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Evidence from a Swedish childcare access reform

by

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Abstract

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 the reform. We find no robust overall effects on the children's 6th grade test scores, but we find evidence of positive effects on test scores for sons of less than university educated mothers and 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 and a lower probability of having a younger sibling may have reduced competition for parental time further and contributed to the improved test scores for girls.

Keywords: Human capital formation; Parental time investments; Cognitive development; Parental leave; Quasi-experiment

JEL-codes: I14; I21; J13I14; I21; J13

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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., 2019, 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, which 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. Prior to the reform, it was optional for municipalities to provide childcare for the older siblings and 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] 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 implied that families maintained a connection to their childcare provider, possibly facilitating the enrollment of the younger child, mothers' return to the labor market, and further fertility

[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. (2019) for a discussion of the importance of one-on-one communication for child development

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 thus 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 child baring during some intense years by lowering transactions 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 Differences-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: 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. The core subjects include Mathematics, Swedish and English and thus 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 is 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 since there is evidence that there are gender differences in sensitivity to the childhood environment (see, e.g., Bertrand and Pan, 2013; Autor et al., 2019; Fort et al., 2019). We also explore the extent to which effects on test scores depend on maternal education, since 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

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

or less constrained by the lack of childcare prior to the reform. Further, SES has implications for the quality of the home environment, as discussed extensively when analyzing effects of parental vs 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. It is not clear in which direction net effects go. 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) by allowing 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 of course also reduce the indirect exposure to spoken language. While less exposed to the older sibling, the 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 longer 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]

In addition to studying child schooling outcomes, we explore a number of possible pathways through which the increased access to childcare of older siblings may affect children's school performance, namely through health, family environment, parental time allocation, and sibling spillovers. While contact with the sibling's childcare environment is likely to imply exposure to viruses and infections, the exposure to microorganisms in the home environment may be less varied if the older sibling spends less time there.[5] The

[4] Effects of childcare are typically stronger for the oldest sibling, since younger siblings are exposed to microorganisms through their older siblings.
[5] 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 thus reduces the risk of developing autoimmune diseases such as asthma and allergies.

reform potentially affected also 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 and timing of younger siblings. It can also affect the length of parental leave, the possibilities to continue working and subsequent labor market attachment. Also 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. Moreover, we explore alternative mechanisms that are unrelated to more one-on-one time per se, via 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 sd (standard deviation). This effect is entirely driven by sons of less than college educated mothers, who gain 0.063 sd. There is no average effect on girls, but we find that test scores improved by 0.086 sd for daughters of university educated mothers.

We explore a number of 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 and fewer infections and respiratory conditions 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 mothers' mental health, family separations, mothers return to work, the child's age at childcare enrollment, parental leave division between parents or from human capital spillovers from the older sibling. Hence, it is not drastic changes of the home environment or changes in the quantity time spent with a parent that resulted in human capital improvements. Instead, an interpretation is that the effect on low SES boys stems from 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 improved test scores of daughters of university educated mothers, in line with Fort et al., (2019). For this group we also find suggestive evidence 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 perhaps 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, that emerges early and persist or grow 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. Fiorini and Keane (2014) and Hsin and Felfe (2014) use time diaries for Australian 1-9-year-olds and for US 0-12-year-olds, respectively, and conclude that children's cognitive development benefits especially from time spent in educational and structured activities with their parents (mothers). Findings suggest that early investments have larger effects than later. This time pattern of effects is also found in Del Bono et al. (2016), analyzing the effects of maternal educational and recreational time inputs with their 3-7-year-olds. These studies typically find that time with a highly educated mother has more impact, and that effects may be larger for firstborns. The Del Bono et al. study also finds that time in childcare had no impact, which relates to findings of Liu and Nordstrom Skans (2010). They find that a reform extending parental leave, which effectively shifted the care of children from formal childcare to mothers during ages 12-15 months, had no overall effects on educational outcomes or health, but that more time with a college educated mother had beneficial effects on educational outcomes. This is consistent with the findings of Fort et al. (2019), who show that increased access to childcare at an early age had a negative effect on the IQ of girls of highly educated mothers, in particular. 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. (2019), 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, in particular for low SES boys, but also for girls of highly educated mothers.

Further, since differential parental time investments and time allocation during childhood are central in understanding birth order effects, we contribute also to this literature. The economic literature on birth order effects shows important differences in a wide range of relevant outcomes, for instance 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:[6] Siblings may, for good and bad, influence each other as "caregivers", teachers and role models. Lei (2019) and Karbownik and Özek (2019) find that the positive effects from more years of schooling of the older sibling, spill over and positively affects also the educational achievement of the younger sibling. Furthermore, Joensen and Skyt Nielsen (2018) show that the education choice of younger siblings is sensitive to the social influence of the older sibling and argue that this is likely due to competition. Siblings are further exposed to indirect spillovers as they share, or compete for, family resources. This is stressed in Björkegren and Svaleryd (2017), who find birth order effects in health to be driven by conditions that could have been avoided with appropriate preventive care. Black et al. (2020) estimate sibling spillovers on student outcomes and find differences in both parental time and financial investments to contribute to the sibling spillovers. Black et al. (2018) document that Swedish parents take a more active role in firstborns' schooling and Lehmann et al. (2018) show that later-born US children receive lower levels of cognitive stimulation at home as early as in the first year of life, although they seem to be receiving the same quality of emotional support from their parents. Although we don't model sibling differences in this paper, 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 reform and control municipalities, discuss sampling and measurement, and provide evidence on how the reform affected childcare enrollment. A

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

graphical analysis is presented and threats to identification are discussed in section 5. Results are presented in section 6. Section 7 concludes.

2 Swedish childcare arrangements, the childcare access reform and the impact on families

The vast majority of 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 to both parents, and although fathers' share of parental leave has increased over time it was only 30 percent in 2019. During the time 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 8 years of life, mothers taking 80–90 percent of leave days. Of these months, 12 were paid at a wage replacement of 80–90 percent up to a cap and three months, or 90 days, 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).

Take up 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 first enrollment age was 18 months (Duvander, 2006) and almost 80 percent of 1–5-year-olds were enrolled in formal childcare in 1999.

