Working paper: Pre-school or parental care - what is the effect on school performance at age sixteen?

Evidence from a Swedish child care reform

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Abstract

This paper uses a Swedish child care reform in 2002 to estimate the causal effect of pre-school atendance on school performance at age 16. The reform increased pre-school availability for children with parents on parental leave with an infant sibling. Using registry data on individuals born from 1993-2001 together with data from two national child care surveys I estimate the effect of the reform on child care utilization and on the school performance at age 16. Pre-school attendance increased with 23 percent and the results on a standardized test in mathematics taken at age 16 increased with 0.06 standard deviations for the populations that gained more access to pre-school (ITT). The effect on other measures of school performance is not statistically significant. The results suggest that different sub-populations as male/female or those with foreign/domestic background benefited equally from the increased time in pre-school.

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I Introduction

During the last 20 years an increasing number of empirical studies on the causal effect of early childhood environment on adult human capital has been published. This field has partly been motivated by policy interest following the build up of child care outside the family and partly by research interest in understanding the life cycle of human capital formation.

Early childhood is typically defined as the period before the age of five, a time during which many critical phases in child development takes place. For instance IQ seem to be malleable only until around age 10 (Jensen (1980) in Cunha et al. (2006)); both first and second language learning are also examples of skills that are much harder to master after childhood (Uylings, 2006). This suggest both *self-productivity* and *dynamic complementary* in the technology of skill formation with the implication that skills begets skills and that later life remediation for adverse early childhood environment is not always possible (Cunha and Heckman, 2007; Cunha et al., 2006). Empirical estimation of the relative effectiveness of different early childhood interventions is thus of interest for both parents and policy makers.

Public spending on early child care and education has increased in many countries. In Sweden, government expenditure on child care increased from 2.1 percent of total expenditure in 1995 to 3.2 percent 20 years later (OECD, 2019). A cost/benefit analysis of public spending on child care must both take into consideration the effect on labor market participation of parents and the effect on child development. Reliable empirical estimates on the latter are largely missing in the Swedish context.

The aim of this thesis is to estimate the causal effect of increased time in out-of-home child care on the school performance at age 16. I use upper secondary school grades and test score on standardized tests in mathematics and Swedish as outcome variables. The empirical strategy uses exogenous variation in childcare utilization introduced by a child care reform in 2002 with heterogeneous impact on different geographical regions and on children with and without younger siblings. By using the trend in school performance for the un-affected individuals as the counterfactual trend for the treated population, I estimate the causal effect of increased time in pre-school. The primary data source is registry data on the universe of lower secondary graduates in Sweden collected by Statistics Sweden. It is combined with survey data from the Swedish National Agency of Education in order to determine the impact on the reform on different municipalities and on childcare utilization.

The results suggest that increased time in pre-school has a positive effect on test scores in mathematics at age 16, but not on test scores in Swedish or on the probability of having a passing grade in the core curriculum (Swedish, mathematics and English). The latter being a requirement to advance to upper secondary school in Sweden. I find no support for large heterogeneity of the effect in either of the sub-populations male/female, foreign/domestic background. But the data does not allow for an in-depths analysis of how the effect varies with the socio-economic status (SES) of households.

One limitation of the external validity of the conclusions that can be drawn from the results is that all treated children had a younger sibling. If the results extend to children without younger siblings or not depends on the causal mechanisms at play, a question that merits further studies. The fact that the effect only is measured on school performance and not labor market outcomes also restrains any ultimate claim on the effects of pre-school attendance on human capital outcomes.

In this thesis I introduce the institutional setting and describe the Swedish child care reform in section II. Relevant previous studies and the contribution of the thesis to the early childhood and education literature is discussed in section III. I present the empirical strategy and the data in detail in IV. Section V contains the main results and relevant specifications checks to test the identifying assumption. I conclude the thesis by discussing the results and potential causal mechanisms in section VI, where I also provide some suggestions for further research. In section VII I summarize the conclusions that can be drawn from the study.

II Institutional setting and the child-care reform

In modern welfare states, an explicit goal for social institutions is to limit the negative impact of adverse childhood environment. A national pre-school program is often a corner stone policy for that endeavor. In the international literature such interventions goes under the collective term Early Childhood Education and Care (ECEC). The link between ECEC attendance and adult human capital has attracted a lot of research attention internationally, but less so in Sweden.[1] In most developed societies these programs are regulated and highly subsidized. Attendance has been increasing in both Europe and North America since the 70's but is substantially higher in Europe where more than 90 percent attend some ECEC program before starting primary school (World Bank, 2019). However, the increase might not reflect a change from parental care to ECEC institutions. During the same period parents' (self reported) child care time has also been increasing in the west (Dotti Sani and

[1] At least from a quantitative perspective, see Persson (2008) for a summary of qualitative research on different aspects of pre-school practices.

Treas, 2016). In line with this result many studies on expanded ECEC find a substantial crowding out of private care arrangements. As a large part of the previous studies on ECEC has been made on US data it is important to note that the institutional setting in the US translates poorly to the Swedish context. Especially since most US programs, with some exceptions, are targeted towards disadvantaged children (Blau and Currie, 2006).

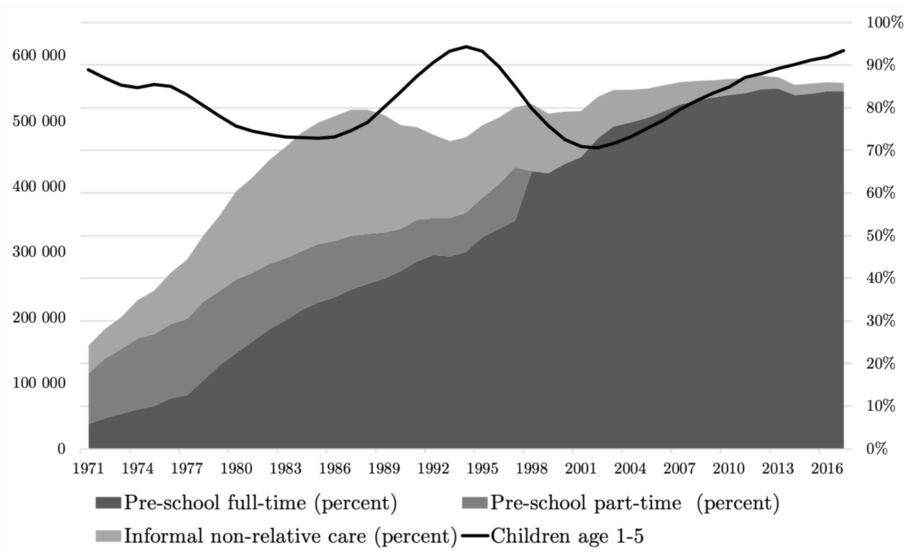
Also in Sweden, public child care began as a targeted intervention, but during the 70's and 80's policy makers decided to expand public child care to promote labor market participation of mothers. During this time there were different modes of public care a two earner family with young children could choose from. The main alternative to pre-school was daycare (*familjedaghem*), a semi public care form in the home of other women (who often cared for their own children at the same time). A slot in pre-school was however preferred by most families. Both because pre-schools were highly subsidized and because national quality recommendations guided the care and educational practices (Martin Korpi, 2015).

By the mid 90's pre-school had become the dominant mode of child care. A contributing factor was a national legislation introduced in 1995 that mandated municipalities to guarantee a pre-school slot for all working or full time studying parent within 3-4 months from applying. By 1998 a national curriculum for pre-schools was adopted and the political responsibility for pre-school policies was shifted from the Ministry of Social Welfare to the Ministry of Education, mirroring a pivot from care to education in both the philosophy of pre-school operators and most municipalities (NEA, 2004b). As shown in Figure 1 just under half (47 percent) of all children aged 1-5 attended a full time pre-school in 1994 - 13 years later it was 80 percent. A part of the increase was a switch from other forms of non-relative child care to pre-school

Daycare children decreased from around 17 percent to 5 percent during that same time.

Figure 1

Child care development in Sweden



Note: Data from Statistics Sweden (Statistikdatabasen, Historisk statistik om utbildning).

The expansion of pre-school did not take place without controversy. The large birth cohorts of the late 80's and early 90's coincided with an economic recession. Higher demand for pre-school thus came at a time of high unemployment rates and large budget deficits for both national an local governments. This resulted in increased local child care fees and a higher child-teacher ratio in order to cut costs. From 1991 to 1997 the municipalities' cost per child in pre-school decreased by some 30 percent while the child care fee increased in most municipalities. During the 80's parents payed around 10 percent of total cost, in the mid 90's fees had increased to on average 20 percent (Martin Korpi, 2015). The increasing cost, in turn, spurred a debate on the effect on labor market participation of childcare cost and availability (Schwarz and Nyman, 1991). As a whole the child care issues became a focal point of the political discussion.

In the last month of the election campaign of 1998 the incumbent Social democratic party pledged to lower the cost of child care by introducing a national cap on local child care fees (*maxtaxa*) (Karlsson, 1998). The promise also included expanded availability for unemployed parents.[2] The Social democrats won the election and during negotiations within the governing coalition it was also decided to expand pre-school availability for parents on parental leave (PPL) with a younger sibling. The result was that practically all children was guaranteed a minimum of 15 hours per week in pre-school, no matter their parents labor market status (Martin Korpi, 2015). The child care reform had three different parts that was gradually introduced.

(i) 1st of July 2001 — 15-hours rule implemented for unemployed parents.

(ii) 1st of January 2002 — 15-hours rule implemented for parents on parental leave.

(iii) 1st of January 2002 — The fee cap was implemented.[3]

(iv) 1st of January 2003 — Universal and free of charge access for the first 15 hour per week for all 4 and 5 year old.

Taken together the national child care reform decreased municipality revenues from child care fees and expanded the number of children eligible for pre-school. To alleviate any potentially negative effects on pre-school quality the national government introduced a child care grant for the municipalities which on average was a fair

[2]Elinder et al. (2009) suggest that this campaign pledge helped the Social democrats to stay in power.

compensation for the increased costs (Wikström, 2007). Most municipalities also managed to deal with the increased demand for a pre-school since there were excess capacity already in place due to falling fertility rates in the late 90's.

III Previous studies

There are many studies of the effect of ECEC on human capital formation, but there is no consensus about the size or even the direction of the effect on later life human capital. According to Morris et al. (2018) one explanation to the lack of consensus is the large heterogeneity of interventions and institutional settings covered and that the external validity of any one study therefore is limited. Given this, at least three aspects are crucial to consider when reviewing the literature, besides the usual concerns for identification strategy and statistical inference.

(i) The characteristics of the program, at what age are children participating? Is it a targeted or a universal program? Is it center or home based? What training (if any) is required of the staff? What is the child-teacher ratio?

(ii) The condition for the children not participating (the control group) Is it parental care in the own home or is it other modes of child care arrangements? General interpretation of effects measured against different control conditions must be made with caution. For instance, most childcare programs will free up time for the parents, time that often is used for wage labor and increase the disposable income for the family. This income effect of subsidized child care could potentially influence the quality of parental care for the remaining part of the day when the child is not attending pre-school.

(iii) The outcome under study. In the general human capital framework a distinction is made between cognitive and non cognitive outcomes is common. In terms of timing, short term outcomes are typically measured just after the program or at the beginning of primary school. Long term effects are for example years of schooling, grades and test scores, teenage pregnancy, criminal record, earnings and welfare dependency.

Dietrichson et al. (2018) has made a systematic review of papers estimating causal effects of ECEC in conditions similar to the Swedish institutional setting. They summarize 26 studies, of which 21 was made on programs in developed countries, however none on Swedish data. Their general conclusion is that effects on grades and test scores in primary or secondary school are ambiguous and often statistically insignificant. In the studies where the outcome under study includes adequate school progression; years of schooling; or adult earnings, effects are in contrast mainly positive and precisely estimated. The most likely explanation, according to Dietrichson et al. (2018), for this seemingly contradictory patterns is that the programs evaluated with grades or test scores was of lower quality than those where long-term outcome measures were used. In the few studies where both test scores and adult outcomes were measured, the average effect was beneficial for both outcomes.

However, not all of the included studies compare pre-school attendance with parental care. Some estimate the effect of a child care subsidy that mainly shifted the mode of care from informal or privately arranged child care to a pre-school institution. As highlighted by Almond et al. (2017) in a discussion on the contradictory results from the universal pre-school expansion in on the on hand Quebec, Canada and on the other hand Norway this has to be taken into consideration. In the Canadian case a new pre-school subsidy resulted in a immediate and persistent decrease in non-cognitive skills with negative outcomes later in life such as lower self reported life quality and increased crime rate. At the same time there were no significant gains for cognitive skills (Baker et al., 2008, 2015). The Norwegian story is the opposite, while the pre-school expansion had no effects on medium term indicators of cognitive skills, such as grades and test scores, there were large positive effects on adult earning and a decreased risk of being on welfare (Havnes and Mogstad, 2011, 2015). The explanation proposed is that the Canadian reform caused middle class children to move into newly opened low quality care institutions from previously being in parental care. In the Norwegian case the expansion was well financed and the pre-schools quality (in terms of child-teacher-ratio) was high. Furthermore the expansion in Norway mainly crowded out private care arrangements and had no effect on mothers' labor supply. In Norway the effect was furthermore largest for the children from the lowest SES households and while the average effect was positive, the long-term effect for children from the highest SES households was negative.

