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Incentives and inequalities in family and working Life

Aino-Maija Aalto

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INSTITUTE FOR EVALUATION OF LABOUR MARKET AND EDUCATION POLICY

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Dedicated to the memory of my father, Esko Aalto.

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[1] In fact, I am convinced that there is no way that I could list all of you. So as a summary: thank you universe, thank you everyone!

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[2] (Translation to Finnish:) Kiitos Salli, että olet opettanut minulle taidon nauraa itselleni, kun siihen on syynsä, ja taidon olla onnellinen muiden onnesta. On mielettömän hienoa olla ystävä ("varamamma"), jolla on niin paljon enemmän elämänkokemusta.

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Uppsala, September 2018
Aino-Maija Aalto

Contents

Introduction

Labour is not growing like mushrooms in the rain as Hobbes (1641) leads as to assume in De Cive. It takes a long time and a lot of investments to raise human capital to the level that is sufficient for market work. In practice, given the education level of the Nordic countries, the usual age to become independent from one's parents or governmental support is much later than the legal age to enter the labour market. In other words, "growing" labour is a big investment in care work and education by families and educational institutions.

However, the time investments in children's human capital is not equally distributed among men and women. Despite the fact that the Nordic countries are often considered as the forerunners in gender issues (see e.g. OECD 2018), women in Nordic countries allocate more time to unpaid work, such as care work, than men do and the labour markets are highly segregated by gender (Nordic Council of Ministers 2015). It is mostly women who work in the health and education sector. Thus, policies and institutions affecting human capital investments in children have different effects on men and women.

This thesis consists of three self-contained empirical essays which investigate different aspects of how institutions affect human capital investments and inequalities in investments between men and women. In the first essay I study whether same-gender role models at school matter for later educational choices. In the second essay I investigate, together with my co-authors, the inequality of health and study whether the health of children of unemployed parents are affected by access to childcare. In the third essay I study the effect of a change in the rules governing the parental-leave allowance level on mothers labour market participation. Hence, I study the importance of three public institutions —parental insurance system, childcare and schooling—on outcomes related to labour market participation, health inequality and career choices.

In the following section I shortly discuss the potential outcome idea and how we approach the research questions empirically to claim causality. I then continue by introducing each one of the three topics of the thesis separately.

Finding the answer to the questions of interest

All three essays in this thesis are in the field of applied microeconometrics. In all three of the papers my aim is to understand the *causal effect* of one factor on the outcomes of interest. The concept of causal effect is frequently used term in today's econometrics. According to Angrist and Pischke (2010) the focus on causal effect and careful research design is still relatively recent phenomena in empirical work in the field of Economics. Each time we are faced with a choice, there is no parallel world that we could study to know what had happened if we had chosen differently. This restriction of life makes it hard to claim causality; we cannot go back in time and fast-forward to see how different the outcome had been if another decision had been made. This problem is the key to *the potential outcome idea*, which was initiated according to Freedman (2006) by Neyman in in the 1920s and developed further by Rubin (1974) and Holland (1986). Hence, to be able to study the effect of interest we need a comparison group to control for the potential counterfactual scenario or well-motivated control variables to take care of the *self-selection*.

When studying an effect of a treatment, the importance of counterfactual, to what scenario we compare the treated to, cannot be overemphasized. The problem arises because we often self-select in front of a choice instead of being randomly allocated to choose in a certain way. Thus, we cannot make a conclusion about the choice affecting the people when it might be that only certain types of people make that specific type of choice. In each of the essays in this thesis, I utilize different identification strategy to answer my questions of interest and conduct sensitivity analyses to test the robustness of the estimated effects.

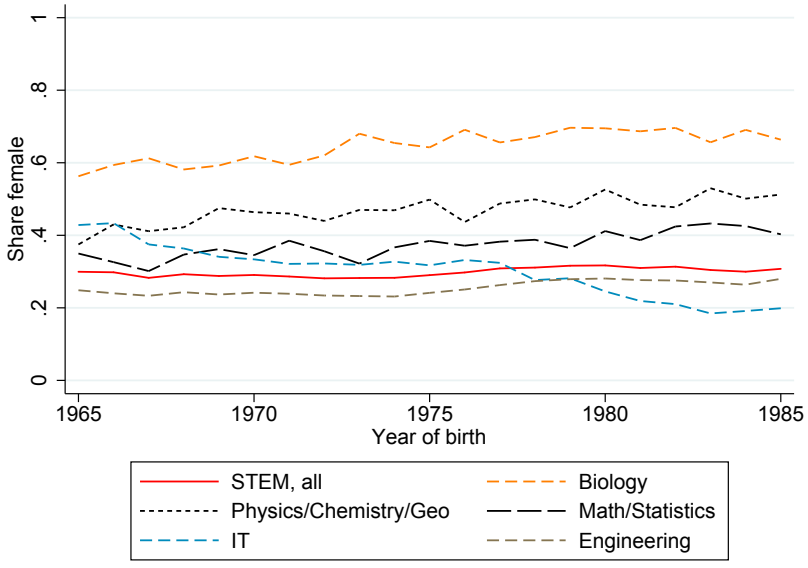
Role models and career choices

Sweden among other Nordic countries is ranked as one of the most equal gender-wise (World Economic Forum 2016). Yet, Swedish labour markets are very segregated to jobs that mostly men hold and to jobs that mostly women hold: only 15–16 percent of men and women work in gender-equal work places (SCB 2016). This gender segregation of labour, not only in fields but also in tasks (e.g. Albrecht et al. 2003), explains a large part of the earnings gap we see between men and women (Blau and Kahn 2017). Math-intensive fields are typically male dominant. The segregation in math-intensive fields plays also an important role in the wage gap between men and women (Card and Payne 2017). Interestingly, countries that are ranked more gender equal have a stronger gender segregation in STEM (science, technology, engineering or mathematics) than less gender-equal countries according to Stoet

and Geary (2018). A striking fact is that even if the achievements in mathematics do not differ between girls and boys in school tests (see e.g. Kahn and Ginther 2017), it is mostly boys who pursue the more math-intensive degrees that often lead to higher-paying jobs. Given that girls do as well as boys in mathematics at school, it is likely that societies are loosing a large potential in human capital due to the gender segregation in occupations.

The likelihood of selecting into math-intensive field of study is the focus of **essay I**. Figure 1 depicts the share of women among those with a university STEM-degree in Sweden in year 2015 across cohorts 1965–1985. Women hold only about 30 percent of the STEM-degrees across generations. However there are notable variations: in biology there are more women than men whereas the share of women with a degree in IT is around 20 percent for the later cohorts. In my analysis I focus on the selection into a math-intensive track in upper secondary school and to a math-intensive field of study at university.

Figure 1. Share of women within each type of STEM-degree by birth year.



Notes: The degree-information is form year 2015 for each cohort.

There is a large literature exploring the potential reasons for the low share of women in STEM-fields (see e.g. Kahn and Ginther (2017) for an overview). According to the social cognitive theory, different role models that we are exposed to early on and through our lives play an important role in shaping our ideas of what is typical to each gender

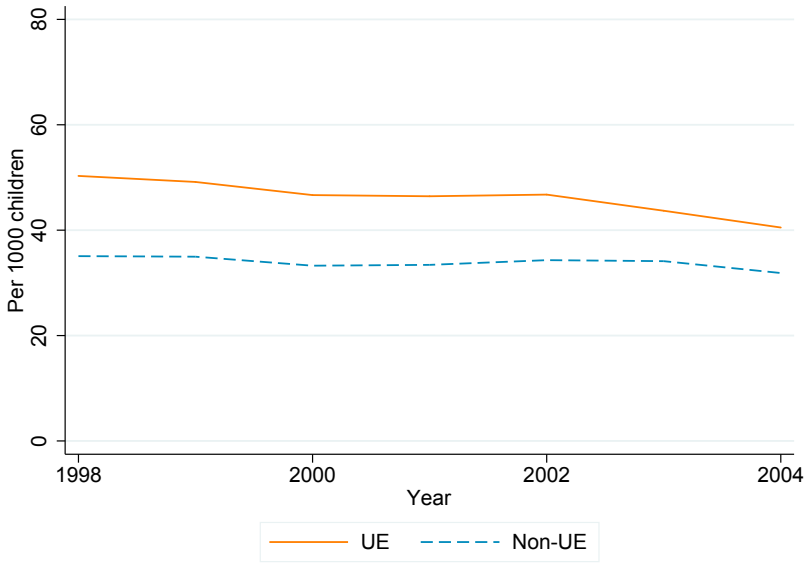
(Bussey and Bandura 1999). One place where we are exposed to role models is at schools. Previous literature on the effect of same-gender role models in educational institutions have mostly focused on the higher education. However, before we start our studies in university, we have already made choices that determine part of our further possibilities to educate ourselves. In my study, I focus on the last three years of compulsory school, the lower secondary school. I study the effect of the share of female math and science teachers on the likelihood of students choosing a math-intensive education path. In particularly, my focus is on the gender gap in STEM. I am interested whether same-gender teachers can affect the preferences of girls to choose a math-intensive field. I utilise Swedish register data for the analysis and I control for family fixed effects to account for the sorting of students into schools.

According to my findings, the gender-gap in graduating from a math-intensive track in upper secondary school is decreased by 16.2 percent and graduating with such a degree in university by 22.5 percent if at the time of lower secondary school the share of female math and science teachers is increased from none to all. I find support for the effect to be driven by role model effect rather than via effect on performance.

Health inequality and early childhood investments

Health plays an important role in determining the possibilities for human capital development. Early life conditions, such as health, can have lasting effects on later outcomes in life (for an overview see Currie and Almond 2011). However, health is not equally distributed among us. According to Deaton (2013), there has been a social gradient in health since the development of medical cures for diseases. Mörk et al. (2014) show in Sweden that children of unemployed are having worse health than those of employed. This pattern can also be seen in Figure 2, where we depict the hospitalization among children aged 2–5 by the labour market status of their parents. We see that children who experience any parental unemployment during a year are more likely to be hospitalized than those whose parents are not unemployed. In **essay II** (co-authored with Eva Mörk, Anna Sjögren and Helena Svaleryd), we study whether access to high-quality childcare affects the health of children with unemployed parents. In earlier literature, the effects of childcare on educational outcomes have been studied more broadly but evidence on the effect on health is still scarce.

Figure 2. Hospitalizations among children aged 2–5 years by parental labour market status.



Notes: A parent is defined as unemployed (UE) if (s)he received any unemployment benefit during a year. Otherwise the parent is defined as non-unemployed (non-UE).

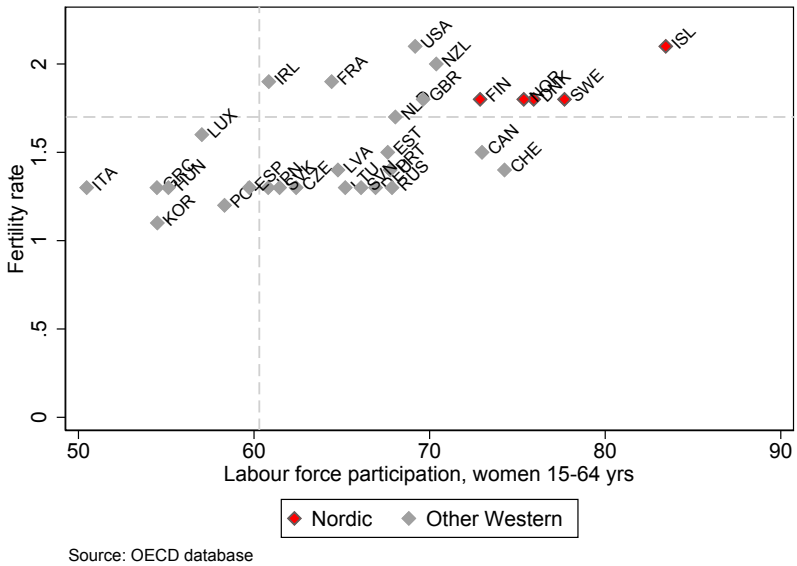
To identify the effect of interest, we exploit time-variation in Swedish municipalities in their regulation of access to childcare to the children whose parents are unemployed. Since 2001 all municipalities have been obliged to offer childcare at least for 15 hours per week for these children. We study the effect of the access to childcare in the short run, around the reform year, by analysing the effect on any hospitalizations and specifically on respiratory, injury or infection related hospitalizations. Additionally, we study whether the access has effects when the children are aged 10–11. At this age the registers allow us to study also a less severe health measure of drug prescriptions.

We find hospitalizations due to infections to increase a year after the reform, for children aged four to five, and find that the effect is driven by children of low-educated mothers. For younger children, aged 2–3, we find no effects. For children aged 10–11, we however do not find access to childcare at an earlier age to have mattered for hospitalizations. For prescriptions at this age, we find that respiratory-related medication is increased for those who had no access to childcare at the time they were younger and experienced parental unemployment. Hence, our results suggest that access to childcare exposes children to risks of infections but that the need for medication is smaller for children who had access while experiencing parental unemployment.

Parental leave policies and labour market participation

Among other Nordic countries, Finland has a relatively high fertility rate as well as a high labour market participation of women (Figure 3). This is a situation that many other developed countries are striving for as the populations age and there is a need to secure future tax-revenues. All of the Nordic countries have also a long history of generous family policies (Johnsen and Løken 2016, Datta Gupta et al. 2008), which include long duration of job protection, universal coverage of family benefits as well as publicly provided childcare. However, there is still relatively scarce evidence of the effect of the level of the paid parental leave on the labour market choices of the mothers. These choices matter as time away from work after a birth of a child often lowers mothers' income.

Figure 3. Women's labour market participation (15-64 year old) and fertility rate of selected Western countries in 2005. Dashed lines show the OECD averages of the measures. Nordic countries are marked separately.



Countries included: Canada, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Japan, Korea, Latvia, Lithuania, Luxenbourg, Netherlands, New Zeland, Norway, Poland, Portugal, Russia, Slovak Republic, Slovenia, Spain, Sweden, Switzerland, UK, US.

In **essay III** I study the importance of a financial incentive, in the form of parental leave allowance, on mothers' decision to stay at home instead of returning to work. To identify the causal effect of the incentive on the probability to work between or after a birth, I exploit a reform that changed the basis of the allowance in Finland. The re-

form made it possible to regain the right for the same level of parental leave allowance as with the previous child, without needing to return to work, if the next child is born within three years. With respect to the implementation date of the reform, the timing of the first child defines whether a parent can become eligible for the reform or not. I use regression discontinuity design to study whether the allowance level matter for mothers' decision to stay at home or return to work between births and whether this decision affects their long run labour market attachment. I find that the mothers decreased their labour market participation between births by three months but there are no effects on the labour market participation after five years of giving birth to the first child. Hence, it seems that the increased parental leave benefit has a short term effect on mothers' labour market participation but this impact does not affect the participation in the long run.

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I. Do girls choose science when exposed to female science teachers?

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1 Introduction

Despite the fact that achievement on written tests in mathematics does not differ between girls and boys (see e.g. Kahn and Ginther 2017), boys are more likely to pursue math-intensive degrees. Since these degrees often lead to higher paying jobs, it is thus possible that societies are losing a large potential in human capital within some of the most productive jobs. A potential explanation for this occupational segregation is gender-specific role models. In this paper, I examine the possible effects of having a larger share of female science and math teachers in Swedish lower secondary schools on the likelihood of girls choosing to continue in math-intensive fields of study.

The previous literature shows mixed evidence regarding the role model effect of a same-gender teacher on the likelihood of studying math-related fields. A prominent paper by Carrell et al. (2010) studies the effect of the share of female professors in introductory science and math classes on the probability to continue in a STEM[1] field among college students in the US Air Force. They find positive effects on continuation and graduation with a STEM degree among female students who perform the highest in mathematics and no effect on male students. Bottia et al. (2015) find similar results when studying the effect of share of female STEM teachers in high school on the likelihood to major in STEM fields at university. They find effects on female students across the entire achievement distribution, not just among the top performers. Similar to Carrell et al. (2010), they find no effect on male students. In contrast to these studies, Griffith (2014) finds no effects on pursuing a STEM degree and Bettinger and Long (2005) find mixed evidence depending on the STEM subject. Canes and Rosen (1995) find no association between the share of female faculty and share of female students.

An alternative explanation why female teachers affect the likelihood that girls choose STEM fields could be that female teachers affect girls' school performance. However, most previous studies conclude that there is no effect or only a small effect of having a same-gender teacher on achievement (Antecol et al. 2015, Griffith 2014, Winters et al. 2013, Ehrenberg et al. 1995, Hoffmann and Oreopoulos 2009 and Holmlund and Sund 2008). An exception is Dee (2007), who finds that same-gender teachers raise the achievement of both boys and girls in different school subjects for 8th grade students in the US, although for mathematics he finds negative effects for girls. Also, Carrell et al. (2010) find positive effects on female college students' test performance but negative effects on boys in introductory science and mathematics courses. A

[1]STEM stands for Science, Technology, Engineering and Mathematics.

strength with this study is that I can investigate whether female teachers have different effects on the school performance for boys and girls.

When the effect of potential role models on educational choices is studied at a higher level of education the sample consists of individuals who have already made earlier decisions about their educational path.[2] Indeed, a large part of the literature has focused on the college level to study the effect of the same-gender role models on educational choices (e.g. Carrell et al. 2010, Price 2010, Bettinger and Long 2005, Canes and Rosen 1995, Robst et al. 1998, Hoffmann and Oreopoulos 2009, Griffith 2014). An exception is Bottia et al. (2015) who study the effect at the high school level. However, even at this stage, students have already chosen some of their courses according to their idea of future studies.

In comparison to the earlier literature, my focus is at a lower level of education. Lower secondary school (grade 7–9, at age 13–15) is the last part of compulsory schooling in the Swedish education system. At that point of education all the students are still exposed to the same national curriculum and have not yet made specific choices about a field of further studies. I study the effect of the share of female math and science teachers in this school level on the probability to continue in a math-intensive program in upper secondary school and to pursue a degree in such a field in university. To further analyse whether the effect is due to role models or that female teachers affect performance of girls, I study the effect on achievement in the national mathematics exam, on the final grade in mathematics and other STEM subjects as well as on GPA at the end of compulsory school. I make use of register data for the full population of Sweden for the cohorts 1982–1995. To control for the endogeneity of teacher sorting across schools, I use sibling fixed effects and compare the effect between girls and boys.

I find that increasing the share of female science and math teachers in lower secondary school decreases the gender gap both in applying to and graduating from a math-intensive track in upper secondary school as well as the gender gap in pursuing a math-intensive degree in university. Within upper secondary school there are two relatively more math-intensive programs that I define as the STEM tracks: natural science and technical track. According to my results, increasing the share of female science teachers from none to all decreases the gender gap in graduating from a STEM track in upper secondary school by 16.2 percent and the gap in pursuing a math-intensive degree in university by 22.5 percent. However, girls and boys are affected differently: while the higher share of female teachers in science affects girls positively in

[2]For instance Card and Payne (2017) show that it is important to have taken certain courses at the end of high school on the likelihood to major in STEM in university.

terms of later choices of STEM, boys are negatively affected but to a notably lesser extent than the positive effect on girls. The effect on graduating with a STEM track in upper secondary school is fully driven by the science track—I find no effects on the more male-dominant technical track. Within the science track the increase from none to all in the share of female science teachers entirely closes the gender gap. The effects on educational choices for girls do not appear to arise via effects on student performance. In line with most of the existing literature, I find no evidence that the share of female science teachers affects their performance differently than boys. Thus, I find evidence that the higher share of female science teachers does increase the female students likelihood to continue in math-intensive education path through choices and not performance which is consistent with a role model effect.

In Section 2, I discuss the conceptual framework related to the potential effect of role models and how there may be heterogeneous effects across students. I then explain the relevant components of the Swedish schooling system in Section 3. In Section 4, I explain the research design and continue in Section 5 to describe the data used to study the research question of interest. In Section 6, I show the main results and conduct some heterogeneity analysis as well as investigate the robustness of the results and try to shed light on potential alternative mechanisms behind the effects. Finally, in section 7, I discuss the findings and conclude.

2 Conceptual framework

Role models of the same gender provide one potential channel for gender-specific preference formation. Bussey and Bandura (1999) explain that according to the social cognitive theory, different role models that we are exposed to early on, and throughout our lives, play an important role in shaping our ideas of what is typical for each gender. For a school-aged child the three main sources of role models are typically members of the family, teachers at school and different characters in entertainment. In this paper the focus is on same-gender teachers at school, and the effect they have on choosing a math-intensive study track. Teachers at school can affect both the performance and the preferences of the students to different subjects, which both in turn might determine further educational choices of the students.

In Sweden, the performance of girls in mathematics is not a concern when considering the reasons for the lower share of women in math-intensive fields. Girls do on average as well as boys in mathematics during school time (see e.g. Figure A2a). Additionally, the earlier literature has found little evidence that the gender of the teacher would matter for performance in math related subjects (Antecol et al. 2015,

Griffith 2014, Winters et al. 2013, Ehrenberg et al. 1995, Hoffmann and Oreopoulos 2009 and Holmlund and Sund 2008). However, while girls are doing as well as boys in mathematics they on average perform notably better than boys across all other subjects (see Figure A2b). This comparative advantage in relation to other subjects is what Card and Payne (2017) conclude to be the main driver of the STEM gap we see today in terms of choice of majors. The fact that girls more often than boys perform well in a variety of subjects, when they also perform well in mathematics, is likely to restrict the possibility to affect their preference to choose a math-intensive field of study; they simply have more options to choose from than many of their male peers.[3]

The fact that there are no effects on achievement does not mean that the preference to opt for STEM later in life could not be affected by having a same-gender teacher in science. Bussey and Bandura (1999) formulate that as we identify with our gender and the stereotypes associated with it, via the role models and the incentives and disincentives we experience in our social environment when behaving in a certain way, it is more likely that a boy forms a stronger belief about his mathematical abilities than a girl, given today's social environment. This assumption is supported by Dahlbom et al. (2011) who show that Swedish girls are less confident than boys in their math skills and by Correll (2001) with US data. Conditional on skill level, if having a same-gender teacher matters for one's confidence, we would expect girls' preferences to be more affected than boys' when they are facing an environment with same-gender STEM teachers as there are more men than women in STEM occupations in general (and thus also, e.g., in films and books).[4] The effect is also likely to be stronger among girls with a high skill-level in mathematics as these skills are a prerequisite to enter a math-intensive field of study.

Not only could a female math and science teacher be a stronger role model for girls than for boys but she might also conduct her teaching in a different way than her male colleague would. It is possible that a female teacher is better at creating a class-room environment that is more suitable for girls to enjoy math-related subjects. For example, Spencer et al. (1999) argue that girls achieve better results in less competitive environment and when the stereotype that mathematics is a masculine field is faded out. If female teachers are better at decreasing gender stereotypes in mathematics, then having a same-gender teacher might affect educational choices not only through the role model channel, but

[3]I conduct robustness checks in Section 6.2 for the possibility of competing role models in other subjects but find no change in the results.

[4]Correll (2001) develops a model along these lines by considering the importance of cultural beliefs about gender and self-assessment as determinants of the gendered occupational choices.

also due to other characteristics associated with the teacher's gender. Related to the gender stereotypes of mathematics, Carlana (2017) shows by using an implicit association test for teachers in Italian schools that female math teachers are less biased in their stereotypes about gender and science than their male colleagues. She also shows that the gender bias has a negative effect on female students' self-confidence in their math skills.

In this paper I use the share of female STEM teachers at the school level as a proxy for female role models. This measure captures a combination of having a female teacher in class and the potential within-school spillover effects to other classes. Being in direct contact with a teacher of the same gender or having multiple same-gender teachers in STEM subjects at a school can likely have different effects on students. The estimated effects will be a combination of direct and indirect exposure to the same-gender role models at school. It is likely that in a smaller school the students are more in direct contact with the teachers. Hence, I test also for the effects separately for larger and smaller schools. As explained above, it is possible that the effect of a higher share of female science teachers could involve other channels for affecting the preference of future education than purely via the teachers acting as role models. I explore this possibility by studying the impact on performance.

3 Swedish schools and STEM education

3.1 Compulsory school

The Swedish compulsory school consists of nine years of schooling. Almost all children start the first grade the autumn of the year they turn seven, and finish their compulsory school the year they turn 16. The majority of compulsory schools are municipality-owned but there are also private voucher schools that are financed by public funding.[5] All compulsory schools are obliged to follow the national curriculum set by the Swedish National Agency. Notably, no skill-based tracking is allowed in the Swedish compulsory schools. As of today, the curriculum in the last three years of compulsory school, in the lower secondary school, has about 23 percent of hours dedicated to different STEM subjects. The teachers in these classes are the ones that I focus on in most of the analysis.

Students' choices of lower secondary schools to attend are mainly determined by the alternatives available within the municipality that one resides in. Municipality run schools give priority to the students

[5]During the research period the share of students in private schools has increased from 5 percent to 13 percent.

who live closest to the school and the choice of lower secondary school is therefore most often determined by the proximity rather than willingness for particular type of school.[6] Different schools may thus face students with different socio-economic background mainly due to housing segregation. In my research design I control for family fixed effects to remove this type of sorting. However, siblings may attend different schools if the family moves, a school closes or a new one opens. In my research sample the majority of the families (70 percent) have all siblings attending the same school.[7]

Municipalities are responsible for organizing the schooling but in practice it is the principals who make decisions about teacher recruitments and negotiate the wages with the teachers. More women than men become teachers, and the lower the level of schooling, the higher is the share of female teachers. The share differs across subjects: there are more female teachers in languages and fewer in mathematical subjects.

3.2 Choice of study after compulsory school

Most students continue to upper secondary school after finishing compulsory school. The upper secondary school consists of different types of programs. The first major choice in the Swedish education system about which field one wants to study is thus the choice of upper secondary school program. All programs are three years long, some are vocationally oriented and some preparatory for higher education.[8] Two programs are substantially more intensive in mathematics than the others: the technical program and the natural science program. Throughout my analysis, I define these two programs as STEM tracks and refer to the natural science track as the science track. These two STEM tracks are both preparatory programs for higher education. The technical program is especially intended for those who aim to continue with engineering studies after finishing their upper secondary school. The natural science track is the most flexible program in terms of further studies.[9]

[6] Voucher schools may have additional queuing systems in the applications if there are more students applying than places available. However, the rules of acceptance have to be accepted by Swedish Schools Inspectorate. In general, no compulsory school can have entrance tests or skill-based acceptance rules. Few exceptions exists for schools that are specialized in art or sports.

[7] I have run the results also for the sub-sample of siblings who attend the same school. The results are not sensitive for this restriction.

[8] All programs give access to some higher-education studies, but the vocational ones give this access only to a restricted number of fields.