During the years 2001–2003, 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 had 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 for those whose parents were unemployed from July 2001 and on parental leave from 2002.[7] Moreover, the reform imposed a uniform fee schedule with a low cap in all municipalities in 2002, and granted all 4–5-year-olds 525 hours/year of childcare

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

free of charge from 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 of older siblings.[8]

For children of parents on parental leave with a younger sibling, the reform hence, implied that the municipalities were obliged to offer a childcare slot of at least 15 hours per week from January 1st, 2002. This part of the reform was motivated by the importance of maintaining a stable childcare environment for the older sibling (Government 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 and meant that the infant's exclusive one-on-one time with the parent on leave during the first year of life increased, but also that the older sibling could remain enrolled in childcare and that the family thus 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

[8] Lundin et al (2008) find no effects on female labor supply of reduced childcare fees. Mörk et al (2013) find heterogenous effects on fertility of the same reform, and Van den Berg and Siflinger (2020) find positive effects on child health as more children were enrolled in childcare. Vikman (2010) finds a substantial effect on maternal job finding rates and Alto et al (2019) find limited overall effects on child health of childcare access for children of the unemployed, but that medication for respiratory conditions in school age was reduced. Studying access to childcare for children of parents on parental leave, Norén (2015) finds no effects on parental leave division among parents. Hallberg (2019) finds positive effects on 9th grade Mathematics test scores of the older siblings who gained child care access.

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 math 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.

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, while 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 cut of 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 child care 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: 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 4.2.

We estimate the following model:

$$Y_{imcd} = \alpha + \delta post_c * sibling_i + \gamma sibling_i + \theta_{mc} + \lambda_d + \boldsymbol{X}_i\beta' + \varepsilon_{imcd}, \quad (1)$$

where Y_{imcd} is the outcome of child i born in municipality m, of birth cohort c, and calender month, d.[9] The variable $post_c$ is an indicator variable taking the value 1 for children born after the reform, from 2002, and 0 for pre-reform cohorts.[10] 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, it captures the intention-to-treat (ITT) effect on the child (or on the parent or family) of the opportunity to enjoy 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 further includes birth month fixed effects, λ_d, accounting for differences in outcomes, e.g., between children born early and late in the year. Finally, the model includes a set of pre-determined family and parental controls, $\boldsymbol{X}_i$, as listed in Table 3. In order to analyze heterogeneous effects of the opportunity to have more one-on-one time, we split the sample by maternal educational 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 reform municipalities. Second, we need

[9] The municipality of birth of the child is proxied by the municipality of residence of the mother, at the year of birth.
[10] Pre-reform differences between reform and control municipalities are displayed in Table 2.

treatment assignment to be exogenous. 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 affects fertility choices of families, we examine carefully the composition of families of children with and without siblings in section 5.1.

Alternative empirical strategies would be to estimate a differences-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 1), we provide the estimates in the Appendix.

4 Data, definitions, and reform effects on enrollment

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

4.1 Sampling and data

We use linked administrative data from the Multi-generation register and from education, health, tax and social insurance registers covering the universe of Swedish children and their families. These administrative records contain family links and demographics, such as age, sex, birth order, and 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 the years surrounding the reform, 1999 and 2002.

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 at the earliest when it turned one years old.[11] 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[12]. This implies that the maximum age difference for treated children is 5 years and 11 months. Infants with a larger age gap to the older sibling[13], 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, month and year of birth of the child, mothers' country of birth as well as their age at first birth. From the Medical Birth Register we create an indicator of low birthweight. Children who are not present in the birth register are removed from the analysis[14]. Children whose mother is not present in tax registers, and hence not a Swedish resident in the child's year of birth or when the child is in school (ages 6–12), are also removed from the sample[15].

We use data from tax and education registers 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 prior to birth. Because the time 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.[16] 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.[17]

The interest of this paper is to estimate the impact of better opportunities for one-on-one time with a parent during infancy[18] on human capital development. We measure

[11] Municipal surveys suggest that childcare access was often even more restrictive for new enrollments of older siblings, than in allowing them to keep their slot as a new sibling was born.
[12] Both restrictions regarding the sibling age difference removes 2.6 percent of the analysis sample.
[13] For this group, the minimum age difference is 5 years and 9 months.
[14] 3.3 percent of the initial sample.
[15] 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 tax registers for the relevant years. 0.25 percent of children have mothers who are not.
[16] 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.
[17] This corresponds to 2-3 years of tertiary education/university depending on the length of elementary education.
[18] 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.

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, available from the National Agency for Education.[19] Sixth grade test scores are good predictors of later test scores and compulsory school leaving GPA, 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 9th 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 any changes to time allocation during the child's first years of life. We also study 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 Patient Registry, containing information on diagnoses for all inpatient care visits since 1987, and outpatient care visits since 2005.[20] In addition, we make use of the Drug Prescription Registry, containing all drugs prescribed since 2006. First, based on the inpatient care register we construct individual indicators of ever being hospitalized for children during preschool age, and hospitalizations relating to mental health for mothers during the first three years since birth [21]. Second, based on inpatient and outpatient registers and medical drug prescriptions we construct indicators of any health care use of the child in school age (age

[19] The test scores are standardized within each school cohort. There are national tests also in grade 3 and 9. The latter are available only for cohorts born prior to the reform, i.e. 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.
[20] The Patient register contains outpatient care visits from 2001, but the coverage is increasing over time and it has full coverage only from 2005. Therefore, we only use outpatient care visits from 2005 onwards. Outpatient care includes specialized health care by medical doctor, but not primary care visits.
[21] Hospitalization related to conditions and complications at birth (perinatal) are excluded.

7-13).[22] For the school age, we also construct two diagnosis-specific indicators.[23] The first captures care for infections/respiratory diseases, common in childhood and plausibly affected by exposure to the childcare environment or to older siblings. [24] 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 thus the timing of preschool enrollment and mothers' return to the labor market. Increased flexibility may also have reduced parental stress and conflict. We therefore estimate effects on the child's age at preschool enrollment[25] and maternal labor earnings during the child's first years of life. We capture family separations by an indicator taking 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 the Appendix H, 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 mother's use of parental leave prior to birth as a measure of prebirth health.

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

The extent to which the childcare access reform affected childcare arrangements of families across municipalities depends on the bite of the restrictions imposed on families prior to the reform. This, in turn, depends both on the supply of and demand for childcare

[22] Medical drug prescription data is available from 2005 and can hence be measured when the studied cohorts are in school age.
[23] Diagnoses in in- and outpatient care are based on main and up to 20 auxiliary diagnoses. See Appendix B, Table A1 for a detailed description of the ICD10 and the ATC codes that correspond to the diagnosis groups used.
[24] 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.