Most studies that find a significant effect of pre-school attendance also concludes that there are heterogeneous effects with regards to SES. The importance of socio-economic factoris for early childhood environment is discussed by Bradley and Corwyn (2002). Mediating mechanism suggested include access to resources (nutrition, access to health care, cognitively stimulating experiences), increased exposure to stress (due to for instance domestic violence, crowded housing or insecure employment) and lifestyle factors like smoking, alcohol consumption, parenting style etc. The same conclusion is drawn from the broader early childhood intervention literature (see Duncan and Magnuson (2013); Almond and Currie (2010); Almond et al. (2017)). In a Swedish context Mörk et al. (2015) finds a SES gradient in hospitalization and other health measures in early childhood along the whole dimension of SES.

Table 1 summarizes the subset of studies in Dietrichson et al. (2018) that was made in developed countries and that has test scores or grades as their outcome variable.[4] The studies are then separated into three panels based on the control condition that the ECEC programs are compared against. Panel I contains the two studies that explicitly looks at changes from low quality to high quality non-parental care. They find no significant effects on test scores or grades. In panel II studies that capture the effect of changing from parental care to a public care institution are listed. The effects are significant and the SES of the parents largely determine the direction of the effect. However both Felfe and Lalive (2010) and Felfe et al. (2015) have some methodological limitations.[5] The general conclusion is that ECEC can have both beneficial and adverse impacts on children and that the home environment is crucial to determine the direction of the effect. Panel III is made up of the studies where the control condition either was hard to determine or was a mix of changing from parental care and crowding out of private care givers in the treated population. In these studies results tend to be insignificant and as discussed negative in Baker et al. (2015).

Zanella et al. (forthcoming) and Drange and Havnes (2019) are thus the only previous studies with a flawless methodology that estimates the impact of a high quality universal pre-school program on cognitive and non-cognitive outcomes in adolescence

[4] One recent study by Drange and Havnes (2019) is also added besides the selection made by Dietrichson et al. (2018).

[5] In the former there are concerns with the exclusion restriction for their IV strategy and the statistical inference is hard to assess as there are no discussion about the standard errors and how they are calculated. In Felfe et al. (2015) inference is made in a setting with state-lever variation and only a small number of states included, making correct calculation of standard errors an issue (Cameron and Miller, 2015).

where parental care was the primary control condition. They both give credence to the hypothesis that the impact can be both beneficial and harmful. It may however appear contradictory that Drange and Havnes (2019) does not find any negative effect for the children from high SES parents. Zanella et al. (forthcoming) comments on this and notes that the child-teacher-ratio was lower in Oslo than in Bologna and that this could explain the absence of a negative effect. It might be the case, but it is not possible to rule out a negative effect for high SES children in Oslo. The estimates are imprecise and the outcome variable was constructed to only report variations in the lower end of the skill distribution.

Table 1

Previous studies from developed countries with test scores and school grades as outcome under study

Article	Country	Time	Staff-child-ratio	Identification	Age	Control condition	Effect
PANEL I - COMPARED WITH MOSTLY OTHER TYPES NON-PARENTAL CARE							
Blanden et al. (2016)	UK	2002-2007	Public: 1:13. Private: 1:8 if no qualified teacher, 1:13 if qualified.	Exploits the staggered implementation of a universal part-time preschool program across Local Education Authorities in England. Compare low and high intensity areas	3-4	Mainly other informal modes of care.	Insignificant on reading. Significant but small on mathematics (0.006).
Havnes and Mogstad (2015)	Norway	1976-1979	1:8	Compare municipalities with high coverage to municipalities with low coverage (above or below median percentage point increase in preschool coverage rates)	3-6	Mostly informal (lower quality) care	Outcome is cognitive at age 18-20 and the effect is insignificant. (NB males were included).
PANEL II - COMPARED WITH MOSTLY PARENTAL CARE							

Felfe and Lalive (2010)	Germany (except Berlin)	1996-2000	East: 1:6.8 West 1:5.1	The authors use the difference in child care offer rates across Germany induced by the former East/West division as an instrument for attending preschool.	0-3	Mainly parental care	Positive effects on both cognitive and non-cognitive. (At least no negative effects according to the authors).
Felfe et al. (2015)	Spain	1991-1996	Max 20 per class	Exploits the variation in the speed of expansion across states. Divide 15 states into treatment and control based on their increase in public child care enrollment of 3-year-olds.	3	Mainly parental care, but part of the control group might have been in preschool.	Positive on reading (0.15) driven by low SES (0.17) and girls (0.19).
Zanella et al. (forthcoming)	Bologna, Italy	2001-2005	0-year-olds: 1:4. 1-2-year-olds: 1:6	Use the threshold in the admission system that determines whether children are offered a preschool slot as an instrument for attendance.	0-2	Parental care or extended family care	Negative impact on IQ (-0.045 for one extra month). Driven by high income families.

Drange and Havnes (2019)	Oslo, Norway	2005-2010	1:3	A lottery determines early access to pre-school in Oslo. Children to winners could enter preschool earlier than other children.	1	Parental care or extended family care	Positive average effect on language (0.165) and math (0.111) test score at age 7. Primarily driven by low SES families (0.25).
PANEL III - AMBIGUOUS CONTROL CONDITION							
Baker et al. (2008, 2015)	Canada	1997-2001	0-3-year-olds: 1:8 4-5-year-olds: 1:10	Introduction of the subsidy on universal preschool for children aged 0-4 in Quebec. They use other provinces of Canada as a control group	0-4	Combination of parental care and non-professional informal	Non-cognitive: Negative. Cognitive: Ambiguous effects (0.26 on PISA math, -0.23 on national math test).
Cascio and Schanzenbach (2013)	Oklahoma & Georgia, USA	1995-1999	1:10	Compare changes in preschool enrollment in the two states that introduced universal preschool initiatives with the rest of the country over the same period.	4	Low SES children moved mostly from parental care to pre-school. High SES children moved mostly from private to public preschool	Not significant.

Kühnle and Oberfichtner (2017)	West Germany	1997-2002	1:7	Exploits the December/January discontinuity to estimate the effect of attend preschool earlier and thereby attending preschool for a longer time.	2-6	Parental care or informal preschool settings (not explicitly described)	Not significant.

There are also two relevant studies on pre-school attendance in Sweden, but non with a quasi-experimental methodology. Andersson (1992) uses a longitudinal dataset on 130 children in Stockholm and Gothenburg and compares the cognitive and non cognitive abilities between children with different age at entry in child care.[6] The results indicate that early entry is positive for both for non-cognitive and cognitive abilities at age 8 and 13. Fredriksson et al. (2010) use a selection on observable framework to estimate the effect of child care on the performance gap between native and immigrant children. They find that the gap decreases when pre-school and daycare attendance increases but only for language skills, not inductive skills.

a. Relation between the present study and the previous literature

This study compares parental care with a high quality universal pre-shcool program. The main contribution is that it, in contrast to Drange and Havnes (2019); Zanella et al. (forthcoming), evaluates a shock to the pre-school attendance for children already attending pre-school, not just the effect of an earlier start. The results in this study are also less likely driven by increased labor force participation for mothers. This is an important contribution since I can exclude the causal mechanism that goes via increased household income when both parents engages in wage labor. Furthermore this study can examine heterogeneous effects conditional on the age of the child when the shock to pre-school attendance occurs.

My study also contribute to the evaluation of the Swedish childcare reform. Since policies aiming at expanding child care opportunities for parents often include large public subsidies it is relevant to evaluate their welfare implications. Baker et al.

[6] With categories age 0-1, age 1—2, age 2—6, and no child care before primary school.

(2008) argues that such policies should be evaluated along three dimensions.

(i) To what extent does public subsidies impact the availability and quality of child care alternatives for parents.

(ii) How is the labor force participation of parents affected.

(iii) How are the children affected by the change in care arrangements?

There are studies on Swedish data that analyzes the first and the second aspect (Lundin et al., 2007; Wikström, 2007) but to my knowledge no one that analyzes the effect on the children. However other studies uses the same, or a similar, variation in pre-school attendance to estimate causal effects on other outcomes. Norén (2015) looks at the effect on the gender balance in parental leave when parents are allowed to keep their older child(ren) in pre-school while on parental leave. She does not find that the policy affected the division of parental leave between mothers and fathers. Vikman (2010) analyze how the right to have your child in pre-school while being unemployed affected the length unemployment spells. The conclusion is that spell lengths decreased for mothers but not fathers. Aalto et al. (2018) looks at the effect of increased pre-school attendance on child health for children with unemployed parents. They find that there is an immediate increase in hospitalization for infectious deceases after children starts attending pre-school and that there are no significant effects on hospitalization at age 10-11. Mörk et al. (2008) find that the lower child care fee caused an increased birth rate by about 4-6 percent.

IV Empirical strategy and data

My empirical strategy hinges on the fact that municipalities had full autonomy to decide on pre-school availability for parents on parental leave (PPL) before January 2002. When a national binding minimum was implemented only municipalities that restricted access for children with infant siblings to less than 15 hours per week were affected. With the effect that children with an infant sibling in those municipalities could attend pre-school to a greater extent after than before.

Information on the local access policies before the reform was collected in surveys conducted by the National Education Agency in spring 1998 (NEA, 1998) and 2001 (NEA, 2001b). The response rate was high, 100 percent in 1998 and 97 percent in 2001.[7] Pre-reform regulations were decided by local governments, but in larger cities there could be sub-municipalital differences, in spite of this fact, NEA has only reported one answer for each municipalities which may introduce a measurement error for in particular the three largest cities Stockholm, Gothenburg and Malmö. The survey questions regarding regulation for PPL was divided into two categories. The first concerning children already attending pre-school when they got a younger sibling and the other children applying for a pre-school slot for the first time while at home with a PPL. Unfortunately the questions and answer alternatives were slightly altered between the first and second survey. The most important difference is that the 1998 wave included the alternative *Access is determined on a case by case basis.* For the 2001 wave it was dropped and the question was slightly rephrased.[8] However,

[7]Although not all municipalities answered all questions.

[8]The question was specified so that respondents should not take into account that municipalities were mandated to provide a pre-school slot for all children with special social needs, no matter their parents labor market situation. It could thus be this *case by case* consideration that the respondents

in both surveys municipalities could first answer whether or not the children could attend pre-school. If yes, the respondent they had to specify for how many hours per week and for how many months.

I categorize the municipalities by their response to the 1998 and 2001 survey, both for children with a slot and for those applying for the first time. If the children could attend more that 15 hours per week the municipality is labeled *generous*. If the children never could attend or lost their place within three months from having a sibling it is deemed *restrictive*. A third category for municipalities where children could attend but for either less than 15 hour per week or for more than 15 hour but for more than 3 month but less than 10 is labeled *partial*. The categorization process is illustrated in table 2. In both 1998 and 2001 the average time in pre-school per week for children with working parents was 31 hour per week (Wikström, 2007). Thus an increased right to attend from 0 to 15 hours per week is a sizable increase, corresponding to about 15 percent of full participation or approximately 10 extra months of full time.[9]

Similar surveys were conducted in 1995, 1996 and 1997 but the results per municipality are no longer available. However, at the aggregate level municipalities became increasingly restrictive from 1995 to 1998, and from 1998 to 2001 they become more generous again (NEA, 2001b, tabel 8), potentially as a response to the forthcoming national reform but could also be due to the decreasing number of children in

in the 1998 survey was referring to.

[9]Children with full access to pre-school from age 1,5 who attended 30 hour per week in 50 weeks per year had on average $30 \times 50 \times 4.5 = 6,750$ hours in pre-school. The reform increased access with $15 \times 50 \times 1.5 = 1,250$ hours. $1,250/6,750 = 0.15$.

Table 2

Categories of pre-school access policy prior to the reform

	Hour per week	**Maximum months**
Generous	≥ 15	> 10
Restrictive	0	0
Partial	> 0	≤ 10
or	< 15	No limit

pre-school age (see figure 1).[10] Table 3 summarizes the answers from the 1998 and 2001 surveys according to my categorization. The first four columns give the number/share of municipalities in each category. In the fifth and sixth column I show the number/share of municipalities who ended up in the same category in both 1998 and 2001.

The municipalities were more generous towards children already in pre-school when they got a younger sibling than those applying for the first time. Looking at the children already in pre-school the number of municipalities that have a generous policy is roughly the same in 1998 and 2001. However more than half of the generous municipalities in 1998 had moved to a more restrictive policy in 2001. Only 12 municipalities had a generous access policy in both 1998 and 2001.

a. Student outcomes at age 16

For the main analysis I use the universe of lower secondary school graduates from 2008 to 2017 collected by Statistics Sweden with information on year of birth. Lower

[10] The national decision to implement the reform in 2002 was made in late 2000 (2000/01:UbU5).