[9] In 2011, the upper secondary school system went through a major change that, among other things, increased the difference between the vocational and the preparatory programs. The technical program was under 1990s a specialisation possibility

4 Empirical strategy

The aim is to study the effect of the share of female STEM teachers on the probability to graduate from a STEM track in upper secondary school or to major in a math-intensive field in university. Analysing the effect of the share of female STEM teachers directly on the full sample of students without additional controls would likely suffer from omitted variable bias as neither teachers nor students are randomly distributed across different schools. Parents with certain characteristics tend to live in specific areas, and teachers might choose their employment location with respect to similar characteristics. Some of these characteristics might matter more for the location choice of female teachers than for male teachers and could also affect the likelihood that the children choose STEM later in life. Previous studies have used varying strategies to tackle this problem. Hoffmann and Oreopoulos (2009), Dee (2007) and Holmlund and Sund (2008) use within-student and within-teacher variation; Bettinger and Long (2005) and Price (2010) instrument the gender of the teacher by using share of courses taught by female faculty; Carrell et al. (2010) and Griffith (2014) use systems where teachers are randomly allocated to classes; and Bottia et al. (2015) control for school and teachers characteristics. In contrast, I use family fixed effects to control for any family-specific unobservable that is correlated with the share of female STEM teachers at a school and that may also affect the likelihood to choose STEM. In other words, I focus on between-sibling variation in the share of female STEM teachers, where the identifying variation comes from sisters in comparison to their brothers. By controlling for family-specific characteristics, I also control for exposure to other types of role models at home such as parents and family-specific consumption of culture (e.g. entertainment) that all siblings are exposed to.

My identification strategy relies on the assumption that the share of female STEM teachers is randomly allocated across children conditional on family fixed effects. In the main specification (Equation 1),

$$\begin{aligned} Y_{ij} = \alpha_i + \beta_1 ShareFT_i + \beta_2 FStudent_i + \\ \beta_3 ShareFT_i * FStudent_i + \gamma_j + \mathbf{X_i} + \epsilon_{ij}, \end{aligned} \tag{1}$$

my explanatory variables of interest is the share of female STEM teachers ($ShareFT_i$) in the school for student i of family j. The coefficient of particular interest is the estimate of the interaction term (β_3) of being a female student ($FStudent_i$) and the share of the female STEM teachers. Hence, I analyse whether the share of female STEM teachers

within the natural science program, but was separated from the natural science program in 2000.

affect boys and girls differently. The coefficient tells us how much the likelihood for girls to choose STEM increases in percentage points, in comparison to boys, if the share of female STEM teachers increases from none to all. I control for the family-specific characteristics (γ_j), and I include year of birth and sibling order as student-specific controls ($\mathbf{X_i}$). The cohort fixed effects take care of potential trends in the likelihood of choosing a STEM field whereas the sibling order controls the possibility that older and younger siblings are differently affected. These are both variables that are not family specific but vary instead at individual level. The outcomes of interest (Y_{ij}) are applying to and graduating from a STEM track in upper secondary school and pursuing a degree in math-intensive field in university. The coefficient on the share of female STEM teachers (β_1) captures the effect of an increase in the share of female teachers on boys. The coefficient on the female-student dummy (β_2) captures the difference between girls and boys in the likelihood to graduate in STEM in the next education level, i.e., the gender gap in STEM. I cluster the standard errors at school level as this is the level where the explanatory variable, share of female teachers, varies.

4.1 Potential threats to identification

The ideal design for studying the effect of the gender of the STEM teacher on the likelihood of choosing a STEM-path later would be a randomisation of both the STEM teachers and the students to these teachers. In reality, however, this type of randomisation is hard to conduct. What we are left with is a set of assumptions to be able to claim causality in the research design. In my empirical model, I control for family characteristics that are shared between siblings by including family fixed effects. This is done to take care of the fact that children are not randomly allocated to schools and some family characteristics could be correlated both with the explanatory variable of interest and the outcomes causing omitted variable bias. Hence, in terms of identification of the effect of interest, sorting of teachers across schools matters only to the extent that siblings of different sex choose schools differently in a manner that correlates with both the share of female science teachers and with the likelihood of choosing to continue in math-intensive field of education. In Section 6.2 I test whether this type of sorting matters by introducing different school characteristics interacted with student gender into the main regression specifications. However, the results remain essentially the same across specifications when the additional controls are included.

There are also at least two other reasons why the estimates cannot be interpreted directly as role-model effects. First of all, I do not observe

direct links between students and teachers as I cannot identify which classes students participate in and which classes certain teachers teach. In smaller schools the same math or science teachers will teach all students. If we assume that direct contact with the teacher has a stronger effect than indirect contact, and the effects operate to same direction, then my estimates for larger schools will however be attenuated towards zero if interpreted as direct effects of having a female teacher. Another potentially confounding aspect is that the share of female STEM teachers may correlate with other unobservable factors at school level. If female STEM teachers for example are better (or worse) teachers, then the effect of the gender is not only via role model effect but also due to the difference in teaching quality. I proxy quality by having a teaching degree in STEM subjects in one of the specifications in Section 6.2 and find no difference in the results.[10] Additionally, I investigate the effect of the share of female teachers on performance to rule out the possibility that the found effects are caused by effects on achievement.

5 Data

The studied population includes all individuals born 1982–1995 who finish compulsory school in Sweden. The main sample consists of students who graduate from a compulsory school at the normal age of 16 or one year before or after. I define siblings as those who have the same mother. A unique identifier for each individual makes it possible to link the lower secondary school graduate-register to background variables and to later choices of upper secondary school tracks and university studies. In addition, the lower secondary school graduates register includes a unique identifier for each school. This identifier makes it possible to add more detailed school-specific information to the research data such as total number of students in the schools. I include those schools where I can identify at least one math/science teacher and which have students in all the grades of lower secondary school (grades 7 to 9). There are about 2,000 lower secondary schools in my sample. The sample of graduates from upper secondary school, who have a sibling, consists of about 1,000,000 students who belong to about 430,000 families.

5.1 Explanatory variable of interest

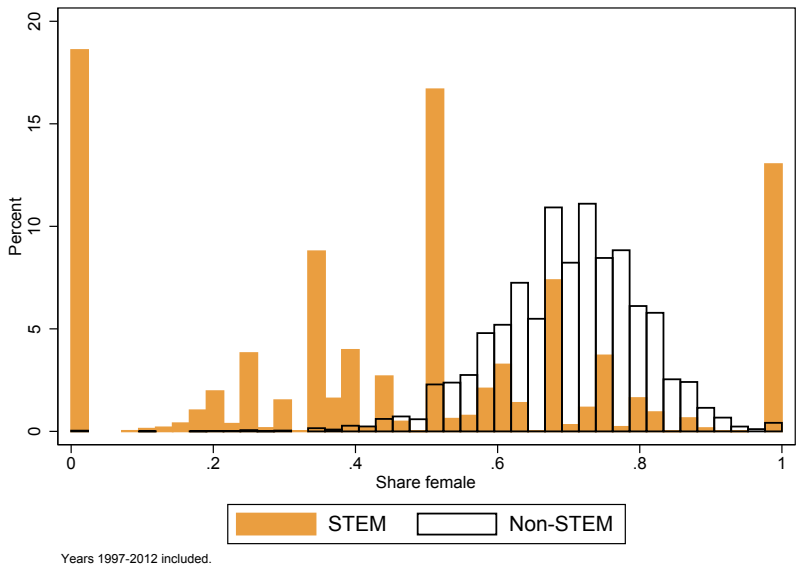
My explanatory variable of interest is the share of female STEM teachers at a school. The share of female STEM teachers and the share of the teachers of other subjects is taken from the teacher register. I

[10] A caveat is that a proper qualification does not guarantee a higher quality of teaching.

can connect children to the school, where they finish their last year of compulsory school.[11] The share is defined the year the children graduate from their schools. The STEM teachers are defined as those who teach science, technical studies and mathematics. These teachers have a common subject identifier in the teacher register.

In Figure 1, I show the distribution of the share of female STEM teachers and the share in other subjects across the years the individuals in the sample finish their lower secondary school.[12] It is apparent from Figure 1 that most teachers in the schools are female but the variation is larger among the STEM teachers. In Figure A3 we can also see that the share of female teachers in STEM subjects has been steadily increasing over the years whereas the increase of females in other subjects has been modest.[13]

Figure 1. Share of female STEM and non-STEM teachers in lower secondary school.



[11]The graduation year is the only year when I can observe the school the students attend.

[12]I have also investigated the variation by age difference between siblings (Table A7) and school size (Table A8). The variation in share of female teachers in somewhat larger in families with larger age differences. With respect to school size, there is slightly more variation in larger schools.

[13]In Figure A3, I also indicate separately the share of female teachers in social sciences. This group of teachers is relevant as they could act as competing role models to teachers in math and science when students consider the alternatives for higher education. We see from the figure that the share of female teachers in social sciences has been relatively stable across the years.

5.2 Outcomes studied

I want to study the effect of same-gender teacher role models on further education and career choices. Hence, I study whether a student applies for a STEM track in upper secondary school, graduates from a STEM track, or pursues a degree in math-intensive field in university. I categorize the science and technical track in upper secondary school as the STEM tracks. I study both application to and graduation from upper secondary school as students may change their track over the course of upper secondary school. The information about applications and graduation at the upper secondary school level come from two separate registers where the tracks are indicated.[14] About 80 percent of each cohort has finished upper secondary school by the year they turn 20. The share of boys and girls who graduate from the science track is fairly similar, but in contrast there are many more boys graduating from the technical track (see Figures A1a and A1b).

The graduation information at university level is taken from the population-wide register LOUISE for the year the students turn 28 years.[15] In line with Kahn and Ginther (2017), I define geosciences, engineering, economics, mathematics, computer sciences and physical sciences as math-intensive majors and refer to them as GEMP fields of study. These fields of study are separated from the life sciences where female participation is already high and which tend to be less math-intensive. The degrees in these GEMP fields are included in my main results for the university-level outcomes. More women than men complete a 3-year university degree by age 28, but notably more men than women major in GEMP (see Figure A1d). Additionally, I also conduct the analysis for various alternative definitions of STEM-majors—the results are not sensitive to different definitions. Due to data limitations, I can observe graduation by age 28 only for the sub-population of my sample who are born in years 1982–1987.

I also study the effect of the share of female STEM teachers on achievement in the national examination of mathematics, the final grade in mathematics, the average final grade in all STEM subjects[16] and on the grade point average (GPA). The data for the national exam is taken from a separate register. I test the effect on GPA as the grades in other subjects matter as well for further education. The exam results are available since 2004 for most of the population who finish 9^{th} grade. The final grades in mathematics and the other STEM subjects as well as

[14] I additionally check for acceptance to the first-ordered track but almost all who apply to a STEM track are also accepted. Hence, the results are essentially the same in both cases.

[15] The median age to graduate is 28 years for university degrees.

[16] STEM subjects defined as those that are thought by the STEM teachers: science, technical studies and mathematics.

the GPA (*meritvärde*) is taken from the compulsory school's graduation register. For across year comparison, I standardise all these measures by school year to have mean zero and standard deviation of one. Girls and boys do very similarly in the national examination of mathematics (Figure A2a) but girls do notably better on average across all subjects when measured by GPA (see Figure A2b).

5.3 Descriptive statistics by sample

Table 1 shows descriptive statistics of the different samples used in the analysis. Column 1 includes all children in the sample with or without a sibling and the second column includes only those with at least one sibling. For the first two samples I can study whether a child applied to and graduated from a STEM track in upper secondary school. The last column shows the sample that is used to study the university level outcome of pursuing a GEMP degree by age 28. The sibling samples, shown in the last two columns, include the individuals who have a sibling born within the same interval of years, i.e., years 1982–1995 and 1982–1987, respectively. As expected, the number of siblings goes down when less years are included. However, the samples are fairly similar. About 40 percent of the individuals have at least one parent with a university degree at the time the child is 16 and about 7 percent of the children have at least one parent with a STEM-degree from university. The number of STEM teachers has increased over time and the share of female STEM teachers has gone up whereas the share of female teachers in other subjects has remained stable. The number of students per school has decreased slightly over the time. The sibling samples are fairly similar to the whole population which suggests that the results for the siblings sample is representative for the whole population.

6 Results

Tables 2 and 3 display the main results across different specifications and samples. Table 2 shows the results for the outcomes at the upper secondary school level: the likelihood of applying for a STEM track and the likelihood of graduating from such a track.[17] Table 3 shows the results in a similar manner for university graduation. In columns 1 and 4 in Table 2, I have included all the individuals from the relevant cohorts irrespective of having a sibling or not. In this specification, I

[17] I have run the regressions also for the outcome of being accepted to a STEM track. The results are shown in Table A4 and show qualitatively the same results as for the applications. This similarity in the results is not a surprise as most who apply to a STEM track are also accepted as is shown in Table A6.

Table 1. *Descriptive statistics (means) of the different samples.*

	$\leq$ 1995, All	$\leq$ 1995, Sib	$\leq$ 1987, Sib
Family background			
# of siblings	2.02	2.45	2.12
Share parents, Uni degree	0.39	0.39	0.38
Share parents, STEM degree	0.07	0.07	0.06
School characteristics			
# of STEM teachers	5.86	5.99	5.01
Share female STEM teachers	0.46	0.46	0.41
# of Soc. Sci. teachers	4.16	4.22	3.65
Share female Soc. Sci. teachers	0.55	0.55	0.55
# non-STEM teachers	40.17	40.13	40.99
Share female non-STEM teachers	0.69	0.69	0.67
# of students	321.16	323.66	338.05
Outcome variables			
STEM-track, application	0.18	0.18	0.19
-Natural Science	0.13	0.13	0.14
-Technical	0.05	0.05	0.04
STEM-track, graduation	0.15	0.15	0.14
-Natural Science	0.11	0.10	0.11
-Technical	0.04	0.04	0.03
GEMP-major, graduation			0.08
N	1,406,670	995,087	252,981

do not control for family fixed effects, but as in all the specifications, I include sibling order and year of birth as controls. These results with the full population are conducted to see whether the results in the sibling sample, where large families are overrepresented, can be extrapolated to the whole population. In the second column for each outcome, I restrict the sample to those who have a sibling and, finally, in the third specification, I include family fixed effects as controls.

The estimate for the full population in column 1 in Table 2 shows a 1.1 percentage point increase for girls in likelihood to apply to a STEM track, in comparison to boys, when the share of female science-teachers is increased from none to all. Relative to the mean this increase translates to 7.9 percent increased likelihood to apply. The estimates for the sibling sample, without family fixed effects, are essentially the same. Given that the estimates in columns 1 and 2 are essentially the same, I conclude that the siblings sample is representative for the whole population of students in lower secondary school. The preferred specification where the family fixed effects are included is shown in column 3 and 6 in Table 2 for application and graduation, respectively. According to these estimates, increasing the share of female science-teachers from none to all decreases the gender gap in applying to a STEM track by 17.4 percent and for

graduating by 16.2 percent. Boys' likelihood to apply decreases, but to a lesser extent than the positive effect on girls.[18]

[18]The gender gap in applications is greater than in graduations. This is due to the fact that girls are more likely to complete a STEM track conditional on applying than boys are (see correlations in Table A6).

Table 2. *Probability to apply to or graduate from a STEM track in upper secondary school.*

	Application			Graduation		
	(1) All	(2) Sib,OLS	(3) Sib,FE	(4) All	(5) Sib,OLS	(6) Sib,FE
Share STEM	0.001	0.003	-0.010**	0.001	0.001	-0.007*
	(0.005)	(0.005)	(0.004)	(0.004)	(0.005)	(0.004)
Girl	-0.091***	-0.091***	-0.092***	-0.067***	-0.067***	-0.068***
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Girl × Share STEM	0.011***	0.012***	0.016***	0.007**	0.009**	0.011**
	(0.004)	(0.004)	(0.006)	(0.003)	(0.004)	(0.005)
N	1,406,670	995,087	995,087	1,406,670	995,087	995,087
Mean outcome, girls	0.139	0.137	0.137	0.118	0.116	0.116
Mean outcome, boys	0.225	0.222	0.222	0.181	0.180	0.180

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

I also find a positive effect on pursuing a degree in a math-intensive field by age 28; according to the result in column 3 in Table 3, the gender gap in obtaining a degree in a GEMP field is decreased by 22.5 percent when the share of female science and math teachers is increased from none to all.[19] Interestingly, the negative effect on boys does not persist into higher education. The OLS estimates for the full population (column 1) differ greatly from those of the sibling-sample (column 2). A reasonable explanation is that the sample covers fewer cohorts and thus oversample families with short spacing between children. Hence, extrapolating the results to the full population of students in the lower secondary school requires more leap of faith for the university level outcome.

Table 3. *Probability to graduate with a GEMP degree by age 28.*

	Degree		
	(1) All	(2) Sib,OLS	(3) Sib,FE
Share STEM	0.009***	-0.001	-0.005
	(0.003)	(0.004)	(0.005)
Girl	-0.079***	-0.073***	-0.071***
	(0.002)	(0.002)	(0.003)
Girl × Share STEM	0.002	0.016***	0.016***
	(0.003)	(0.004)	(0.006)
N	511,854	252,981	252,981
Mean outcome, girls	0.052	0.043	0.043
Mean outcome, boys	0.130	0.110	0.110

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

To investigate these findings further, I study the two STEM tracks in upper secondary school separately in Table 4. The estimates for applications to the two separate tracks are shown in the first two columns and the last two show the results for graduation from these tracks. The

[19]Same specifications are also run for two different definitions of STEM-degrees: one where biology is included and economics not, and one where neither biology or economics are included. These results are shown in Table A1 and A2. The results are essentially the same also in the two different definitions of mathematical fields of study. I have also run the regressions separately for the likelihood to obtain a medical degree. I show the results for this outcome in Table A3. I find no effect on the likelihood to pursue a medical degree for either sex by increasing the share of female STEM teachers in the lower secondary school.

positive effect on applying and graduating is entirely driven by the science track—no effect is found for the more male-dominant technical track. According to the results, having all female science teachers at lower secondary in comparison to none entirely closes the gender gap in the science track. Given these findings, the effect on the decreased gender gap in pursuing GEMP degree is likely to be driven by the effect via the science-track graduates.

Table 4. *Probability to apply to or graduate from a STEM track, separately for science and technical tracks.*

	Application		Graduation	
	(1) Science	(2) Technical	(3) Science	(4) Technical
Share STEM	-0.011***	0.001	-0.008**	0.001
	(0.004)	(0.003)	(0.003)	(0.003)
Girl	-0.018***	-0.074***	-0.011***	-0.056***
	(0.003)	(0.003)	(0.002)	(0.002)
Girl × Share STEM	0.017***	-0.001	0.012***	-0.001
	(0.004)	(0.004)	(0.004)	(0.004)
N	995,087	995,087	995,087	995,087
Mean outcome, girls	0.120	0.017	0.103	0.015
Mean outcome, boys	0.132	0.090	0.111	0.070

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

It could be that female science and math teachers affect achievement in mathematics and hence not only act as role models but increase the scores of female students. If achievement was affected, the effects would be a combination of the effect on possibilities, in terms of grades, and preferences via the same-gender role models. The effect on achievement could be caused by different channels: teachers could have gender-specific ways of teaching that works best for the same-gender students or it could be that teachers favour students of same gender in their grading. In Table 5, I run the same specification as for the main results (Equation 1) on the results at national examination of mathematics, final grade in mathematics, average of final grades in STEM subjects and grade point average. All of the outcomes are standardized by school year to have a mean zero and standard deviation of one. I find no effects of the interaction term in any of the outcomes. Hence, girls and boys are not differently affected by the share of female science

teachers. These findings support the interpretation that girls are more likely to choose a STEM field if they are exposed to female role models.

Table 5. *Effect on performance across different measures of achievement at the end of lower secondary school.*

	(1) Exam	(2) Math grade	(3) STEM avg	(4) GPA
Share STEM	0.001	-0.014	-0.111***	-0.015
	(0.022)	(0.021)	(0.039)	(0.010)
Girl	-0.009	0.092***	0.154***	0.322***
	(0.012)	(0.013)	(0.012)	(0.006)
Girl × Share STEM	0.017	-0.001	-0.002	0.005
	(0.022)	(0.023)	(0.021)	(0.011)
N	577,807	607,822	607,822	1,001,210
Mean outcome	0.009	0.003	0.081	0.025

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls. All outcomes are standardize by school year to have mean zero and standard error of one.

6.1 Heterogeneity

Both Carrell et al. (2010) and Bottia et al. (2015) found the largest effects of female role models for female students who performed particularly well in mathematics. As I explained in Section 3, this result could be caused by female students having a low confidence in their skills in mathematics despite performing well. If same-gender teacher role models matter for increasing confidence in STEM skills, then we would expect especially those with the needed skill-level to be affected the most. Additionally, this group of female students is the one who would likely to be the most suitable to pursue a degree in a math-intensive field as they already perform well in mathematics. Additionally, I investigate the main results by school size as in a smaller school the students are more likely to be in direct contact with the teachers.

I define the top-performing students as those who belong to the top 25^{th} percentile with respect to the national examination in mathematics in 9^{th} grade, and study this group of students separately. As the national examinations data starts from year 2004, it is only possible to study the school-track choices in upper secondary school for this part of the analysis. The results are shown in the first two columns of Table 6. In line with previous studies, I find the point estimates to be large for the top-performing girls, in comparison to boys, but these estimates are not

statistically significant from zero. As expected, top-performing students are more likely to choose a STEM track. Interestingly, the gender gap in applying and graduating is notably larger among the top-performing students than among all.

Table 6. *Top 25^{th} percentile students in mathematics on the probability to apply to or graduate from a STEM track.*

	(1) Application	(2) Graduation
Share STEM	-0.014	-0.042
	(0.043)	(0.043)
Girl	-0.154***	-0.150***
	(0.028)	(0.027)
Girl × Share STEM	0.027	0.045
	(0.049)	(0.048)
N	141,288	141,288
Mean outcome, girls	0.333	0.320
Mean outcome, boys	0.484	0.455

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

6.2 Robustness of the mechanism

In Tables 7 and 8 I conduct multiple robustness checks for the main results by interacting the gender dummy with different school-level characteristics and investigate whether the main effect is affected. I test the possibility of potential competing role models by controlling for the share of female teachers in social sciences in columns 1 and 5 in Table 7 and in column 1 in Table 8. The results remain qualitatively the same. Hence, it seems like the share of female teachers in mathematics and science is of particular importance as role models for girls. This result supports the idea that there are fewer females role models outside of school for science and hence the teachers in these subjects have a stronger effect on later educational choices.

The gender of a teacher can be correlated with other teacher or school characteristics that may affect student preferences. In columns 2 and 6 in Table 7, and in column 1 in Table 8, I control for the share of STEM teachers at the schools who have a degree for teaching in the subject-specific area. The effect remains essentially the same. For additional

robustness, I control for mean GPA at the school level and the number of students at the school in the last two column of each outcome. If we would see a difference in the main effect of the interaction term of girl and the share of female STEM teachers this would indicate that school quality matters for the found effect (mean GPA of the students used as a proxy for quality) or the size of the school. The main results remain the same after including these additional school-level controls. Additionally, I investigate the effect by school size (Table A9). When I divide the sample by the median school size of the schools, I find that the results remain qualitatively similar in both groups but only the results for the smaller schools remain statistically significant. The smaller schools are also the ones where the students are more likely to be in direct contact with the teachers.

Table 7. *Sensitivity analysis by including different school characteristics as controls for the probability to apply to and graduate from a STEM track.*

	Application				Graduation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share STEM	-0.014***	-0.010**	-0.009**	-0.010**	-0.011**	-0.007*	-0.006	-0.007*
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Girl	-0.092***	-0.093***	-0.130***	-0.086***	-0.070***	-0.067***	-0.102***	-0.060***
	(0.005)	(0.004)	(0.003)	(0.005)	(0.004)	(0.003)	(0.003)	(0.004)
Girl × Share STEM	0.018***	0.015***	0.016***	0.015***	0.013**	0.012**	0.012**	0.010**
	(0.006)	(0.006)	(0.005)	(0.006)	(0.005)	(0.005)	(0.004)	(0.005)
Girl								
× Share Soc. Sci.	Yes	No	No	No	Yes	No	No	No
× Share Qualified	No	Yes	No	No	No	Yes	No	No
× GPA	No	No	Yes	No	No	No	Yes	No
× School size	No	No	No	Yes	No	No	No	Yes
N	919,720	995,087	994,862	995,087	919,720	995,087	994,862	995,087
Mean outcome, girls	0.136	0.137	0.137	0.137	0.116	0.116	0.116	0.116
Mean outcome, boys	0.221	0.222	0.222	0.222	0.180	0.179	0.179	0.179

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls as well as the main effect of the variable that is interacted with the girl dummy. Columns (1) and (5) include schools where I can identify both science and social science teachers. Columns (3) and (7) include those individuals for whom I can observe the end of school GPA.

Table 8. *Sensitivity analysis by including different school characteristics as controls for the outcome of graduating with a GEMP degree in university.*

	Degree			
	(1)	(2)	(3)	(4)
Share STEM	-0.006	-0.004	-0.003	-0.004
	(0.006)	(0.005)	(0.004)	(0.004)
Girl	-0.072***	-0.075***	-0.086***	-0.055***
	(0.005)	(0.003)	(0.003)	(0.005)
Girl × Share STEM	0.020***	0.012**	0.015***	0.015***
	(0.007)	(0.006)	(0.005)	(0.006)
Girl				
× Share Soc. Sci.	Yes	No	No	No
× Share Qualified	No	Yes	No	No
× GPA	No	No	Yes	No
× School size	No	No	No	Yes
N	210,554	252,981	252,793	252,981
Mean outcome, girls	0.041	0.043	0.043	0.043
Mean outcome, boys	0.104	0.110	0.110	0.110

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls as well as the main effect of the variable that is interacted with the girl dummy. Column (1) includes schools where I can identify both science and social science teachers. Column (3) includes those individuals for whom I can observe the end of school GPA.

7 Concluding discussion

In this study, I investigate whether female teachers in science at school increase the likelihood of female students to apply to and graduate from a STEM track and further pursue a degree in a math-intensive field. I find that an increase in the share of female math and science teachers increases the likelihood in both levels of education. However, this effect does not come without affecting also the male students. Whereas the increase in the share of female STEM teachers increases the likelihood of female students to choose STEM, it decreases the likelihood of boys. I find that increasing the share of female math and science teachers at lower secondary school from none to all decreases the gender gap in graduating from a STEM track by 16.2 percent and by 22.5 percent in the math-intensive degrees in university. The effects are stable across a variety of robustness checks. Additionally, I find no effect on achievement at the end of compulsory school for girls. The fact that the performance

is not affected speaks for the favour of role model effect. Furthermore, I found the effect to be remain statistically significant only for the smaller schools. In small schools it is more likely that the students are in direct contact with their teachers. Hence, also this finding supports the role-model framework.

In comparison to Bettinger and Long (2005), who studied the effect of the share of female math and science teachers in upper secondary school on the likelihood to apply to and graduate from math-intensive fields, my estimates are moderate. According to the results of Bottia et al. (2015), girls are 19.7 percent more likely to start and 35 percent more likely to graduate with a math-intensive major if the share of female math and science teachers is increased from one standard deviation below the mean to one above. In contrast, Carrell et al. (2010) do not find an effect of the teacher's gender in introductory courses on the probability for graduating with a STEM degree among all students but find the gender gap to be nearly closed among the highest performing female students when the share of female professors in the introductory courses is changed from none to all. These two studies are very different methodologically: Bottia et al. (2015) control for variety of individual controls to identify the effect of the share of female teachers in STEM at upper secondary school level on choosing STEM as major, whereas Carrell et al. (2010) study the direct linkage of introductory STEM-course teacher's gender on the probability to continue in STEM in college and are able to argue for random allocation of teachers. However, in contrast to my findings both of the studies find the strongest effect among the high-performing female students. I do not find support for such a result in my analysis. Neither Bottia et al. (2015) nor Carrell et al. (2010) find negative effects on boys for the increased share of female science-teacher exposure.