[25] We follow Duvander and Viklund (2017) who proxy preschool enrollment using detailed data on parental leave benefits from the database MiDAS, administered the by Social Insurance Office. 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 do as 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, because these are possibly difficult to assign the correct date.

for older siblings[26]. We capture the bite of the reform using measures of actual enrollment before the reform. Nationwide administrative registers on childcare enrollment are unfortunately not available for the time 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, just after the reform. These surveys were addressed to parents of children in the ages 1–12 years 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[27] children in preschool age 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 difference between these groups likely imposed stronger restrictions on childcare enrollment for families on parental leave than municipalities with a small enrollment difference.[28]

Table 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 group	Sample	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 1 presents 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

[26] See Appendix A, for a discussion about formal restrictions, as reported by the municipalities.
[27] 69 000 children 1999, 72 000 children 2002.
[28] Figure A 1 in Appendix C displays the distribution of the pre-reform difference in enrollment between these groups.

children of parents on parental leave (top panel)[29] 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, reflecting 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. These children had access either because the supply of slots was excessive or because they had special needs[30]. In the bottom quintile (group 1), defined as 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 and implies 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, is 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 B, Figure A 2, 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, and we see a clear positive correlation.

Using the top and bottom quintiles to define reform and control municipalities, based on the enrollment rates presented in Table 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 child in preschool-age (see

[29] This group consists of 6,773 observations in 1999 and 7,568 observations in 2002.
[30] 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 time. 10 percent is thus the upper bound for the fraction of households with especially high gains from childcare access not captured in the estimations.

Appendix C, Table A 2). The first stage is estimated formally in a differences-in-differences model (Appendix C, equation 2), and results are presented in Appendix C, Table A 4. The formal first stage estimate confirms the results in Table 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 different form zero[31].

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.[32] Low demand due to high fees prior to 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.

4.3 Data description

Table 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 control (column 3) and reform (column 4) municipalities separately.[33] Reform municipalities are somewhat disadvantaged compared to the country as a whole 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

[31] Estimating heterogeneity with respect to maternal education with an interaction term results in an insignificant coefficient for the interaction.
[32] In this case, we would have a weak first stage.
[33] Appendix C Figure A 3 shows the geographical distribution of the quintile groups, indicating that both reform and control municipalities are well dispersed across Sweden.

control group of municipalities. Comparing childcare quality as measured by the child-teacher ratio, cost per child and the mean age at enrollment, the reform municipalities are representative of the country as a whole. 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, the standardized test score in grade 6, based on individual data, reveals that test scores in reform municipalities are somewhat below the country average. In the empirical specification, we include municipality-specific cohort fixed effects accounting for both level and trend differences in outcomes between municipalities.

Table 2 Municipality, child and family characteristics at municipal level prior to 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

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

Table 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, note that 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 less likely to have a university educated mother. In order to examine if 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 in column 5 the DD-estimates of the reform impact on each covariate, using the specification in equation (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 in childcare age. Also, there is a larger decline in the fraction of mothers with low (compulsory) education among the treated children. In Appendix D, Table A 5 we provide 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 not likely that this change in composition is due to the reform. In the empirical analysis, we therefore control for predetermined characteristics.

Table 3 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)	(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, birth month fixed effects.

5 Graphical analysis and threats to identification

Before turning to the formal analysis in section 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 allows us to assess the validity of the parallel trends assumption underlying our identification strategy. We also further 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.[37] This analysis aims to detect if decisions regarding fertility, child spacing, or relocation patterns are correlated with the reform such that a changing composition of families may confound any effects of the reform. We also investigate if 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 1 show the average and predicted test scores of children in birth cohorts 1999 to 2003 in reform (full drawn line) control (dashed line) municipalities for treated children (blue) who at birth had a sibling of childcare age and control group (black) who did not have an older sibling of child care age. The vertical line at 2001.5 indicates the cutoff for the pre- and post-reform data points. Because of the reform, siblings of children born from 2002 onward had access to childcare regardless of where the family lived.

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

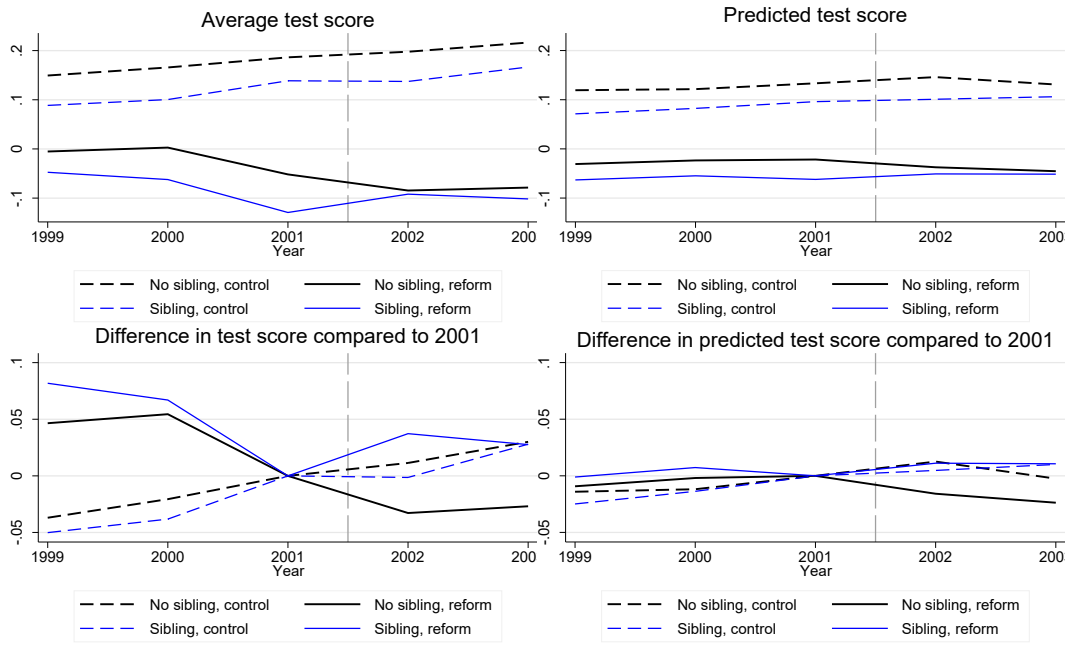
[37] The predicted test scores are obtained from a regression model relating average test scores to child and parental controls for the years 1999–2001. See equations 3 and 4 in Appendix E.

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 1, showing 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. It is therefore 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, thus revealing more clearly any impacts of the reform. In 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 be driven by changes in predetermined characteristics, i.e., by compositional changes that are correlated with the reform. In Appendix F, Figure A 4, 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.

Figure 1 Average and predicted test scores at age 13 of children with and without siblings of child care age in reform and control municipalities.



Note: The empirical specification is found in Appendix, equations 3 and 4.

