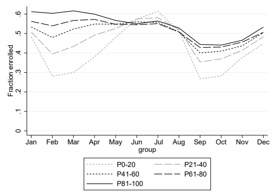
(a) Fathers earnings



(b) Total earnings



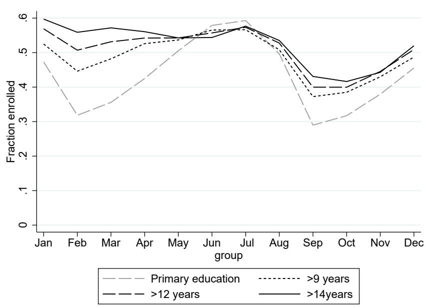
(c) Disposable income



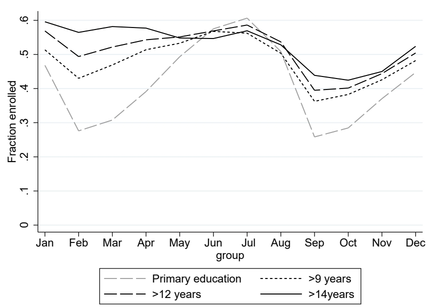
Note: Fraction of children enrolled in August and January respectively, using the exact date of enrollment. Quintiles refer to different income measures, before birth.

Figure F6: Fraction enrolled in August/January, by education

(a) Fathers



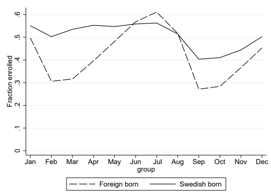
(b) Mother



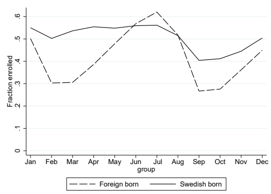
Note:Fraction of children enrolled in August and January respectively, using the exact date of enrollment.

Figure F7: Fraction enrolled in August/January, by different measures

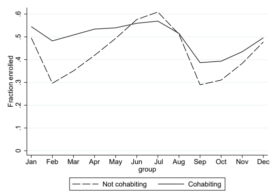
(a) Father immigrant



(b) Mother immigrant



(c) Cohabiting



Note: Fraction of children enrolled in August and January respectively, using the exact date of enrollment. Cohabiting is measured the year the child turns one.

Table F2: Enrollment in August or January, by different income measures

	(1) Earnings	(2) Earnings	(3) Earnings	(4) Disposable income
	Mother	Father	Total	Total
Q5=1	0.415	0.432	0.431	0.419
Q5=2	0.477	0.478	0.476	0.473
Q5=3	0.504	0.501	0.506	0.504
Q5=4	0.521	0.517	0.525	0.522
Q5=5	0.545	0.540	0.551	0.549

Notes: The fraction of enrollments in August or January, before age 2. This is using the adjusted date of enrollment. August include observed enrollments between June 21st and September 7th; January include observed enrollments between December 21st and February 7th.Earnings refers to labor market earnings, total earnings is the total parental earnings. Disposable income is the total disposable income reported by statistics Sweden.

Table F3: Enrollment in August or January, by education

	(1) Compulsory	(2) Upper secondary	(3) University <3 years	(4) University >=3 years
Education mother	0.419	0.482	0.512	0.532
Observations	130,857	481,002	147,919	386,571
Education father	0.435	0.489	0.511	0.528
Observations	152,111	576,723	158,660	270,040

Notes:The fraction of enrollments in August or January, before age 2. This is using the adjusted date of enrollment. August include observed enrollments between June 21st and September 7th; January include observed enrollments between December 21st and February 7th.Highest educational level of mothers and fathers the birth year of the child. Earnings refers to labor market earnings, total earnings is the total parental earnings. Disposable income is the total disposable income reported by statistics Sweden.