Table 3

Pre-reform access policies

Panel I: Children already in pre-school						
	1998		2001		Both	
	n	pct	n	pct	n	pct
Restricted	217	(75.87)	204	(73.12)	173	(90.58)
Partial	7	(2.45)	41	(14.70)	6	(3.14)
Generous	28	(9.79)	34	(12.19)	12	(6.28)
Case by case	34	(11.89)				
N	286		279		191	

Panel II: Children applying for pre-school						
	1998		2001		Both	
	n	pct	n	pct	n	pct
Restricted	206	(72.03)	237	(83.75)	169	(98.26)
Partial	1	(0.35)	23	(8.13)	0	(0)
Generous	24	(8.39)	23	(8.13)	3	(1.74)
Case by case	55	(19.23)				
N	286		283		172	

[i] The raw survey data was received via email from Swedish National Agency of Education. Categorization was made as described in Table 2.

secondary school is mandatory in Sweden and students typically graduate the year they turn 16. My data thus most likely capture the entire birth cohort from 1993 to 2000 and most of the 2001 cohort. It is presumably not fully represented due to grade repetition and late primary school entry.[11]

The outcome variables of interest are grades and standardized tests scores. At the end of lower secondary school all students are graded in the 17 subjects included on the national curriculum. Before 2013 there were three passing grades (G, VG, MVG) and one fail (IG). From 2015 forward there are five passing grades (E, D, C, B, A) and one fail (F). If a student had not attended enough lectures or handed in the required tasks they did not receive any grade NEA (2017b).

The official GPA are then calculated as the sum of the 16 best passing grades where $G, E = 10$; $D = 12,5$; $VG, C = 15$; $B = 17,5$; $MVG, A = 20$. If a student had no passing grade the GPA is not reported, making it a variable ruing from 10-320. From the grade it is also calculated weather or not the student met the national requirement to advance to upper secondary school (*gymnasiet*). This requirement is a passing grade in English, mathematics and Swedish (or Swedish as a second language). For each individual I have a indicator variable $Pass$ that is one if the individual met the requirements and zero otherwise.[12] The standardized tests are taken in the last year of lower secondary school and consists of four sub-tests in

[11]Grade repetition was also rarely used for these birth cohorts (OECD, 2018). However, the point estimates in my main results are robust to excluding the 2001 birth cohort from the sample, but with larger standard errors as a consequence. This could also be a problem for the 2000 cohort. But less than 0,06 percent of students in the earlier cohorts graduate more than one years late.

[12]After 2010 it changes so that you had to have a passing grad in those three *core curriculum* and five additional passing grades (NEA, 2017b). But for comparability over birth cohorts I use the old definition. In the later cohort this has the consequence that about 0.1 percent of the students who are codes as passing actually did not.

mathematics and three tests in Swedish.[13] The results on sub-tests are graded by the same scale as the subject grades and are then weighted to a subject test grade. In order to have a full test grade individuals had to take all sub-tests.[14] The tests are not constructed to be comparable between test years (NEA, 2017a). But comparison within birth cohorts is possible.

b. Identification strategy

To estimate the causal effect of increased time in pre-school on lower secondary school outcomes I employ both a difference-in-difference (DD) and a triple difference (DDD) strategy. Both use the fact that the national reform in 2002 forced 73 percent of the municipalities in Sweden to drastically expand pre-school access for PPL with a younger sibling. 12 percent of the municipalities were unaffected by the reform and in the other 15 percent access increased slightly. The reform thus constitutes a natural experiment where children in the treated municipalities gained better access to formal child care after the reform and children in control municipalities were largely unaffected. However the reform coincided with a sizable reduction in child care fees, which poses a threat to identification. In this section I address how I control for the fee-reduction and other potential confounders.

The main treatment/control regions are the 185 municipalities who had a restrictive/generous access policy for at least three years before the reform (Column 5,

[13] The students that follow the curriculum for Swedish as a second language take a separate test, but the results are reported in the same variable.

[14] Data is thus missing for more individuals when using test scores as the variable of interest.

Panel I in Table 3).[15] The municipalities who granted partial access are few, and can not credibly be assigned to either treatment or control group.[16]

The earliest possible pre-school entry age in Sweden is 12 months, but since the duration of payed parental leave was 14-15 months during this time most children started pre-school at age 1,5 and by age three, 99 percent of those who attended some pre-school had started (Arwidsson Hansen and Cedstrand, 2013, Tabel 43).[17] After the summer in the year the children turned 6 they transitioned into a different pre-school setting with more focus on school readiness (*förskoleklass*). This intermediary step between pre-school and lower secondary school was introduced during the 90's and became free of charge for parents in all municipalities in 1998. Although not mandatory, more than 95 percent of 6 year old's attended from 1999 and forward. It means that children could attended pre-school at most for half a year the year they turned 6. In this study I primarily focus on children who had a sibling from age 2 to 6, but not during the first or second calendar year after their birth year (see note (i) in Table 4), in order to capture the years when they were most likely to attend

[15]The assumption is that a municipality that had the same policy in 1998 and 2001 did not change during the years in between.

[16]My main results are robust to instead only using the 2001 survey results to define treatment and control municipalities (i.e. Column 3, Panel I in Table 3). However, when using the 2001 only treatment/control definition both the first stage and the ITT effect size becomes weaker, which is what to expect.

[17]In 2002 the payed parental leave policy was extended from 14 to 15 months, the additional month was earmarked for fathers. Duvander and Johansson (2010) estimate that the effect of the reform was an increase in payed parental leave uptake by fathers with on average 6 days, and 7 days for mothers. Since all children in my sample is born before 2002, the effect of the change in national parental leave policy only affected the children in my sample though the parental leave for younger siblings.

pre-school.

Universal pre-school access at 15h per week for PPL was implemented in January 2002, thus individuals in treatment municipalities who had a sibling born in 2002 or later were affected. Individuals who had a sibling in 2001 were at least partially treated, especially those with a sibling born late in the year. I do not have the exact date or month of birth for the individuals in the sample, only birth year. Thus having a sibling in 2001 is a *fuzzy* indicator for treatment that can be thought of as a *phase in* period.

Table 4 shows how child care access is determined across birth cohorts and time. The dark gray marks the cells in which the population with a sibling in the treatment municipalities are treated. The less dark represent the *phase in* period. The light gray marks the cells in which having a sibling means that you could keep your pre-school slot in the control municipalities, and loose it in the treatment municipalities. As Table 4 show only the birth cohorts from 2000 and forward are fully treated and the 1995 birth cohort is the youngest non-treated birth cohort. For the 1995 birth cohort and earlier it is only possible to determine the impact of sibling-events later than 1998.

The identifying assumption for my DD strategy is that in absence of the national reform the school results for children who had a sibling between age 2-6 would have developed along parallel trends in treatment and control municipalities. As the variation is at the municipality level I aggregate the data by municipality, sibling-event-year (1998-2006) and birth year and use panel data for my estimations.[18] When

[18]The results in the DD and DDD models are robust to instead using individual level data, but aggregating is the conservative approach (Bertrand et al., 2004).

aggregating only children who had a sibling born between 2-5 years after their own birth year are included. To make sure that all individuals correctly can be assigned a *sibling-event-year* those who have siblings born in more than one year in this span are dropped. Individuals who have a sibling born in $birthyear+1$ or $birthyear+6$ are also dropped to avoid contamination from other siblings born close in time. Some individuals with these characteristics was potentially affected by the reform, but most were either to young or to old too be fully exposed.[19]

Following Bertrand et al. (2004) I regress my outcome variables on a set of exogenous background variables (indicators for gender, foreign born, one or two parents born abroad and birth order) and calculate residuals before aggregating the data.[20] This controls for potential changes in the composition of individuals, between birth cohorts, not related to the treatment. I use the panel to estimate the difference-in-difference in a regression framework using the following model:

$$y_{m,\tau,t} = \theta_m + \theta_\tau + \theta_t + \beta \mathbf{I}(REFORM)_{m,\tau \geq 2002} + \varepsilon_{m,\tau,t}, \tag{1}$$

where $y_{m,\tau,t}$ is a measure for average lower secondary school results in municipality m, for individuals who had a sibling in between the age of 2-6 in year τ and was born in year t. θ_m is the municipality fixed effects that will capture all unobserved time invariant municipality characteristics and θ_τ is the fixed effects of the sibling-event-years $\tau \in \{1998, 1999, \ldots, 2006\}$. This will capture all year specific national shocks to children in families with a newborn, like changes in the payed parental leave legislation. θ_t is the birth year fixed effects that capture national trends in school results

[19] The same is true for some of the individuals who had a sibling the second year after their birth but my main results are robust to excluding them from the sample.

[20] This is done on the full dataset, containing birth cohorts 1993-2001, before applying any sample restrictions.

like grade inflation and shocks introduced by national policy.[21] $\mathbf{I}(REFORM)_{m,\tau\geq2002}$ is a dummy that is one if the individual has a sibling in 2002 or later and lived in one of the treatment municipalities, and otherwise zero. β is my coefficient of interest and can be interpreted as the difference-in-difference between having a sibling before and after the reform in treated and control municipalities. As the children who had a sibling in 2001 also were affected by the reform, but for a shorter time, I use with different specifications as a robustness test. One where I use $\mathbf{I}(REFORM)_{m,\tau>2001}$ and one where both the variables $\mathbf{I}(PHASEIN)_{m,\tau=2001}$ and $\mathbf{I}(REFORM)_{m,\tau\geq2002}$ is included in order to control for the phase in period. The error term $\varepsilon_{m,\tau,t}$ contains all unobserved municipality by sibling-event and birth year specific shocks to human capital formation process of children. If the identifying assumption holds it is independent of the left hand side of (1). Since I have aggregated the data and weight the regressing by the numbers of individuals in each cell, the bias from auto-correlation in the error term, when calculating my standard errors, is greatly reduced (Cameron and Miller, 2015). But to account of remaining correlation between birth cohorts within municipalities I cluster the standard errors at the municipality level.

The weakness of the DD strategy is that it cannot control for unobserved municipality and time specific shocks. This is a problem since the expanded access to pre-school for PPL coincided with the national child care fee cap (maxtaxa). From 2002 and forward the fees were practically the same in all municipalities. Before there were large heterogeneity both between municipalities and between households within municipalities since fees were related to household income (Lundin et al.,

[21] During the primary and lower secondary school years of these birth cohorts many new national legislation and policies were introduced: a new grading scale (2011), earlier grades (2012), more standardized tests etc. For a full list of reforms and discussion on their effects see SOU 2013:30.

2007). Just controlling for the average size of the reduction in each municipality is thus not enough. The price elasticity of child care might also have been different in different regions.[22] It is thus possible that, in particular high earners in the control municipalities abstained from sending their older children to pre-school while on PL before the reform because of high cost, even if the municipality had a generous policy. If that is the case a reduction in fees might have increased enrollment in the control municipalities, even though the access policy did not change.

In order to control for this I use a triple difference (DDD) strategy where the identifying assumption is that the relative difference in the outcome variables between children with and without younger siblings would have been the same before and after the reform in treated and control municipalities in absence of the reform (Gruber, 1994). This is a rather week assumption that only requires that there were no other shocks to the human capital formation process (i.e. primary and lower secondary school) that affected children with and without younger sibling differently in treatment and control municipalities and coincided with the reform. As local education policies seldom target these groups differently once children pass the age of 6 it seems like a plausible assumption. If it also can be assumed that having another child within 1-7 years after an older sibling is not related to parental income, it will also effectively control for the fee reduction.[23]

[22]In rural areas for instance there might also be both a time cost and a pecuniary cost of transporting children to and from a pre-school.

[23]This assumption can be empirically tested, but in the scope of this thesis I do not have the data that would allow me to do it. One potential source of correlation between having a sibling and parental income could however be the parents age are on average higher for children with one or two older siblings, and thus potentially also the income, it is also one motivation for adding birth order in my set of control variables.

To do this I separate the individuals in my data into three categories:

(i) Those who had a sibling as describes in the DD-section ($n = 151,903$).

(ii) Those who did not have any sibling between the years $birthyear{+}2$ to $birthyear{+}5$ ($n = 268,318$).

(iii) Those who do not fall under category (i) or (ii) ($n = 54,561$).

Individuals in (iii) are dropped and after that I assign a dummy to the remaining observations that is 1 if they are (i) and 0 if they are (ii). Individuals who had a sibling before 1998 are dropped. I then aggregate the dataset over municipality and birth year partitioned by the dummy for having a sibling.[24] The DDD estimation is made within a regression framework, where I estimate the model:

$$y_{m,t,s} = \theta_{m,t} + \theta_{m,s} + \theta_{t,s} + \beta(\mathbf{I}(REFORM)_{m,t\geq 1999} \cdot \mathbf{I}(SIBLING)_s) + \varepsilon_{m,t,s}, \quad (2)$$

where $y_{m,t,s}$ is the mean of the outcome variable for individuals born in year $t \in \{1993, 1994, \ldots, 2001\}$, living in municipality m, who did or did not have a sibling s during their pre-school years. $\theta_{m,t}$ is the birth year-by-municipality fixed effects that captures any cohort and municipality specific shock other than the reform, like the childcare fee reduction. It also capture municipality specific grade inflation.[25] $\theta_{m,s}$ is the municipality by having a sibling fixed effects accounting for fixed municipality specific differences in for instance mothers' age at child birth. $\theta_{t,s}$ are the birth year by having a sibling fixed effect capturing time trends in the effect of having a

[24] I use the same method of calculating residuals before aggregating as in (1).