5.1 Test of covariate balance

In Table 4, we perform a formal test of covariate balance, as displayed in the right panel of Figure 1, by examining if 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 grade 6, 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 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 to a large extent driven by differences in parental human capital.

Table 4 DD-model of effects on average test scores, predicted test score and parental educational background

	(1) Average test score	(2) Predicted test score	(3) Parental educational 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

Note: 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.

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 in 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 5. The results show precisely estimated zero effects, thus 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, were unaffected.

Table 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 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 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, and in particular 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.

6 Results

We investigate effects on child human capital, as measured by sixth grade test scores, of a reform which 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, e.g., Francesconi et al. (2016), Bertrand and Pan (2013) and Autor et al. (2019). We study several potential

mechanisms, i.e., effects on child and maternal health, and family environment. We are particularly interested in the extent to which the reform can be tied to changes in quality and quantity of parental time investments. We therefore 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.

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

Table 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 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, but 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 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 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 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

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 DD-model. The child and parental controls are listed in Table 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 7 provides results of split sample regressions for boys and girls and by maternal education. Figures A 5 -A 6 in Appendix F, suggest that the parallel trends assumption is not violated for this split sample analysis. Moreover, we have verified that the covariate imbalance detected in section 5.1 is not aggravated by splitting samples.[38]

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[39] (less than college) mothers, who gain 0.063 sd, 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

[38] Results are available from the authors.
[39] Using three educational groups, separating out 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.

estimates for the control municipalities, displayed in Appendix G, Table A 16, show small and insignificant estimates for the studied subgroups.

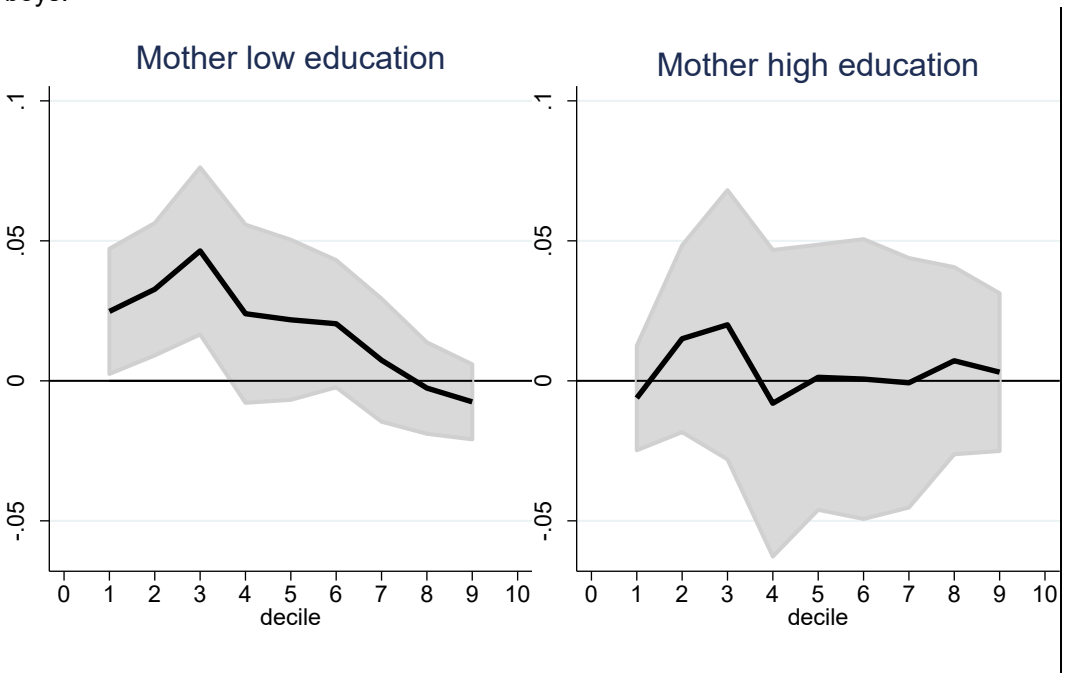
Table 7 Main results: Effects of better opportunities for one-on-one time on average test scores, by gender and maternal education

	(1) All	(2) boys	(3) girls
		All	
	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	
	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	
	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

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 3.

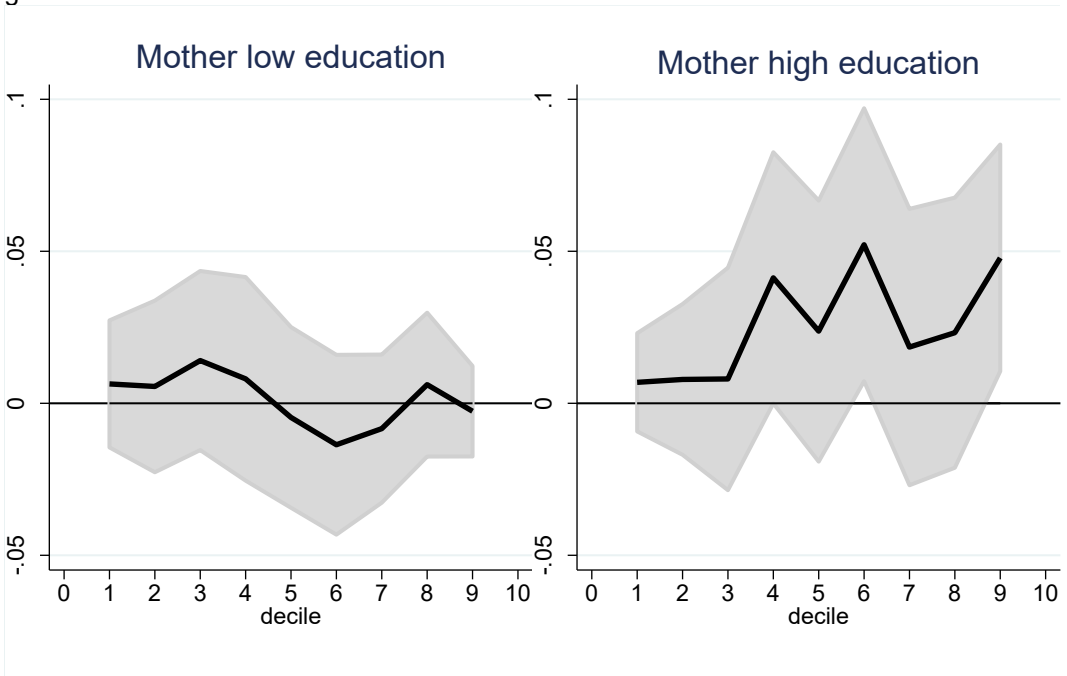
Before we explore possible mechanisms for the positive effects on 6th grade test scores, Figure 2 and Figure 3 present the results when estimating our DD-model on an indicator for having a test score above a particular decile of the test score distribution, for boys and girls separately. The figures 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.