The negative effect on male students of having a female STEM teacher has been found in some of the previous studies, especially those studying student performance on exams. For example, Carrell et al. (2010) found a negative effect on male students, above the positive effect on female students of having a larger share of first-year math and science courses taught by female professors, on the following STEM-course performance. However, they found no such effect on the probability to graduate with a STEM degree. Hoffmann and Oreopoulos (2009) found a negative effect on male students' probability to continue a course if the instructor was female and found no effect on female students. Price (2010) finds the effect of a larger share of courses taught by a female teacher during the first year of university to decrease the probability of male students continuing in the field and no effect on female students. Bottia et al. (2015) however do not find the negative effect on male students when studying the effect of share of female-STEM teachers on the likelihood

to major in STEM on female and male students nor on declaring a STEM major.

Overall this paper contributes with evidence of the importance of role models at an earlier stage when no choices of educational path has yet been done, whereas the earlier literature has focused mainly on the higher level of education. I find share of female teachers to matter for the later educational choices and the robustness checks speak in favour of the role-model effect. Still a full understanding of the exact channel through which this effect operates needs more research. The gender of a teacher is likely to correlate with other characteristics that could also affect the likelihood of continuing in math-intensive education. Carlana (2017)'s work on the importance of gender stereotypes among teachers and their effect on student performance is one way forward, and it would be interesting to conduct such a study at a larger scale of both students and teachers to better understand the mechanisms that lead to gendered career choices.

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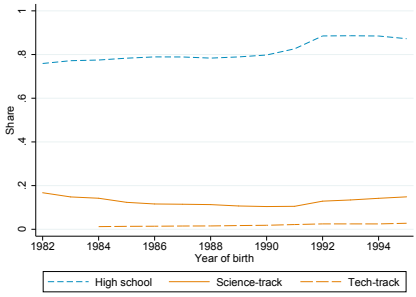
Appendix

A Development of outcome variables over time

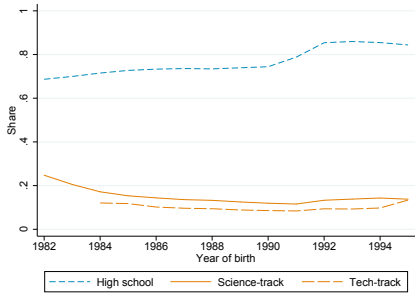
Figure A1 shows the development of the outcome variables for graduation from a STEM track in upper secondary school and for pursuing a degree in a GEMP field in university. In Figures A1a and A1b we see that about 80 percent of each cohort has finished their upper secondary school by the year they turn 20. The share of girls and boys who graduate from the science track is fairly similar, but in contrast there are many more boys graduating from the technical track. This gender imbalance in the technical track does, to a large extent, account for the gender difference in the STEM tracks (Figure A1c). The reform of the early 2000s that separated the technical track from the science track is apparent in the figures. Additionally, the reforms of year 2011 increased the share of students graduating from any upper secondary school program, which also affects the shares in both of the STEM tracks. Interestingly, even though the share of female STEM teachers has increased in the lower secondary schools (Figure A3), we do not see much of a change in the share of female students choosing STEM in upper secondary school over the research period. Additionally, I study the effect on pursuing a degree in a math-intensive field at the university level for those whom I can observe this outcome at age 28 (cohorts born 1982–1987). In Figure A1d we see that more women than men complete a 3-year university degree by age 28, but notably more men than women major in GEMP.

Figure A1. Share of 20 year olds who have graduated from upper secondary school and those with a STEM track by sex, and share of female and male students who have any university degree and specifically GEMP degree by age 28.

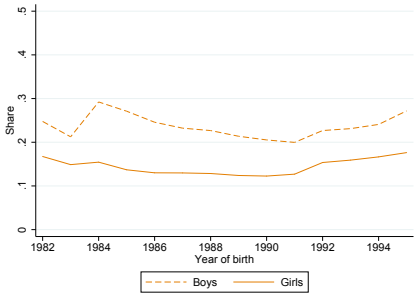
(a) Girls, 20 yrs



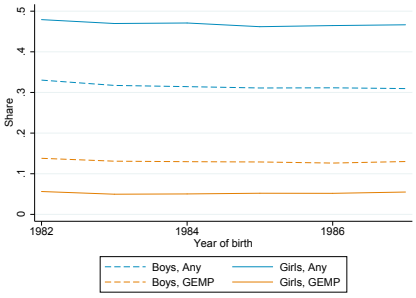
(b) Boys, 20 yrs



(c) STEM-track, 20 yrs



(d) Uni- and GEMP-degree, 28 yrs



Notes: Technical track was separated from the science track in 2000. In year 2011 a reform related to the requirements of completion was implemented in the upper secondary school.

B Sensitivity to different definitions of STEM fields

Across papers focusing on STEM fields, the definition of them differs. I followed Kahn and Ginther (2017), who define the specific group of more math-intensive fields to include geosciences, engineering, economics, mathematics, computer sciences and physical sciences as GEMP fields. However, to test the sensitivity of results the to the definition, I conduct the same specifications as in Table 3 with couple of alternative definitions. In Table A1, in comparison to the GEMP fields, I include biology from the life sciences and exclude economics. In table A2 I also exclude economics but do not take biology into account. Finally, in Table A3, I investigate whether the probability to pursue a medical degree is affected by the share of female science teachers at lower secondary

school. I find the effect on the gender gap to be slightly lower when economics is excluded and biology included, the effect in column 3 in Table A1 is a decrease of 18.5 percent in the gender gap when the share of female STEM teachers is increased from non to all. When biology is excluded from the definition, the effect is a 22.5 percent decrease in the gender gap (Table A2). In Table A3 we see that the share of female science teachers in lower secondary school does not matter for the likelihood to pursue a degree in medical studies (including medical studies to become a doctor, dentist or veterinarian).

Table A1. *Probability to graduate with a STEM degree by age 28.*

	Degree		
	(1) All	(2) Sib,OLS	(3) Sib,FE
Share STEM	0.008**	-0.000	-0.003
	(0.003)	(0.004)	(0.005)
Girl	-0.072***	-0.067***	-0.065***
	(0.002)	(0.002)	(0.003)
Girl × Share STEM	0.000	0.012***	0.012**
	(0.003)	(0.004)	(0.006)
N	511,854	252,981	252,981
Mean outcome, girls	0.060	0.048	0.048
Mean outcome, boys	0.131	0.111	0.111

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

Table A2. *Probability to graduate with a STEM-degree w/o biology included by age 28.*

	Degree		
	(1) All	(2) Sib,OLS	(3) Sib,FE
Share STEM	0.008**	-0.001	-0.005
	(0.003)	(0.004)	(0.005)
Girl	-0.079***	-0.073***	-0.071***
	(0.002)	(0.002)	(0.003)
Girl × Share STEM	0.001	0.016***	0.016***
	(0.003)	(0.004)	(0.006)
N	511,854	252,981	252,981
Mean outcome, girls	0.050	0.041	0.041
Mean outcome, boys	0.127	0.107	0.107

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

Table A3. *Probability to graduate with a medical degree by age 28.*

	Degree		
	(1) All	(2) Sib,OLS	(3) Sib,FE
Share STEM	0.002**	0.002**	-0.000
	(0.001)	(0.001)	(0.002)
Girl	0.009***	0.008***	0.008***
	(0.001)	(0.001)	(0.001)
Girl × Share STEM	0.002*	0.001	0.001
	(0.001)	(0.002)	(0.002)
N	511,854	252,981	252,981
Mean outcome, girls	0.019	0.016	0.016
Mean outcome, boys	0.009	0.008	0.008

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

C Figures

Figure A2. Share of boys and girls in each decile of the grade-distribution of 9th grade national exams in mathematics and deciles of GPA.

(a) National exam *(b)* GPA

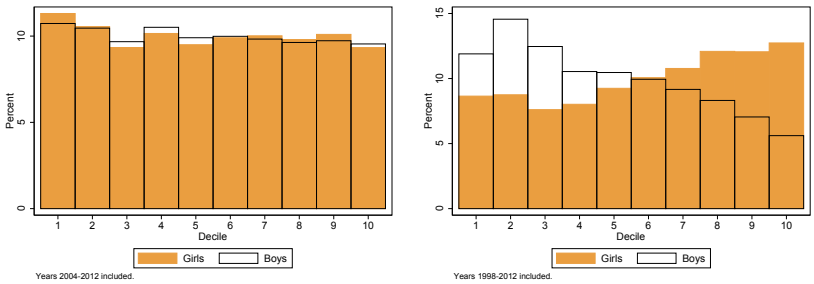
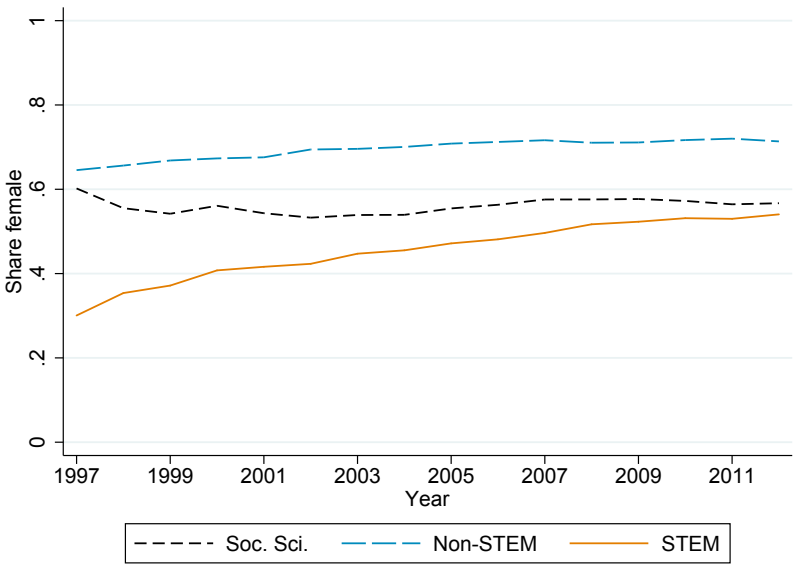


Figure A3. Share of female STEM and non-STEM teachers across years in lower secondary school.



D Tables

Table A4. *Probability to be accepted to a STEM track in upper secondary school.*

	Accepted		
	(1) All	(2) Sib,OLS	(3) Sib,FE
Share STEM	0.002	0.003	-0.010**
	(0.005)	(0.005)	(0.004)
Girl	-0.090***	-0.090***	-0.091***
	(0.002)	(0.002)	(0.003)
Girl × Share STEM	0.011***	0.012***	0.015***
	(0.004)	(0.004)	(0.006)
N	1,406,670	995,087	995,087
Mean outcome, girls	0.138	0.136	0.136
Mean outcome, boys	0.223	0.220	0.220

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

Table A5. *Sensitivity analysis by including different school characteristics as controls for the probability to apply to and graduate from a science track.*

	Application				Graduation			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Share STEM	-0.012***	-0.011***	-0.010***	-0.011***	-0.009**	-0.007**	-0.007**	-0.008**
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.003)
Girl	-0.017***	-0.019***	-0.051***	-0.013***	-0.010***	-0.013***	-0.040***	-0.006*
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)	(0.002)	(0.003)
Girl × Share STEM	0.014***	0.016***	0.016***	0.016***	0.011**	0.010***	0.012***	0.011***
	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Girl								
× Share Soc. Sci.	Yes	No	No	No	Yes	No	No	No
× Share Qualified	No	Yes	No	No	No	Yes	No	No
× GPA	No	No	Yes	No	No	No	Yes	No
× School size	No	No	No	Yes	No	No	No	Yes
N	919,720	995,087	994,862	995,087	919,720	995,087	994,862	995,087
Mean outcome, girls	0.118	0.120	0.120	0.120	0.101	0.101	0.101	0.101
Mean outcome, boys	0.129	0.132	0.132	0.132	0.107	0.108	0.108	0.108

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls. Columns (1) and (5) include schools where I can identify both science and social science teachers. Columns (3) and (7) include those individuals for whom I can observe the end of school GPA.

Table A6. *Correlation between application, acceptance and graduation from a STEM track in upper secondary school, separately for girls and boys and the two different tracks.*

	Girls			Boys		
	Application	Acceptance	Graduation	Application	Acceptance	Graduation
			STEM, both tracks			
Application	1.0000			1.0000		
Acceptance	0.9918	1.0000		0.9889	1.0000	
Graduation	0.7792	0.7799	1.0000	0.7666	0.7686	1.0000
			Natural science track			
Application	1.0000			1.0000		
Acceptance	0.9924	1.0000		0.9933	1.0000	
Graduation	0.7826	0.7832	1.0000	0.7860	0.7862	1.0000
			Technical track			
Application	1.0000			1.0000		
Acceptance	0.9895	1.0000		0.9853	1.0000	
Graduation	0.8008	0.8023	1.0000	0.7640	0.7683	1.0000

Table A7. *Variation in share of female STEM teachers by maximum age difference between siblings in a family.*

	Mean	SD	Min	Max	Observations
All					
Overall	0.464	0.259	0.000	1.000	N = 995087
Between		0.212	0.000	1.000	N = 432361
Within		0.153	-0.364	1.279	T-bar = 2.302
1-3 yrs					
Overall	0.474	0.252	0.000	1.000	N = 468058
Between		0.218	0.000	1.000	N = 228731
Within		0.128	-0.201	1.224	T-bar = 2.046
4-6 yrs					
Overall	0.459	0.259	0.000	1.000	N = 314962
Between		0.205	0.000	1.000	N = 129262
Within		0.160	-0.341	1.209	T-bar = 2.437
Over 6 yrs					
Overall	0.450	0.274	0.000	1.000	N = 197259
Between		0.195	0.000	1.000	N = 67076
Within		0.196	-0.379	1.265	T-bar = 2.941

Table A8. *Variation in share of female STEM teachers by size of the schools.*

	Mean	SD	Min	Max	Observations
<315 students					
Overall	0.447	0.224	0.000	1.000	N = 505604
Between		0.200	0.000	1.000	N = 271842
Within		0.114	-0.276	1.199	T-bar = 1.860
≥315 students					
Overall	0.482	0.290	0.000	1.000	N = 489483
Between		0.254	0.000	1.000	N = 264978
Within		0.151	-0.346	1.297	T-bar = 1.847

Table A9. *Main results by school size for applying to and graduating with a STEM track and pursuing a math-intensive degree in university. School size defined by median size of schools.*

	Small school			Large school		
	(1) Application	(2) Graduation	(3) Degree	(4) Application	(5) Graduation	(6) Degree
Share STEM	-0.011*	-0.003	-0.009	-0.002	-0.004	-0.002
	(0.007)	(0.006)	(0.007)	(0.010)	(0.009)	(0.008)
Girl	-0.092***	-0.065***	-0.066***	-0.095***	-0.072***	-0.076***
	(0.005)	(0.004)	(0.004)	(0.006)	(0.005)	(0.005)
Girl × Share STEM	0.018**	0.011	0.018**	0.012	0.009	0.013
	(0.008)	(0.007)	(0.008)	(0.011)	(0.010)	(0.011)
N	491,096	491,096	112,923	510,347	510,347	146,806
Mean outcome	0.172	0.141	0.068	0.188	0.155	0.084

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Robust standard errors clustered at school level. All specifications include sibling order and year of birth as controls.

II. Childcare –A safety net for children?

Co-authored with Eva Mörk, Anna Sjögren and Helena Svaleryd

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1 Introduction

Children of unemployed parents have worse health than children whose parents are working.[1] Studying Swedish children, Mörk et al. (2014b) find that children with at least one unemployed parent are 17 percent more likely to be hospitalized in a year than children whose parents are employed. Since poor childhood health has been shown to have persistent effects on child development and adult outcomes, understanding how to improve the health outcomes of disadvantaged children is highly relevant.[2] In this paper, we ask whether access to high quality childcare at age 2–5 affects health outcomes among children with unemployed parents. We study the immediate effects on physical health as well as the effects at age 10–11 on physical and mental health. To this end, we use rich register data on hospitalizations and drug prescriptions and exploit a Swedish reform that improved access to formal childcare for children with unemployed parents. Before the reform, municipalities varied with respect to whether they offered childcare to children with unemployed parents. After the reform, offering childcare to these children became mandatory. Comparing the change in health of children of unemployed parents residing in municipalities that had to change policy with the corresponding change for children of unemployed parents living in municipalities that already before the reform offered childcare to these children, we estimate the causal effect of having access to childcare in a difference-in-differences framework.

There is vast evidence that childcare improves cognitive outcomes especially among disadvantaged children.[3] Less is known about the causal effects of childcare on children's health outcomes. There is however a large literature studying the associations between attending childcare and short run health outcomes such as the prevalence of respiratory

[1] This has been shown using U.S. data for birth weight (Lindo 2011) and parental reported health and mental health (Schaller and Zerpa 2015), in Scandinavia for hospitalization (Mörk et al. 2014b; Christoffersen 2000), and physiological problems (Sund et al. 2003; Kaltiala-Heino et al. 2001;Christoffersen 1994), in Slovakia for self-rated health and long-term well-being (Sleskova et al. 2006) and in Holland for behavioral problems (Harald et al. 2002)

[2] In her survey, Currie (2009) present evidence that low birth weight has been found to reduce test scores, the likelihood of high school graduation and earnings, and that individuals with better self-rated health during childhood have higher incomes as adults. Mörk et al. (2014a) find similar results for Sweden.

[3] Most of these studies find that access to high quality childcare improves cognitive outcomes for disadvantaged children (e.g. Felfe and Lalive 2018; Fitzpatrick 2008; Gathmann and Sass 2017; Felfe et al. 2015; Drange and Havens forthcoming; Cornelissen et al. forthcoming). The quality of the home environment, and thus the alternative mode of care, as well as child age when attending childcare, seem to matter for the effects of childcare on cognitive development (see Cascio 2015 for an overview).

infections, diarrheal illness and the use of antibiotics. These studies typically find that attending childcare is associated with a temporary higher prevalence of diseases and antibiotics prescriptions, followed by a period with a reduced likelihood of illness, and no changes in the long run (see e.g., Lu et al. 2004 and de Hoog et al. 2014 Ball et al. 2002). These studies point to an acquired immunity, in line with the so-called *hygiene-hypothesis* suggested by Strachan (1989), but that the immunity effect seems to fade out over time.

Papers aiming at estimating causal effects of childcare using experimental and quasi-experimental methods are less common. The earlier evidence of immediate effects of attending childcare on physical illnesses such as infections and colds is supported by findings in Baker et al. (2008) and van den Berg and Siflinger (2018). Baker et al. (2008) find negative effects of childcare on a number of child health related outcomes (reported by parents), such as whether the child is in excellent health or experienced throat or ear infections, when universal childcare was introduced in Quebec. The negative effects seem to persist later in life (Baker et al. 2015).[4] In a study of Southern Sweden using register data, van den Berg and Siflinger (2018) find that cohorts with longer exposure to a regime of low childcare fees, and potentially higher childcare enrollment, tend to have more infections at a younger age but fewer infections at ages 6–7.[5] They term this a *substitution effect*. Liu and Skans (2010) instead, do not find any effects on hospitalizations of 1–16 year olds of a parental leave reform which likely led families to substitute formal childcare for parental care during the child's second year of life.[6]

Earlier evidence on mental health is more mixed. After the reform in Quebec, parents reported that their children showed more aggression, and had worse motor and social skills, once being enrolled in childcare and in the long run there is evidence of increased criminal activity for boys (Baker et al. 2008 and Baker et al. 2015). In contrast, the study by van den Berg and Siflinger (2018) finds that cohorts with potentially higher childcare enrollment, were less likely to experience behavioral problems, such as developmental and behavioral disorders. Similarly, Yamaguchi et al. (2017), in a study on Japanese data, find that childcare reduced inattention and hyperactive behavior among children aged 2.5 of low-educated mothers. There is also evidence that enrollment age may matter for the health effects of attending childcare. Kottelenberg

[4]In particular, Baker et al. (2015) find negative effects on self-reported health and life satisfaction also at ages 12–20. They also find lasting negative effects on non-cognitive skills and higher rates of youth crime, especially for boys.

[5]The analyzed fee reductions were the result of a reform in 2001 which harmonized the fees across municipalities and substantially lowered them.

[6]Parental leave was extended from 12 to 15 months in 1989.

and Lehrer (2014) study the Quebec reform and show that the negative effects of childcare are mostly driven by children who started childcare at early ages.[7] For children aged 3 there are instead benefits, in terms of better development scores, of attending childcare.

We contribute to the existing literature in several ways. First, we focus on the effects of childcare access on children of unemployed parents. This group of children is arguably of particular policy relevance, because of their vulnerability. Second, compared to much of the earlier literature on childcare, we focus on child health and also provide estimates relating to childcare exposure at toddler and preschool age. In addition, we are able to follow the children and explore effects at 10–11 years of age. Third, our study has some data related and methodological advantages. We use register data on in-patient care and prescription drugs to measure child health outcomes. These are arguably more objective than the parent reported outcomes used in the Canadian studies. In particular, there is a risk that the way parents evaluate and report their children's health status may be affected by the fact that children are in childcare. An advantage, in relation to the study by van den Berg and Siflinger (2018), is that we are able to control for health trends since we rely on regional variation in reform exposure to identify causal effects.[8]

We rely on register data to measure health outcomes. More specifically, we use data from the National Patient Register, which contains information of all hospital stays in Sweden, including detailed information about diagnoses. Incidences that lead to hospital stays are of course rather serious and we are not be able to pick up less severe health problems with these data.[9] As a complement, we therefore also analyze prescriptions for medical drugs. Unfortunately, prescription drug data are only available from 2005, which implies that we will not be able to use these outcomes in the short-run analysis.

Since Swedish register data do not include any information on childcare attendance we do not know which children attend childcare. Hence,

[7] Kottelenberg and Lehrer (2013) also show that the findings in Baker et al. (2008) are robust to the inclusion of additional years of data, implying that the negative effects originally found are not due to initial implementation problems.

[8] van den Berg and Siflinger (2018) use the fact that different cohorts were exposed to lower childcare fees for different number of years, depending on their age when the maximum fee reform was introduced. Hence, even though they control for time trends in a flexible way, they are unable to control for cohort-specific time shocks. In a sensitivity analysis, they investigate whether the effects on health are heterogeneous with respect to the reform-induced reduction in childcare fees, which differed between municipalities, but do not find any statistically significant heterogeneity.

[9] On the other hand, one might argue that it is the more severe health problems that are likely to result in negative long run outcomes and therefore are most interesting.

our estimates should be interpreted as intention-to-treat effects.[10] Using survey data, we show that enrollment increased substantially, by 20-percentage points, among children with unemployed parents in treated municipalities compared to enrollment in control municipalities, implying the existence of a first stage.

Our results show that access to childcare did not affect hospitalization rates for children aged 2–3, for any of the diagnoses that we investigate. This result is in line with Liu and Skans (2010) who find no effect on hospitalization of the increase in parental care during children's second year of life. For preschool children, 4–5 year old, we find that access to childcare caused an increase in hospitalization for infections the first year after the reform. This result supports findings in a number of correlation studies of a temporary increase in the risk of infections when children first enroll in childcare.

In the medium run, we find no evidence of an effect of earlier childcare access on hospitalizations at age 10–11. Neither do we find effects on prescriptions of antibiotics. As for ADHD-medication and psycholeptics (prescribed to treat anxiety or sleeping problems), estimates point to that gaining access to childcare may have increased mental health problems, but standard errors are large and we cannot rule out zero or positive effects. Prescriptions for respiratory conditions at age 10-11, however, declined by five percent for children who had access to childcare when parents were unemployed. This result supports either the hygiene-hypothesis or the presence of a substitution effect as found in van den Berg and Siflinger (2018).

As earlier evidence shows that family characteristics, such as the education level of parents, matter for the impact of childcare on cognitive skills, we study whether there are heterogeneous effects with respect to the education level of the mother. We find that the immediate increase in infections among preschool children is entirely driven by children whose mothers have only compulsory education. Because we find that the parents with a low education were equally likely to be employed when the child was younger, we can rule out that their increased hospitalization rates was a result of these children's lesser exposure to childcare at an early age. A potential explanation could instead be that being exposed to the childcare environment has larger effects on children with unemployed parents because the parents do, to lesser extent, seek for appropriate preventive and primary care.

The rest of the paper is organized as follows. In Section 2, we discuss potential effects on child health in general and on hospitalization in

[10] Lack of information in who is actually treated by the reform may introduce measurement error in our treatment variable. Estimates may hence suffer from attenuation bias.

particular, when a child is cared for at home by an unemployed parent instead of attending center-based childcare. Thereafter, in Section 3, we describe the institutional setting as well as the reforms that allow us to identify the causal effects. Section 4 presents our quasi-experimental strategy and Section 5 the data. We then turn to the results in Section 6. Finally, Section 7 concludes.

2 How can the mode of care be expected to affect child health?

This paper focuses on the short and medium-run health consequences for children with unemployed parents who are either cared for at home by their unemployed parent or attending center-based childcare. In this section, we discuss why being at home with an unemployed parent or attending center-based childcare might have different health consequences for a child.

In childcare the child is attended by professional staff, trained in early childhood education and development, in a facility especially designed for children. This may increase the likelihood of early detection of health problems, and hence exposure to preventive health measures, reducing the need for hospitalization. Furthermore, this may also reduce the risk of injuries and poisoning and stimulate the child's psycho-social development. However, a group of children is also a fertile environment for spreading child related viruses and infections (Lu et al. 2004; de Hoog et al. 2014; Ball et al. 2002). While serious illnesses may have negative long run effects, it has been argued that contacting minor infections early in life can build a child's immune system and lead to fewer infections later, the so called hygiene-hypothesis (Strachan 1989). Currie and Almond (2011) however also discuss the possibility that poor health in early childhood can make the child more sensitive later on. Results from observational studies tend to show that entering childcare only gives rise to a timing effect on *when* the child get infections and respiratory conditions.[11] This effect is what van den Berg and Siflinger (2018) call a substitution effect. Being in a large group of children might also be stressful for sensitive children, and may thus lead to more anxiety and aggression; a hypothesis that is supported by empirical evidence in e.g. Baker et al. (2008).[12]

[11] Ball et al. (2002), for example, finds that attendance at large daycare centers was associated with more common colds during the preschool years but less during early school years. This acquired immunity was however waned by age 13.

[12] That children in childcare may also suffer from fewer one-to-one interactions with adults is also supported by evidence by Fort et al. (2017) who find attending childcare at ages 0–2 to reduce IQ at ages 8–14 for children in advantaged families.