[25] One source of municipality specific grade inflation that has been discussed in the literature in the increased incidence of independent schools (*friskolor*) in some municipalities, with more competition between schools as a result. The effect of this on grade inflation is however debated (see Böhlmark and Lindahl (2012) and Vlachos (2010)).

younger sibling. $\mathbf{I}(REFORM)_{m,t\geq 1999}$ is a dummy that is one if the individual lived in a treatment municipality and is born 1999 or later and had a sibling after the reform and otherwise zero. β is the parameter of interest and can be interpreted as the difference-in-difference-in-difference (hence triple difference).[26] The error term $\varepsilon_{m,t,s}$ include both the idiosyncratic error and unobserved shock that effects children with and without siblings differently within each municipality-by-birth cohort combination. To account for correlation in error term within municipalities, standard errors are clustered at the municipality level. As with the DD-strategy there are multiple ways of dealing with the phase-in period. For the triple difference it is the 1998 and 1997 birth cohort that is partially treated.[27] Thus I explore different ways to specify the reform variable. (1) Use $\mathbf{I}(REFORM)_{m,t\geq 1998}$ instead to include more observations in the treated period. (2) Include another policy variable $\mathbf{I}(PHASEIN)_{m,t\in\{1997,1998\}}$ in order to control for the phase in period. (3) Exclude the 1997 and 1998 birth cohort from the estimation.

[26]The interpretation become more intuitive in a clean two regions, two periods, two groups setting. Say that we only have a treatment (T) and control (C) region; one period before the reform (pre) and one after (post); one group who had a sibling (S) and one who did not (N). The we can note that:

$$\beta = \big((y_{T,post,S} - y_{T,pre,S}) - (y_{C,post,S} - y_{C,pre,S})\big) - \big((y_{T,post,N} - y_{T,pre,N}) - (y_{C,post,N} - y_{C,pcre,N})\big)$$

[27]In the 1997 cohort of the individuals who had a sibling, 12,5 percent had their sibling in 2002, and 20,1 percent had it in 2001. From the 1998 cohort 33,6 percent had a sibling after 2002 and 32,3 percent in 2001.

c. Child care utilization

The ultimate goal of this study is to estimate the causal effect of attending pre-school instead of parental care in the home. Even if the identifying assumption for my empirical strategies hold, this is not the interpretation of the β in (1) and (2). The correct interpretation is the causal effect of gaining the opportunity to spend more time in pre-school. The increase in *potential* time in pre-school after the reform in the treatment municipalities was approximately 10 months of full participation. But sending your child to pre-school while on PL is an individual decision and not all families used their newly gained right. In addition some parent might have keep their children in pre-school but for less than the maximum 15 hours per week. The β averages out the effect over all the children in the treatment group, no matter their actual time in pre-school. It is thus to be interpreted as the *Intention-To-Treat* effect (ITT). And as there are no individual data on pre-school attendance from this time it is not possible to identify the *Treatment-On-the-Treated* (TOT) effect with full precision. There are however some advantages with the ITT as it captures the full effect of the reform, including peer-effects on children not attending pre-school (Havnes and Mogstad, 2011). In terms of public policy it is also relevant to know the ITT when forced participation is not an option. But for research purpose it is the TOT that is of ultimate interest. I thus use data from a the national child care survey 1999 (NEA, 2001a) and 2002 (NEA, 2004a) to estimate how much of the increase in pre-school attendance for children with PPL can be attributed to the policy. This estimate is used to scale the estimated ITT with the average increase in pre-school attendance in the treatment municipalities. The surveys were made on a large number of randomly selected parents of 1-5 year old children from all municipalities ($n \approx 60000$). The response rate was above 90 percent in both 1999 and 2002 and the results are weighted by the total population. For this study NEA has prepared a dataset where the answers from both surveys has been summarized by municipality for three categories of parents (i) both parent work or study full time, (ii) at least one parent is on parental leave with a younger sibling, (iii) others including households with one unemployed parent.[28] There were 10 answer alternatives for mode of child care but in my data the answers are compiled to either formal child care (public pre-school, independent pre-school or day care) or non-formal child care (care in the home by the parent, a relative or private nanny).

As discussed in section IV.b. it is important to control for the municipality specific shock to the child care fees that coincided with the reform when estimating the effect. Thus the triple difference strategy translates naturally to estimating the effect on child care utilization. Consequently I estimate the following model:

$$CC_{m,t,j} = \theta_{m,t} + \theta_{m,j} + \theta_{t,j} + \gamma(\mathbf{I}(REFORM)_m \cdot \mathbf{I}(PL)_j \cdot \mathbf{I}(t = 2002)_t) + \xi_{m,t,j}, \tag{3}$$

where $CC_{m,t,j}$ is the share of parents with household characteristics j (on parental leave or working/studying) answering that their children attend some formal child care in municipality m in survey year t. $\mathbf{I}(REFORM)_m$ is one for the treated municipalities and zero for the control group. $\mathbf{I}(PL)_j$ indicate if the parents are on parental leave or working/studying. γ is the the triple difference and captures the effect of the reform on pre-school attendance for children with PPL. The regressions are weighted by the number of parents in each cell.

[28] Received by email from Hanna Karlsson Ruiz, Avdelningen för analys at NEA (2019-05-13)

d. Sample restrictions

Before runing any regression I exclude all individuals who migrated to Sweden after the age of 2. All individuals with an international migration history between age 2 and graduating from lower secondary school are also excluded.[29]

Adopted children are excluded since I do not know the year of adoption, as will individuals who have adopted siblings that are within an age span of 6 years older or 6 years younger. This is because I only have birth year for adopted siblings and not the year of adoption, the birth year of an adopted sibling will thus not identify the shock to pre-school attendance in the same way a for a biological sibling.

Furthermore, I exclude individuals who have younger half siblings born within 1-6 years after their own birth year. It is ambiguous how such sibling relations affected the right to pre-school before the reform. For instance children who lived exclusively with only one of their biological parents, the fact that the other had another child should not affect their pre-school attendance.

The sample restrictions described above limits my sample from just over 900 000 individuals to around 770 000 individuals. Table 13 in the appendix summarizes the sample before and after restrictions.

[29]It is unclear to what extent newly arrived children attended pre-school. When migrating to Sweden with young children parents were allowed to use the payed parental leave which at least should have postponed pre-school entry. The main results are however robust to including individuals who migrated to Sweden up to age 5.

e. Descriptive statistics

In Table 5 I present some summary statistics from before (1999, 2000, 2001) and after (2002, 2003, 2004) the reform for treatment and control municipalities. The data, come from a publicly available database of comparative figures on the municipality level (NEA, 2019). Aggregated results from the child care survey is also added.

The first panel contains pre-school relevant variables such as the share of 1-5 year old in pre-school, child-teacher-ratio, public cost per citizen for child care etc. Some general demographic characteristics are also reported. The second panel is the results from the two child care surveys that was made in 1999 and 2002.

The share of 1-5 year old in pre-school increased more in the treated than the control municipalities (C: +2.6 vs T: +11.8). The share was however higher in the control municipalities both before and after, indicating that there are some underlying differences in the demand for pre-school. The treatment and control municipalities are initially rather different along all dimensions reported, but the trend from before to after is similar across the variables. The notable exception is the child-teacher-ratio in pre-school which was roughly the same before the reform, but it decreased in the control group and slightly increased in the treatment group after. It suggests that the treatment municipalities had just enough resources to compensate for the increased number of children in pre-school, but that the control group could use the new pre-school grant to invest in more teachers. In the control municipalities child-care cost per citizen was a bit higher both before and after, which it may seem paradoxical as neither the age structure, nor the child-teacher-ratio motivates this (at least before the reform). The most likely explanation is higher administrative costs in the control municipalities (especially real-estate cost) as they are mostly (but not exclusively) urban areas.[30]

The parental survey show that a large majority of parents in the control group used the possibility have their children in public child care while on PL. It might seem surprising that about one forth of PPL in the treatment group also had their children in child care. This is likely due to two factors, (a) children with special social needs had always the right to attend pre-school and (b) the survey was made in a particular week and it asked for the families current child care arrangements. It means that some of the children might have just had a younger sibling, but not yet been sent home from pre-school.[31] However the increase in child care utilization was substantial in the treatment municipalities after the reform, and in comparison to the control group it was more than three times as large.

Turning to the sample used for estimation, Table 6 summarize some key variables for the individuals with a sibling. Column 1-3 reports the averages for the individuals in the control municipalities before, in the phase in period and after the reform. Column 4-6 does the same in the treatment municipalities. Both maternal education level and number of younger siblings are measured at the age of graduation and should thus be interpreted with caution as they might be endogenous to the reform. With that caveat we can see that individuals in the control municipalities are positively selected in terms of lower secondary school results and have about 1/3 of a standard deviation higher GPA and test scores both before and after the reform. The same is true for passing lower secondary school, but here the difference is smaller, only 10 percent of a standard deviation.

[30]Half of them are located in the capital region (*Stockholms Län*).

[31]As Table 2 show some restrictive municipalities allowed the children to stay for 1-3 month after having a sibling.

If having a sibling at different age affect how a shock to the time in pre-school impacts an individual's skill level, compositional changes in the age at sibling event could bias the result, but the variables birth year and sibling event year indicate that the pattern is the same in treatment and control group over time.

In Table 7 I summarize the same variables as in Table 6, but now by birth year include the children who did not have a sibling during their pre-school years.[32]

Children who had a younger sibling are positively selected in both treatment and control group in comparison to those who did not have a younger sibling. The no-sibling group could have younger siblings after the age of 6, but very few does. On average they had 0.1 younger siblings when graduating from lower secondary school.

Since children who had a sibling before 1998 are dropped from the sample, those who had a sibling and are born before 1996 were slightly older when they got their sibling than the later cohorts. If the effect of pre-school attendance depends on the age of the child, this might introduce a bias in my estimation. This can be solved by either limiting my control group to the 1995 and 1996 birth cohorts, or including the children who had a sibling born before 1998, even though I can not know how a sibling event affected their right to pre-school. These are useful specifications checks that I will run in order to test the robustness of my results.

[32] Note that the individuals included in columns (7)-(12) are basically the same same as those in 6, but that the categorization is different.

Table 4

Schematic depiction of treated birth cohorts

Birth year	Year 1998	1999	2000	2001	2002	2003	2004	2005	2006	Expected graduation
1993	5	6	7	8	9	10	11	12	13	2009
1994	4	5	6	7	8	9	10	11	12	2010
1995	3	4	5	6	7	8	9	10	11	2011
1996	2	3	4	5	6	7	8	9	10	2012
1997	1	2	3	4	5	6	7	8	9	2013
1998	0	1	2	3	4	5	6	7	8	2014
1999		0	1	2	3	4	5	6	7	2015
2000			0	1	2	3	4	5	6	2016
2001				0	1	2	3	4	5	2017

[i] Every cell represents the average age of individuals born in a year (rows) at a certain time (column). In the year of birth the average age is of than 0, but it is written for simplicity.

[ii] Since it is only possible identify individuals and sibling by birth year I have to determine if a individual has a sibling within each cell of the table. The interpretation is that an individual born in t and had a sibling in $t+i$ was somewhere between $i-1$ and $i+1$ years old. If birth date is homogeneously spread out over the year the average individual within a cell that have a sibling was i years old at the sibling-event. This assumption should hold up for sibling-event-years>birthyear+2 but for closer parings the distribution is skewed to the right (Statistics Sweden, 2017, diagram 3.1). The average age at a sibling event in those cells that are marked 2 are somewhere between $2,25$-$2,5$, and less than 10 percent are younger than 1,5 years. In those marked 1 the average age is most likely 1,5.

[iii] The number of children who get a younger sibling at birthyear+2 and birthyear+3 exceeds those who get a sibling at birthyear+4 and birthyear+5 by far. Thus the *sibling-events-weight* of a specific cohort is tilted to the left side of the table. For example in the 1997 cohort 67,2 percent of those who had a sibling had before 2001.

Table 5

Summary statistics for municipalities

	Control		Treatment	
	Before	After	Befor	After
	(1)	(2)	(3)	(4)
PANEL I MUNICIPALITY CHARACTERISTICS				
Share of 1-5 y.o. in PS	76.48	78.04	62.74	73.52
	(4.27)	(3.30)	(8.15)	(6.26)
Child-teacher-ratio in pre-school	5.33	5.08	5.42	5.47
	(0.27)	(0.35)	(0.57)	(0.43)
Public child care cost per citizen	5261.81	5835.09	4476.77	5133.79
	(428.63)	(607.58)	(702.32)	(842.45)
Public education cost	10108.12	11114.24	11523.82	13329.99
	(1185.18)	(1360.76)	(1209.50)	(1204.78)
Population share 1-5 y.o.	5.10	5.08	5.44	5.28
	(0.49)	(0.44)	(0.80)	(0.78)
Children in families on welfare (pct)	10.61	8.46	9.11	7.37
	(3.70)	(3.00)	(5.81)	(4.87)
Population with low education (pct)	13.93	12.74	22.02	18.98
	(3.67)	(2.94)	(4.90)	(4.14)
Foreign citizen (pct)	5.42	7.49	2.41	3.76
	(1.28)	(2.85)	(1.48)	(2.53)
Degree of urbanization	97.59	97.79	81.06	81.11
	(6.95)	(6.77)	(12.92)	(12.87)
Observations	12	12	173	173
PANEL II CHILD CARE SURVEY	1999	2002	1999	2002
Share in child care by parents status				
Parental leave (with younger sibling)	79.33	90.63	25.89	64.71
	(17.01)	(14.52)	(11.24)	(15.11)
Work/study	92.21	94.85	88.50	93.38
	(2.05)	(2.01)	(4.34)	(3.69)
Other	64.45	69.84	51.95	62.78
	(7.22)	(6.82)	(10.92)	(8.69)
Observations	12	12	161	161

[i] Standard deviations are reported in the parenthesis.