Figure 2 Effects of better opportunities for one-on-one time over the test score distribution, boys.



Note: 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 3. Grey area shows 95-percent confidence interval, with standard errors clustered at the municipality level.

Figure 3 Effects of better opportunities for one-on-one time over the test score distribution, girls.



Note: 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 3. Grey area shows 95-percent confidence interval, with standard errors clustered at the municipality level.

6.1.1 Robustness of main results

Table 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 a number of ways. First, we present the differences-in-differences estimated 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 Tables A 6- A 8. The results show that the reform impact is only present in the municipalities that were most restrictive prior to the reform.

Second, as has already been 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 have so far defined treatment status based on the year of birth and therefore included children born 2001 in the group of untreated children. However, depending on the month of birth, their first year covers also 2002, i.e., when the older sibling gained access to childcare, and they are thus partially treated if parents used the opportunity to (re)enroll the older sibling. Therefore, we present i) an analysis excluding the 2001 cohort entirely and ii) an analysis where the cohort of 2001 is included, but treatment defined as the share of the first year of life that the older sibling had access to childcare.[40] The results and the corresponding placebo analyses show patterns very similar to the main results, and are presented in Appendix Table A 9 and Table A 10.

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 if our results are robust to excluding firstborns from the sample of control children, thus 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 A 11 and Table A 12. 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 differences-in-differences model and we also estimate a triple difference model. In the alternative DD-model presented in Table A 13, treated

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

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 1) showed that the parallel trends assumption before the reform was far from satisfied 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 G, Table A 14, does 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 Table A 15 effectively removes confounding effects, but the many dimensions make the estimate less transparent. The results are, however, very similar to the results of the main analysis.

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 if 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, in particular effects on maternal mental health and family separations.

6.2.1 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 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 relating 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.[41]

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 registers 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 negative, indicating reductions in care use. For boys there is a reduction in care relating to mental health, by 11 children per 1000 or 10 percent relative to the pre-reform mean. The estimate is, however, significant only at the 10 percent level, and correcting for multiple hypothesis testing, the estimate is no longer significant at conventional levels. Yet, the consistently negative estimates suggest that, if anything, improved health during school age cannot be discarded as a possible mechanism through which school results were affected.

Table 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	Any Mental	Any Infec/Resp
		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

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 3. Note that health outcomes are measured per 1000 individuals.

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

Table 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 (weakly) significant.

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, boys with low educated mothers and girls with high educated mothers, there is suggestive evidence of worse preschool health. For boys, there also appears to be an improvement in mental health in school age, which could have contributed to the improved school results for this group. The placebo estimates, presented in Appendix G, Tables A 17- A 18, generally show smaller and insignificant estimates.

Table 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	Any Mental	Any Infec/Resp
		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 income		
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 income		
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

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 3. Note that health outcomes are measured per 1000 individuals.

6.2.2 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 to which the reform affected the quality and quantity of parental time investments during early childhood. To capture 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 10.

Table 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,		
			Low maternal 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
Control mean	10.47	0.135	0.0900	12.03	537.2
			High maternal 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
Control mean	4.838	0.0401	0.0822	12.54	575.9
			Girls		
			Low maternal 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
Control mean	8.957	0.125	0.0896	12.04	533.8
			High maternal 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
Control mean	5.714	0.0464	0.0657	12.59	577.7

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 3. Note that 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[42]. Neither is there any 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. In column 3 of Table 10, there is, however, an indication 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 per cent relative to the mean, and significantly different from zero at the 10 percent level. The corresponding placebo estimates are presented in Appendix G and do not show a similar decrease in fertility for this group (see Appendix G, Table A 19).

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

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 if parental leave uptake was affected, both in terms of division and intensity, and in line with Norén (2015), we find no evidence of that (see Appendix H, Table A 20). Neither do we find any indication of changed pre-birth health since parental leave use prior to giving birth remained unaffected. Another possible mechanism, is that the increased access to childcare effected 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 H, Table A 22). We find it unlikely that these negative effects would drive the positive effects we find. Our findings thus contradict Hallberg (2020) who finds a positive reform effect from gaining access to childcare on the 9th grade test score in mathematics for older siblings. [43]

7 Conclusion

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 which 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 some 30 percentage points. We identify causal effects on human capital formation using a DD-approach which compares 6th grade test scores in core subjects of infants with and without a sibling of childcare age in municipalities that were affected by the reform. While 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

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

by 0.043 sd (standard deviation). 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 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.

Because there is no statistical difference in the estimated first stage with respect to maternal education, 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 through which the increased opportunities for more one-on-one time may have affected the human capital development of children. While we find little support for overall effects on child health, but it is possible that less behavioral and psychiatric problems and fewer infections and respiratory conditions 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. Further, 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. Although speculative, it is further possible that more and early exposure to the siblings' childcare environment is part of the explanation for the reduction in respiratory conditions and infections during school age, in line with Lu et al. (2004) and Ball et al. (2000), but there is no reason why this effect should only be present for a specific group.

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 be the mechanism behind the improved test scores also of daughters of college educated

mothers. Fort et al. (2019) argue that girls are more likely to benefit from the cognitive stimulus of this interaction than boys, especially in highly educated 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. Neither do we find evidence that mothers 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. They point 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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Appendix

A. Definitions of diagnoses

Table A 1 Definitions of health variables and corresponding ICD10 and ATC codes

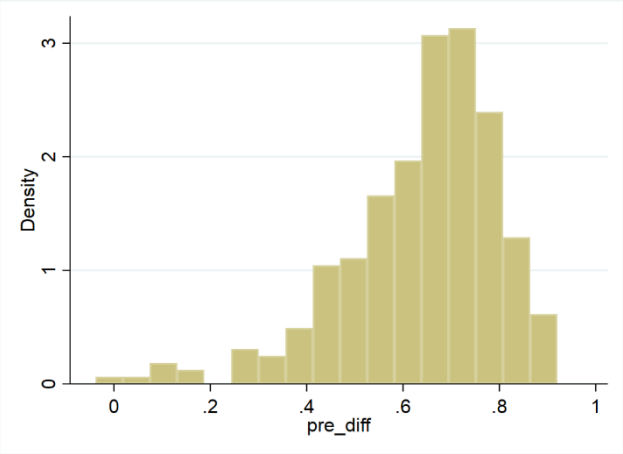
Variable	Definition	Example
Hospitalizations and outpatient care		
Hospitalization	=1 if admitted to hospital for any reason	
Infection	=1 if admitted to hospital with the ICD10 diagnosis code A00-A99, B10-B84, B90-B99, B00-B09, B85-B89	Infectious diarrhea, mononucleosis, chicken pox.
Respiratory	=1 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

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 National Education Agency 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.