Compared to a facility designed for the care of children, the home environment of most children is full of potentially dangerous everyday objects. Yet, parents are experts on their own children, and can focus on the individual child to a larger extent than childcare personnel who have many children to attend and care for. Being away from parental care for many hours per day might also be detrimental for children's attachment to their parents, and thereby their psychological well-being later on (see, e.g., NICHD-ECCRN 2003). However, experiencing unemployment may be stressful and thereby negatively affect the quality of parenting.[13] Parenting quality may also be affected by having children at childcare centers if parents learn parenting skills from teachers or other parents, or if parents experience less stress when they do not have to activate and care for their children full-time. Yamaguchi et al. (2017) show that enrollment at childcare centers improved parenting quality among low-educated mothers. Other indirect effects of childcare access may be that unemployed parents could find a new employment sooner when they can spend more time on job search, which will increase family income.[14]

Whether a child's health will benefit or be harmed by spending time at home due to parental unemployment instead of at a childcare facility is thus an open question. In particular, effects are likely to differ depending on the health outcome considered. Also, the effects can be expected to differ depending on the quality of the care provided by parents and childcare facilities. This is relevant for diagnoses related to injuries and respiratory conditions, where home conditions, such as child safety awareness and indoor environment, as well as caregivers' awareness of early signals of illnesses, are likely to matter. We would expect an increased likelihood of infections when children first attend childcare (or start school if they have not attended childcare during early ages). However, whether infections require hospital care is likely to depend on their severity, but also on the quality of preventive or primary care available to the child. Finally, when it comes to mental health and behavioral problems in the medium-run, Canadian evidence points to increased anxiety and aggression as a result of increased childcare enrollment, while evidence from Japan shows that childcare enrollment among children of low-educated mothers reduces inattention and hyperactivity symptoms. In this study, we attempt to capture effects on physical and

[13] E.g. Eliason and Storrie (2009) and Browning and Heinesen (2012) show that individuals that experience job loss due to a plant closure experience negative health consequences. Furthermore, Eliason (2011) and Huttunen and Kellokumpu (2016) find an increased risk of divorce following a job loss.

[14] However, if the value of the parent's time (leisure) at home is higher when there is no need to care for the child, unemployment duration may instead increase. Vikman (2010) finds a 17 percent increase in the likelihood that mothers find employment when childcare is available. She finds no similar effect for unemployed fathers.

mental health by investigating hospitalizations related to injuries, infections, respiratory conditions, as well as prescription of medical drugs related to infections (anti-infectives), respiratory conditions and mental and behavioral problems (ADHD-medication and psycholeptives).

3 Childcare and health care in Sweden

3.1 Childcare and the reform

In the year 2000 as many as 66 percent of Swedish children aged 1–5 attended publicly financed childcare. Swedish childcare is heavily subsidized and of high quality and it is the municipalities that are responsible for organising the childcare. Before July 2001, municipalities were obliged to provide childcare for children whose parents were either working or full time students, from when the child turned one until school start (i.e. in the fall of the year the child turns six).[15] The average enrollment age for children born 1999 was 18 months (Duvander 2006). Whether to also provide childcare for children whose parents were unemployed or on parental leave with a younger sibling was determined locally by each municipality.[16] In July 2001, a new law came into place requiring municipalities to offer preschool for at least 3 hours per day or 15 hours per week to children whose parents were unemployed. This paper exploits this policy change to isolate a causal effect on child health of access to childcare for children with unemployed parents.

The aim of the policy was to increase childcare enrollment among disadvantaged children and to facilitate job finding for unemployed parents (primary mothers). There were other policy changes in 2002 and 2003 that also potentially increased enrollment in childcare among children with employed parents or with parents who did not participate in the labor force. In 2002 there was a reduction in childcare fees, in 2003 children whose parents were on parental leave with a younger sibling were granted access for 15 hour per week of childcare, free of charge. Additionally, 4 and 5 year old children were offered free childcare for 525 hours per year.[17]

[15] Compulsory school formally starts at age seven, but most children enroll in a voluntary preschool year from age six organized by schools. Parents in Sweden are entitled to 16 months of paid parental leave.

[16] Municipalities were however obliged to provide a childcare slot for children who were judged to be in special need of childcare, regardless of parental employment status.

[17] In 2002, childcare fees were harmonized across municipalities and average fees were also reduced, implying that the share of childcare costs paid by parents was reduced from 16 to 10 percent. After the reform, parents payed three percent of household income for the first child up to a maximum to 145 euro per month. The fees for the second and third enrolled child were lower and the fourth child was free of charge.

Swedish childcare is of high quality. In their family database, OECD uses two main types of information to capture childcare quality: child-to-staff ratios and the minimum qualifications required for childcare staff. Sweden rates high in both dimensions.[18] Both before and after the reform, child groups were relatively small (around 17 children per group) and the child-to-staff ratio low (around 5.3–5.5 children per staff). About 50 percent of the childcare employees have a university degree from a preschool teacher-training program and 40 percent of childcare employees have an appropriate vocational high school degree specializing in the care of young children in day care.[19] Important for the results in the present study is that there is no indication that the quality of childcare changed, as a consequence of the studied reform; staff ratio and child groups remained stable compared to before the reform (Mörk et al. 2013). One reason why the staff ratios did not decrease is that central government introduced additional intergovernmental grants to compensate for cost increases caused by the reform.

3.2 Health care for children

When studying health outcomes of children based on hospitalizations and drug prescriptions, a potential concern is that factors such as family income or other characteristics affect access to care. We argue that this is a limited problem in our setting. There is universal health insurance coverage in Sweden. The Child Health Program provides vaccinations and preventive care with regular checkups from birth to school start after which the School Health Care Program takes over the responsibility. These programs are free of charge and have almost 100 percent enrollment.[20] Also dental care is free of charge until age 20. Patient fees, for both primary and hospital care are heavily subsidized. There is also a high-cost protection policy in place, implying that there is a low maximum amount that families have to pay during a year. During the period studied in this paper, the cap on health care expenses was SEK 900 during a twelve months period. In most counties, persons below 20 did not pay any patient fees.

Lundin et al. (2008) and Mörk et al. (2013) analyze the introduction of a maximum fee and find no effects on parental employment but some positive effects on fertility. van den Berg and Siflinger (2018) study the effect on child health by comparing the health of children in cohorts which paid higher fees with the health o children with cohorts which paid lower fees.

[18] https://www.oecd.org/els/soc/PF4-2-Quality-childcare-early-education-services.pdf

[19] The information about childcare quality is taken from The Swedish National Agency for Education's yearly reports "Beskrivande data om barnomsorg, skola och vuxenutbildning".

[20] See Socialstyrelsen (2014).

For hospital care, counties were not allowed to charge more than 80 SEK per day and night. For prescription drugs, individuals paid the full cost up to 900 SEK, after which costs were reduced gradually implying that nobody had to pay more than 1,800 SEK during a twelve months period.[21] The cap on health care expenditures also applies to children. Families add up the expenditure on patient fees and prescriptions respectively for all their children (0–18 year olds). The caps above applied to the total costs for all children (Ds2011:23). Thus, health care costs should not make up an obstacle for receiving care, not even for low-income families.

4 Empirical strategy

Our aim is to investigate how access to childcare affects the health of children with unemployed parents. We investigate both short-run effects, i.e. health outcomes in the same year that children have or do not have access to childcare and effects in the medium run, i.e. health outcomes measured when the children are 10–11 years. Because we do not have access to data on actual attendance, we estimate reduced form effects. Section 5.1 provides evidence using survey data that the reform implied a substantial increase in enrollment among children of unemployed parents. Below we present the identification strategy for the short-run and medium-run effects respectively.

4.1 Indentifying the short-run effects

When analyzing how access to childcare affects short-run health outcomes of children exposed to parental unemployment we estimate the following event type difference-in-differences (DD) specification:

$$Y_{imt} = \sum_{t=1998}^{2004} \mu_t TREATED_{imt} + \delta \mathbf{X_{it}} + \pi UE_{mt} + \tau_t + \varphi_m + \epsilon_{itm} \quad (1)$$

where Y_{imt} is the health outcome for child i exposed to parental unemployment in municipality m during year t, and $TREATED_{imt}$ is a dummy variable taking the value one for children who at time t live in municipalities that changed their policy due to the reform, and where τ_t and φ_m are year and municipal fixed effects, respectively. We also control for the municipal unemployment rate for individuals aged

[21] Costs were reduced with 50 percent for the amount between 900 and 1,700, with 75 percent for the amount between 1,700 and 3,300 and with 90 percent for amounts between 3,300 and 4,300, and with 100 percent for amounts above 4,300.

25–34 years (UE_{mt}) as well as child-specific characteristics including dummy variables for child age (in months at the end of the year) and sex, number of siblings age 0–10 years and birth order, maternal age at first birth as well as maternal and paternal level of education (compulsory schooling, upper-secondary schooling, higher education or unknown), and region of birth (non-Nordic), captured by the vector $\mathbf{X_{it}}$. The parameters of interest are the μ_t for, $t \in [1998, 2004]$.[22] We have chosen year 2000 (the year before the reform was introduced) as the reference year to which the other years are compared. If being at home with an unemployed parent or being cared for in center-based care matters for child health we would expect $\mu_t \neq 0$ for $t \in [2002, 2004]$, where a negative (positive) sign would indicate better (worse) health outcomes for children with unemployed parents when having access to childcare than when being home. Looking at the estimated coefficients for the years before the reform (i.e. $\mu_{1998,1999}$) we can observe whether the trends in health among children exposed to parental unemployment in treated and control municipalities were the same. If $\mu_t \neq 0$ for $t \in [1998, 2000]$, we would worry that the assumption of parallel trends is violated, and we would have reasons to doubt that the estimated effects capture causal effects.

4.2 Identifying the medium-run effects

To identify the medium-run effects, i.e. effects on health outcome when the children are 10–11 years old, we exploit the fact that children have been denied access to childcare during different number of years, depending, on the one hand, on exposure to parental unemployment, and on the other hand on the municipal policy for offering childcare to children with unemployed parents. More specifically, we estimate the following triple difference (DDD) specification:

$$\begin{aligned} Y_{imt} = \alpha NOACCESS_{icm} + \beta UNEMP_i + \delta UNEMP_i * cohort_c \\ + \gamma UNEMP_i * municipality_m + \rho \mathbf{X_i} + \theta_{cm} + \epsilon_{icm} \end{aligned} \quad (2)$$

[22] We limit the study to the years 1998–2004. One reason is that information on municipality policy regarding access to childcare for unemployed is based on responses to a survey conducted in 1998 and 2001. Estimating the model including years before 1998, would rest on the assumption that municipalities had the same childcare access policy for earlier years. Another potential problem with extending the sample period is that other policies may have affected the studied groups. Since the new regulations was introduced in the summer of 2001, and it is unclear to what extent the municipalities that previously had not offered childcare for children with unemployed parents were able to offer the slots already in the second half of 2001, it is unclear whether we can expect to see any effects in 2001.

where Y_{imc} is the health outcome at age 10–11 for child i in municipality m and birth cohort c, $UNEMP_i$ counts the number of years during which the child was exposed to parental unemployment between ages 2–5, and $NOACCESS_{icm}$ counts the years during which unemployment exposure coincided with not having access to childcare (because of having an unemployed parent and living in a treatment municipality before the reform), which differ between cohorts. Since the reform was implemented in July of 2001, that year is counted as a half year. θ_{cm} are municipality-specific cohort-effects and $\mathbf{X_i}$ is a vector of child specific control variables (the same as for the short-run analysis, but measured when the child is two). The parameter of interest is α that shows how exposure to parental unemployment without access to childcare affects health 5–6 years after the child was in childcare age (at age 10–11).

4.3 Threats to identification

The identifying assumption that must hold for the difference-in-differences specification to estimate causal effects is that health of unemployed children in the treated municipalities would have been similar to that in the control municipalities if they had had access to childcare already before the reform. This assumption will not hold if there are other changes in society that affected hospitalization rates in treated and non-treated municipalities differently. Although this cannot be formally tested, we can study whether the assumption is plausible. We do this by studying the development of health of children exposed to parental unemployment in treatment and control municipalities before the reform.

In addition, it might also be the case that the reform affects both selection into unemployment and unemployment duration.[23] In order to investigate whether selection on the extensive margin is important, we investigate how sensitive the estimated effects are to the inclusion of a number of parental controls, such as parental education, country of origin and maternal age at first birth. If the point estimates remain relatively unchanged when controlling for these parental controls, we will conclude that selection into unemployment is not a serious issue. We will deal with the potential selection on the intensive margin by not conditioning on the length of parental unemployment, but only consider being exposed to any parental unemployment during a year (see Section 5.2 for details and for a longer discussion).

[23] In a study of the same reform as the one studied in this paper, Vikman (2010) finds a 17 percent increase in the likelihood that mothers find employment when childcare is available. She finds no similar effect for unemployed fathers.

The identifying assumption that must hold for the DDD-specification, to estimate causal effects, is that there are no differences in how health develops for children by parental employment status across cohorts in treated and untreated municipalities that is unrelated to childcare enrollment. Ideally, we would like to test this identifying assumption using a placebo-specification, but due to the limited time-period for which we observe prescription data, this is not possible.

As discussed earlier, the analyzed reform was followed by other childcare reforms that may also have increased enrolment in childcare. Most importantly, childcare became cheaper, both through the implementation of a maximum fee in 2002, and through the introduction of 525 hours of free-of charge preschool per year for all children aged 4–5. For the short-run analysis, these additional reforms would be problematic only if they increased enrollments among children with unemployed parents to a larger extent in the control municipalities than in the treated municipalities (for example if childcare was more expensive in these municipalities before the reform). In this case, there would be no detectable first stage effect on the enrollment rates of the studied group in the treated municipalities and we would likely underestimate the effect of having access to childcare with the strategy outlined in Section 4.1. The same is true for the identification of the medium-run effects, except that we here also need the reforms not to increase enrollment among children with employed parents more than among children with unemployed parents. Below, we use survey data on childcare enrollment in order to investigate how the reform package affected childcare enrollment in the different groups and whether there is a "first stage". If such a first-stage exists, we are assured that the studied reform had an effect on enrollment rates over and above possible enrollment effect of other reforms that were implemented during the same period.

5 Data and measurement issues

In this section, we first discuss how we identify treated and control municipalities, thereafter we present the individual level data and discuss how we measure health outcomes and parental unemployment. Finally, we present some descriptive statistics.

5.1 Treatment and control municipalities

Prior to the reform in 2001, municipalities could choose to provide childcare access for children whose parents were unemployed. After the reform, all municipalities were required to offer at least 15 hours of childcare to this group of children. In this paper, we exploit the resulting

change in municipal policies to estimate causal effects of the availability of childcare for children with unemployed parents. More specifically, we will compare municipalities that changed policy because of the reform to municipalities that already before the reform gave children with at least one unemployed parent access to childcare.

Information about which policies that were in place in different municipalities before the reform is taken from surveys conducted by the Swedish National Agency for Education in 1998 and 2001. In the surveys, municipalities were asked about their childcare fees and childcare policies in general. They were asked what happens if i) a child already had a slot and a parent became unemployed and ii) a child did not have a slot and at least one parent was unemployed. We consider a municipality as treated if children with unemployed parents did not get a slot or they lost their slot if a parent became unemployed according to both of the survey rounds. Applying these criteria, we identify 75 treatment municipalities.

Based on the responses to the survey, only seven municipalities can be defined as untreated municipalities, in the sense that they did not have restrictions on the access to childcare for children with unemployed parents. However, these municipalities are very different from the reform municipalities, with lower unemployment rates and different trends in hospitalization rates before the reform. For the remaining 207 municipalities, the policies with respect to offering childcare to children with unemployed parents are not quite clear, making it difficult to identify suitable control municipalities based on survey responses. An alternative strategy to identify a suitable set of control municipalities is to select them based on actual enrollment rates of children of unemployed and employed parents prior to reform. For this purpose, we use another survey conducted by the National School board in 1998 and 2002, the Parent survey, which asked parents about childcare enrollment.[24] Our aim is to identify 75 suitable control municipalities where children to unemployed parents had access to childcare already before the reform. We therefore select municipalities where i) the enrollment rates of children in prior to the reform were similar regardless of parental unemployment status and ii) the differences in enrollment between children with employed and unemployed parents did not change as the reform was introduced.[25] Using this procedure we identify 75 municipalities that are not likely to have been affected by the reform.

Figure A2 in Appendix shows that treated and control municipalities are scattered across Sweden. Table A 1 shows descriptive statistics for

[24]The survey was stratified to make the responses representative at the municipal, as well as at the national, level.

[25]The selection of control municipalities is further described in Appendix Figure A1

the treated and control municipalities. The treatment municipalities are in general less populated, have fewer children in childcare age, are to a larger extent run by a left-wing majority and have somewhat higher unemployment rates. As expected, childcare enrollments rates are lower, and so is municipal spending on childcare. In the empirical analysis, we control for these differences through the inclusion of municipality fixed-effects and by controlling for the municipal employment rate. Importantly, the child-to-staff ratio, which is a proxy for childcare quality, is similar in both treatment and control municipalities, and does not change in connection with the reform, see Figure A3 in the Appendix.

We use the Parent surveys to investigate how the enrollment rates for children with unemployed parents changed due to the reform, see Table 1 for the results.[26] Columns 1–2, provide the difference-in-differences estimates of the change in enrollment for children with unemployed parents, comparing treatment to control municipalities before and after the reform (column 1 without controls and column 2 including controls for parental education and child age and sex). The estimated effect is very stable and suggests a 20–21 percentage points increase in enrolment as a result of the reform. Because there were other reforms during the same period that may have affected enrollment of other groups, we also provide DDD estimates where we include children of employed parents as a further control groups. The results are presented in columns 3–4. The estimated effect is now slightly smaller, 19.2–19.8 percentage points, but still very similar to the DD estimates.[27] Comparing this 19–20 percentage points increase in enrollment to the pre-reform enrollment rate of 57 percent for children with unemployed parents implies an increase by 34–37 percent increase in enrollment.[28]

[26] Information on enrollment and average hours in childcare in control and reform municipalities for children of employed and employed parents is displayed in Table A2.

[27] We have also estimated the first stage for different educational background. The increase in enrollment is similar across maternal education groups.

[28] Pre-reform enrollment is not zero for children with unemployed parents. Before the reform, these children could be granted access to childcare if the family was considered in extra need by social services in the municipality. Another reason could be that the parent had only been unemployed for a short time and the child had not yet lost the childcare slot.

Table 1. *First stage: the effect of the reform on enrollment in childcare among children of unemployed parents.*

	(1) DD	(2) DD	(3) DDD	(4) DDD
Reform-effect	0.203***	0.211***	0.192***	0.194***
	(0.036)	(0.032)	(0.035)	(0.033)
Observations	5306	5306	45533	45533
R-squared	0.078	0.144	0.073	0.093
Time and municipal FE	Yes	Yes	Yes	Yes
Controls	No	Yes	No	Yes
N	308,623	308,623	308,623	308,623
Mean outcome	29.77	5.16	15.81	3.24

* p<0.10, ** p<0.05, *** p<0.01

Notes: Robust standard errors in parenthesis. In columns (1) and (2) DD = unemployed in treatment and control municipalities. In columns (3) and (4) DDD = unemployed and employed in treatment and control municipalities. Controls include: parental education indicators (compulsory, upper secondary or higher), child age dummies and child sex.

5.2 Individual level data

We base the analysis on population wide Swedish register data from Statistics Sweden, the National Board of Health and Welfare, and the Public Employment Service. Population registers allow us to link parents to children and contain information on sex of the child, child age in months, number and age of siblings and parental age. Matched to these data are taxation and education registers with information on parental earnings, and education, as well as information about residential municipality. Information about parental unemployment is available in the data from the Public Employment Service. Health outcomes are taken from The National Patient Register (Hospital discharge register (NPR)) and from the Prescription Drug Register (PDR), both from the National Board of Health and Welfare. NPR contains information on all patients who are discharged from in-patient care in Swedish hospitals and include detailed diagnoses, whereas PDR, which exists since 2005, contains records of all over-the-counter sales of prescription drugs, with information on the patient and on the active substance. During the years around the childcare reform, high quality data on out-patient care did not exist for the whole of Sweden, but only for a few counties, hence our focus on in-patient care.

We sample all children born 1993–2002 and their parents. When analyzing short-run effects we keep, for each year 1998–2004, the children who are 2–5 years old at the end of the year and who were exposed

to parental unemployment at some point during that year. We define child exposure to parental unemployment as having at least one parent who is registered as unemployed at the Public Employment Service at least one day during a specific year. A reason for including children with very little exposure in the group is that previous research has shown that the length of unemployment was affected by the reform (Vikman 2010) and that unemployment duration is hence endogenous to the reform. However, as is clear from Figure A4 in the Appendix, the majority of children experience considerably longer parental unemployment spells, and as much as 18 percent of the children with unemployed parents experience parental unemployment during the whole year. The length of spells changes over time, but the pattern of change is similar in the treatment and control municipalities.[29]

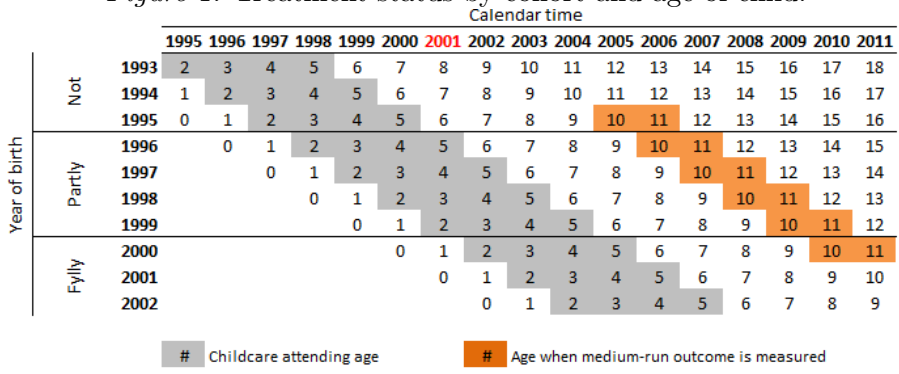
When analyzing medium-run effects, our sample includes children exposed to parental unemployment and children whose parents are employed, where we define a child as having employed parents if neither parent is register as unemployed during the year and both parents have annual earnings that exceed a threshold defined as two times the *price base amount*.[30] Due to data limitations, we restrict this sample to children born 1995–2000.[31] Since the new policy was introduced in July 2001 access to childcare differs across cohorts. Figure 1 shows in what ages a child with unemployed parents did not have access to childcare depending on the birth year of the child. For example a child born in 1995 with unemployed parents did not have access to childcare at any age, whereas a child born 1997 had access to childcare at age 5 if the parent was unemployed. Cohorts born between 1996 and 1999 are partially treated and cohorts born in 2000 had access to childcare their whole childhood.

[29] The way we define exposure to unemployment implies that we may have measurement error in our treatment variable (having access to/not having access to childcare) and the estimates may therefore suffer from attenuation bias.

[30] Price base amount (*prisbasbelopp*). The amount is based on the consumer price index and adjusted annually by the government. Between years 1998 and 2004 the amount has been 36400–39300 SEK in nominal value (roughly 4000 Euro).

[31] Data on prescriptions is only available 2005-2011.

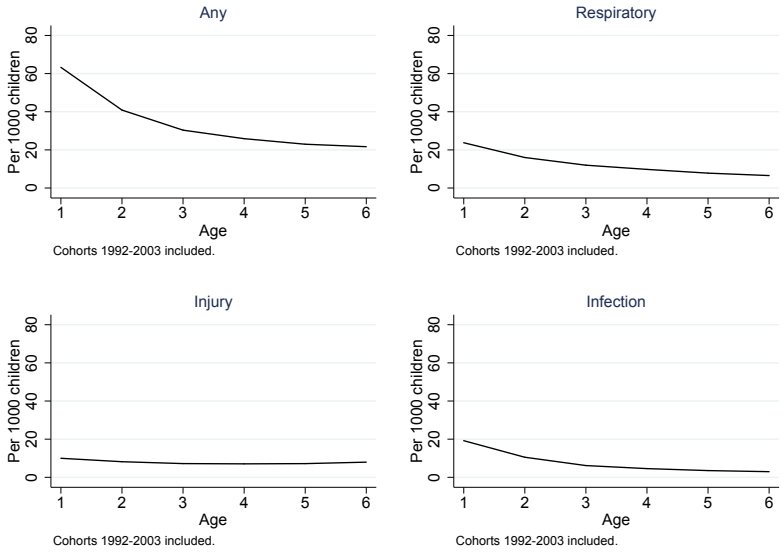
Figure 1. Treatment status by cohort and age of child.



In the short-run analysis, we measure child health using the in-patient register. We consider a child as hospitalized during a year if (s)he is observed in the in-patient care register at least once in a year. In addition to investigating effects on hospitalization for any diagnosis, we also investigate the effect of access to childcare on diagnoses related to injuries, respiratory diseases and infections.[32] These diagnoses groups make up some of the most common reasons for why children are hospitalized. They also cover conditions that, as argued in Section 2, could be affected if a child attends childcare rather than stays at home with parents. Figure 2 shows hospitalization rates by age for the different diagnoses. Hospitalizations clearly vary by age of children. The risk of being hospitalized is highest among the youngest children and decreases as children get younger, especially during the first three years. Since the health of 2–3 year old children is different from the health of 4–5 year old children and since, as discussed in the introduction, earlier studies have found that enrollment age may matter for the effects of childcare attendance on health as well as on cognitive and non-cognitive development, we study the two age groups separately.

[32]We have also considered effects on total number of hospitalizations, but this turns out to be a very noisy measure. Hence, these results are not presented in the paper. The ICD10 codes of the diagnosis considered in this study are listed in Table A3 in Appendix. Hospitalization for a specific condition is based on the diagnosis codes for the main diagnosis and the first 5 auxiliary diagnoses in the register. Both hospitalization and drug prescriptions are measured as in 1,000 children.

Figure 2. Hospitalization per 1,000 children across different diagnosis groups by age.



The benefits of using hospitalization records when measuring health are that hospitalization can be regarded as a relatively objective measure (as opposed to self-reported health measures). Moreover, hospitalizations capture rather serious health events, which are likely to have long-run effects. However, although in some sense objective, hospitalization is still depended on a professional judgement by a physician, based on the child's health status, and on the fact that the child has been taken to the hospital, i.e. care-seeking behavior of the child's parents. The seriousness of health conditions that require hospital care is however such that one should expect children who need care to eventually get to the hospital. Also, neglecting to seek primary care, may result in a need for hospital care. As described, earlier hospital care for children is not expensive in Sweden. Thus, the cost should not be an obstacle for seeking care, even for poor families.

In the medium-run analysis, we measure child health when children are 10–11 years of age, using data from both the in-patient and the drug prescription registers. From the in-patient register, we create a dummy variable indicating whether the child was hospitalized any time during the considered age-span. From the drug prescription register, we first construct indicators for if the child has been prescribed any 1) anti-infectives and 2) medication for respiratory conditions in the calendar years the child is 10 and 11 years old. These medication groups match the hospitalization diagnoses studied, but capture also less severe conditions. Second, in order to capture effects on the child psychiatric

health and behavioral problems, we create indicator for being prescribed ADHD-medication or psycholeptics, i.e. medications for sleeping problems and anxiety. The ICD10 codes of the diagnoses and the ATC codes of the drugs considered in this study are listed in Table A3 in Appendix. Hospitalizations of young children for psychiatric conditions are very rare, which is why drug prescriptions are of special interest.