Table F4: Enrollment in August or January, by different measures

	(1) No	(2) Yes
Swedish mother	0.424	0.510
Observations	242,391	933,134
Swedish father 0.426	0.510	
Observations	248,835	926,690
Cohabiting	0.437	0.500
Observations	134,475	1,037,821

0.8 Notes: The fraction of enrollments in August or January, before age 2. This is using the adjusted date of enrollment. August include observed enrollments between June 21st and September 7th; January include observed enrollments between December 21st and February 7th.Swedish indicates birth in Sweden. Cohabiting refers to the year the child turns one.

3.G Heterogeneity in average age

I find that the average age at childcare enrollment is steadily increasing with SES, with the top quintiles almost overlapping (Appendix Table F1). Findings are similar for alternative income measures, using instead the earnings of the father, total earnings or disposable income. It is also robust to alternative measures of SES, including level of education and immigrant status (see Appendix Tables G1-G3 for quintile averages). Comparing the top and bottom, the difference is almost two months, but this ranges between 48 days in May to 75 days in December (Appendix Figure G1a). Thus, the average difference in age at enrollment by SES differs across month of birth, which is consistent with heterogeneity in the propensity to enroll in August or January. However, the removal of uncertain enrollment dates is not constant across SES, for the top and bottom about 19 percent of the households are excluded while the corresponding fraction is 14 percent for mid-SES households. Consequently, the level differences in age with respect to SES observed in my sample are not necessarily reflecting the true differences in average age for the full population. In fact, there are discrepancies relative to existing research.[46] There is however no

[46] Using an approach similar to Duvander and Viklund (2017), I impose more restrictions to analyze only the credible dates as this is crucial in my setting, while

indication of differences in exclusion across the year by SES (Appendix Figure G1b), thus this raise no concern regarding the heterogeneity in seasonal variation.

Table G1: Average age at enrollment by different income measures

	(1) Earnings	(2) Earnings	(3) Earnings	(4) Disposable income
	Mother	Father	Total	Total
Q5=1	457.988	464.957	462.946	458.010
Q5=2	486.053	485.476	486.254	485.135
Q5=3	499.038	501.549	501.427	503.073
Q5=4	515.565	511.097	518.376	515.438
Q5=5	516.993	516.564	520.047	517.040

Notes:Earnings refers to labor market earnings, total earnings is the total parental earnings. Disposable income is the total disposable income reported by statistics Sweden.

Table G2: Average age at enrollment by education

	(1) Compulsory	(2) Upper secondary	(3) University <3 years	(4) University >=3 years
Education mother	455.068	489.737	500.304	517.810
Observations	130,857	481,002	147,919	386,571
Education father	465.255	492.545	505.862	514.955
Observations	152,111	576,723	158,660	270,040

Notes: Highest educational level of mothers and fathers the birth year of the child. Earnings refers to labor market earnings, total earnings is the total parental earnings. Disposable income is the total disposable income reported by statistics Sweden.

they focus on the average age. They find the average age at enrollment to be highest for the 4th quintile and the highest difference to be one month. Hall and Lindahl (2018) find the complete reversed relationship with respect to education group, but similar variation.

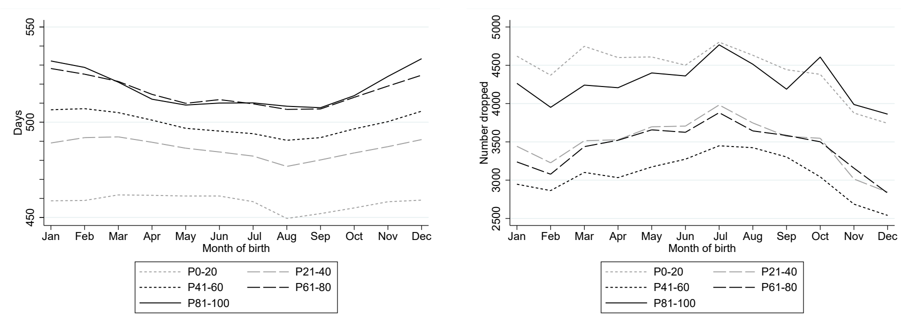
Table G3: Average age at enrollment, different measures

	(1) No	(2) Yes
Swedish mother	457.705	505.111
Observations	242,391	933,134
Swedish father	458.938	505.110
Observations	248,835	926,690
Married	496.099	494.237
Observations	693,575	481,950

Notes: Swedish indicates birth in Sweden. Married refers to marital status the birth year of the child.