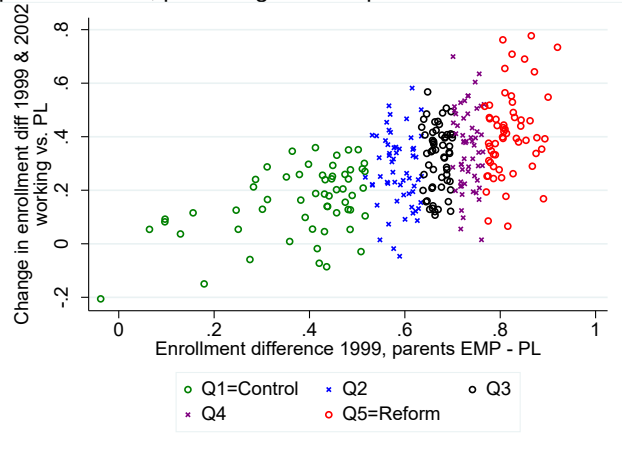
C. Variation in pre reform childcare enrollment

Figure A 1 Distribution of pre-diff, municipality level



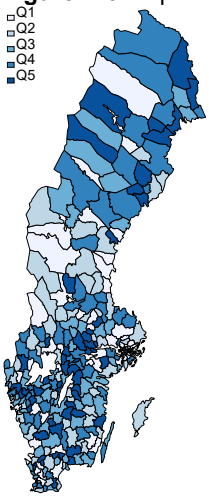
Source: NAE Parental Surveys

Figure A 2 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 A 3 Map of municipalities by pre-diff quintiles



D. Changes in enrollment: First stage

Table A 2 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	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: NAE Parental Surveys

Table A 3 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: NAE Parental Surveys

Estimation of the first stage

Formal estimation of the first stage is based on the following equation:

$$Y_{imc} = \alpha + \delta post_c * reform_m * work_i + \gamma work_i * reform_m + \eta post_c * reform_m$$
$$\varphi\ work_i * post_c\ +\ work_i + post_c + \theta_m + \boldsymbol{X}_i\beta' + \varepsilon_{imc}, \qquad (2)$$

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 A 4 DD of first stage using parental survey

	Reform municipalities	Control municipalities
	All	
Both parents work*post	0.442***	0.123**
	(0.037)	(0.047)
Observations	15,435	16,679
	Mother low education	
Both parents work*post	0.423***	0.104**
	(0.030)	(0.040)
Observations	13,664	13,519
	Mother high education	
Both parents work*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.

E. Control municipalities: Descriptives

Table A 5 PLACEBO: Municipalities, child and family characteristics at municipal level prior to the reform

	(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.

F. Predicting test scores

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

$$Y_{im} = \boldsymbol{X}_i\beta' + \theta_m + \varepsilon_{im}, \qquad (3)$$

$$\hat{Y}_{im} = \alpha + \delta post_c * reform_m * sibling_i + \gamma sibling_i * reform_m + \varphi\, sibling_i * post_c + \mu sibling_i + \theta_{mc} + \varepsilon_{imc}, \qquad (4)$$

G. Graphical analysis and threats to identification

Figure A 4 Residualized test scores for children with and without childcare age siblings in reform and control municipalities.

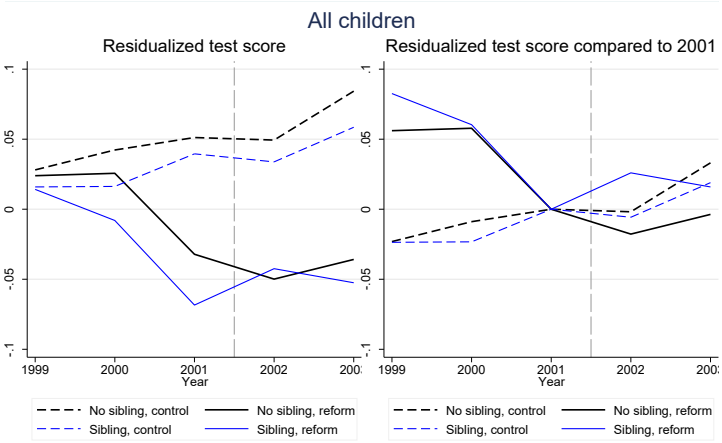


Figure A 5 Residualized test scores for children with and without childcare age siblings in reform and control municipalities, boys