[ii] In the first panel means are weighted by the total number of 1-5 year old in the relevant year (Statistics Sweden). In the second the I weight the averages with the weights provided by NEA for the child care survey for each year respectively.

[iii] Some observations are missing from the treatment group in the child care survey from 2002. This is mostly small rural municipalities.

Table 6

Summary statistics for DD-strategy

	Control			Treatment		
	Sibling event year			Sibling event year		
	≤ 2000	2001	≥ 2002	≤ 2000	2001	≥ 2002
	(1)	(2)	(3)	(4)	(5)	(6)
GPA	239.3	244.3	249.4	219.8	224.6	229.8
	(60.6)	(56.4)	(56.5)	(58.3)	(55.4)	(56.8)
Pass	0.95	0.94	0.95	0.93	0.93	0.93
	(0.23)	(0.23)	(0.21)	(0.26)	(0.25)	(0.26)
Mathematics test score	13.1	13.3	13.6	11.6	11.9	12.0
	(5.5)	(5.2)	(4.9)	(5.5)	(5.2)	(5.2)
Swedish test score	14.2	14.7	15.0	13.3	13.6	13.7
	(3.8)	(3.8)	(3.6)	(3.9)	(3.9)	(4.1)
Graduation year	2011.9	2013.9	2015.8	2011.9	2013.9	2015.8
	(1.3)	(1.1)	(1.1)	(1.3)	(1.0)	(1.1)
Birth year	1995.8	1997.9	1999.8	1995.9	1997.9	1999.8
	(1.3)	(1.0)	(1.1)	(1.3)	(1.0)	(1.1)
Sibling-event-year	1998.98	2001.00	2003.23	1998.98	2001.00	2003.21
	(0.82)	(0.00)	(1.11)	(0.82)	(0.00)	(1.11)
Birth order	1.32	1.34	1.36	1.35	1.33	1.33
	(0.71)	(0.73)	(0.78)	(0.74)	(0.71)	(0.70)
No. younger siblings	1.19	1.20	1.19	1.19	1.19	1.19
	(0.48)	(0.50)	(0.47)	(0.47)	(0.47)	(0.47)
Mother's education (age 16)						
Low	0.24	0.22	0.20	0.31	0.28	0.23
	(0.43)	(0.41)	(0.40)	(0.46)	(0.45)	(0.42)
Medium	0.29	0.27	0.30	0.32	0.34	0.36
	(0.45)	(0.44)	(0.46)	(0.47)	(0.48)	(0.48)
High	0.45	0.51	0.50	0.36	0.37	0.40
	(0.50)	(0.50)	(0.50)	(0.48)	(0.48)	(0.49)
Foreign born	0.01	0.01	0.01	0.00	0.01	0.01
	(0.09)	(0.08)	(0.10)	(0.06)	(0.07)	(0.08)
Born in Sweden with						
Two foreign parents	0.16	0.17	0.16	0.08	0.08	0.08
	(0.36)	(0.38)	(0.37)	(0.27)	(0.26)	(0.27)
One foreign parent	0.13	0.14	0.14	0.08	0.08	0.09
	(0.34)	(0.35)	(0.35)	(0.28)	(0.28)	(0.28)
No foreign parent	0.70	0.68	0.68	0.84	0.84	0.83
	(0.46)	(0.47)	(0.47)	(0.37)	(0.37)	(0.38)
Share female	0.49	0.50	0.48	0.48	0.49	0.48
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Observations	8,352	2,742	9,145	40,523	12,753	41,811

[i] Standard deviations in parenthesis. The number of observations are slightly less than reported for some variables since data is missing for some individuals, this is mainly a concern for the test score variables.

[ii] The three dummies for maternal education level is constructed from the three digit SUN2000 codes. Low: SUN<330 which includes everyone with 2 years of upper secondary school or less. Medium: 330≥SUN<526 which include everyone with more than 2 years upper secondary and less than 2 years of tertiary. High: 526≥SUN which includes everyone else. Those with SUN=999 are codes as missing.

Table 7

Summary Statistics for DDD-strategy

	No younger sibling during pre-school age						One younger sibling between age 2-5					
	Control			Treatment			Control			Treatment		
	Born $\leq$ 1996 (1)	Born 1997-1998 (2)	Born $\geq$ 1999 (3)	Born $\leq$ 1996 (4)	Born 1997-1998 (5)	Born $\geq$ 1999 (6)	Born $\leq$ 1996 (7)	Born 1997-1998 (8)	Born $\geq$ 1999 (9)	Born $\leq$ 1996 (10)	Born 1997-1998 (11)	Born $\geq$ 1999 (12)
GPA	224.8	228.1	239.7	205.1	210.8	218.6	238.6	240.3	251.1	217.7	223.2	231.0
Pass	0.92	0.92	0.93	0.89	0.90	0.90	0.94	0.94	0.96	0.92	0.93	0.93
Mathematics test score	11.53	12.81	12.72	10.28	11.48	11.01	12.62	13.73	13.64	11.03	12.38	12.00
Swedish test score	13.49	14.14	14.45	12.46	13.12	13.25	14.08	14.58	15.06	13.09	13.58	13.74
Graduation year	2010.4	2013.5	2016.0	2010.4	2013.5	2016.0	2011.2	2013.5	2016.0	2011.2	2013.5	2016.0
Birth year	1994.4	1997.5	2000.0	1994.4	1997.5	2000.0	1995.2	1997.5	2000.0	1995.2	1997.5	2000.0
Sibling-event-year							1998.8	2000.6	2003.1	1998.8	2000.6	2003.1
Birth order	2.00	1.96	1.97	2.14	2.07	2.05	1.35	1.34	1.34	1.38	1.34	1.31
No. younger siblings	0.08	0.08	0.07	0.07	0.07	0.07	1.19	1.20	1.19	1.19	1.19	1.19
Mother's education (age 16)												
Low	0.32	0.30	0.26	0.41	0.38	0.34	0.25	0.23	0.19	0.33	0.28	0.23
Medium	0.26	0.28	0.29	0.28	0.30	0.33	0.28	0.29	0.30	0.32	0.34	0.37
High	0.40	0.41	0.44	0.29	0.31	0.32	0.45	0.47	0.51	0.35	0.37	0.40
Foreign born	0.01	0.01	0.01	0.00	0.00	0.01	0.01	0.01	0.01	0.00	0.00	0.01
Born in Sweden with												
Two foreign parents	0.16	0.17	0.17	0.07	0.09	0.08	0.17	0.16	0.16	0.08	0.08	0.08
One foreign parent	0.16	0.16	0.17	0.10	0.10	0.10	0.13	0.14	0.14	0.08	0.08	0.09
No foreign parent	0.68	0.66	0.65	0.82	0.81	0.81	0.69	0.69	0.69	0.83	0.84	0.83
Share female	0.48	0.49	0.49	0.49	0.48	0.49	0.49	0.49	0.49	0.48	0.49	0.49
Observations	22,750	9,977	14,730	110,322	45,205	65,334	6,017	5,374	8,848	28,650	25,921	40,516

[i] The standard deviations are not reported for space economy reasons, but they are similar to those in Table 6. The number of observations are slightly less than reported for some variables since data is missing for some individuals, this is mainly a concern for the test score variables.

V Results

In this section i present my main empirical findings and provide support for the assumptions underlying my identifications strategy. First I present graphical evidence for the parallel trends assumption. Then the impact of the reform on child care utilization in the treatment is presented. This will be the baseline to scale my ITT estimates to the TOT effect. In Table 9 the main result is presented with a following section on heterogeneous effects. In the last part I run a number of specifications checks, including a placebo test in order to establish the robustness of my results.

a. Examining the parallel trends assumption

The results from my analysis can be interpreted as a causal effect of pre-school attendance only if the post reform trend in the outcome under study in the control municipalities is a good counterfactual trend for the treatment municipalities. The standard way to test this is to graphically inspect the pre-refrom trend in the treated and non-treated regions and assess if it diverges or converges over time (Abadie and Cattaneo, 2018). In Figure 2 I plot the residuals used to estimate my regression for all individuals in my sample who were born from 1993-1998 and did not have a sibling in 2001 or after. Those who had a sibling in 2000 and before are the sibling-sample. Since no individual included was directly affected by the reform the trends should be parallel. The gray area represents a 95 percent confidence interval for each mean, Note that it is the latest birth cohorts that are most relevant for assessing the trends, In the birth cohorts 1993 and 1994 a lot fewer individuals who had a sibling are includes, as anyone who had a sibling before 1998 is excluded from the sample. Thus the confidence interval is a lot larger.

Trends seem parallel for all outcomes and there are no strong evidence for separate trends in the treatment and control municipalities. More importantly for the assumption underlying the triple difference strategy - the relative difference between individuals who did, and did not get a sibling during their pre-school years seems to be parallel. The possible exception would be the *Pass* variable where the confidence interval is so large that the trends are hard to assess with accuracy.

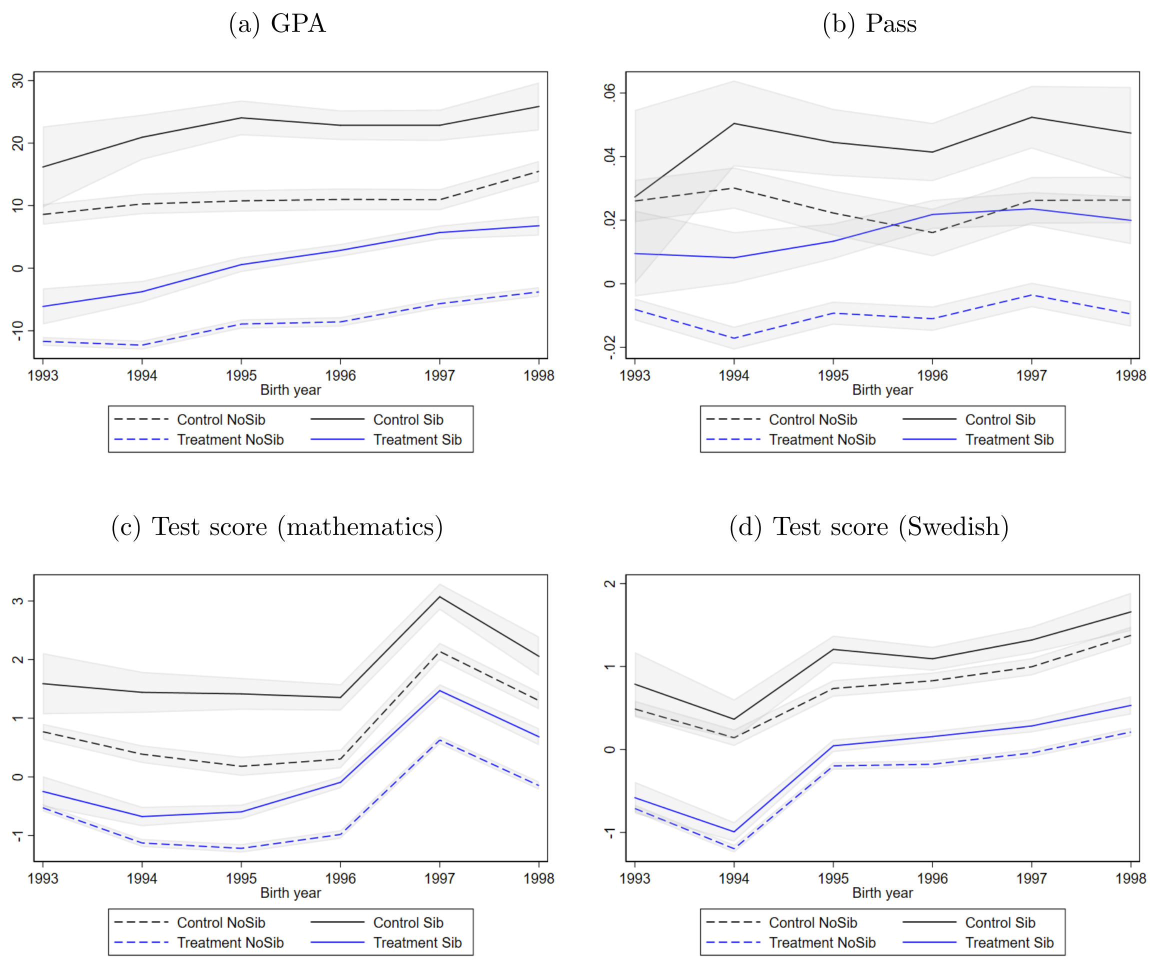
b. Effect on child care utilization

As shown in Table 3, the share of PPL who had their child in formal child care increased three times as much in the treatment than the control municipalities from 1999 to 2002. In Table 8 I present the results from a more formal analysis where I estimate (3). Column 1 is the difference-in-difference where only the PPL are compared in the treatment and control group before and after the reform. Column 2 is the triple difference where the PPL are compared with all other parents and in column 3 I use only the working/studying parents as my within municipality control group. This column is includes since the working/studying parents might be a better counter factual for the PPL than all other parents. But as Table 8 show the results are very similar.

The results show that the increase in pre-school attendance for children with PPL that can be attributed to the reform was 22,9 percent. I use this estimate to scale my *intention to treat* estimates: $TOT = ITT/0.229$. When interpreting the TOT parameter it is important to keep in mind that the sample restriction is not the same in the two settings. The child care survey includes the whole population of children born from January 1997 to August 2001 and my estimation sample for the ITT effect contains more birth cohorts and are restricted to those who had

Figure 2

Pre-reform trend for the outcome variables



either exactly one or none full sibling during their pre-school years. For the TOT parameter to be unbiased one have to assume equal effect of the reform on child care utilization in the restricted and unrestricted sample (Havnes and Mogstad, 2011). If the excluded children were more (less) likely than those included to attend pre-school it will introduce a downward (upward) bias.