Using prescriptions of drugs, we pick up less severe health problems and these data are hence a valuable complement to the hospitalization data. Drug prescriptions require a diagnosis by a health professional and are thus an indication of an objective evaluation of the child's health condition. However, care-seeking behavior is likely to play an important role in the likelihood of getting a prescription. Moreover, only actual purchases are registered which may introduce a further social element if economic conditions influence the parents' likelihood of collecting the medication. Since, as described in Section 3.2, a high-cost protection in place for prescription drugs, also low-income household should be able to afford to collect prescribed medications.

For natural reasons, register data does not contain any information about the quality of parental care. Given earlier evidence of larger positive effects of childcare on cognitive outcomes for children of lower socio-economic status (see, e.g. Liu and Skans 2010; Felfe et al. 2015; Havnes and Mogstad 2011), we use maternal education as a proxy for parental care when looking for heterogeneous effects.

5.3 Descriptive statistics

Table 2 shows descriptive statistics for children with at least one unemployed parent for the pre-reform period 1998–2000 and the post-reform period 2002–2004 by treatment status of the municipality. Mothers in the treated municipalities are somewhat younger at first birth, and both parents are less likely to be born outside the Nordic countries and less likely to have higher education. These differences in background characteristics motivate the inclusion of control variables in the estimations. Children in the treatment municipalities are more likely to be hospitalized, and over time, hospitalization rates decline in both treatment and control municipalities.

Table 3 shows descriptive statistics for children aged 10–11, for the first and last cohorts in our sample. Whereas the first cohort (born in 1995) only had access to childcare in case of parental unemployment if they lived in a control municipality, the last cohort (born in 2000) had access to childcare regardless of where they lived. It turns out that children in the treatment municipalities are somewhat more exposed to parental unemployment than those in the control municipalities, which

is as expected given the differences in parental education level that was observed in Table 2. Furthermore, children in treatment municipalities have somewhat worse health outcomes than those in the control municipalities, as is evident for both hospitalization and drug prescriptions. What is most striking from the table is the sharp increase in prescriptions of ADHD medication and psycholeptics (*mental*) between the two cohorts. However, this pattern exists both in the treatment and in the control municipalities.

Table 3. *Descriptive statistics for 10–11 year old children.*

	Cohort 1995		Cohort 2000	
	Control	Treatment	Control	Treatment
Years of UE exposure	1.35	1.58	1.47	1.71
	(1.31)	(1.33)	(1.59)	(1.65)
Share with some UE exposure	0.58	0.64	0.55	0.60
	(0.49)	(0.48)	(0.50)	(0.49)
Hospitalizations per 1000 children				
Any hospitalization	30.84	32.93	27.05	33.76
	(172.89)	(178.46)	(162.22)	(180.62)
Respiratory	5.73	5.24	4.45	5.25
	(75.46)	(72.21)	(66.54)	(72.30)
Injury	15.74	16.58	14.96	17.08
	(124.48)	(127.69)	(121.41)	(129.57)
Infection	3.57	4.79	3.38	4.14
	(59.64)	(69.02)	(58.08)	(64.20)
Mental	1.10	1.20	1.51	1.64
	(33.21)	(34.57)	(38.84)	(40.49)
Hospital visits	39.65	43.64	39.52	45.65
	(341.26)	(320.65)	(427.29)	(309.82)
Prescriptions per 1000 children				
Mental prescription	6.68	8.38	17.85	21.02
	(81.44)	(91.13)	(132.39)	(143.45)
Antibiotics prescription	96.84	101.13	106.12	122.11
	(295.75)	(301.51)	(308.00)	(327.42)
Respiratory prescription	88.85	99.08	116.22	130.58
	(284.54)	(298.77)	(320.49)	(336.96)
N	38,940	17,552	3,5752	15,224

6 Results

We first present how hospitalization rates related to various diagnoses among 2–5 year old children of unemployed parents are affected by having access to childcare. Then, we turn to the analysis of the medium-run

effects (at ages 10–11) on hospitalization and drug prescriptions, including behavioral disorders, of not having access to childcare at ages 2–5.[33]

6.1 Effects on child health in the short run

In order to get at short-run effects on child health of having access to childcare when parents are unemployed we estimate equation 1. As motivated in Section 5.2, we estimate the model separately for children aged 2–3 and 4–5 years old.

The descriptive statistics in Table 2 showed that parents differed somewhat in characteristics in the treatment and control municipalities. We are therefore interested in whether the results are sensitive with respect to the inclusion of control variables. This is especially important since selection into unemployment could differ depending on whether children with unemployed parents have access to childcare or not. Hence, we estimate the model using three different sets of controls. First, we only control for a number of child-specific characteristics (sex, age in months and birth order). Second, we add parental controls for maternal and paternal education, age of mother at first birth, whether the mother or father is of non-Nordic origin and number of children of the mother. Third, we add the municipal unemployment rate (among 25–34 year olds). Results, available in Table A4 in the Appendix, show that the point-estimates of our main interest (the estimates of μ_t in Equation 1) are very similar across specifications. We take this as evidence of similar sorting into unemployment in treatment and control municipalities before and after the reform. In order to increase precision, we focus on the full specification in the rest of the paper.

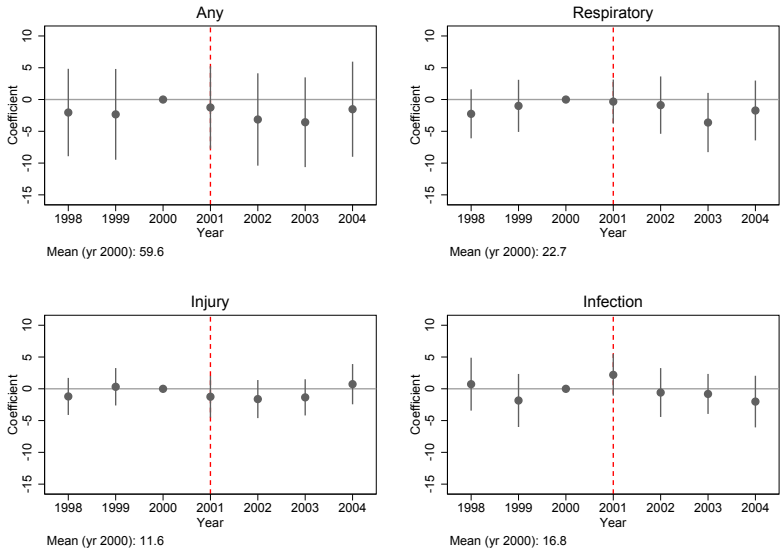
Figure 3 shows the differences in hospitalization rates between children of unemployed parents in control and treatment municipalities for children aged 2–3 years, compared to differences in hospitalization rates in the year before the reform (year 2000). Looking at the coefficients for the years 1998 and 1999, we can assess whether there are indications of different trends in hospitalization rates for children in treated and control municipalities already before the analyzed reform of 2001, in which case we would be worried that the identifying assumption of parallel trends is violated. The coefficients for the years 2002, 2003 and 2004 measure the effect of having access to childcare for children with unemployed parents. Since the reform was introduced in the middle of 2001, and it is uncertain how quickly the municipalities implemented

[33] We have also conducted all estimations excluding Stockholm from the control municipalities, given that Stockholm is much bigger than the other control municipalities and might behave differently, since it is the capital of Sweden. The results excluding Stockholm are very similar to those presented in the paper, including those for the first stage, and are available upon request.

the reform, we are not certain to what extent children with unemployed parents actually had access to childcare in the treated municipalities this year. We present the coefficient for that year for completeness.

Looking at the pre-reform coefficients, the point estimates are both statistically and economically insignificant which ensures us that, before the reform, hospitalization rates developed similarly in control and treatment municipalities. This is however also true for the post-reform coefficients, indicating that having access to childcare or not did not affect hospitalization. For any hospitalization and for hospitalization due to respiratory diagnoses, there are some indication of a decrease in hospitalization with around 3–4 less children per 1,000 in one to two years following the reform, but these effects are not statistically significant and, at least for any hospitalization, small in size.

Figure 3. Difference-in-differences specifications for 2–3 year old children. Outcome: annual hospitalizations per 1,000 children.



Notes: All regressions controls for child sex, child age (months in end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

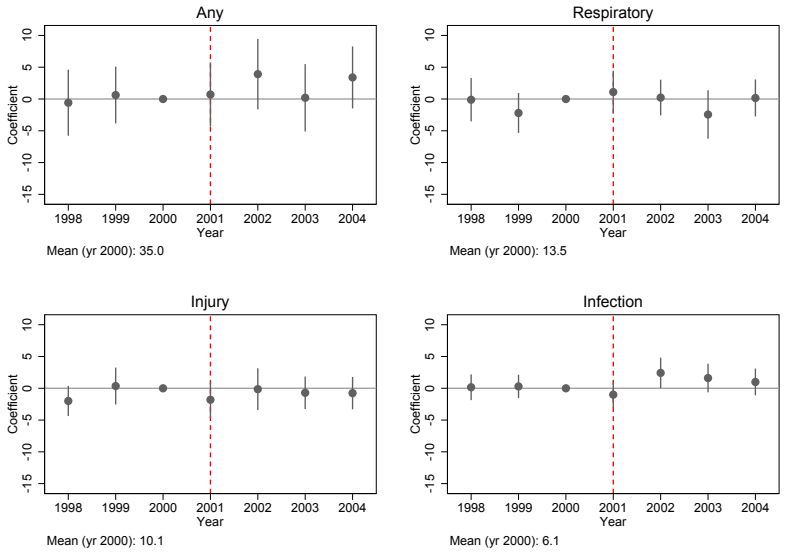
Next, we turn to the preschool aged children (aged 4–5). The corresponding results are presented in Figure 4. Also for this age group, there are no indications of differential health trends before the reform was implemented in 2001. Once the reform is introduced however, there

is a statistically significant, positive effect for the year 2002 on hospitalizations for infections, which indicates that children with unemployed parents were more likely to be hospitalized due to infections once they had access to childcare. The increase in hospitalization when having access to childcare is 2.42 more children per 1,000 children hospitalized annually, which corresponds to an increase of more than 40 percent.[34] The effect lasts only for the first year the children had access; the estimates for the years 2003 and 2004 are not statistically different from zero.[35]

[34] In Section 5.1 we studied the change in enrollment using survey information on enrollment reported by parents at a particular point in time and found that enrollment among children with unemployed parents increased with about 20 percentage points. Is it appropriate to use this estimate to calculate an IV estimate? Since our analysis regard as treated children those whose parents have been unemployed at least one day during the year, it is not straightforward to infer the reform induced increase in childcare attendance from the estimations using survey data, where unemployment is measured at the time of the survey. Yet, if we do, the first stage estimate from the survey implies that we ought to multiply our estimates with 5.

[35] Estimating the model, excluding Stockholm, the effect persists also in 2003, at least at the 10-percent level.

Figure 4. Difference-in-differences specifications for 4–5 year old children. Outcome: annual hospitalizations per 1,000 children.



Notes: All regressions controls for child sex, child age (months in end of the year), birth order, parental education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

6.2 Heterogeneous effects

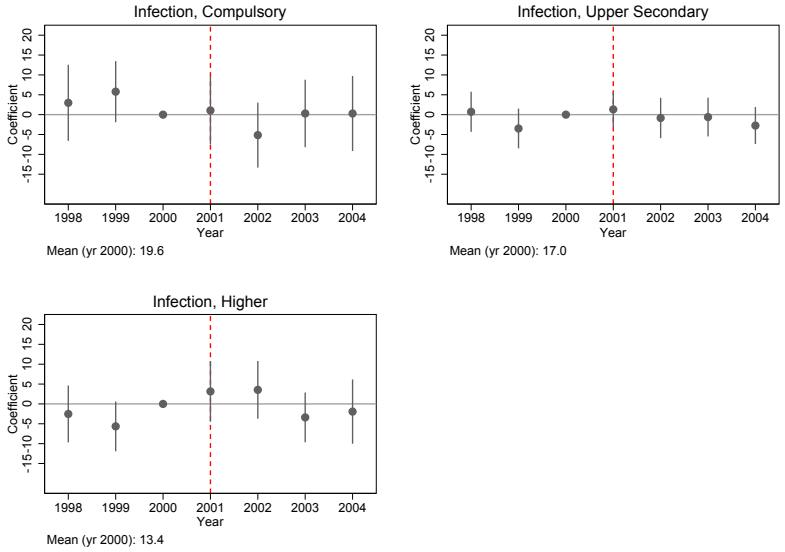
To gain understanding about the effect on infections we study whether certain groups of children are more affected by access to childcare than others. As earlier literature shows that family background can be important for the impact of attending childcare we study heterogeneous effects by mother's education level.[36] For completeness, we do this for both age groups.

Figure 5 shows the resulting estimates for children aged 2–3 and Figure 6 shows the same for children aged 4–5 (see Table A5 and Table A6 in the Appendix for point estimates and standard errors). From the estimates in Figure 5 it is clear that the found zero-effect remains; there are no statistically significant effects for any maternal education level for the younger children. Turning to the older children (Figure 6), results clearly show that the found effect for the whole group is driven by

[36] We have also analyzed whether the effects differ depending on child sex or by maternal or paternal unemployment. Our results, available upon request, do not show any such patterns, although the effects for paternal unemployment are somewhat noisier.

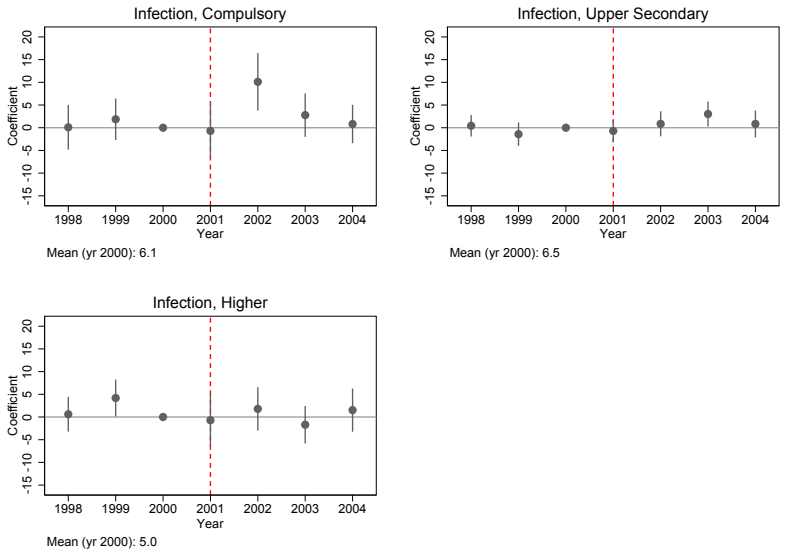
children of mothers with only compulsory education. For these children, there is a large increase in the risk of hospitalization due to infections in 2002, the first year when children with unemployed parents had access to childcare; 10 more children per 1,000. There is also a statistically significant effect on hospitalization for infections in 2003 for children whose mothers have upper secondary schooling and among children of highly educated mothers there is statistically significant effect before the reform, suggesting pre-reform trends for this group.

Figure 5. Infection related hospitalization (per 1,000 children) by education level of mother for 2–3 year old children.



Notes: All regressions controls for child sex, child age (months in end of the year), birth order, father's education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

Figure 6. Infection related hospitalization (per 1,000 children) by education level of mother for 4–5 year old children.



Notes: All regressions controls for child sex, child age (months in end of the year), birth order, father's education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

There are at least two potential explanations for why children with low educated mothers have more infections. First, it could be that these children in general are more vulnerable to health shocks. The same exposure to germs, viruses and bacteria may have worse consequences for children with parents with low education because the children are less resilient and/or because they do not receive appropriate preventive and primary care. Second, it could be that these children were less likely to attend childcare when younger and therefore are more vulnerable once they enroll. Remember that during the pre-period, when these children were younger age, childcare was only available for children whose parents were working or being full-time students.

To investigate to what extent children, whose parents were unemployed when they were 4–5 year olds, were likely to attend to childcare when they were younger, we look at the share of mothers who either were unemployed or received student benefits when their children were 2–3 year olds. Table A7 in the Appendix shows these shares by maternal education. There are no indications that mothers with only compulsory education were previously unemployed to a larger extent than mothers

with upper secondary education. Hence, it is fair to say that among 4–5 year olds whose parents are unemployed, those with compulsory educated mothers are as likely to have been exposed to childcare at younger ages as those with more educated mothers. A more probable explanation for their higher risk for hospitalization is hence that children with compulsory educated mothers are more likely to need hospital care either because they are more vulnerable at the outset or because they do not receive as appropriate preventive care as other children.

6.3 Effects on child health in the medium run

Next, we turn to medium-run effects and estimate equation 2, where we measure health outcomes when children are aged 10–11. The results are presented in Table 4.[37] The parameter estimate shows how many more children per 1,000 who are hospitalized/prescribed a drug at ages 10–11 if they experienced any parental unemployment during a year when they were aged 2–5, when there was no access to childcare as opposed to experiencing unemployment when there was access to childcare.

Starting with the results for hospitalization, there is no indication that childcare access when parents are unemployed has any effect on hospitalization at ages 10–11, the estimates are both statistically and economically insignificant. Turning to results for prescriptions of ADHD-medication or psycholeptics (*mental*), our point estimate is negative, suggesting a reduction of about 10 percent when compared to the population mean, implying that *not* having access to childcare is beneficial. However, the standard errors are of the same magnitude as the estimate, implying that a confidence interval of 95 percent spans effects of [-3.39, 0.97], corresponding to a change of -24 percent to 7 percent. Also for antibiotics, we do not find evidence of any statistically significant effect and the estimate is small relative to the population mean. However, for respiratory conditions, not having access to childcare, increases the risk of needing prescriptions for respiratory problems at ages 10–11; a year of no access increases the likelihood of drug prescription by 6 percent, compared to the population mean. This effect is in line with the hygiene hypothesis, or the substitution effect suggested in van den Berg and Siflinger (2018), which states that little exposure to infections and respiratory illness in early childhood, may shift problems to a higher age.

[37] We have also investigated whether the effects differ with respect to maternal education. The point estimates are similar for all education levels but the standard errors are large, so it is difficult to draw any firm conclusions. Therefore we do not present these results in the paper, but they are available upon request.

Table 2. *Descriptive statistics for 2–5 year old children who experience parental unemployment in pre-reform years 1998–2000 and post-reform years 2002–2004 by treatment status of the municipality.*

	Pre (1998–2000)		Post (2002–2004)	
	Control	Treatment	Control	Treatment
Mother				
Age at first birth	25.40	24.60	26.14	25.12
	(5.56)	(5.12)	(6.12)	(5.70)
Compulsory education	0.21	0.20	0.19	0.17
	(0.41)	(0.40)	(0.39)	(0.38)
Upper secondary education	0.54	0.62	0.51	0.61
	(0.50)	(0.48)	(0.50)	(0.49)
Higher education	0.23	0.17	0.29	0.21
	(0.42)	(0.38)	(0.45)	(0.40)
Unknown education	0.02	0.01	0.02	0.01
	(0.15)	(0.10)	(0.14)	(0.11)
Non-Nordic born	0.27	0.13	0.30	0.16
	(0.44)	(0.33)	(0.46)	(0.37)
Father				
Compulsory education	0.21	0.20	0.18	0.17
	(0.41)	(0.40)	(0.38)	(0.38)
Upper secondary education	0.52	0.61	0.52	0.63
	(0.50)	(0.49)	(0.50)	(0.48)
Higher education	0.24	0.16	0.27	0.18
	(0.42)	(0.37)	(0.45)	(0.38)
Unknown	0.03	0.02	0.03	0.02
	(0.17)	(0.13)	(0.16)	(0.14)
Non-Nordic born	0.29	0.14	0.33	0.17
	(0.46)	(0.34)	(0.47)	(0.38)
Hospitalizations per 1000 children annually				
Any hospitalization	47.74	49.76	42.31	45.38
	(213.21)	(217.45)	(201.29)	(208.13)
Respiratory	18.38	19.35	16.24	16.98
	(134.31)	(137.77)	(126.39)	(129.20)
Injury	10.27	11.51	9.88	11.01
	(100.81)	(106.64)	(98.90)	(104.35)
Infection	11.78	11.70	10.08	10.49
	(107.91)	(107.52)	(99.90)	(101.87)
Observations	208,417	109,950	158,017	78,387

Table 4. *Hospitalizations and prescriptions at age 10–11.*

	Hospitalizations					Prescriptions		
	(1) Any	(2) Respiratory	(3) Injury	(4) Infection	(5) Psychiatric	(6) Mental	(7) Antibiotics	(8) Respiratory
No access, years	-0.233	0.060	0.220	0.123	-0.203	-1.210	0.787	7.178***
	(1.174)	(0.489)	(0.782)	(0.465)	(0.247)	(1.111)	(2.179)	(2.563)
UE, years	-0.447	-0.733***	2.435***	-0.338*	-0.354***	4.727***	83.898***	63.221***
	(0.593)	(0.261)	(0.394)	(0.185)	(0.113)	(0.426)	(1.258)	(1.202)
N	308,623	308,623	308,623	308,623	308,623	308,623	308,623	308,623
Mean outcome	29.77	5.16	15.81	3.24	1.29	14.23	133.07	125.74

* p<0.10, ** p<0.05, *** p<0.01

Notes: Control variables at age 2: Sex of child, age of child in months at the end of the year, sibling order number, education level of parents, mother's age at first birth, non-Nordic parents, number of children 0–10 years in the family, municipal UE rate (among 25–34 years old), municipality, year of birth and interaction of municipality and year of birth and interaction of years of unemployment and year of birth as well as year of birth and municipality. Standard errors are clustered at municipality level.

7 Concluding comments

In this paper, we have evaluated whether access to childcare for children with unemployed parents affects child health by investigating short run effects on hospitalization and medium run effects on hospitalizations and medical drug prescriptions. Using a nationwide reform, which obliged all municipalities to provide at least 15 hours of childcare for children with unemployed parents, we exploit the exogenous change in childcare provision for this disadvantage group. The reform potentially changed the mode of care from care at home by an unemployed parent to at least 15 hours per week in a high quality center based childcare. The expected effects of hospitalization are ambiguous. At centered based childcare the child is attended by professional staff, trained to detect early health problems and reduce the risk of accidents and poisoning. However, the child is part of a larger group which increases exposure for infections and the child may receive less attention and adult and parental time.

We find that among preschool children (4–5 year old) access to childcare led to an increase in hospitalization for infection. However, within one year after the reform the hospitalization rate due to infections among children with unemployed parents falls back again. A potential explanation for this pattern is that when children in age 4–5, who have not previously had access to childcare, are more sensitive to infections when they first start to attend childcare. Once they have attended childcare for some time they have built up resistance and the hospitalization rate falls back to its original level. The effect on hospitalization due to infections is entirely driven by children with low-educated mother. This may suggest that more vulnerable children are more likely to be hospitalized when exposed to infections because they may not get the same preventive and primary care. Among toddlers (children age 2–3) there is no evidence that the mode of care matters for hospitalization in the short run. In the longer run, at age 10–11, we find no effects of childcare access on hospitalizations, but a possible increase in the prescriptions of drugs related to respiratory conditions for children who did not have access to childcare when the parents were unemployed. This result lends supports to the hygiene-hypothesis or the presence of a substitution effects shifting the risk of contraction illness in time.

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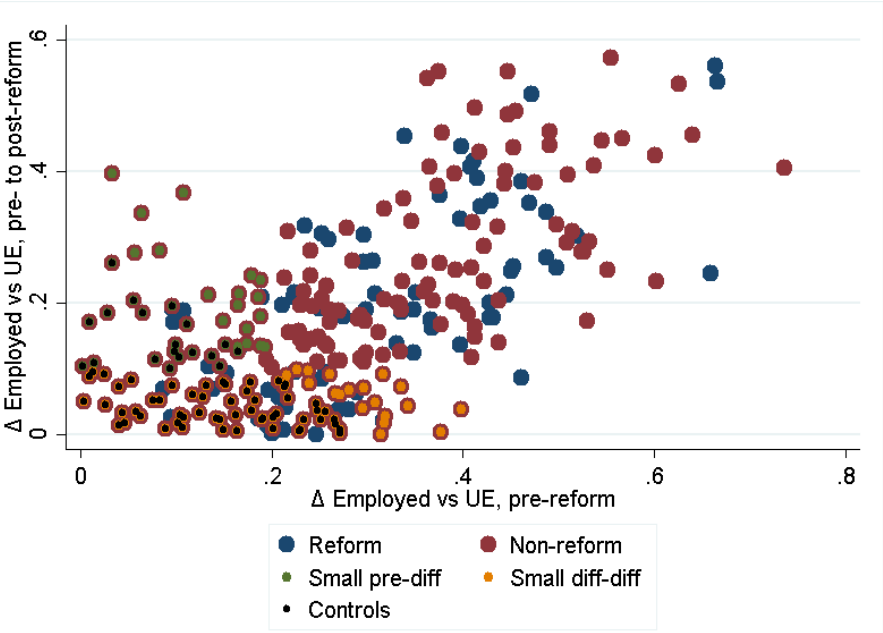
Appendix

A Selection of control municipalities

In order to estimate effects of the reform we need to identify a set of control municipalities in which the reform did not significantly change the access to childcare for children with unemployed parents. To this end we study enrollment rates before and after the reform. Information on enrollment among children with unemployed and employed parents is collected from the Parent survey conducted by the National School board in 1998 and 2002. The survey is answered by parents and pertains to the status of them and their child at a specific point in time. This is not an exact measure of the enrollment rates in different groups since, for example, the status of the parent may change over the year and there is no information on how long the parent have been unemployed. We define *pre-diff* as the difference in enrollment rate between children of employed parents and unemployed parents in 1998 in a given municipality, and the *diff-diff* as the change in the enrollment difference between children of employed and unemployed parents between the two surveys.

Figure A1 plots the municipal *diff-diff* against the *pre-diff*. Blue dots represent the 75 reform municipalities that, according to the municipal survey, had restrictions for unemployed parents prior to the reform. The red dots are all other municipalities. A small green dot has been placed in the middle of the red dot for the 75 non-reform municipalities with the smallest pre-reform enrollment difference (*pre-diff*). A slightly smaller yellow dot is marking the 75 municipalities with the smallest change in enrollment difference (*diff-diff*). We choose as our control municipalities the 75 municipalities that have the smallest sum of *pre-diff* and *diff-diff*. These are marked by a tiny black dot.

Figure A1. Difference in differences from 1998 to 2002 and the difference in childcare enrollment in 1998 by parental employment status in Swedish municipalities.



B Figures

Figure A2. Map over the treated and control municipalities.