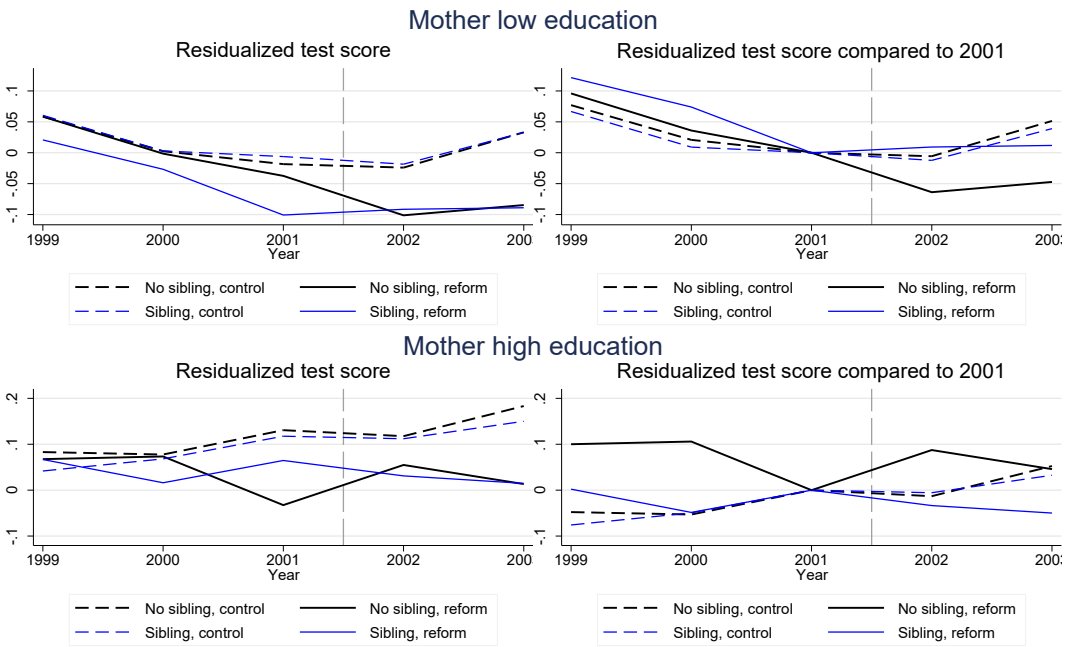
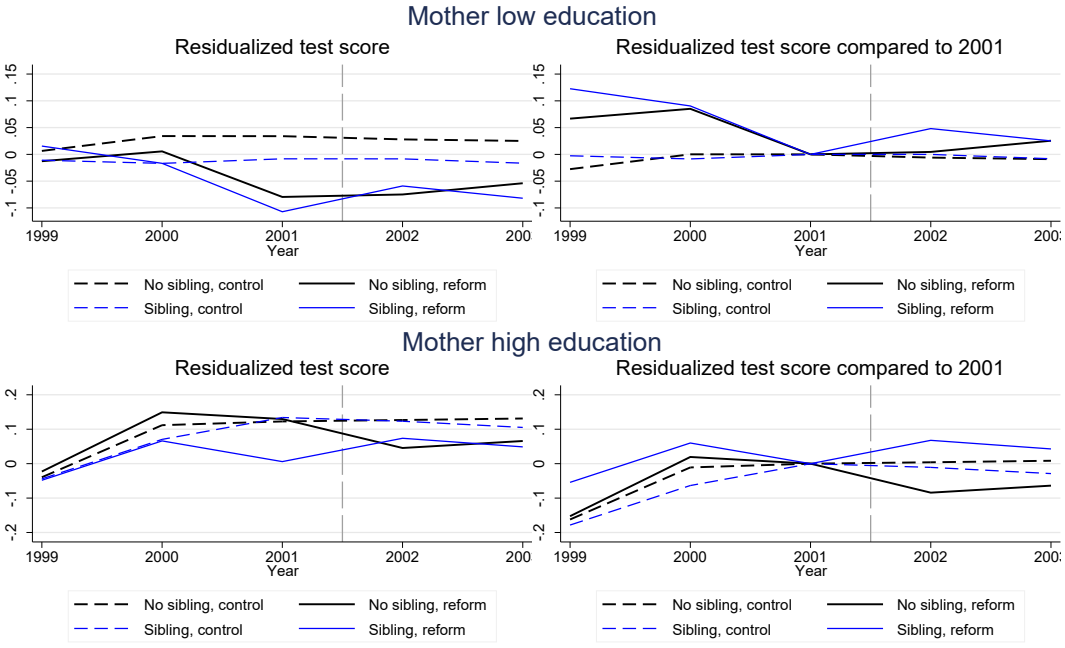


Figure A 6 Residualized test scores for children with and without childcare age siblings in reform and control municipalities, girls



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 A 6-Table A 8 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 A 6 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

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 3.

Table A 7 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

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 3.

Table A 8 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
	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

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 3.

Sensitivity to the reform year 2001

The year 2001 can be considered a phase in and there are different ways of treating this. Below, we look at two different approaches.

Table A 9 Sensitivity of results to partial treatment of the 2001 birth cohort

	(1) All children	(2) Boys	(3) Girls
	2001 cohorts dropped		
		All	
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
		Low maternal 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
		High maternal 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	
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
		Low maternal 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
		High maternal 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

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 3.

Table A 10 PLACEBO: Sensitivity of results to partial treatment of the 2001 birth cohort

	(1) All	(2) Boys	(3) Girls
	2001 cohorts dropped		
		All	
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
		Low maternal 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
		High maternal 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	
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
		Low maternal 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
		High maternal 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

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 3.

Sensitivity to sibling status

Table A 11 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	0.052*	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
		Low maternal 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
		High maternal 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

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 3.

Table A 12 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
		Low maternal 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
		High maternal 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

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 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:

$$Y_{imc} = \alpha + \delta post_c * sibling_i + \gamma sibling_i + \theta_{mc} + \lambda_d + \boldsymbol{X}_i\beta' + \varepsilon_{imc}$$

$$Y_{imc} = \alpha + \eta post_c * reform_m + post_c + \theta_{reform,c} + \lambda_d + \rho_m + \xi_c + \boldsymbol{X}_i\beta' + \varepsilon_{imc}, \quad (6)$$

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 ξ_c, respectively. Similar to the main specification, the model includes covariates as specified in Table 3, and month of birth fixed effects.

Table A 13 DD-model Average test scores, comparing children with siblings in reform and control municipalities.

	(1) All	(2) Boys	(3) Girls
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 low 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 high 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 parenthesis, , *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including birth month fixed effects, child and parental controls (as specified in Table 3), municipality fixed effects, cohort fixed effects and separate time trends.

Table A 14 PLACEBO: DD-model Average test scores, comparing children without siblings in reform and control municipalities.

	(1) All	(2) Boys	(3) Girls
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 low 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 high 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 parenthesis, , *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including birth month fixed effects, child and parental controls (as specified in Table 3), municipality fixed effects, cohort fixed effects and separate time trends.

Differences in differences in differences estimation

Combining the two DD-models, we can estimate a triple-differences model. The empirical specification is as follows:

$$Y_{imc} = \alpha + \delta post_c * reform_m * sibling_i + \gamma sibling_i * reform_m + \varphi\, sibling_i * post_c + \mu sibling_i + \theta_{mc} + \lambda_d + \boldsymbol{X}_i\beta' + \varepsilon_{imc}, \quad (7)$$

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 3.

Table A 15 DDD-model Average test scores, comparing children with and without siblings in reform and control municipalities.

	(1) All	(2) Boys	(3) Girls
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 low 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 high 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 parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DDD-model including municipality by cohort fixed effects, birth month fixed effects, and child and parental controls (as specified in Table 3).