Table 8

Reform effect on child care utilization

	Share in child care	Share in child care	Share in child care
	(1)	(2)	(3)
Reform x 2002	0.255***		
	(0.0405)		
Reform x 2002 x PPL		0.229***	0.233***
		(0.0472)	(0.0447)
Observations	65,443	535,038	423,024

Note: Each column represents a separate regression using the STATA command `reghdfe` (Correia, 2016). Column 1 include a dummy for survey year, and municipality fixed effects. Column 2 and 3 include municipality fixed effects separately interacted with a dummy for survey year and a dummy for PPL and an interation between the suvey year dummy and the dummy for PPL. The standard errors are calculated using robust standard errors clustered at 173 municipalities and are reported in parentheses. The regression are weighted by the number of children in each cell.

* Significant at 5 percent,

** Significant at 1 percent,

*** Significant at 0.1 percent

c. The effect of pre-school on school results at age sixteen

I estimate (1) and (2) separately for the four outcome variables of interest. The results are presented in Table 9. Panel A are the results from estimating the DD-model and point estimates are small and insignificant on all outcomes except test scores in Swedish, where the estimate is negative and highly significant. The results from the DDD-model is reported in Panel B. The point estimate for GPA indicate a positive treatment effect, but it is only significant at the 10 percent level (t-value= $1,86$). On *Pass* the estimate is small and statistically insignificant. For the two test scores the results on the test in mathematics is positive and significant, and the negative effect on the Swedish test from Panel A is now much smaller and statistically insignificant.

I scale my ITT estimates to the TOT. For GPA this is $2.1/0.23 = 9.1$ which represents improving one grade from fail to pass or to improve four steps on the passing dimension on the grade scale. For the test scores in mathematics the TOT is $0.33/0.23 = 1.4$ which is the equivalent of half a step on the passing end of the grade scale. These estimates is equivalent to 13 and 23 percent of a standard deviation for the respective outcomes.[33] The magnitude of the result on mathematics is similar to those found in for instance Drange and Havnes (2019) and Baker et al. (2015).[34] Although the point estimates on *Pass* and test score in Swedish are positive I can not rule out a negative effects of pre-school attendance. If the lower value for the 95 percent confidence interval is scaled to a TOT it would suggest -0.2 s.d. for test score in Swedish and -0.1 s.d. for *Pass* (equivalent to -2 percentage points).

d. Heterogeneous effects on school results

As discussed in section III there are reason to believe that there are heterogeneous effects of changing from parental care to a pre-school institution. The quality of care in the home is often correlated with household income and parental education level Dearing and Taylor (2007). There are also evidence that suggest a gender in pre-

[33] I use the standard deviation for the entire population before applying the sample restrictions. For the GPA variable the s.d. is stable over time, but since the standardized test can differ in difficulty between years the s.d. ranges from between 5-6 points between years. I take the conservative approach and use 6.

[34] Baker et al. (2015) find positive results for PISA test in mathematics by about 0.2 s.d. but at the same time a almost equal in size but negative impact on a domestic standardized test.

Table 9

ITT-effect on school results at age sixteen

			Test Score	
	GPA	Pass	Mathematics	Swedish
	(1)	(2)	(3)	(4)
Panel A: Difference-in-Difference				
Reform x Post	0.0441	-0.00558	-0.0672	-0.248***
	(1.052)	(0.00386)	(0.113)	(0.0544)
Municipality FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Sibling-event-year FE	Yes	Yes	Yes	Yes
Observations	115,326	115,261	108,545	107,463
Panel B: Triple difference				
Reform x Post x Sib	2.110	0.00163	0.333***	-0.0669
	(1.132)	(0.00394)	(0.0918)	(0.0616)
Municipality-by-birth year FE	Yes	Yes	Yes	Yes
Municipality-by-sibling FE	Yes	Yes	Yes	Yes
Birth year-by-sibling FE	Yes	Yes	Yes	Yes
Observations	383,644	383,468	356,662	357,325

Note: Each column in Panel A and B represents a separate regression using the STATA command `reghdfe` (Correia, 2016). The outcome variables are the averages residuals calculated from regressing the outcome on a gender dummy, four categories of family migration background and a full set of birth order dummy (only including full sibling). The residuals was calculated using the STATA command predict. The standard errors are calculated using robust standard errors clustered at 185 municipalities and are reported in parentheses.

* Significant at 5 percent,

** Significant at 1 percent,

*** Significant at 0.1 percent

school practices Persson (2008). One additional aspect of pre-school is the increased socialization with other children and as a child's developmental stages vary with age we might suspect that the age at the shock to pre-school attendance happened might influence the results.

Just looking at the results in Table 9 we can hint at some variation in the treatment effect. As the *Pass* variable only measures student outcomes on the margin of pass/fail we would expect effects to be significant for that outcome if the positive effect was driven by individuals who struggle to succeed in lower secondary school. That is however not the case. That the effect seem to be larger and more precisely estimated on GPA than *Pass* indicates that the gain in school results is not on the pass/fail margin.

To more thoroughly investigate the heterogeneous effects hypothesis I present results in Table 10 from (2) for a subset of the population that represent groups where we suspect heterogeneous effects. I use domestic or foreign background and mothers' education level as two different attempts to capture SES. An important caveat is that the mothers' education level is measured at age 16 and not at child birth. The latter would have been more adequate for this purpose, as the education level of the parents might be endogenous to the reform. The results where the population in selected on basis of that variable must thus be interpreted with caution.

The estimates in Table 10 are quite stable across groups. For GPA we see some variation in the point estimate indicating that children with foreign background gained more from attending pre-school. However, with so few observations the precision is low. Results on test scores in mathematics are significant for all groups except those with low-educated mothers and the point estimates are very similar, although slightly higher for men than women.

The estimates from low/high educated mothers deserve specific consideration. They are the opposite of what most of the literature suggest. The impact for children with low educated mothers is insignificant for all outcomes except *Pass* where it is negative and precisely estimated. For individual with high educated mothers all outcomes except test score in Swedish is significant, and higher than for most other groups.

To investigate this further, I estimate (2) separately on three dummy variables indicating mother's education level as the dependent variable. The results are presented in Table 11 and suggest that the mothers who, due to the reform, could send their older child to pre-school while on PL were less likely to have a low education level 10-15 years later. There was a, in terms of effect size, similar increased probability of having a medium education level in the same group. The probability of having a high education level seems not to have changed. One interpretation is that some of the extra time that mothers gained from sending one child to pre-school was used for self-education. Another explanation is that there were different fertility trends in the treatment and control regions for women with different education level.[35] Estimating the same model, but with fathers' education level give almost the same result which could be interpreted as support for both hypothesis. Treating the education level as an outcome of the reform or as a potential confounder have implication for both the empirical strategy and the interpretation of the results. If it is an outcome of the reform, controlling for it will block that causal path and bias the results. It might however, better isolate the causal mechanism of the actual pre-school experience. If it is a confounder excluding it will also bias the results, but in the other direction.

[35] Graphs showing the pre-reform trends for the mothers' education level is presented in Figure 6 in the appendix.

Controlling for the parents education level will thus be an important robustness test. Note that the plausibility of either interpretation is intimately connected to what assumptions one are willing to make about the correlation between the education level of parents at the birth of a child and the education level of the same parents 16 years later. It seems reasonable that individuals would wait to have children until they reached their preferred education level, but as Sweden is a country with a developed and accessible system for adult education and training there are plenty opportunities for parents to improve their education level post family formation. According to Hallberg et al. (2011, Figure 2) one quarter of female university students had children in 2004. They also conclude that parents are on average more effective students than non-parents. There are furthermore no restrictions for attending university while collecting payed parental leave from the government (as long as you also care for your child) (Försärkingskassan).

e. Robustness and specification tests

In section V.a. I provide graphical evidence for the identifying assumption that underlies my identification strategy. However, my estimates might be sensitive for the specification of my regression. A particular concern is what birth cohorts to include in the pre- and post-period. I thus run a number of specification checks on all my outcome variables, including a placebo test where the reform is moved back 4 years in time.[36] In Table 12 I present the results for math test score since that is where my main result suggest a statistically significant estimate, and in Table 14 in the appendix I include similar tables for the other three outcome variables.

[36] I move the reform 4 years back rather than the more conventional 1 or 2 year approach in order to avoid contamination from the phase-in period

The specification checks are presented in column 1-8 and is done by: (1) Include the 1998 birth cohort in post rather than pre reform period. (2) Include a dummy for the phase-in period (birth cohorts 1997 and 1998) interacted with the treatment group and sibling dummy. (3) Drop the phase in period from the regression. (4) Exclude the 2001 birth cohort and thus narrowing the post period. (5) Exclude the two large cities Stockholm (C) and Malmö (T) from the sample.[37] (6) Exclude the 1993 and 1994 birth cohort and thus limiting the pre-reform period. (7) The placebo reform where post is redefined to be the birth cohorts 1995-1997, later birth cohorts are dropped. (8) I drop all individuals who had a younger sibling in 2001 and 2002. This is an attempt to address the fact that having a sibling might be endogenous to the reform and thus be a channel though which there could be selection into treatment (an issue I will explore further in the section V.f.). (9) Control for parental education level at age 16.

All specification checks suggest that my main results i robust to different model specifications. When the large cities are excluded the point estimate is almost the same, but it becomes less precisely estimated. The coefficient from the placebo test is not significantly different from zero, even at the 10 percent level. The effect size becomes smaller when I control for parental education, but is still significant at the 5 percent level. The same is true for the other outcome variables, with the exception that the estimate on the test scores in Swedish which becomes negative and significant at the 5 percent level once I control for parental education.

[37]The third large city in Sweden, Gotenburg, is neither in the treatment or the control group.

Table 10

Heterogeneous effects

			Test Score		
	GPA	Pass	Mathematics	Swedish	n
	(1)	(2)	(3)	(4)	(5)
Female	2.057	0.00590	0.278**	-0.0775	187,513
	(1.086)	(0.00381)	(0.101)	(0.0658)	
Male	2.162	-0.00237	0.382**	-0.0539	196,131
	(1.828)	(0.00665)	(0.129)	(0.111)	
Swedish background	2.020	0.00281	0.356**	-0.0712	305,013
	(1.288)	(0.00401)	(0.110)	(0.0683)	
Foreign background	3.016	0.000412	0.342*	0.0280	78,602
	(1.888)	(0.00708)	(0.154)	(0.125)	
Low educated mothers	-1.845	-0.0207**	0.163	-0.160	129,511
	(1.875)	(0.00719)	(0.138)	(0.112)	
High educated mothers	3.416***	0.0117***	0.353***	-0.0233	131,836
	(0.947)	(0.00335)	(0.102)	(0.0555)	
Younger	2.064	0.00426	0.289*	-0.0574	339,778
	(1.248)	(0.00460)	(0.122)	(0.105)	
Older	1.195	-0.00337	0.297*	-0.0628	312,184
	(1.868)	(0.00480)	(0.151)	(0.0988)	
Municipality-by-birth year FE	Yes	Yes	Yes	Yes	
Municipality-by-sibling FE	Yes	Yes	Yes	Yes	
Birth year-by-sibling FE	Yes	Yes	Yes	Yes	

Note: Each estimate in column 1-4 is a result from a separate regression using the STATA command `reghdfe` (Correia, 2016). Every row represens a subset of the sample. Swedish background means that the individuals have two parents who are born in Sweden. Foregin background indicated that at least one of the parents are born abroad. Low and high education level is defined in the note to Table 6. Younger are the individuals who had a sibling at, on average, age 2-3, older the individuals who has a sibling at, on average, 4-5. Column 5 report the number of observations for the regression with GPA as outcome, for the test score variables it is slightly smaller. The outcome variables are the same as in Table 9. The standard errors are calculated using robust standard errors clustered at 185 municipalities and are reported in parentheses.

* Significant at 5 percent,

** Significant at 1 percent,

*** Significant at 0.1 percent

Table 11

Triple difference on mothers' education level at age 16

	Share of mothers with education level		
	Low	Medium	High
	(1)	(2)	(3)
Reform x Post1999 x Sib	-0.0250**	0.0226**	0.00457
	(0.00870)	(0.00775)	(0.00854)
Municipality-by-birth year FE	Yes	Yes	Yes
Municipality-by-sibling FE	Yes	Yes	Yes
Birth year-by-sibling FE	Yes	Yes	Yes
Observations	383,644	383,644	383,644

Note: Each column represents a separate regression using the STATA command `reghdfe` (Correia, 2016). The outcome variables are the average percent of mothers with the specified education level. The education level variable is defines in the note to Table 6. The standard errors are calculated using robust standard errors clustered at 185 municipalities and are reported in parentheses.