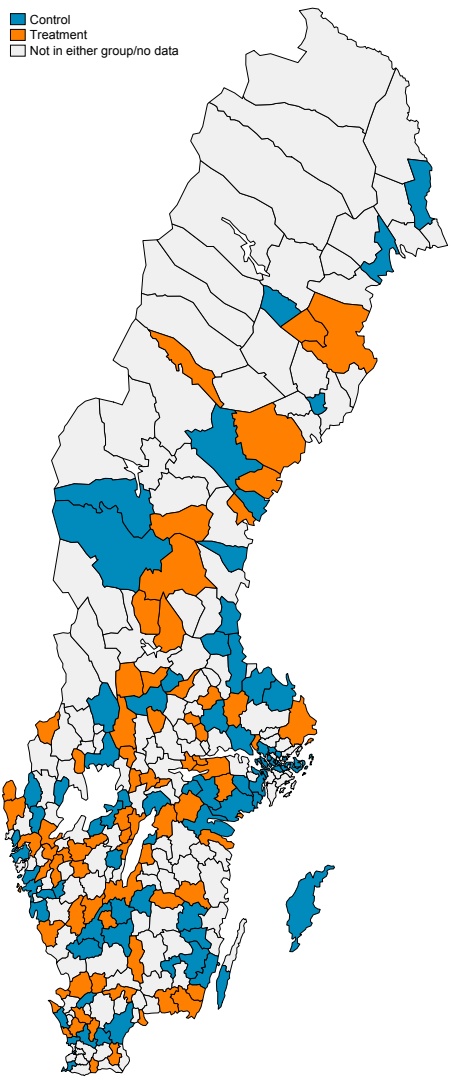


Figure A3. Child-staff ratio in municipal childcare in treatment and control municipalities in 1998–2005.

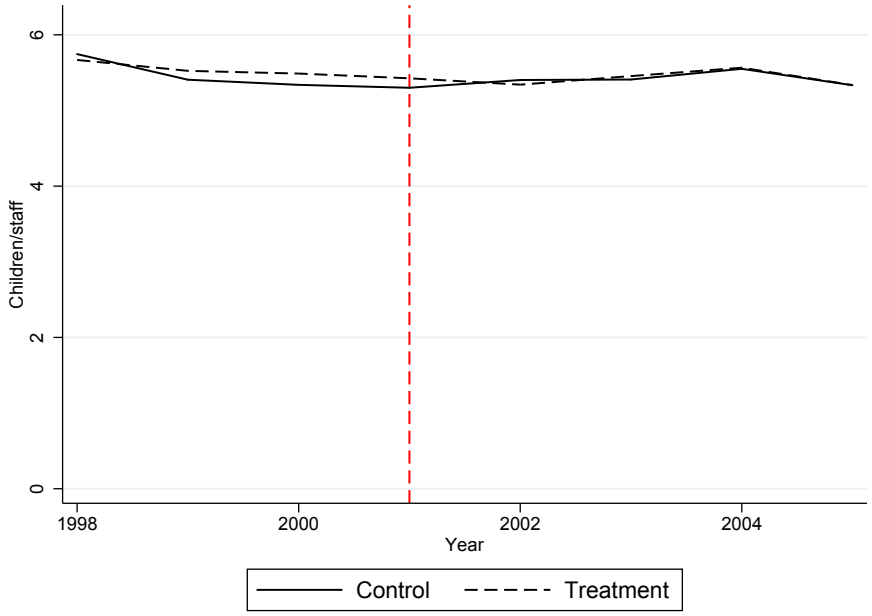
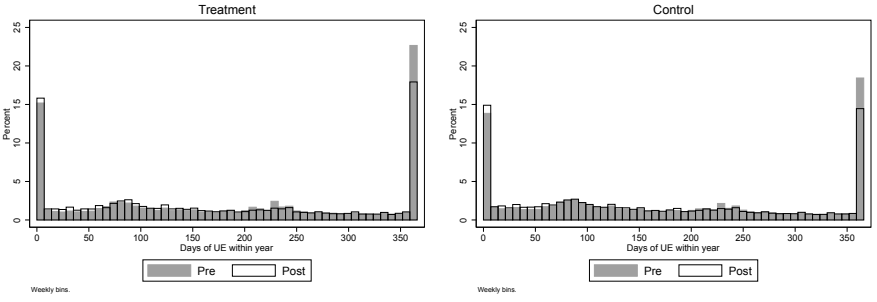


Figure A4. Number of days in a year for which children with unemployed parents experienced parental unemployment in treatment and control municipalities pre (1998-2000) and post (2002-2004) reform.



C Tables

Table A1. *Municipal level characteristics in 2001 by treatment status.*

	Control	Treatment	All
Municipal budget, spending/capita			
Sports and recreation	951.45	963.23	1,007.52
	(321.29)	(318.78)	(335.83)
Childcare	4,598.17	4,034.55	4,324.60
	(796.13)	(547.01)	(779.36)
Family support	2,074.75	1,940.51	1,957.50
	(798.93)	(566.11)	(723.00)
Education	12,541.64	12,650.48	12,728.06
	(1,394.10)	(1,184.22)	(1,392.85)
Other municipal characteristics			
Left wing majority	0.33	0.37	0.39
	(0.47)	(0.49)	(0.49)
Population share 1-5 yr	5.11	4.86	4.99
	(0.80)	(0.55)	(0.70)
Childcare participation	65.73	59.56	62.35
	(8.62)	(8.74)	(9.78)
Staff at childcare/child (municipal)	5.44	5.47	5.50
	(0.15)	(0.11)	(0.62)
Staff at childcare/child (private)	5.54	5.70	5.50
	(0.16)	(0.22)	(0.81)
UE rate 25-34 yr	10.39	11.11	11.15
	(3.61)	(3.24)	(3.63)
Population	45,389	20,738	30,827
	(100,929)	(20,250)	(58,499)
N	75	75	289

Table A2. *Enrollment in reform and control municipalities from Parental Survey.*

	Pre-reform 1999		Post-reform 2002	
	Reform	Control	Reform	Control
Unemployed				
Enrollment	0.57	0.78	0.81	0.82
Hours/week	20.85	24.83	20.96	24.39
Employed				
Enrollment	0.90	0.93	0.95	0.97
Hours/week	30.41	33.21	30.68	32.94
Total				
Enrollment	0.85	0.91	0.94	0.95
Hours/week	29.55	32.15	29.80	32.05

Table A3. *ICD10 diagnosis codes of the study.*

Variable	Definition	Example
	Hospitalizations	
Hospitalization	=1 if admitted to hospital for any reason	
Infection	=1 if admitted to hospital with diagnoses code ICD10: A00-A99, B00-B99	Infectious diarrhoea, mononucleosis, chickenpox.
Respiratory	=1 if admitted to hospital with diagnoses code ICD10: J07-J08, J19-J39, J48-J99	Upper and lower respiratory infections.
Injury and poisoning	=1 if admitted to hospital with diagnosis code ICD10: S00-S99, T00-T98	Broken arm or ankle, overdose of medication.
Mental	=1 if admitted to hospital with diagnoses code ICD10: F10-F09, F51-F83, F85-F89, F99	Anxiety disorder, developmental agnosia, disorder of psychological development.
	Prescriptions	
Mental	=1 if received prescription to medication with ATC codes: N06B, N06A, N05	ADHD, depression, insomnia.
Antibiotics	=1 if receives presciption to medication with ATC code J01	Ear infection, urinary infection.
Respiraotry	=1 if receives prescription to medication with ATC codes R01-R06	Asthma-related, cough.

Table A4. *Difference-in-differences estimates across different set of covariates.*

	Children age 2–3 years			Children age 4–5 years		
			Any diagnosis			
1998	-2.328	-2.285	-2.043	-0.569	-0.453	-0.586
	(3.536)	(3.526)	(3.479)	(2.635)	(2.645)	(2.628)
1999	-2.129	-2.116	-2.333	0.717	0.768	0.622
	(3.603)	(3.598)	(3.614)	(2.272)	(2.271)	(2.259)
2001	-1.106	-1.062	-1.249	0.721	0.692	0.713
	(3.308)	(3.311)	(3.282)	(2.576)	(2.562)	(2.553)
2002	-3.081	-3.300	-3.139	3.988	3.908	3.913
	(3.651)	(3.660)	(3.677)	(2.777)	(2.810)	(2.805)
2003	-3.171	-3.582	-3.572	0.618	0.490	0.196
	(3.571)	(3.551)	(3.572)	(2.667)	(2.676)	(2.683)
2004	-0.882	-1.375	-1.523	3.918	3.729	3.395
	(3.702)	(3.722)	(3.787)	(2.461)	(2.475)	(2.467)
			Respiratory			
1998	-2.378	-2.316	-2.251	-0.098	-0.057	-0.111
	(1.976)	(1.974)	(1.949)	(1.727)	(1.731)	(1.729)
1999	-0.990	-0.972	-0.998	-2.149	-2.120	-2.199
	(2.087)	(2.083)	(2.075)	(1.468)	(1.464)	(1.468)
2001	-0.201	-0.169	-0.322	1.077	1.036	1.097
	(1.763)	(1.763)	(1.753)	(1.693)	(1.683)	(1.699)
2002	-0.701	-0.815	-0.881	0.277	0.202	0.223
	(2.232)	(2.253)	(2.283)	(1.442)	(1.443)	(1.424)
2003	-3.417	-3.619	-3.616	-2.174	-2.286	-2.438
	(2.348)	(2.343)	(2.358)	(1.929)	(1.918)	(1.928)
2004	-1.466	-1.684	-1.725	0.397	0.267	0.162
	(2.365)	(2.362)	(2.382)	(1.503)	(1.505)	(1.471)
			Injury			
1998	-1.308	-1.292	-1.199	-1.965*	-1.903	-1.992*
	(1.472)	(1.474)	(1.476)	(1.187)	(1.190)	(1.202)
1999	0.379	0.380	0.320	0.275	0.295	0.355
	(1.476)	(1.479)	(1.489)	(1.468)	(1.464)	(1.468)
2001	-1.337	-1.307	-1.245	-1.809	-1.839	-1.807
	(1.655)	(1.656)	(1.657)	(1.459)	(1.461)	(1.458)
2002	-1.712	-1.749	-1.615	-0.081	-0.106	-0.137
	(1.512)	(1.519)	(1.515)	(1.638)	(1.640)	(1.661)
2003	-1.360	-1.424	-1.345	-0.737	-0.741	-0.707
	(1.450)	(1.451)	(1.441)	(1.268)	(1.266)	(1.292)
2004	0.803	0.749	0.731	-0.646	-0.683	-0.760
	(1.598)	(1.599)	(1.602)	(1.275)	(1.278)	(1.288)
			Infection			
1998	0.611	0.553	0.729	0.180	0.150	0.163
	(2.141)	(2.139)	(2.106)	(1.022)	(1.025)	(1.027)
1999	-1.658	-1.679	-1.837	0.358	0.334	0.296
	(2.090)	(2.085)	(2.112)	(0.934)	(0.931)	(0.927)
2001	2.323	2.328	2.196	-0.949	-0.947	-1.002
	(1.647)	(1.645)	(1.657)	(1.081)	(1.080)	(1.070)
2002	-0.700	-0.798	-0.587	2.477**	2.452**	2.421**
	(1.965)	(1.967)	(1.945)	(1.211)	(1.214)	(1.212)
2003	-0.685	-0.843	-0.805	1.743	1.716	1.611
	(1.588)	(1.611)	(1.593)	(1.154)	(1.149)	(1.138)
2004	-1.834	-2.077	-2.011	1.153	1.100	0.994
	(2.018)	(2.035)	(2.055)	(1.076)	(1.072)	(1.069)
N	315,562	315,562	315,562	321,576	321,576	321,576
Child controls	Yes	Yes	Yes	Yes	Yes	Yes
Parental controls	No	Yes	Yes	No	Yes	Yes
Municipal controls	No	No	Yes	No	No	Yes

* p<0.10, ** p<0.05, *** p<0.01

Notes: Child controls: child sex, child age (months in the end of the year), birth order. Parental controls: parental education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family. Municipal controls: municipal unemployment rate. All specifications include municipal and year fixed-effects. Standard errors are clustered at the municipal level.

Table A5. *Difference-in-differences estimates, heterogeneous effects by maternal education level for 2–3 year old children.*

	Compulsory Infection	Upper Secondary Infection	Higher Infection
1998	2.981	0.716	-2.527
	(4.850)	(2.558)	(3.627)
1999	5.786	-3.485	-5.647*
	(3.895)	(2.536)	(3.173)
2001	1.051	1.319	3.150
	(4.326)	(2.237)	(3.862)
2002	-5.153	-0.839	3.543
	(4.140)	(2.569)	(3.678)
2003	0.294	-0.608	-3.400
	(4.299)	(2.468)	(3.171)
2004	0.286	-2.742	-1.934
	(4.780)	(2.354)	(4.097)
N	60,455	175,186	73,345
Mean outcome	19.6	17.0	13.4

* p<0.10, ** p<0.05, *** p<0.01

Notes: All regressions controls for child sex, child age (months in end of the year), birth order, father's education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

Table A6. *Difference-in-differences estimates, heterogeneous effects by maternal education level for 4–5 year old children.*

	Compulsory Infection	Upper Secondary Infection	Higher Infection
1998	0.098	0.438	0.608
	(2.485)	(1.208)	(1.934)
1999	1.866	-1.417	4.194**
	(2.317)	(1.302)	(2.051)
2001	-0.693	-0.701	-0.709
	(3.094)	(1.225)	(2.897)
2002	10.106***	0.862	1.787
	(3.208)	(1.394)	(2.423)
2003	2.776	3.016**	-1.698
	(2.422)	(1.383)	(2.087)
2004	0.820	0.846	1.517
	(2.146)	(1.490)	(2.402)
N	62,207	181,747	72,205
Mean outcome	6.78	6.29	5.51

* p<0.10, ** p<0.05, *** p<0.01

Notes: All regressions controls for child sex, child age (months in end of the year), birth order, father's education (four categories), mother's age at first birth, father and mother region of birth (Nordic, non-Nordic) number of children aged 0–10 in the family, municipal unemployment, municipal and year fixed effects. Standard errors are clustered at the municipal level.

Table A7. *Past experience of children aged 4–6 with unemployed parents during the post-period.*

	Compulsory	Upper Secondary	Higher
Any parental UE while 2-3 years	0.90	0.90	0.87
	(0.29)	(0.30)	(0.34)
Months parental UE while 2-3 years	13.72	13.41	11.44
	(8.01)	(8.06)	(8.04)
Student benefit while 2-3 years	0.18	0.13	0.27
	(0.39)	(0.34)	(0.45)

III. Financial disincentive to return to work –do mothers react?

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1 Introduction

Women typically reduce their participation in the labour market when having a child. This reduction leads mothers onto a lower income trajectory than fathers. The time away from work can also be harmful for work-related human capital (Mincer and Polachek 1974) and it can worsen mothers' contacts with the work-life (Calvó-Armengol and Jackson 2004), which both could decrease the chances of employment in the long run and also affect the wage level. Additionally, time at home with children could contribute to a greater specialisation between home production and market work within families (Becker 1993). Most families in the Nordic countries have more than one child. Theoretically it is however not clear whether one long period away from the labour market (short birth-spacing, not returning to work between births) is better or worse, in terms of long run labour market consequences, than multiple shorter periods away from work (children with a longer spacing, returning to work between births). In this paper, I study the importance of the level of the parental leave allowance on mothers' decision to stay at home or return to work between births and how this decision affects their long term labour market attachment.

Labour market policies and financial support for families can create incentives for parents to stay at home. Across countries there is a wide variety of differences in the parental-leave systems, in terms of the benefits they pay, the length of job-protected leave and so forth. These policy instruments are used by policy makers in their attempt to affect both the labour market participation and the fertility rate. In this paper I focus on the Finnish parental leave system and a reform that created exogenous variation in the level of the parental-leave allowance.

As the parental-leave allowance in Finland is based on previous earnings, which is likely to be correlated to labour market participation, we cannot regress the labour market outcomes directly on the level of allowance. Instead, we need exogenous variation in the level of the allowance to identify the effect of financial incentives on labour market participation. In 2005, Finland introduced a reform that made it possible for a sub-group of mothers to retain the earnings-related allowance level for the next child without needing to return to work between births. The aim of the policy was to alleviate poverty in families where children are born relatively close to one another. The eligibility for the same allowance is conditioned on children being born within three years apart. This reform created random variation in the allowance level at the time of a higher order birth. I use this variation to identify the effect of the level of the allowance on labour market participation between births and in the long run. The mothers I study were not aware of the policy change at the time of the birth of their first child. Hence, the intro-

duction of the policy creates the possibility to study the effect with a regression discontinuity design, which I later explain in more detail.

The results show that affected mothers decreased their labour market participation with three months in the short run in response to the raised benefit levels. The decrease is highest among middle-income mothers. The effects on labour market participation are however temporary; in the medium run, 5–8 years after the first birth, there are no effects on the probability of working. Hence, it seems like the reform managed to increase the allowance for families with short spacing between children without persistent negative effects on the labour market outcomes. My findings are in line with papers that have studied the importance of the length of the parental leave and the length of the benefit period during these leaves on the labour market outcomes in the medium and long run (Lalive and Zweimüller 2009, Lalive et al. 2014, Schönberg and Ludsteck 2014). Lalive et al. (2014) find that return-to-work is delayed in the short run due to an extension in the paid leave-period but long-run labour market outcomes are unaffected. Baker and Milligan (2008) find using Canadian data that extensions of parental leave expand mothers propensity to stay at home but also increase the job continuity. In German context Schönberg and Ludsteck (2014) find the same effect on staying at home in the short run but that the effect of extensions of job-protected leave have very small effect on long-term labour market outcomes.

However, the closest papers to the one at hand are the ones that have studied a similar reform in Sweden. Sweden introduced a similar eligibility to the previous parental-leave allowance, "speed-premium", in 1980s with a 30 months threshold-spacing. The reform has affected the labour market participation negatively in the short run (Ginja et al. 2017) and increased take-up of parental leave by mothers (Moberg 2016). Additionally, the reform in Sweden reduced the spacing of births (Hoem 1993,Andersson et al. 2006 Björklund 2006). I find no indication that the Finnish reform had effects on fertility outcomes.[1]

My main contribution in this paper is to study the importance of a financial incentive on the decision to stay at home or return to work between births within an arguably exogenous setting. I study the effect of a rise in the parental-leave allowance level on mothers' labour market

[1]There is also a separate literature studying the effect of spacing on other outcomes than fertility and labour market. Ginja et al. (2017) study the health effects and educational attainment of shorter spacing in Sweden using the speed premium rules in the parental-leave system and find no effects on health around 24 or 30 months cut-offs of birth-spacing but positive effect on educational attainment. Same reform with a different set-up is used by Pettersson-Lidbom and Skogman Thoursie (2009) who find that the educational outcome of older sibling is affected negatively by the shorter spacing.

attachment between births and analyse whether this change affects also the medium-run labour market outcomes. The effect is studied in an institutional environment where the period of job protection around a birth of a child is long (until the youngest child turns three) and does not alter due to the reform. I add to the literature that studies the importance of parental-leave policies on mothers labour market choices.

In Section 2 I introduce the relevant parts of the Finnish family policy. In Section 3 I describe the reform that I use to identify the effect of interest and the research design. In Section 4, I describe the data and the sample restrictions, and investigate the threats to identification. I then continue to the main results in Section 5. Finally, in Section 6, I conclude with discussion of the main results relative to the previous literature.

2 Family policies in Finland

Finland, together with the other Nordic countries, has a long history of generous family policies (Datta Gupta et al. 2008, Vikat 2004, Johnsen and Løken 2016). The policies include long durations of job protection, universal coverage of family benefits, as well as paid leave schemes with high replacement rates. In addition, public childcare is offered by municipalities as a subjective right for every child. The duration of job protection is one of the longest in the OECD countries (OECD 2017); a parent can stay at home until the youngest child in the family turns three years without losing his or her job.

At the time of the reform (October 2005), the Finnish parental-leave scheme consisted of *maternity* leave, *paternity* leave, *parental* leave and a short *bonus-leave* for fathers, during which parents are eligible for earnings-related allowance. Maternity and paternity leave are dedicated to the respective parent but the division of parental leave is determined by mutual agreement by the parents. Despite the fact that parents can, and are, actively encouraged to share the amount of time spent on parental leave, fathers generally take a small share of the days: fathers took up 3.4 percent of the days in 2005 (Social Insurance Institution of Finland statistics). Depending on how early the mother starts the maternity leave, the child is 10–11 month old when the earnings-related leaves end.[2] All the three leaves—maternal, paternal and parental—have to be used consecutively and thus cannot be used when the child is older. Exception to this rule is the one month bonus-leave for fathers which can be used until the child turns three.

[2]The maternity leave starts 5–8 weeks before the expected date of birth in order to protect both the mother's and child's health.

The parental-leave allowance level is based on the earnings according to the previous tax declaration, i.e. earnings two years prior to the birth of a child. On average, the allowance covers about 70 percent of the previous earnings. The effective replacement rate is lower for higher income levels.[3] In case the previous earnings are too low to entitle the parent to the earnings-related allowance, the parent receives the minimum parental-leave allowance. In my sample, the minimum allowance is about one third of the mean earning-related allowance received by those on leave with their first child. These features of replacement rate may affect who of the mothers react to the reform strongest.

In comparison to the other Nordic countries there is one distinguishable feature in the Finnish family policies: until the youngest child in the family turns three, one parent can stay home with job protection and receive flat-rate home-care allowance. This addition to the benefit scheme for families with small children is conditional on the child not taking part in public daycare. Similar home-care allowance ("cash-for-care") has been found to negatively affect the long-term labour market participation of mothers in Norway (Drange and Rege 2013). Over 80 percent of mothers receive this allowance after the earnings-related allowance period has ended (Haataja and Juutilainen 2014). This feature of the Finnish family policies has been contributing to the lower participation rate of younger children in daycare and lower rate of employment for mothers with young children in Finland as compared to its Nordic neighbours (Johnsen and Løken 2016).[4] The existence of the home care allowance and the job protection attached to it are likely to affect the impact of the reform I describe in the next section. As there already existed a three year job protection, the increase in the allowance level may have less impact on the way parents time their return to work.

[3] About 80 percent of the mothers who receive earnings-related allowance fall into the 70 percent replacement-rate category. The mothers whose earnings exceed the earnings-threshold above the common replacement rate get a 40 percent coverage for the exceeding part. And those whose earnings are even higher (about 5 percent of all mothers) get 25 percent replacement for the earnings above the second threshold.

[4] On top of the basic flat-rate home-care allowance, families with multiple small children are eligible for sibling supplements. Additionally, if the family is poor a means-tested part can be added to the allowance. Some municipalities supplement the home-care allowance by an amount that varies across municipalities. Kosonen (2014) uses this variation and finds the supplements to decrease the labour market attachment of mothers.

3 The reform and the research design

3.1 The reform

Before October 2005, the parental-leave allowance was by default based on the earnings of the previous tax declaration. As many parents have their children within two to three years from the previous, they had no time to gain enough earnings to be eligible for a high earnings-related allowance with the subsequent child. In October 2005 a reform package was put in place with the aim to prevent poverty in families with small children. The focus of this paper is on the reform that was targeted to families with relatively short spacing between births.[5] After the reform, parents could retain the same level of earnings-related allowance as they had received with their previous child, as long as the spacing to the next child was no more than three years by birth date of the previous and expected due date of the next child. Thus, after the reform mothers had no incentive to return to work between the births in order to receive the same earnings-related allowance as with the previous child. The disincentive to return to work between births and the financial incentive to space children within three years were unintended side-effects of the poverty-alleviation reform.

The reform was passed in parliament in December 2004 and took effect for higher-order births after the 1th of October 2005. The reform was first discussed in the media during the governmental budget discussions in the summer of 2004. The main newspapers published articles about the reform already in the summer and it was further reported in the local newspapers in the autumn of 2004. In the summer of 2005, the reform was discussed on a popular internet forum for families with small children. Thus, it seems that the information about the reform spread quite fast and extensively. As the focus in this paper is on parents who already had a child, it is likely that they quickly became fully informed about the reform. Parents who already have one child know the parental-leave system and are likely to hear about the relevant changes even via their social connections with other parents.

[5]The reform package also included another change in the qualifying grounds for earnings-related allowance. This other part was aimed to alleviate poverty in families where the parents have temporary job contracts. According to the reform, a parent who has been working at least one month before the due date of a child and could continue in the same job for at least half a year could receive earnings-related allowance based on the income of just one month. This reform is most likely to affect mothers at the lower end of the income-distribution. As I focus on mothers who have received earnings-related allowance with their first child, this other reform is unlikely to affect their decision with respect to labour market participation or fertility. The mothers who are more likely to be affected by this part of the reform package are those who have received minimum allowance with their previous child or are low-income workers who have not yet had their first child.

3.2 Treatment

The treatment created by the reform gives covered parents the right to maintain the same level of allowance as with the previous child, without returning to work, if the next child is born within three years. The timeline of the reform with respect to the birth date of the first child is captured in Figure 1. The parents who had their first child before October 2002 could not be eligible for the reform at the time of the birth of their possible second child. These parents are non-eligible because even if they had their second child within three years, they could not have the child after the implementation date of the reform—the reform-date is over three years away from the birth of the previous child. Instead, those parents who had their *first child since* October 2002 (the first vertical dashed line in Figure 1) could be eligible for the reform as long as the due date of the *second child* was *after* the implementation date. It is this threshold, first birth after 1st of October 2002, that I use in my research design to define the treated.[6] Parents who had their first child after January 2005 (second vertical line in Figure 1) were always eligible for the reform provided they had their second child within three years. In terms of the decision to work between the births, mothers who already knew that they will have their child after the implementation date and within three years could react relatively fast. Given that the reform created an incentive to stay at home between births we would expect to see, if anything, a decrease in the labour market attachment of mothers between births.

Figure 1. Timeline of the reform with respect to the birth date of the first child.

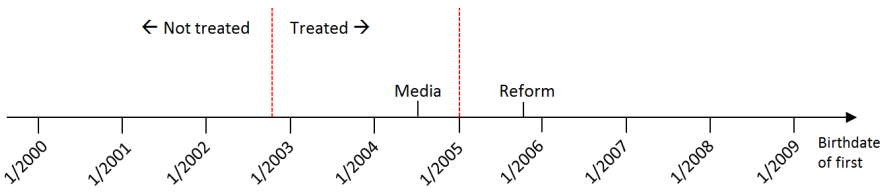


Figure 1. Notes: The first vertical dashed line depicts the reform date in terms of the time of birth of the first child. The second vertical dashed line depicts the point of time after which all first births make parents eligible for the reform. Between the lines, there is a likelihood to be treated conditional on having the second child within three years and after the reform-date.

[6] An alternative threshold to study is that of the reform date. However, given the media spread before the reform it is possible that parents have altered the timing of the births around this threshold.

Figure 2 shows that the effect of the reform on the qualifying grounds for earnings-related allowance with the second child was sizeable. The figure depicts the change for those who had received earnings-related allowance with their first child and who had their second child within three years. We see a notable change in the qualifying grounds. This is the change in the allowance level that I exploit to investigate the effect on labour market participation. The share of mothers who began receiving allowance based on the replacement rate of previous earnings, has been about 40 percent since the implementation of the reform. The share of mothers receiving minimum allowance is almost non-existing after the reform, a marked change from the 20 percent during the pre-reform period. The decrease in the share of mothers receiving allowance based on current earnings is due to mothers who have re-entered the labour market between births but whose earnings would qualify them for a smaller earnings-related allowance than the one enabled by the reform, i.e. the allowance with the previous child exceeds the one that they would receive based on the more recent earnings. The few minimum-allowance receivers that still exist after the reform are those whose earnings-related allowance was just above the minimum-allowance level at the time of the first birth. As the minimum-allowance level has gradually increased over time, some at the margin have received minimum allowance instead of earnings-related at the time of the second birth.[7]

[7]The slight decrease earlier in the share of mothers receiving minimum allowance is due to a reform in January 2003. Since then unemployment benefits have also been considered as qualifying grounds for the parental-leave allowance. I only include those mothers who have received earnings-related allowance during the leave with their first child. Hence, the reform in 2003 should not be a concern for my research design.