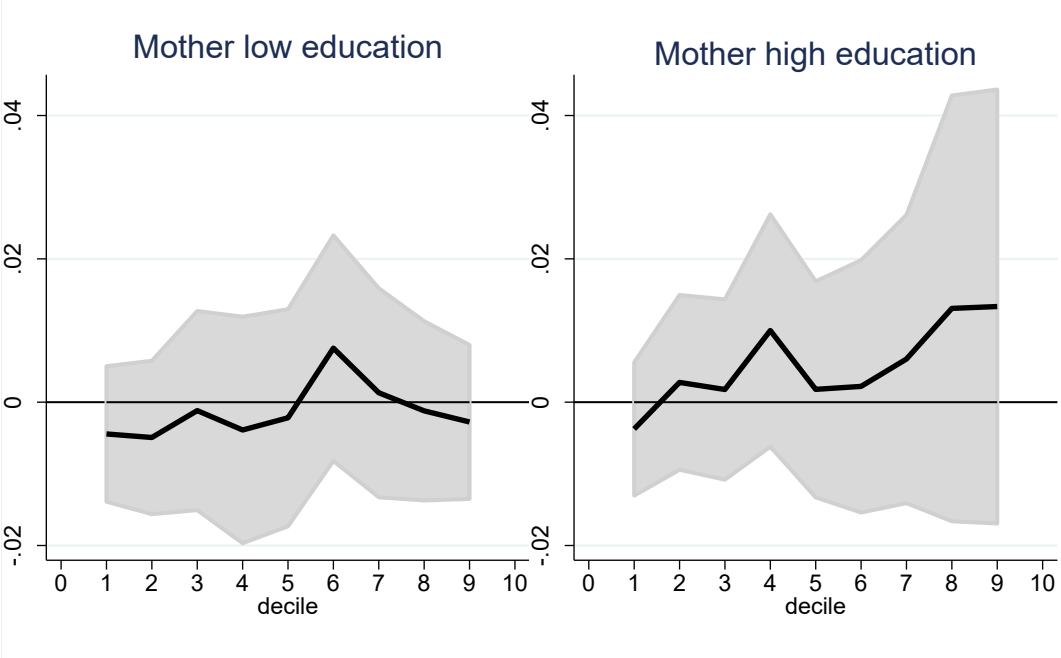
H. Control municipality PLACEBO analysis:

Table A 16 PLACEBO: Main results by gender and maternal education

	(1) All	(2) Boys	(3) Girls
		All	
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 low 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 high 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

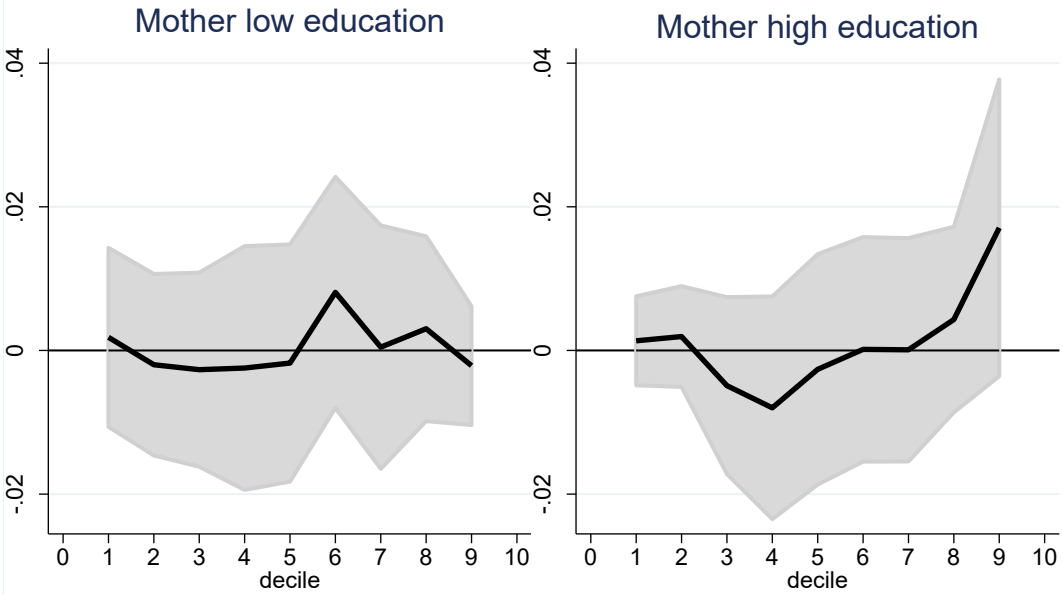
Note: Robust standard errors clustered at the municipality level in parentheses, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 3.

Figure A 7 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 A 8 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 A 17 PLACEBO: Effects on health in primary school age for the younger sibling

	(1)	(2)	(3)	(4)
	Preschool	School		
	Inpatient Any	Any	Any Mental	Any Infec/Resp
		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

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 3.

Table A 18 PLACEBO: Effects on health in primary school age for the younger sibling, by maternal education

	(1)	(2)	(3)	(4)
	Preschool	School		
	Inpatient Any	Any	Any Mental	Any Infec/Resp
		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 income		
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 income		
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

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 3.

Table A 19 PLACEBO: Effects on family and childhood environment

PLACEBO	(1) Mother mental health	(2) Parents separated	(3) Younger sibling	(4) Mother earnings	(5) Age at enrollment
			Boys,		
			Low maternal 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
			High maternal 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		
			Low maternal 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
			High maternal 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

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 sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 3.

I. Additional analyses

Table A 20 Parental leave benefits

	(1)	(2)	(3)
	PL first year		PL before birth
	Father	Mother	
		Boys	
		Low maternal 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
		High maternal 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	
		Low maternal 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
		High maternal 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 parenthesis, *** 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 3.

Table A 21 PLACEBO: Parental leave benefits

	(1)	(2)	(3)
	PL first year		PL before birth
	Father	Mother	
		Boys	
		Low maternal 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
		High maternal 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	
		Low maternal 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
		High maternal 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 parenthesis, *** 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 3.

Table A 22 Sibling spillovers: DD-model of more one-on-one time on Average test scores for younger and older sibling respectively

	(1)	(2)	(3)	(4)	(5)	(6)
	Younger sibling			Older sibling		
	All	Boys	Girls	All	Boys	Girls
			Low maternal 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
			High maternal 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 parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including the main covariates referring to the younger sibling: sibling status specific cohort effects, municipality by cohort fixed effects, birth month

fixed effects, and controls for the list of predetermined characteristics in Table 3. In addition, these estimations include also cohort fixed effects for the older sibling.

Table A 23 PLACEBO: Sibling spillovers: DD-model of more one-on-one time on Average test scores for younger and older sibling respectively

	(1)	(2)	(3)	(4)	(5)	(6)
	Younger sibling			Older sibling		
	All	Boys	Girls	All	Boys	Girls
			Low maternal 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
			High maternal 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 parenthesis, *** p<0.01, ** p<0.05, * p<0.1. Results from separate estimations of full DD-model including the main covariates referring to the younger sibling: sibling status specific cohort effects, municipality by cohort fixed effects, birth month fixed effects, and controls for the list of predetermined characteristics in Table 3. In addition, these estimations include also cohort fixed effects for the older sibling.