* Significant at 5 percent,

** Significant at 1 percent,

*** Significant at 0.1 percent

Table 12

Specification checks and placebo test

	Post1998	Phase-in	Drop Cohorts: 1997 & 1998	Drop Cohorts: 1993 & 1994	Drop Cohorts: 2001	Drop Large Cities	Drop SY 2001 & 2002	Placebo reform	Control parental edu
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Reform x Post1999		0.478***	0.483***	0.281**	0.341***	0.355	0.304**	0.263	0.212*
x Sib		(0.119)	(0.120)	(0.0897)	(0.0836)	(0.227)	(0.113)	(0.182)	(0.0864)
Reform x Post1998	0.398***								
x Sib	(0.0884)								
Reform x Phase-in		0.333*							
x Sib		(0.138)							
Municipality-by-birth year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Municipality-by-sibling FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Birth year-by-sibling FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	356,662	356,662	276,811	283,147	317,006	303,560	326,854	190,558	356,661

Note: Each column represents a separate regression using the STATA command `reghdfe` (Correia, 2016). The outcome variables are defines as in Table 9. For column 9 I also include dummies for the three education levels for mothers and fathers before calculating the residuals. The standard errors are calculated using robust standard errors clustered at 185 municipalities and are reported in parentheses.

* Significant at 5 percent,

** Significant at 1 percent,

*** Significant at 0.1 percent

f. Threats to identification

One problem with the data is that I only observe each individual's home municipality at primary school graduation. This is not necessarily the same as the home municipality during ones early childhood. Individuals who changed home municipality between their pre-school years and graduation will thus be assigned to the wrong municipality in my regressions. Migration between treatment and control municipalities could also generate inconsistencies from selective migration. If I had access to home municipality in early childhood, the natural way would be to use municipality of birth as an instrument, but this is not possible with the current data. Instead I use a different dataset with panel data covering the total population from 1997 to 2014.[38] With this data I can calculate the average migration pattern between treatment and control municipalities for specific birth cohorts. Figure 3 summarizes the results for the 1998 birth cohort.[39] Each arrow represents the migration flow between the three regions from early childhood (age 2-5) to age 16.[40] I also test the robustness of the result by doing the same analysis after excluding the large cities Stockholm and Malmö (Gothenburg is neither in the treatment or control group) with very similar results.

In the control group 86 percent of individuals who lives there at age 16 could also be observed in a control municipality during their pre-school years. In the treatment group the same figure is 89 percent. Under the assumption that migration between the three groups is unrelated to both the parents preference for pre-school and the

[38] The dataset is provided by Statistics Sweden and was put together for the Ministry of Education.

[39] The results are similar for other birth cohorts.

[40] Statistics Sweden (2003) show that between municipality migration is at it's lowest between the age of 7 and 16, i.e. during the primary, and lower secondary school years.

children's sensitivity to pre-school attendance, bias can only be introduces in two ways. Either via migration between treatment (T) and control (C) municipalities or via migration from the other municipalities. We can see that the net effect of migration between T and C is 4,95 percent from T to C. If there is a positive (negative) treatment effect this will bias my results downwards (upwards). The migration from T or C to the other municipalities will not bias the results, but movers in the other direction might.

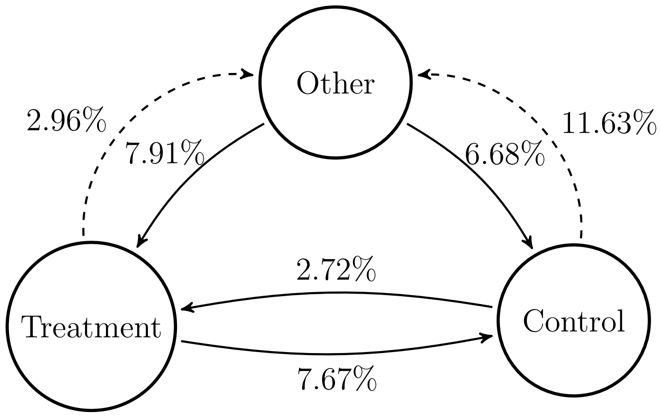
The other municipalities can be excluded from the estimations for several reasons: either (1) they have not replied the pre-school availability survey in 1998 or 2001 ($n = 20$), or (2) they changed policy between 1998 and 2001 ($n = 44$), or (3) they answered *case-by-case* in 1998 ($n = 34$) or (4) granted partial access for children with PPL both years ($n = 6$).[41] Under the assumption that the individuals who moved from the other group to T or C are similar, this migration pattern will only introduce attenuation bias to my results. It is also worth noting that the flow into T and C from the other municipalities are approximately the same in size. Thus under the assumption that between-municipality-migration is not correlated with determinants of the outcome variables, and that there is a positive treatment effect this will only introduce downward bias on my results.

I cannot test this assumption and one cannot rule out that there were some, so called, Tiebout sorting in migration and residential patterns with respect to child care preferences. It does not seem implausible before the reform, as it was advantageous to live in a *generous* municipality if you were planed on having more than one child. However, Boadway and Mörk (2004) investigate the evidence for such sorting

[41] In terms of pre-reform child care utilization for PPL the average in the other group was 41 percent which is closer to the treatment (26 percent) than the control (79 percent group).

Figure 3

Migration pattern between early childhood and age 16

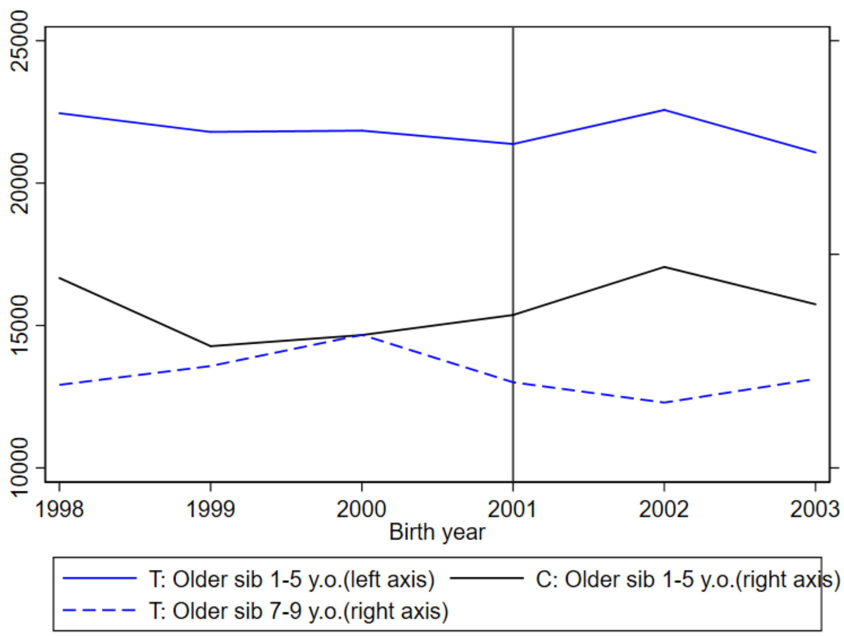


in the Swedish context and does not find support for the hypothesis that Tiebout mechanisms drive between municipality migration in Sweden.

The timing of the sibling births is another potential way in which there could be selection into treatment. It was well known to the public that the reform was going to be implemented in January 2002 as the legislation was adopted in in parliament in late autumn 2000 and was publicly debated (Martin Korpi, 2015). Thus parents who had a child in pre-school and were planing to have another baby might have waited until after the reform. In order to investigate this I graph the number of children born with an older sibling at age 1-5 for years 1998-2003 in the treatment and control group. If there were selection into treatment we would expect a decrease in the number of children born before the reform and a corresponding increase just after in the treatment group, but not in the control. This is also what Figure 4 hints at. In order to be sure that I am not just picking up a general trends in fertility, I also include a reference category - children born with a 7-9 year older sibling in the treatment municipalities. As there were no incentives to wait for the reform if the older child already had started primary school. But as we can see, the trends are not parallel around the reform year. I would need data with higher resolution, for instance month of birth to further investigate this issue. However as Table 12 my results are robust to dropping all individuals who had a sibling in 2001 and 2002.

Figure 4

Children with older sibling by birth year



VI Discussion

Assessing the internal validity of the results is both a matter of critically examine the identifying assumption underlying the empirical strategy and the statistical inference. The identifying assumption for the DD-estimates are not likely to hold, especially since the child care fee reduction coincided with the expanded child care access for PPL. The DDD model is thus the preferred specification. The triple difference also has the benefit of isolating the effect from changes in the municipality specific pre-school quality. As the reform increased the number of children in pre-school a temporary drop in pre-school quality is likely in the treatment group. That the DD-estimates are insignificant and even negative suggest that this could be an important mechanism. The municipalities not affected by the reform seem to have improved the quality of their pre-schools (at least in terms of child-teacher ratio) - most likely explanation is that they were compensated by the central government for a pre-school expansion that did not happen. For the statistical inference I do standard t-tests for the significance level of my estimates. As I both aggregate the individual data before estimating, and compute cluster robust standard errors with a large number of clusters the distribution of the standard error should be well approximated with the *t-distribution*. But as I do multiple testing the significance level might be exaggerated.

What are the implications of the results both for further research and for public policy? The motivation for this study is to estimate the medium- to long-term effects on human capital of increased time in institutional child care relative to parental care in early childhood. My results show that there is a positive effect on test scores in mathematics, which can be seen as a proxy for cognitive ability.[42] However, using test score as outcome variables has its inherent limitations. Jacob and Rothstein (2016) discuss the problem with scaling of test scores and as test scores only have an ordinal scale, the interpretation of a one point increase in the average test score in a population can have multiple interpretations. One way to get around this problem is to either use different outcome measures such as labor market outcomes (that has a well defined cordial scale) or to anchor the test scores in outcomes as adult earnings

[42]However Balart et al. (2018) uses data from the international PISA tests to suggest that there is also a considerable non-cognitive aspect of test performance.

as suggested by Cunha and Heckman (2008). This is done by Fredriksson et al. (2012), they show that the results on the Swedish standardized tests are positively correlated with adult earning.[43]

The TOT effect size on test scores in Mathematics is around 0.25 of a standard deviation. It corresponds well to the studies included in Table 1 that find a positive and significant effect. Another useful reference point is Fredriksson et al. (2012), since the also estimate the effect of a educational intervention targeted at a broad population of Swedish children and uses similar outcome variables. They estimate the effect of class sizes in the first years of primary school and find that a class size reduction of 7 students (average class size was 24,4) results in a 0.16 s.d. increase on test scores. It is hard to compare the two results directly as the interventions differ in character. However the technology of skill formation proposed by Cunha et al. (2006) with dynamic complementarities and self productivity suggest larger effects of earlier interventions - such a pre-school - than later interventions.

One contribution of this study to the ECEC literature is that it estimates the causal effect of changing from parental care to pre-school without expecting higher household income from increased wage labor. The benefit is that effect of pre-school attendance is isolated to factors not related to changes in material resources in the home. The downside is that the children not attending pre-school were at home with a parent who also had to care for a newborn. Zanella et al. (forthcoming) who find a negative effect of early pre-school start hypothesize that it is the difference in child-adult ratio between parental care and pre-school that is the mechanism behind

[43] In table A3 in the appendix they conclude that the correlation coefficient is 0.33. However it is important to note that the tests they use are not exactly the same as their population of interest is born between 1967 and 1982.

the negative results. In their Italian setting the child-adult ratio is similar to the Swedish, but the child-adult ratio in the home is lower in their setting, since they do not limit the sample to multiple child families.

Other papers also discuss the quality aspect of parental care. As all children both attend pre-school and spend time with their parents it is the combination of those two modes of care that is ultimately important. My triple difference strategy controls for potential changes in the general pre-school quality at municipality level - but it does not limit the causal mechanism to go through changes to the quality of parental care. The reform did not only give children more time in pre-school, it also gave the parents more time without having to care for two young children. Less time in parental care could in fact mean higher quality of the parental care given. This hypothesis is explored by Felfe and Lalive (2010) and Cascio and Schanzenbach (2013). They use time-study data to examine how the quality of maternal care changes when the time in maternal care decreases due to a pre-school expansion. Both find that the time allocated to *qualitative* care activities as reading, doing arts projects and singing to the child increased while total time with the children decreased. Although I do not have access to any time study data to examine this mechanism in a Swedish context, it seem plausible. Especially as parents with two infant children at home constantly had to divide their attention between the two before the reform.

The results does not indicate any significant heterogeneity in the effects size or direction. But as discussed in section V it is not possible, without further assumptions, to interpret variable *mother's education level* as a unbiased proxy for the household conditions during childhood. And without other indicators like household income, welfare dependence or single parenthood my analysis of the SES gradient in the effect is limited to foreign/domestic background. And for those groups the results are still rather similar. There are two possible explanations for the absence of a clear SES gradient in the results. First of all, the children with greatest needs might not have been affected by the reform, even though their parents were on parental leave. The legislation before the reform stipulated that children with special social or personal need were entitled to a pre-school slot, no matter their parents labor market position. What constituted a valid need was determined on a case-by-case basis, but examples mentioned are among other, difficulty with language, disabilities, disabled parents, longterm sick parents etc. For the children who qualifies for this quota attendance was free of charge for 15h per week. There are no reliable estimates on how large this group was, but Lillvist and Granlund (2010) conclude, on a sample of 571 pre-schools in two Swedish regions that in between 4-17 percent of pre-school children have special needs that could qualify. The other potential explanation is that the benefit of pre-school for children from low SES household primarily comes from the extra household income that typically is associated with increased childcare availability.