Figure 2. Share of mothers who receive maternal-leave allowance on different basis, at monthly level.

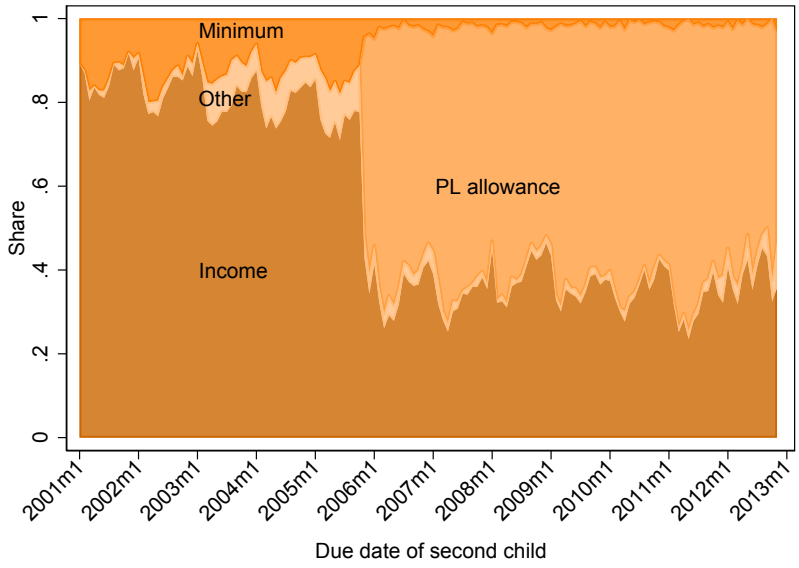


Figure 2. Notes: The figure shows the allowance basis at the time of the second birth for mothers who received earnings-related allowance with their first child and had their second child within three years. The figure shows the share of mothers who have the allowance based on different qualifying grounds. Income-related allowance is based on the income in the last tax-declaration. Minimum allowance is received by those whose pre-birth earnings have not been high enough to qualify for income-related allowance. Other basis are cases where the allowance is based on rehabilitation benefit or unemployment benefit. PL allowance is the basis that was introduced by the reform of October 2005 and is based on the previously received parental-leave allowance.

The change in the qualifying grounds of the allowance provides a first step towards the analysis of whether the reform affected the decision to work between births. To analyse whether the reform also had behavioural effects in terms of potential changes in the labour market participation of mothers or spacing of births, we need to quantify its effects on the size of the allowance. Given that the mean allowance is about 1,000 euros per month, net of tax, for those mothers who received earnings-related allowance with their first child, the financial disincentive to return to work is sizeable. If we consider receiving minimum allowance with the second child as a counter-factual scenario, the financial incentive to stay at home for a mean-allowance receiver would be about 650 euro per month for a period of 10 months.[8]

[8] All monetary values presented in year 2016 value.

3.3 Research design

I am interested in understanding the effect of the parental-allowance level on the labour market outcomes of mothers. If I were to regress a simple OLS with the allowance level on the outcome of interest the estimate would likely be biased as the allowance level is depended on the pre-birth earnings. The pre-birth earnings are correlated with other characteristics of mothers which may affect the labour market outcomes and the spacing of births. I use the year 2005 reform to overcome this problem as it created a clear cut-off date for those who were treated and those who were not with respect to the date of birth of the first child. This set-up gives good grounds for a regression discontinuity design. The reform applied to all higher order births but my focus is on the mothers who were having a second birth as this is the most frequent higher order birth.

To identify the causal effect of the allowance level on the labour market attachment of mothers, I exploit the exogenous variation that the reform created in the allowance level for the second births close to October 2005. In the research sample the parents could not anticipate the reform at the time of the birth of their first child—they could not change the timing of their first child any more at the time of the first media announcements. Hence, it is as if random which parents were exposed to the reform and who were not. As the the change in the qualifying criteria for the allowance is restricted to a birth interval of maximum of three years from the birth date of the previous child to the due date of the next child, the cut-off date in terms of the birth date of the first child is 1th of October 2002 (see timeline in Figure 1 for clarification). The births that occurred before October 2002 could not make the parents eligible for the changed basis of the allowance by the time of the birth of the second child. Hence, mothers who had their first child before October 2002 form the control group and mothers who had their birth after this date form the group that is treated. The research sample consists of around 750–900 first births per month. The mothers who can react to the reform are those who become pregnant early 2005 or later such that the age difference to the previous child is not over three years and the next child's due date is after the implementation date. I sample mothers who had their first child 6 months before the cut-off date (control) of 1th of October 2002 and 6 months after the date (treatment). The number of births is evenly distributed around the cut-off date: about half of the mothers belong to the pre-reform group (5,027) and half to the post-reform group (4,887). I return to the formal tests of randomness around the cut-off in Section 4.2.

As the reform is conditional on having a second child, I focus on those mothers who had a second child within five years. This group of mothers

constitutes 69 percent of all the first-birth givers around the cut-off date in my sample.[9] I discuss the potential endogeneity problem that this restriction raises later in this section. I argue that in the research design that I use, it is not a problem.

I implement a sharp regression discontinuity design where I estimate a linear regression in the form:

$$Y_i = \alpha + \beta_1 treat_i + \beta_2 date_i + \beta_3 treat_i * date_i + \epsilon_i \quad (1)$$

The main coefficient of interest is β_1, which estimates the marginal impact of the allowance around the cut-off date on the outcome variable of interest, Y_i, for mother i. For mothers whose first child was born before October 2002 the variable $treat_i$ takes the value zero and for those who had their first child after October 2002 it takes the value one. Hence, in this setting, birth date of the first child ($date_i$) is the assignment variable that defines the treatment status. The cut-off date is 1^{th} of October 2002 which is set to zero. The variable $date_i$ measures the distance to the cut-off date in days. Finally, ϵ_i is the error term. I use triangular kernel for the weighting of the observations. In the following section I discuss the identifying assumptions in the research design.[10]

4 Data and threats to identification

4.1 Data

In the main analysis, I employ a register-based sample of 60 percent of mothers who have given birth to their first child 6 months before and 6 months after October 2002. For these mothers, I have information from the Population Register on all their births at monthly level until the year 2013. This information is then connected to the registers of the Social Insurance Institution of Finland with information on the receivers of family benefits. The registers include information on the exact date of birth, the expected date of birth of children as well as the the amounts and the qualifying grounds and timing of the parental allowances. Information on mothers' education, age, labour market history, earnings, socio-economic status and regional characteristics are added from different administrative registers of Statistics Finland.

I restrict my analysis to mothers who received earnings-related allowance with their first child, whose first birth was a singleton, whose

[9] Within 10 years from the first birth 78 percent of these mothers have had a second child.

[10] For further discussion about the the regression discontinuity design, see for example Lee and Lemieux (2010) or Angrist and Pischke (2009).

first child did not die within the first 27 months[11] and who had the same partner the year after the birth of the first child. The reason for these restrictions is that being a single mother, giving a birth to multiple children or losing your child are all factors that are likely to limit the effect of the reform. The analysis are conducted for those mothers who have a second child within five years as the reform is constructed such that it only could affect the ones with further births. I show later in Section 4.3 that the fertility patterns were not affected due to the reform.

I study whether the reform affected the labour market attachment of mothers between births and the probability of being employed five to eight years after the birth of the first child. I use two measures of labour market attachment. Firstly, the registers include information about the exact calendar months a mother has a job-contract. This information is used to study the effect on labour market attachment between the births in case the mother has her second child within five years. However, having a job-contract does not reveal whether the mother actually worked, it only tells that the mother has a contract with an employer. Therefore I also study the number of months a mother worked within a year. For this measure I do not know the calendar months that the work has taken place. As I only know the number of months of work within a year for this measure, I use the following three years from the birth year as a proxy for how much the mothers worked between births. I construct a dummy for whether a mother worked 12 months within a certain year after the year of birth of the first child, and use that to measure the medium-run labour market participation.

4.2 Fulfilment of the identification assumptions

The main identification assumption of the regression discontinuity design is that the individuals do not have perfect control over the assignment variable around the cut-off. In my set up it is important that the parents do not have perfect control over the birth date of their first child, and most importantly that they cannot adjust this date with respect to the reform. This is indeed the case in this research set-up: at the time of the cut-off date no-one was aware of the up-coming reform that could affect their later level of allowance. The cut-off date is determined by the birth date of the first child as 1th of October 2002 but the up-coming reform was not discussed earlier than in the summer 2004. Hence, it was impossible for the parents to consider the up-coming reform when planning the timing of their first child. Given this close to

[11] On average pregnancy lasts 9 months. If a previous child dies within 27 months this is likely to postpone the birth of a subsequent child over the 36 months' time limit of the reform (27+9).

random assignment of treatment, it is plausible to interpret the results as causal.

To investigate this claim of randomness around the cut-off date formally, I investigate the distribution of the births for the whole research period (half a year around the cut-off date) and conduct the McCrary test (McCrary 2008) to account for the possibility of violating the continuity assumption in the assignment variable. The distribution of the births in Figure A1 shows some seasonality in the number of births throughout the entire period but there is no apparent jump in the number of first births around the cut-off date. The even distribution of the births is further demonstrated by the McCrary test, where the density of the assignment variable is used as the outcome variable to test for possible discontinuities. Figure A2 depicts the McCrary test's discontinuity measure with different bandwidths. There is no statistically significant discontinuity at the 95 percent level in the assignment variable with a bandwidth of less than a half a year.

Additionally, if the assignment into the pre- and post-reform period is random the characteristics of mothers should be the same on both sides of the cut-off. I conduct a covariate-balance test by running regression 1 on some important observable background characteristics. The age at first birth and the education level are important determinants of further fertility behaviour, whereas the share of allowance of total disposable income of the household (for the first child) is a good indicator of the importance of the public support for families. Local unemployment rate is relevant when we consider the labour market possibilities of the mothers. These regression results are shown in Table A1 in the Appendix. The similarity of mothers on both sides of the cut-off date are also shown in Figure A3 in weekly bins. There are no statistically significant jumps in the observable characteristics at the cut-off, except for a marginal change for years of education at a 10 percent confidence interval. The mothers are on average about 29 years old at the time of their first birth and have on average 14 years of education. On average, the allowance accounts for almost 30 percent of household income during the year the first child is born and local unemployment rate was about 11 percent at the time.

For the RD design framework it is also important to consider the bandwidth for the analysis. If we expand the bandwidth, i.e. the time span of first births around the cut-off, we increase the precision as more individuals are included but the further away we go from the cut-off, the larger is the bias in the estimate as it is harder to argue for randomness of those individuals on either side of the cut-off as parents can then adjust

the timing of their first child by taking into account the existing rule.[12] Instead of the suggested bandwidth by the McCrary test (see dashed vertical line in Figure A2), I use a data-driven bandwidth selection, which estimates the means-squared-error optimal bandwidth for each outcome variable separately. These optimal bandwidths are marked in the graphical presentations of the results with dotted vertical line. For ease of comparison, I additionally mark the optimal bandwidth of the outcome "months of job contract" to each graph with dashed line. The optimal bandwidth for the outcome of months of job contract between births is about 100 days. This bandwidth is very close to the one suggested by the McCrary test. In the tables, I report the point-estimates according to the bandwidth of 100 days. Importantly, the results are shown visually with a variety of different bandwidths to transparently demonstrate the robustness of the results with respect to the different bandwidths.

4.3 Potential endogeneity due to timing of births

Given the type of reform that gives rise to the exogenous variation in the allowance level, restricting the analysis to those who have two children within relatively short birth interval is reasonable. However, restricting the sample to those having a second child within five years raises a potential endogeneity problem, due to the fact that the reform encourages mothers to have their second child within three years. If the mothers respond to the reform by altering the timing of the second child, this would change the composition of mothers in the treatment group and also affect their labour market outcomes. I argue that in my research set up, the potential endogeneity is not a problem. The parents who had their first child close to the threshold date (October 2002) had little time to react in terms of timing of their second child for eligibility of the reform. This argument is based on the fact that it is close to biologically impossible for the mothers in my research sample to react to the reform by altering the timing of their births (getting pregnant usually takes time and pregnancy lasts on average for nine months). As I study the mothers half a year before and after the cut-off date, they would have to get immediately pregnant in order to be eligible for the allowance basis of the reform. For example, parents who had their first child in October 2002 would have to have their second child during the month of October 2005 when the reform was implemented. The reform was signed in December of the previous year, which gives these example parents only one month time to conceive. Due to biological restrictions

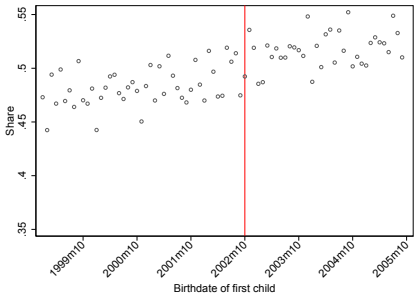
[12] Within the time line I use for the analysis, parents have no possibility to alter the timing of their first child.

it is fairly unlikely that anyone, even the ones 6 months away from the cut-off, had the possibility to alter the timing of their second child when they learned about the reform.

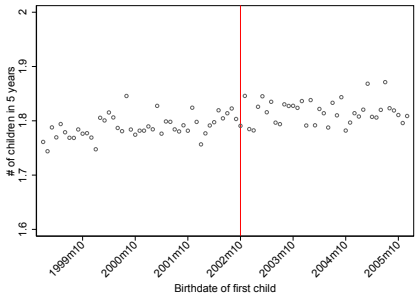
Figure 3 shows the trends in the share having a second child within three years for the period 1999–2005 (Figure 3a) and the trend of number of children within five years from the first one for all mother who have had their first child within the time interval (Figure 3b). The number of children within five years seems very stable. If anything, there has been a trend of closer spacing of births already pre-reform. This shortening of spacing prior to the reform is evident from the Figures 3c and 3d. In Figure 3c I compare the spacing pre-reform (1999–2002) to post-reform (2003-2005). In Figure 3d instead, I compare two pre-reform years (2000 and 2001), when the reform has not had any effects yet. There has been a shift for shorter spacing already pre-reform and this shortening of spacing has continued after the reform but not to a larger extend. I also investigate the change in number of children within 10 years and find no effects on the outcome. The distribution of the number of births and the estimates in line with Equation 1 are shown in Figure A4.

Figure 3. Trends and distribution of spacing and number of children pre and post reform.

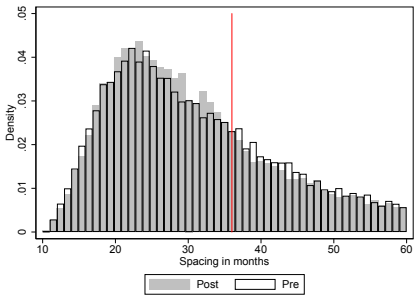
(a) Trend, second within 3 years



(b) Trend, # of children within 5 years



(c) Spacing, pre- and post-reform



(d) Spacing, pre-reform

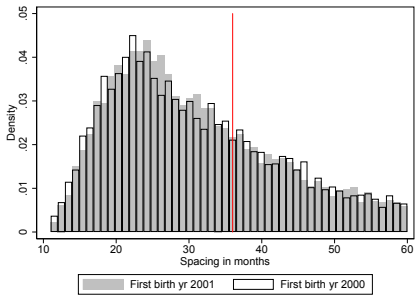


Figure 3. Note: The solid vertical line depicts the reform date with respect to the birth date of the first child in Figures (a) and (b). In Figures (c) and (d) the solid vertical line depicts the 36-months spacing threshold.

Already before the reform, there was a shift in the spacing distribution towards closer spacing and the shifting seems not to have increased after the reform. Given these findings, I conclude that the reform has not had effect on the average spacing between the first and the second birth nor has it affected the total fertility rate. Instead, there has been a general upward trend in the share who have had their second child within three years but the total number of children has remained stable. In Sweden a similar reform in the qualifying grounds for the parental leave allowance was implemented in the 1980s and according to multiple studies this reform did affect the timing of higher order births. In Finland, at the time of the reform in 2005, the share of families having their second child within three years was already relatively high in comparison to other countries. Hence, the fact that the reform did not affect the timing decision of second child might not be that surprising.

5 Results

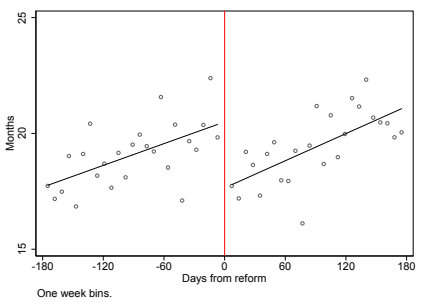
In this section, I first show the results for the full sample of earnings-related allowance receivers. I then study heterogeneous effects in terms of mothers' income to understand better who are the mothers who reacted to the reform. Finally, I run a robustness check with a placebo cut-off date a year earlier, when no one was eligible for the reform.

5.1 Labour market attachment in the short and medium run

In Figure 4, we can see the weekly averages around the cut-off date in terms of labour-market attachment between births. There is no clear pattern; if anything it seems that there is a slight decrease after the reform. To investigate this pattern further, I have depicted average allowance level for mothers who have had their second child within three years with respect to the birth date of the first child in Figure 5. The further the first birth has happened after the cut-off date the more likely it is that the parents are eligible for the new rule as the second birth has to take place within three years but after the implementation of the reform for them to be eligible. Due to this increased likelihood of being eligible, we see in Figure 5 that the average allowance level increased gradually until year 2005 at the time of the second child. The increase is notable in the gross benefit level: from about 40 to 70 euro per day. However, around the threshold where I study the effect, the change is modest in the allowance level between those who were treated and those who were untreated. This is driven by the fact that only few mothers just at the cut-off met the requirement of having the second child within three years but after the implementation date.

Figure 4. Labour market attachment between births if second child born within 5 years in weekly averages.

(a) Job contract



(b) Months of work

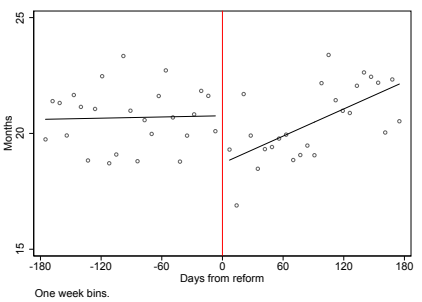


Figure 5. The average daily allowance, at monthly level.

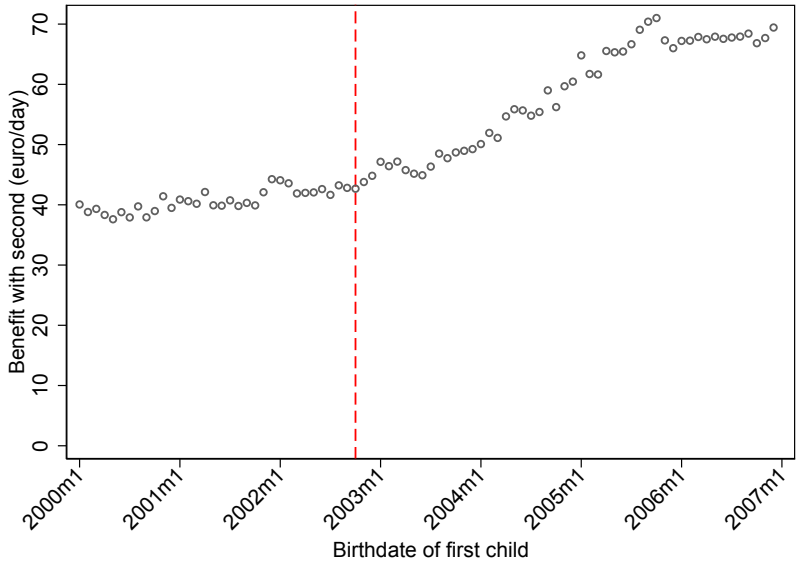


Figure 5. Notes: The daily allowance at the time of the second birth for mothers who received earnings-related allowance with their first child and had their second child within three years.

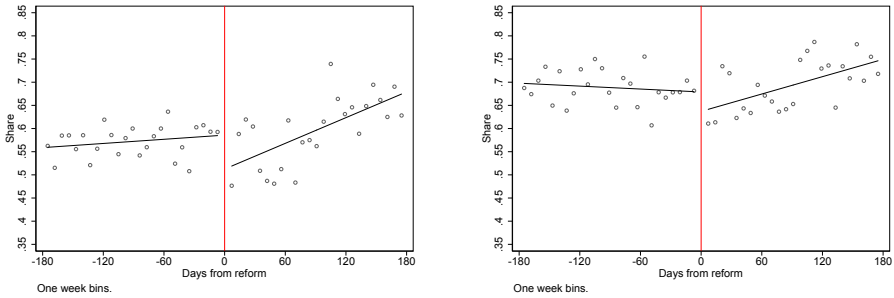
Figure 6 shows the averages for the medium-run outcomes. We see that the share of mothers who work a full year (12 months) increases the further we go from the birth of the first child. However, there seem not to be clear differences across those who were treated and those who were not.

The regression discontinuity estimates are shown in the Figures 7 and 8, and the point estimates at bandwidth of 100 days are shown in Table A2 and A3.[13] We can see a slight decrease in short-run labour market attachment, regardless how we measure it. Hence, despite the fact that the mothers had not much time to react to the reform, we find an effect in the short-run results. On average, mothers who were eligible for the reform have fewer months of job contract (2.8 months) between births than those who were not eligible. Job contract is a more accurate measure of labour market attachment than months worked, as the former is measured in exact months and the latter at yearly level. The number of months worked captures a similar story: mothers worked

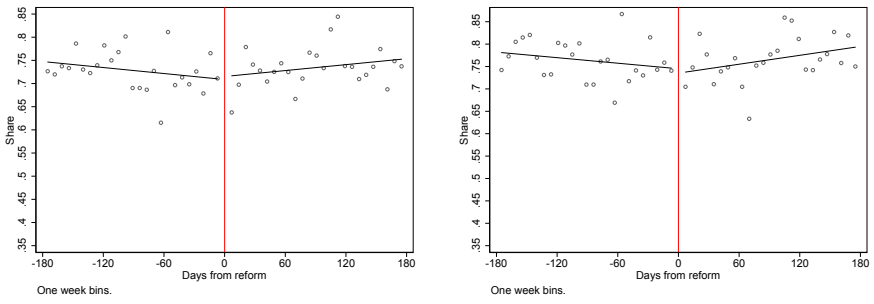
[13]Means and estimates for labour market attachment (working every month within a year) after 2–4 years from the first birth of a child are presented in Figures A5 and A6 and in Table A2. The point-estimate for full time work after two to three years indicates that it is 5 percent less likely for the mothers who are eligible for the reform to work over the whole year. The effect diminishes over the next years.

1.8 months less between births when they could retain the same level of parental leave allowance without returning to work. However, this effect is short-lived, as can be seen in Figure 8: after eight years from the birth of the first child, the effect is essentially zero. The decrease in the short run did not affect the medium-run labour market participation.[14]

Figure 6. Labour market attachment in the medium run after birth of first child if second child born within 5 years in weekly averages.
(a) After 5 years *(b)* After 6 years



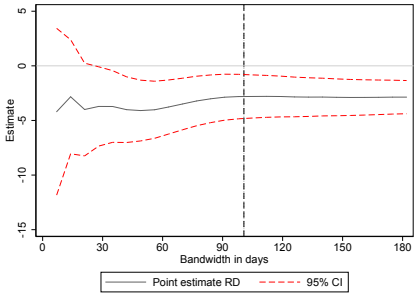
(c) After 7 years *(d)* After 8 years



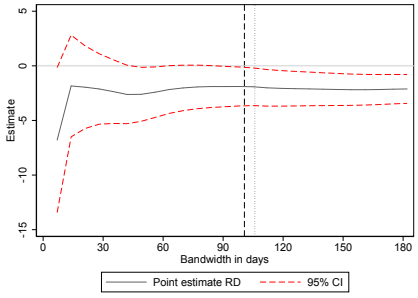
[14]These results are for the sample of mothers who had their second child within five years from the first. I have also investigated the sample of all mothers who had their first child around the cut-off date, without restriction on further births. The averages for these mothers are shown in the Figures A7 and A8, the estimates in Figures A9 and A10 and the point-estimates for bandwidth of hundred days in Table A4. As one would expect, the effect is less visible when mothers who had no further births are included. However, the effect after second year from birth is still very similar to those who had a second child within five years as this group of mothers constitutes the majority of the sample.

Figure 7. Estimates for labour market attachment between births if second child born within 5 years.

(a) Job contract



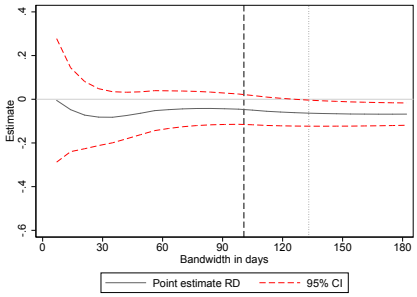
(b) Months of work



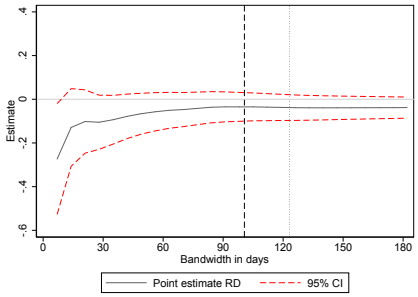
Notes: The dotted vertical line depicts the optimal bandwidth for each outcome in question. The dashed one depicts the same for the outcome "job contract between births" for each of comparison across outcomes.

Figure 8. Estimates for labour market attachment in the medium run after birth of first child if second child born within 5 years.

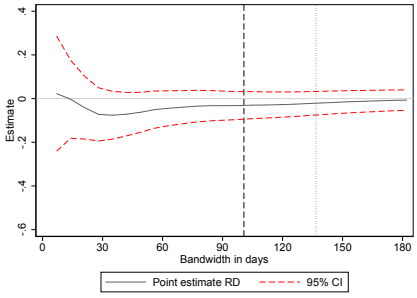
(a) After 5 years



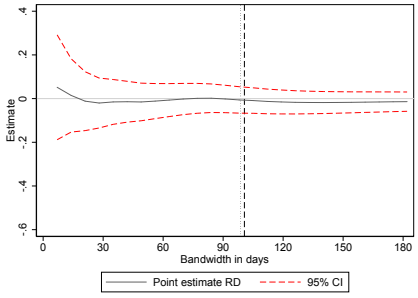
(b) After 6 years



(c) After 7 years



(d) After 8 years



Notes: The dotted vertical line depicts the optimal bandwidth for each outcome in question. The dashed one depicts the same for the outcome "job contract between births" for each of comparison across outcomes.

5.2 Heterogeneous effects by income

The level of the allowance that mothers receive with their first child (the financial incentive to react to the reform) is a measure of their earnings prior to the birth of their first child. Those who receive a low earnings-related allowance, whom I define as the bottom 20th percentile, are less likely to react to the reform as the financial incentive is small for them. As mothers with high earnings-related allowances are by definition high-wage earners, they likely have a closer attachment to the labour market than others and are likely less responsive to the policy change. I explore this heterogeneity in this section according to the 20th percentile division of the low and high allowance-receivers.