Figure G1: Seasonality of enrollment, heterogeneous effects

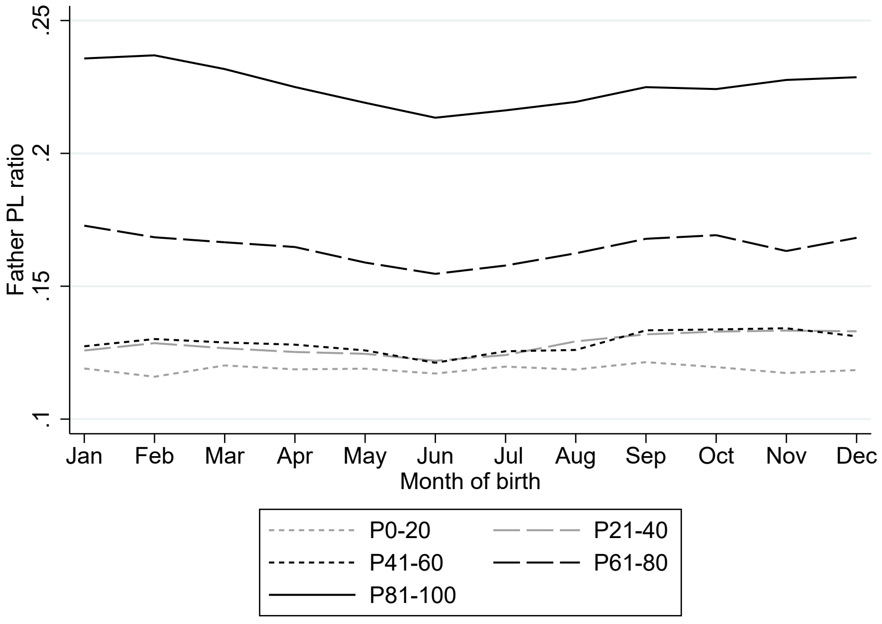
(a) Age at enrollment **(b)** Number droppped



Notes: Low SES (left) refers to bottom 20%, High SES (right) refers to top 20%. Quintiles refer to the pre-birth earnings of the mother, measured as the highest earnings the 2 years before birth.

3.H Implications

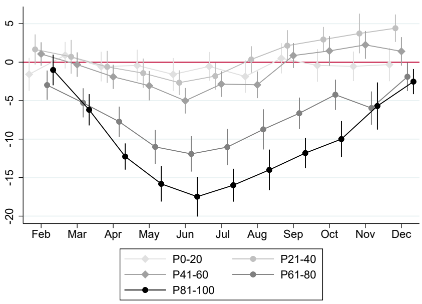
Figure H1: Father's ratio of parental leave before childcare enrollment



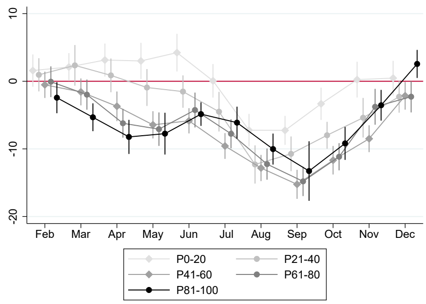
Notes: Raw averages of fathers fraction of parental leave before childcare enrollment, including unpaid days. Quintiles refer to the pre-birth earnings of the mother.