VII Conclusion

In this thesis I present evidence in support for a positive effect on lower secondary school results of increased pre-school attendance. After children to parents on parental leave could attend pre-school for 15 hours per week instead of being at home with their parent and younger siblings they did better on standardized tests in mathematics at age 16. The fact that they did not improve their performance on tests in Swedish suggest that the gains from pre-school is primarily linked to logical/numerical skills. The effect size is comparable to both international and Swedish studies that find a positive effect of early childhood education. The results suggest that the equivalent of 10 months extra pre-school attendance lead to 0.25 standard deviation higher test score in mathematics at age 16. If the positive effect is sustained thought secondary school and adulthood can not yet be determined.

The empirical strategy control for two set of counfounders that otherwise would bias the true effect of pre-school attendance: changes in pre-school quality and short-term changes in household income due to increased wage labor. However, there might be other causal mechanism than the benefits from attending pre-school at play. The first hypothesis is that part time pre-school access improves the quality of parental care for the time the child is not in pre-school. The other is that some of the time that parents gained from increased access to pre-school was used to improve their education and future labor market opportunity. Both these merits further study before definite conclusions can be drawn.

In contrast to much of the literature on ECEC I do not find much evidence for a SES gradient in the effect size or direction of the effect. There are multiple potential explanation for this, but one rather likely is that children with special social needs, and thus potential large gains from pre-school, already could attend pre-school before the reform. Even when their parents were on parental leave if could be motivated why they needed it.

I would like to thank the staff at the Division for Policy Analysis and International Affairs at the Swedish Ministry of Education and Research for allowing my to use their data for this thesis. They have also provided me with valuable insights about the Swedish education system and policies. My supervisor, Mårten Palme (Stockholm University), has also been of great help in my work. The Swedish National Agency for Education has also been very helpful in supplying me with the survey data, a special thanks goes out to Hanna Karlsson Ruiz who helped me with data from the child care survey. In addition: thank you Björn Olsson, Lovisa Folkestad, Monir Bounadi, Alexander Näsström and Dany Kessel for in various ways helping me in the thought and writing process.

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Appendix

Figure 5

Map over treatment and control municipalities

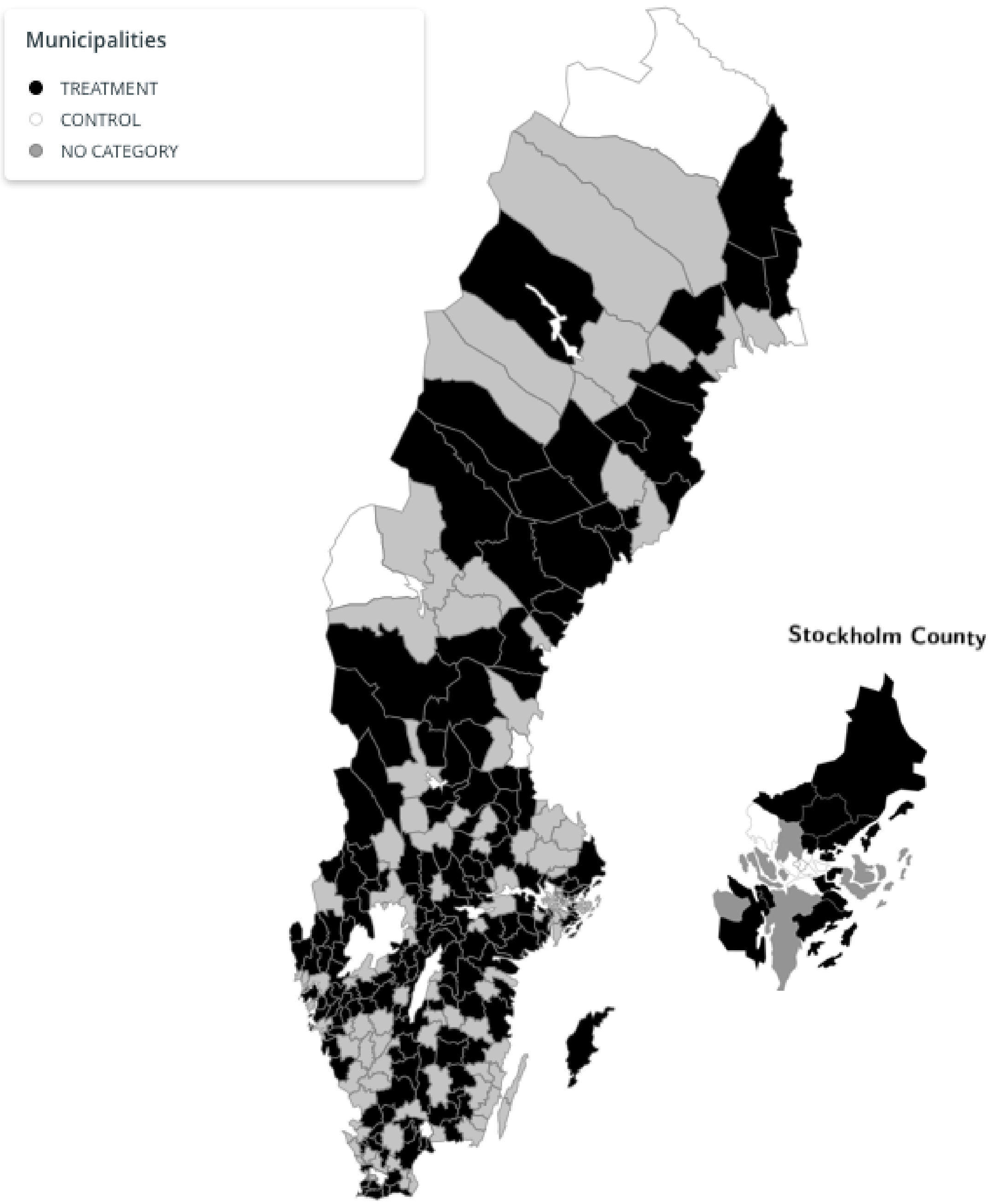


Table 13

Sample restrictions

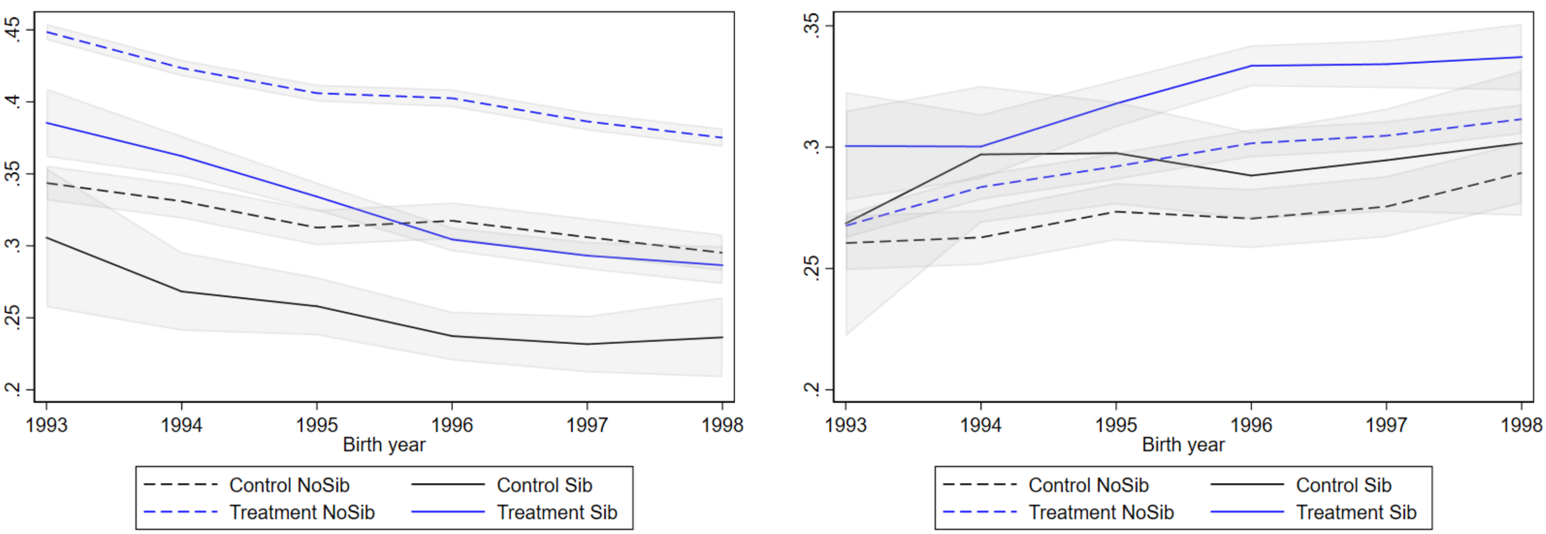
	(1) Full sample	(2) After restrictions
GPA	211.5	217.9
	(67.4)	(62.0)
Pass	0.88	0.91
	(0.33)	(0.29)
Test Score (Math)	11.10	11.35
	(5.61)	(5.49)
Test Score (Swedish)	12.86	13.15
	(4.31)	(4.06)
Graduation year	2012.9	2012.8
	(2.6)	(2.6)
Birth year	1996.8	1996.8
	(2.6)	(2.6)
Birth order	1.74	1.77
	(0.95)	(0.94)
No. younger siblings	0.64	0.64
	(0.91)	(0.84)
Mother's education (age 16)		
Low	0.36	0.34
	(0.48)	(0.47)
Medium	0.30	0.31
	(0.46)	(0.46)
High	0.33	0.35
	(0.47)	(0.48)
Foreign born	0.10	0.01
	(0.31)	(0.07)
Born in Sweden with		
Two foreign parents	0.10	0.11
	(0.29)	(0.31)
One foreign parent	0.10	0.10
	(0.30)	(0.31)
No foreign parent	0.70	0.79
	(0.46)	(0.41)
Share female	0.49	0.49
	(0.50)	(0.50)
Observations	924,057	776,636

Figure 6

Pre-reform trend for the mothers' level of education

(a) Low

(b) Medium



(c) High

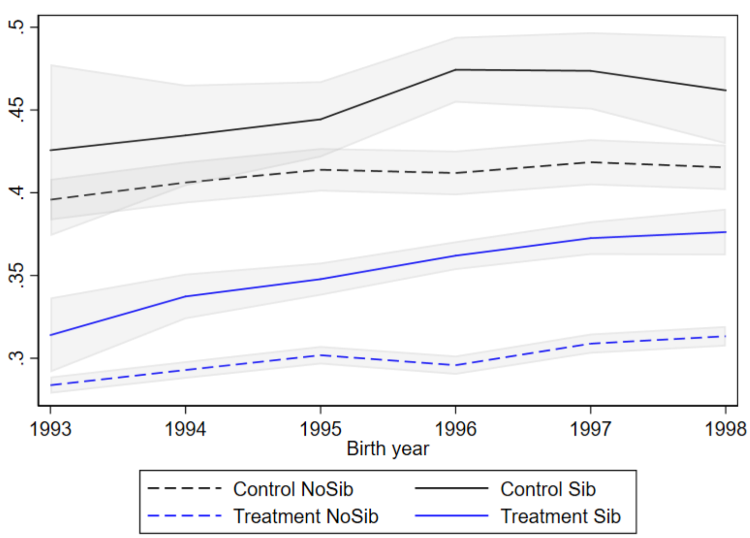


Table 14

Specification checks on GPA, Pass and Swedish Test Score

			Drop Cohorts			Drop	Drop SY		Control
	Post1998	Phase-in	1997 & 1998	1993 & 1994	2001	Large Cities	2001 & 2002	Placebo reform	parental edu
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
GPA									
Treat x 1998	2.499*								
x Sib	(1.232)								
Treat x Phase in		1.432							
x Sib		(1.725)							
Reform x Post1999		2.743	2.750	2.060	1.789	2.970	2.201	0.154	0.618
x Sib		(1.529)	(1.531)	(1.048)	(1.158)	(2.896)	(1.363)	(1.906)	(1.030)
Observations	383,644	383,644	297,167	304,975	340,478	325,473	351,950	204,205	383,643
PASS									
Treat x 1998	0.00545								
x Sib	(0.00486)								
Treat x Phase in		0.00391							
x Sib		(0.00547)							
Reform x Post1999		0.00336	0.00334	0.00199	0.00317	0.00742	0.00252	-0.00594	-0.000998
x Sib		(0.00425)	(0.00426)	(0.00357)	(0.00412)	(0.00741)	(0.00471)	(0.00763)	(0.00363)
Observations	383,468	383,468	297,049	304,799	340,349	325,404	351,792	204,190	383,467
Test Score Swedish									
Treat x 1998	-0.0175								
x Sib	(0.0644)								
Treat x Phase in		0.0331							
x Sib		(0.0857)							
Reform x Post1999		-0.0525	-0.0488	-0.0655	-0.0363	0.0229	-0.132	-0.0107	-0.121*
x Sib		(0.0666)	(0.0679)	(0.0598)	(0.0731)	(0.123)	(0.0674)	(0.154)	(0.0518)
Observations	357,325	357,325	275,833	282,094	323,847	303,553	327,132	195,405	357,324

Note: Each column represents a separate regression using the STATA command reghdfe (Correia, 2016). The outcome variables are the averages residuals calculated from regressing the outcome on a gender dummy, four categories of family migration background and a full set of birth order dummy (only including full sibling). The residuals was calculated using the STATA command predict. For column 9 I also include dummies for the three education levels for mothers and fathers before calculating the residuals. The standard errors are calculated using robust standard errors clustered at 185 municipalities and are reported in parentheses.

* Significant at 5 percent,

** Significant at 1 percent,

*** Significant at 0.1 percent