Figure 9. Distribution of the size of the financial incentive, per month.

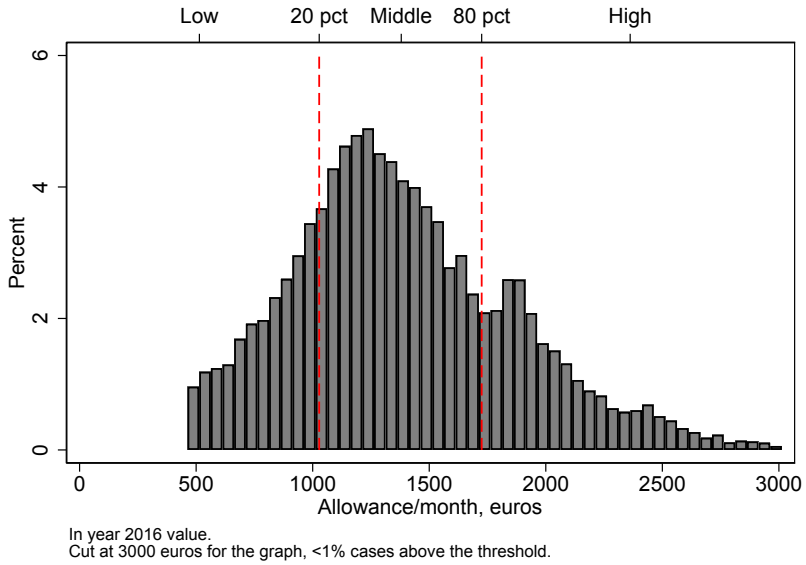
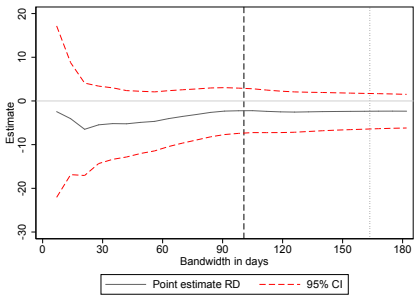


Figure 9. Notes: The financial incentive is the allowance received at the time of the first child. The vertical dashed lines depict the 20th and 80th percentiles of the distribution, which are the thresholds for the low-, middle- and high-income mothers used in this paper.

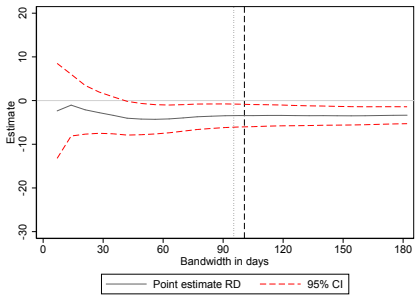
In Figure 10 we see that the previously shown results are strongest among the middle-income mothers (Figure 10b). For the middle-allowance receivers the decrease in months of job contract after the reform is 3.4 months. For the low- and high-income mothers the point-estimates are also negative but less than for the middle-income mothers. The lack of statistical significance in the results for low- and high-income mothers could be due to the smaller sample sizes.

Figure 10. Estimates for labour market attachment between births if second child born within 5 years with respect to the size of the allowance at the time of first birth.

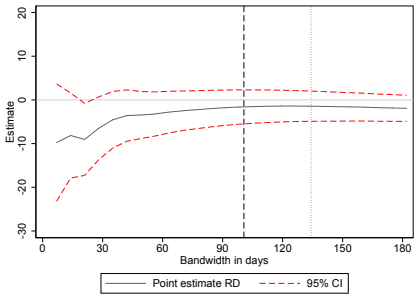
(a) Job contract, low (≤20pct)



(b) Job contract, middle (>20pct and <80pct)



(c) Job contract, high (≥80pct)

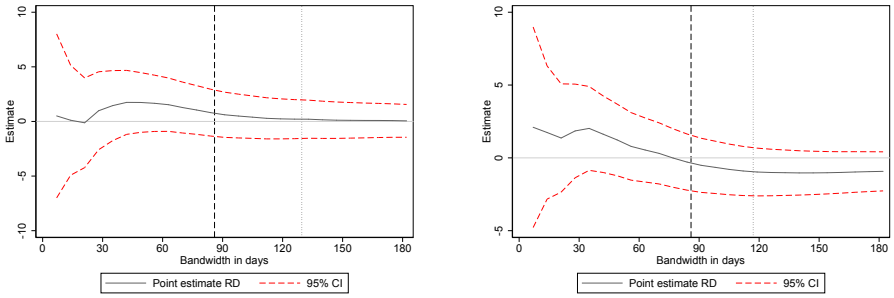


Notes: The dotted vertical line depicts the optimal bandwidth for each outcome in question. The dashed one depicts the same for the outcome "job contract between births" for each of comparison across outcomes.

5.3 Placebo timing: one year before the reform

As a robustness check for the results, I conduct the same analyses as previously but with a cut-off date that should not trigger any effects in terms of labour market attachment due to the reform. I use the same date in terms of month for the cut-off date but a year earlier, i.e. 1^{st} of October 2001, and first births half a year before and after this date as the sample of mothers. At this point in time no mother could be eligible for the reform. As shown in figure 11, no effects are found at this placebo set-up. This finding makes the found effects around the actual reform date more plausible.

Figure 11. Placebo timing: estimates for labour market attachment between births if second child born within 5 years.
(a) Job contract *(b)* Months of work



Notes: The dotted vertical line depicts the optimal bandwidth for each outcome in question. The dashed one depicts the same for the outcome "job contract between births" for each of comparison across outcomes.

6 Concluding discussion

In this paper I have studied the effect of a poverty-alleviation reform on labour market outcomes of mothers. As a side-effect, the reform created a disincentive to return to work between births for mothers with children spaced maximum of three years apart, and a financial incentive to have children within three years apart. The disincentive to work between births decreased mothers' labour market attachment between births. According to my results, this decrease of a few months, did not translate to an effect on labour market attachment in the medium run. The reform affected many families with a positive income shock without the need to change plans about timing of the next child: most families who have a second child have it within three years form the previous. As such, the reform met its initial goals of increasing the level of allowance for families where children are spaced close without persistent effects on the labour market outcomes. The reform did not seem to shorten spacing between births—instead there had been a general trend towards shorter spacing already pre-reform. In total, it seems that the reform met its goals without deteriorating the labour market attachment of mothers or fertility responses.

It should be noted that the reform under study in this paper did not affect the job-protected leave period during early childhood. A parent could stay at home with home-care allowance already pre-reform and gain right for leave until the youngest child in the family turns three. This institutional rule has likely shaped the idea of optimal timing of second child already before the introduction of the speed-premium reform. The fact that only the level of the allowance differed from earlier without change in the period of allowance payment or the period

of job protected leave differs from the set up of some recent studies. Lalive et al. (2014) and Schönberg and Ludsteck (2014) as well as Baker and Milligan (2008) use the change in length to study the responses of mothers in terms of labour market outcomes. However, the results are fairly similar; only short term effects are found. Both Ginja et al. (2017) and Moberg (2016) also find short-run effect on the labour market outcomes of parents in the Swedish context. Ginja et al. (2017) find that the income of mothers is decreases whereas Moberg (2016) finds that mothers who are eligible for the higher level of allowance stay longer on parental leave.

It could still be that at times of high unemployment the speed-premium matters more both in terms of labour market attachment and fertility decisions. If there are more short-term contracts that end before starting the parental leave and it is hard to find a new job, it might be more appealing to have second child within the three years. On the other hand, it could be that in worse labour market conditions those with job contracts feel more pressure to return to work between births. Thus, it would be interesting to study the role of the rule for births after 2008 when the economic crisis hit Finland.

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Appendix

A Figures

Figure A1. Distribution of the assignment variable 180 days around the cut-off date.

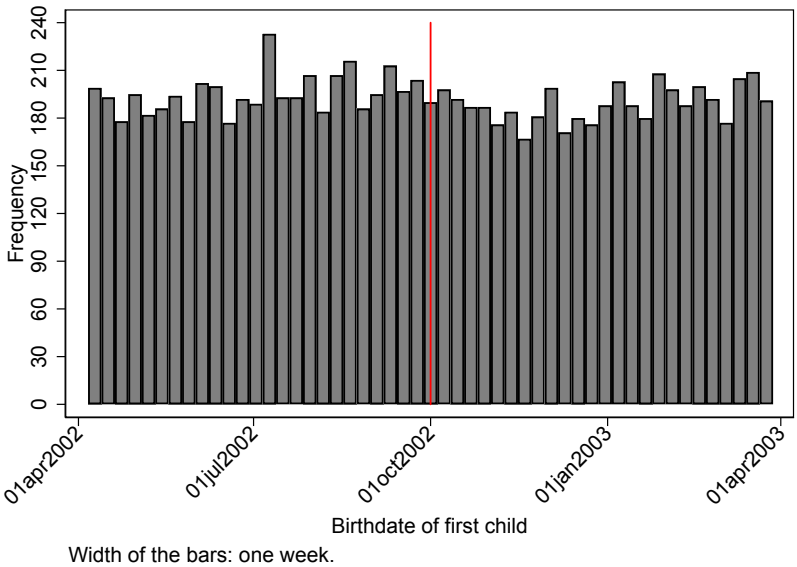


Figure A2. McCrary test for the discontinuity of the assignment variable, birth date of the first child, with different bandwidths. The optimal bandwidth according to the McCrary test is depicted with the dotted vertical line.

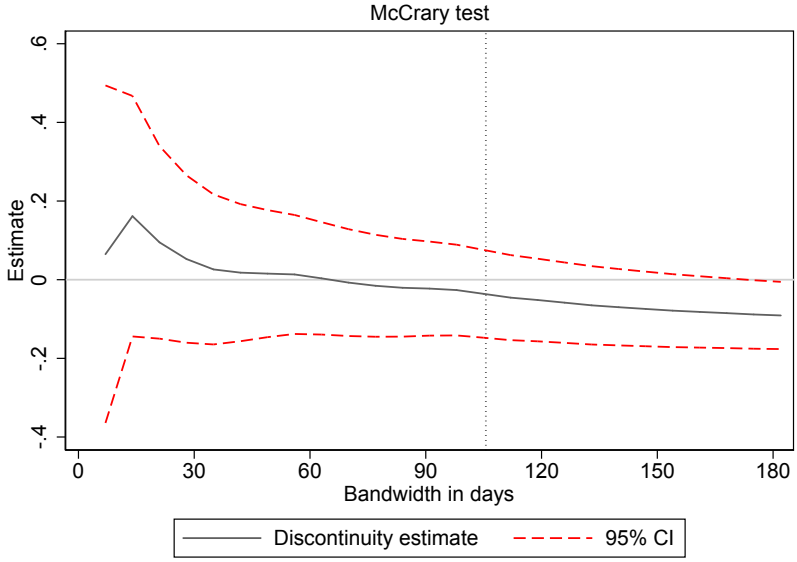
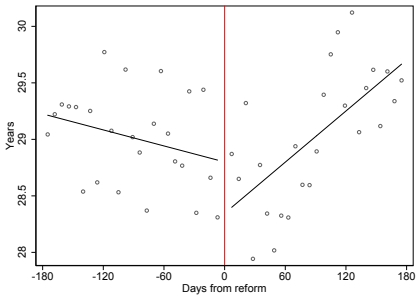
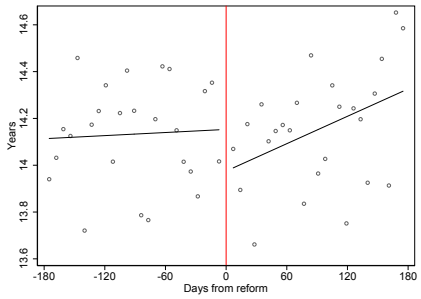


Figure A3. Covariate balance around the cut-off date, which is marked with a solid vertical line, for mothers who had their second child within five years from the first one. One week averages of the background characteristics for mothers during the year their first child was born.

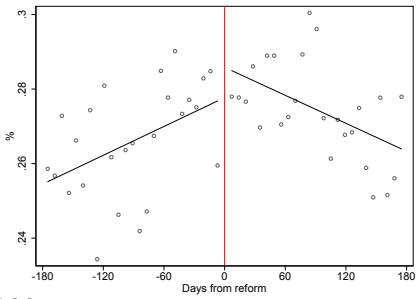
(a) Mother's age at first birth.



(b) Mother's years of education.



(c) Allowance/household income



(d) Local unemployment rate.

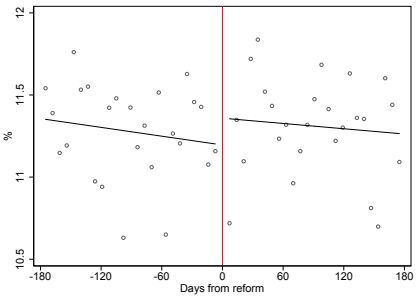


Figure A4. Number of children in 10 years pre and post reform.
(a) Density, # of children in 10 years *(b)* Estimates

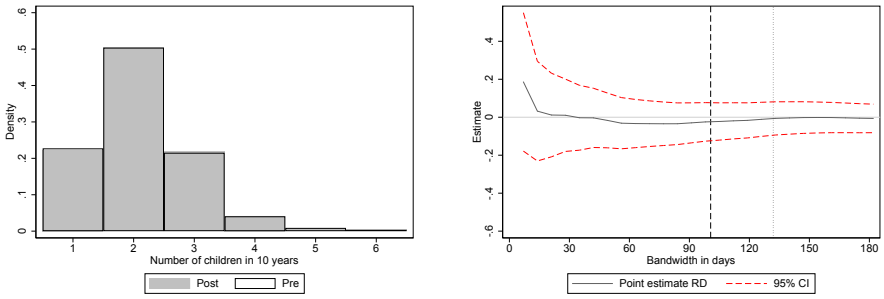
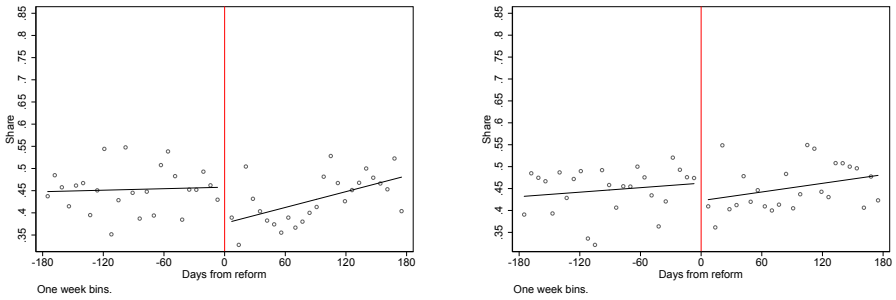


Figure A5. Labour market attachment in the short run after birth of first child if second child born within 5 years in weekly averages.
(a) After 2 years *(b)* After 3 years



(c) After 4 years

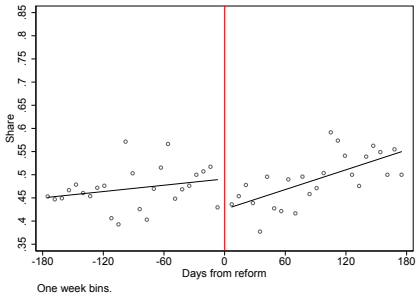
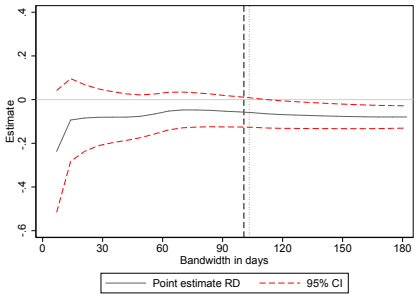
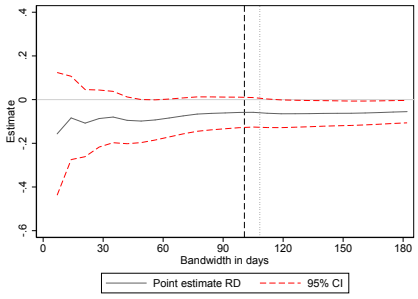


Figure A6. Estimates for labour market attachment in the short run after birth of first child if second child born within 5 years.

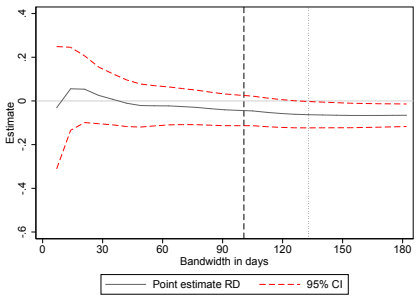
(a) After 2 years



(b) After 3 years



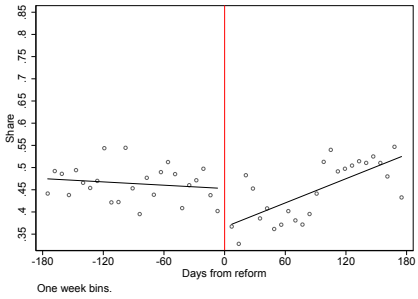
(c) After 4 years



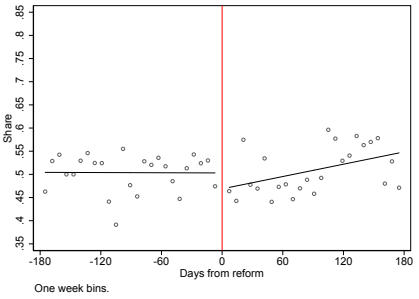
Notes: The dotted vertical line depicts the optimal bandwidth for each outcome in question. The dashed one depicts the same for the outcome "job contract between births" for each of comparison across outcomes.

Figure A7. Labour market attachment in the short run after birth of first child for all mothers who had their first child around the cut-off in weekly averages.

(a) After 2 years



(b) After 3 years



(c) After 4 years

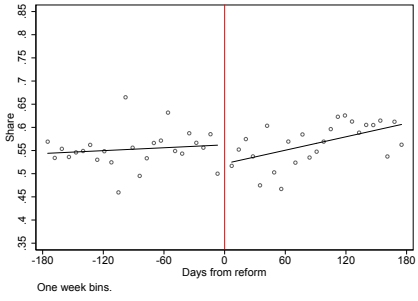
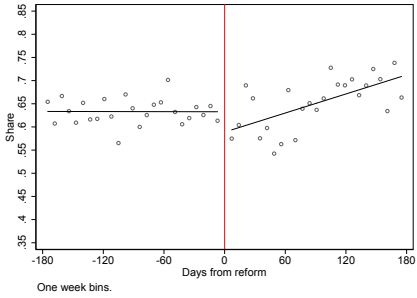
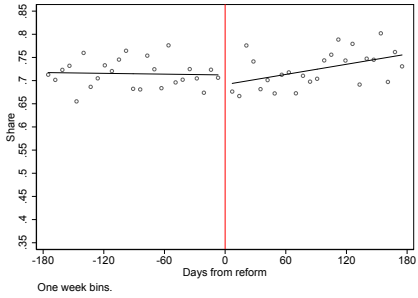


Figure A8. Labour market attachment in the medium run after birth of first child for all mothers who had their first child around the cut-off in weekly averages.

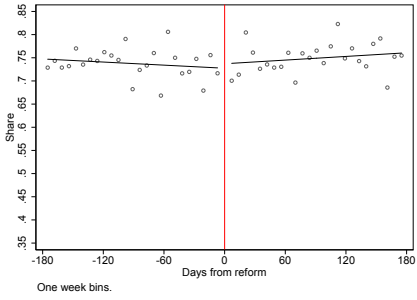
(a) After 5 years



(b) After 6 years



(c) After 7 years



(d) After 8 years

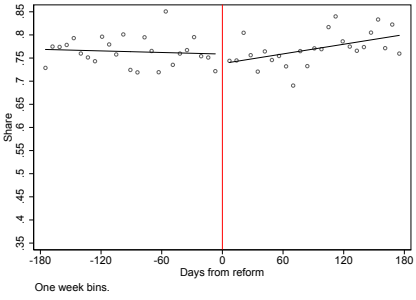
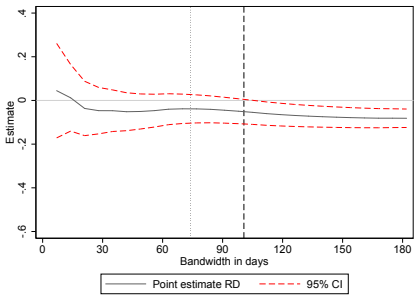
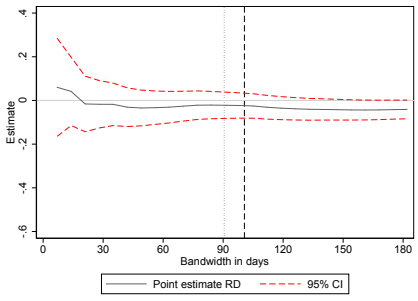
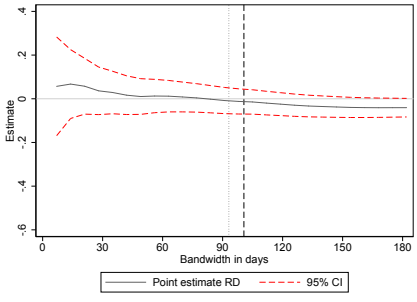


Figure A9. Estimates for labour market attachment in the short run after birth of first child for all mothers who had their first child around the cut-off.
(a) After 2 years *(b)* After 3 years





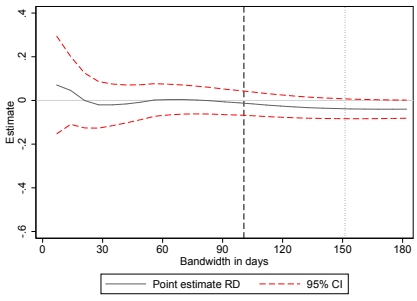
(c) After 4 years



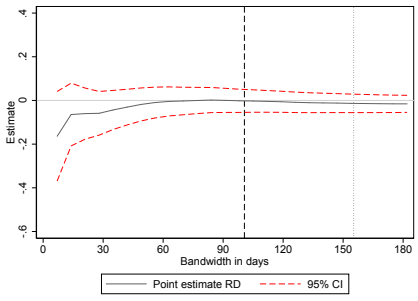
Notes: The dotted vertical line depicts the optimal bandwidth for each outcome in question. The dashed one depicts the same for the outcome "job contract between births" for each of comparison across outcomes.

Figure A10. Estimates for labour market attachment in the medium run after birth of first child for all mothers who had their first child around the cut-off.

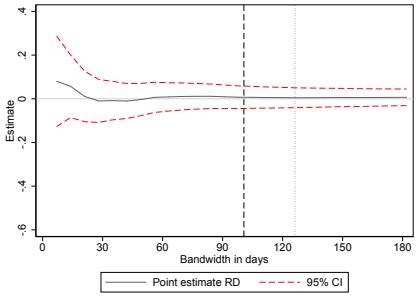
(a) After 5 years



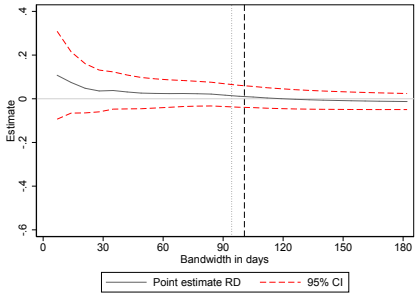
(b) After 6 years



(c) After 7 years



(d) After 8 years



Notes: The dotted vertical line depicts the optimal bandwidth for each outcome in question. The dashed one depicts the same for the outcome "job contract between births" for each of comparison across outcomes.

B Tables

Table A1. *Covariate balance test with bandwidth based on the optimal bandwidth of the outcome "job contract between births" for mothers who had their second child within five years from the first one..*

	(1) Age	(2) Years of education	(3) Allowance/Hh inc.	(4) Local UE rate
Coefficient	0.111	-0.333*	-0.001	-0.172
	(0.286)	(0.192)	(0.014)	(0.269)
Bandwidth	100.0	100.0	100.0	100.0
N (left of cut-off)	1971	1975	1959	1971
N (right of cut-off)	1806	1810	1787	1806
Order of local polynomial	1	1	1	1
Kernel	Triangular	Triangular	Triangular	Triangular

* $p < .10$, ** $p < .05$, *** $p < .01$

Table A2. *Point-estimates for the labour market outcomes in the short run with bandwidth based on the optimal bandwidth of the outcome "job contract between births" if second child born within 5 years.*

	Between births		Working after		
	(1) Job contract	(2) Months work	(3) 2 years	(4) 3 years	(5) 4 years
Coefficient	-2.805***	-1.889**	-0.057	-0.058*	-0.044
	(1.029)	(0.900)	(0.035)	(0.035)	(0.035)
Bandwidth	100.0	100.0	100.0	100.0	100.0
N (left of cut-off)	1975	1975	1975	1975	1975
N (right of cut-off)	1810	1810	1810	1810	1810
Order of local polynomial	1	1	1	1	1
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular

* $p < .10$, ** $p < .05$, *** $p < .01$

Table A3. *Point-estimates for the labour market outcomes in the medium run with bandwidth based on the optimal bandwidth of the outcome "job contract between births" if second child born within 5 years.*

	Working after			
	(1) 5 years	(2) 6 years	(3) 7 years	(4) 8 years
Coefficient	-0.046	-0.035	-0.031	-0.007
	(0.035)	(0.033)	(0.032)	(0.031)
Bandwidth	100.0	100.0	100.0	100.0
N (left of cut-off)	1975	1975	1975	1975
N (right of cut-off)	1810	1810	1810	1810
Order of local polynomial	1	1	1	1
Kernel	Triangular	Triangular	Triangular	Triangular

* $p < .10$, ** $p < .05$, *** $p < .01$

Table A4. *Point-estimates for the labour market outcomes in the short and medium run with bandwidth based on the optimal bandwidth of the outcome "job contract between births" for all mothers.*

	Working after						
	(1) 2 years	(2) 3 years	(3) 4 years	(4) 5 years	(5) 6 years	(6) 7 years	(7) 8 years
Coefficient	-0.051*	-0.023	-0.013	-0.012	-0.00193	0.007	0.010
	(0.029)	(0.030)	(0.029)	(0.028)	(0.027)	(0.026)	(0.025)
Bandwidth	100.0	100.0	100.0	100.0	100.0	100.0	100.0
N (left of cut-off)	2844	2844	2844	2844	2844	2844	2844
N (right of cut-off)	2639	2639	2639	2639	2639	2639	2639
Order of local polynomial	1	1	1	1	1	1	1
Kernel	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular	Triangular

* $p < .10$, ** $p < .05$, *** $p < .01$

Table A5. *Point-estimates for the labour market outcomes w.r.t. size of the allowance, with bandwidth based on the optimal bandwidth of the outcome "job contract between births".*

	(1) Low	(2) Middle	(3) High
Coefficient	-2.216	-3.436***	-1.600
	(2.617)	(1.319)	(1.991)
Bandwidth	100.0	100.0	100.0
N (left of cut-off)	361	1198	416
N (right of cut-off)	304	1112	394
Order of local polynomial	1	1	1
Kernel	Triangular	Triangular	Triangular

* $p < .10$, ** $p < .05$, *** $p < .01$