Figure H2: Days of parental leave benefits by parent

(a) Father's PL before enrollment



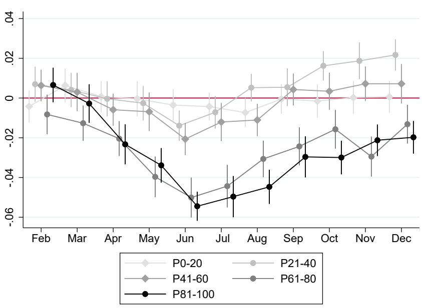
(b) Mother's PL before enrollment



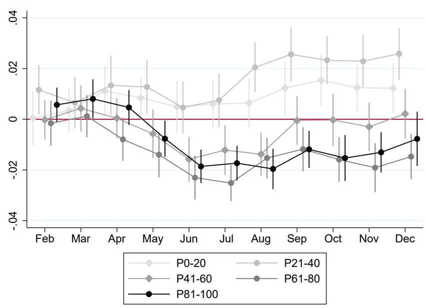
Notes: Regression estimates for days of paid and unpaid parental leave, before childcare enrollment for fathers(left) and mothers (right). By month of birth. The covariates include parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level. Quintiles refer to the pre-birth earnings of the mother.

Figure H3: Father's parental leave before enrollment

(a) More than 60 days of PL, regression estiate



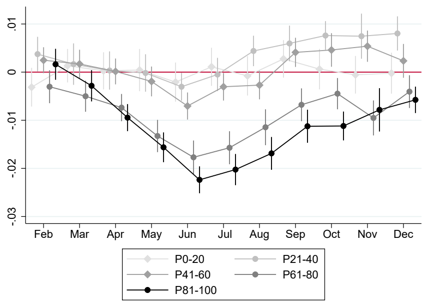
(b) Any PL



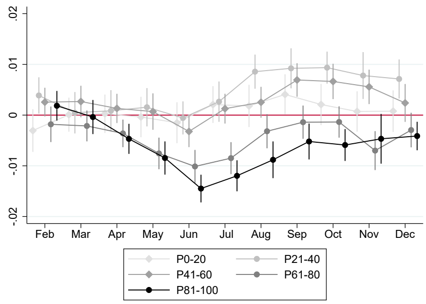
Notes: Regression estimates for the father's uptake exceeding zero days (left) and 60 days (right), before childcare enrollment. By month of birth. The covariates include parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level. Quintiles refer to the pre-birth earnings of the mother.

Figure H4: Fathers ratio of parental leave benefits

(a) Days by enrollment



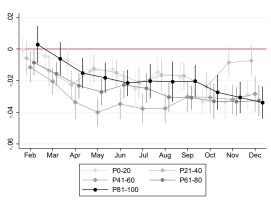
(b) Days by age 8



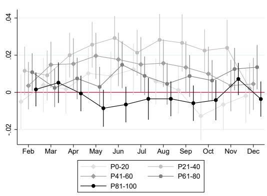
Notes: Regression estimates for the father's fraction of parental leave benefits excluding unpaid days before childcare enrollment (left) and by age 8 (right). By month of birth. The covariates include parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level. Quintiles refer to the pre-birth earnings of the mother.

Figure H5: Additional household outcomes

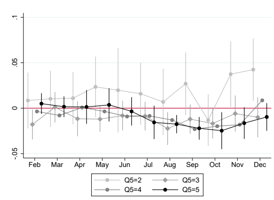
(a) Probability of younger sibling



(b) Cohabiting in 8 years



(c) Household earnings in 8 years



Notes:Regression estimates of having a younger sibling (left). Cohabiting 8 years after birth (middle), where cohabiting is proxied using the status of the household provided by statistics Sweden. Total parental earnings in 8 years (right), divided by total pre-birth earnings (measured as the highest total labor earnings 2 years before birth). For earnings, the bottom quintile is relatively variable and presented on the left y-axis. By month of birth. Because low SES contains low (zero) earnings, this group in more volatile and presented separately (left). The covariates include parity of the child and for each parent labor earnings, age, education (4 levels), sector of employment, and indicators for immigration and self employment. In addition, indicators of parental cohabitation and marriage. The estimations include cohort by municipality fixed effects and standard errors clustered at the municipal level. Quintiles refer to the pre-birth earnings of the mother.

Sammanfattning

I det första kapitlet, **Parental leave Quotas: Peer Effects and Workplace Related Costs**, studerar jag hur det ökade uttaget av föräldraledighet i och med den första och andra pappamånaden, bidrar till förändrat uttag av föräldraledighet bland andra fäder på arbetsplatsen. Analysen bygger på registerdata och effekter är estimerade i en s.k. Regression Discontinuity (RD) modell, där jag använder barns födelsemånad i förhållande till reformen som exogen variation. Jag finner inga effekter av introduktionen av pappamånaden, men att pappor vars kollega får en andra månad reserverad tar ut en del av sin pappaledighet, ca 7 dagar, tidigare. Detta är relevant då föräldraledighet vid en ung ålder sannolikt har större effekt på framtida fördelning av föräldraansvaret, jämfört med föräldraledighet efter inskolning på förskolan. Effekten av de två reformerna, bland fäder såväl som kollegor, skiljer sig inte signifikant mellan olika typer av arbetsplatser. Att pappors föräldraledighet inte påverkades av huruvida kollegor fick sitt barn innan eller efter första pappamånaden är i linje med att introduktionen uppfattades som mer påtvingad.

I det andra kapitlet, **Human Capital Effects of Opprtunities for One-on-one Time with Parents: Evidence from a Swedish Childcare Access Reform**, samskrivet med Anna Sjögren (IFAU), så tittar vi på hur ökade chanser till egentid med en förälder under spädbarnstiden, påverkar barnets kognitiva utveckling. Vi finner att ökad tillgång till egentid inte hade några signifikanta effekter generellt, men att pojkar gynnades, särskilt de med lågutbildade mammor. Vi ser även indikation på att flickor till högutbildade mammor verkar gynnas av reformen. Vi utforskar olika tänkbara mekanismer och den troliga orsaken är förbättrade möjligheter till anknytning mellan barn och förälder. Vi använder oss av den del av maxtaxereformen som 2002 gav äldre syskon tillgång till förskola minst 15 timmar i veckan, när deras föräldrar var föräldralediga. Innan 2002 varierade tillgången mellan kommuner och vi fokuserar på de kommuner där skillnaden i förskola var som störst mellan barn till föräldralediga och barn till arbetande föräldrar. Genom att jämföra barn med syskon i förskoleålder, med de som inte har det, födda innan och efter reformen, så fångar vi effekten av ökade möjligheter till egentid, i en s.k. difference-in-differences (DD) modell.

I det tredje kapitlet, **Seasonality of Childcare Enrollment**, undersöker jag kopplingen mellan födelsemånad och ålder vid inskolning. Trots platsgaranti så innebär det svenska förskolesystemet en tydlig säsongsvariation i antalet lediga platser, där majoriteten av platserna blir tillgängliga i augusti när äldre barn börjar skolan, följt av januari när åldersgrupper omstruktureras. Då datum för inskolning inte finns tillgängligt så estimerar jag det med hjälp av uttag av föräldrapenning. Jag ser att många barn börjar förskolan i augusti eller januari, vilket innebär att ålder vid inskolning varierar över födelsemånad. Detta gäller framförallt barn till föräldrar med högre socioekonomisk status. Eftersom betald föräldraledighet räcker ett år så medför en längre föräldraledighet lägre finansiell ersättning, vilket innebär att ekonomiska förutsättningar avgör möjligheten att vänta med inskolning tills en önskvärd plats blir ledig. Detta har potentiella konsekvenser för matchningen till förskola. Vidare kan föräldraledighetens längd ha konsekvenser för hushållet i termer av fördelning av föräldraledighet och ansvar, syskon och karriär, och tidigare forskning har visat att ålder vid inskolning är potentiellt viktigt för humankapitalutvecklingen hos barn.

Swedish Institute for Social Research

Dissertation Series

If not otherwise stated, the dissertation has been submitted at Stockholm University. The dissertations in the Swedish language contain an English summary.

1. Ante Farm (1986): *A Model of the Price Mechanism*

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1. Parental Leave Quotas: Peer effects and workplace related costs
2. Human Capital Effects of Opportunities for One-on-one Time with Parents: Evidence from a Swedish Childcare Access Reform
3. Seasonality of Childcare Enrollment

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ISBN 978-91-7911-962-1
ISSN 0283-8222

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