Maternal employment, child care, and long-run child outcomes*

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PRELIMINARY WORK - PLEASE DO NOT CITE.

Abstract

Today, 65 percent of women in the OECD countries with small children (aged 3-5) are working. However, we know little about the long-run consequences of maternal employment for these children. Using unique historical Danish data combined with high-quality administrative data, we examine the effects of maternal employment on children's earnings and schooling at age 35. For identification, we use an IV strategy. As an instrument for maternal labor supply we use variation in child care openings across local child care authorities. We find that maternal labor supply increases children's schooling (1.3 years) and earnings (25 percent) .

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1 Introduction

Today, female labor participation is high even for women with smaller children. For example, in the OECD countries, 65 percent of all mothers to children aged three through five are working (OECD, 2014).

This figure raises the question: in the long run, is this good or bad for the children? For policy makers (and parents) this question is important because maternal labor supply is evaluated against the cost of and potential negative effects of non-parental child care.

Several studies exist on the topic of maternal labor supply, child care, and child outcomes. Most of these studies investigate either the direct effect of child care, child care subsidies or maternity leave on either child outcomes or maternal labor supply such as Baker et al. (2008); Carneiro et al. (2011); Havnes and Mogstad (2011a); Dustmann et al. (2012).

A somewhat minor part of this literature focus on the direct effect of maternal labor supply on child outcomes and entirely on children's short- or medium-run outcomes. Thus, we know little about the long-run effects of maternal employment.

Combining unique historical Danish data with a classical IV strategy, we try to close this gap in the literature. We estimate the effect of mothers working when the children are four years old on children's length of schooling and earnings at age 35.

The current literature finds both negative and positive effects of maternal employment on child outcomes. For example, Ruhm (2004) estimate the effect of maternal employment when the children are age one through three. He finds that employment has a negative impact on children's reading and maths scores at age five and six. In addition, Waldfogel et al. (2002) find negative effects of maternal employment when the children are around one-year-old on children's cognitive development at age seven and eight. In contrast, they also find positive effects of maternal employment when employment is measured at age two and three. For medium-run effects, Dunifon et al. (2013) finds e.g. positive effects of maternal employment (employment measured at age one through tree and one through 15) on children's GPA at

age 15.

In 1964, to meet the demand for female labor supply during the 1960s' economic upturn, the Danish government introduced universally accessible and heavily subsidized child care for all women.[1] After this date, child care expanded rapidly but varied across municipalities, the local child-care authorities. For the period 1966-1976, we use these differences in access to formal child care across municipalities and time as an instrument for maternal labor supply.

For two reasons, Denmark is ideally suited for exploiting such a natural experiment. First, Denmark introduced universally accessible child care in the 1960s, making it possible to investigate earnings for a large population at age 35. Second, as the Danish government fairly quickly saw the need for expanding child care, alternative modes of care such as private care were uncommon. For example, in 1973, only 6 percent of all 3-6-year-old enrolled children were in private care.[2]

We identify working mothers as those who are working more than 33 percent of full time (the mean). Using this indicator for maternal employment, we find that that schooling increases by 1.3 years and log earning increases by 25 percent.

Dealing with data over a ten-year period raises the issue of other events diluting our results. For example, historical data suggests that the quality of care varied over time and thus likely varied across municipalities. We take these issues into account. However, we prove that linear and quadratic trends eliminate any correlation between child care openings and parental income and education, and we add these trend specifications to our model.

The remainder of this paper proceeds as follows. In section 2 we review the related literature. Section 3 describes the institutional background. Section 4 focuses on the identification and empirical strategy, whereas sec-

[1] Already in 1919 child care was subsidized (30 percent) in Denmark. However, at that time subsidies supported only child care for low-income families.

[2] At this time, private child care meant a pedagogical untrained woman who took care of a couple of children besides her own. Thus, this mode of care had a different quality.

tion 5 describe the data and presents the descriptive statistics. Section 6 shows and discusses the results and robustness checks whereas we sum up our findings in section 7.

2 Literature review

As most families in western countries rely on non-parental child care, there is an ongoing debate whether putting children in institutional care is god or bad for the children and the society as a whole.

In general, the literature on this topic either focuses on the impact of institutional child care on child outcomes or on the impact of maternal employment on child outcomes. However, disentangling the effects of maternal employment and child care is difficult because the estimated effect of child care often also reflects an effect of maternal employment and vice versa. We aim at investigating the effect of maternal employment through new child care opportunities and thus, we are one of the first studies that explicitly take into account both maternal working hours and universal institutional child care.

There is a large body of literature examining the association between maternal employment and child outcomes but a substantial amount of this literature focus on the effects of parental leave during the first years of childhood. A recent literature review by Ruhm and Waldfogel (2012) summaries the literature on parental leave on child outcomes when the child is less than two years old. They conclude that most studies focus on the effects of maternity leave reforms on female labor supply, fertility decisions and to some extend also infant health, but only a few studies investigate the medium and long-term effects of maternity leave. Examples of the few studies that examine the effect of maternal leave on child longer-run outcomes are Dustmann and Schönberg (2012); Rasmussen (2010); Liu and Skans (2010); Carneiro et al. (2015). In general the results are mixed. Carneiro et al. (2015) study the impact of a reform that increased maternity leave possibilities. They find that mother and child spending more time together has a positive effect on child wages at age 30 and a negative effect on children's

high school dropout. Whereas Dustmann and Schönberg (2012); Rasmussen (2010); Liu and Skans (2010) find no evidence of any general effects of extended maternity leave on various longer run child outcomes such as GPA at age 16 and high school enrollment. For higher educated mothers, Liu and Skans (2010) find a positive effect of extending paid maternity leave from 12 to 15 months on GPA at age 16.

There is a large literature relating the effects of non-parental child care and child outcomes and this literature can be divided into two separate branches: targeted child care programs versus universal programs. The first branch is surveyed in Currie and Almond (2011) and finds overall positive effects on child development when evaluating programs such as The Perry Preschool and The Abecedarian. Thus as most of these programs target disadvantaged families, results are difficult to generalize.

The second branch consists of studies analyzing the effects of universal institutional child care. This is a smaller strand of literature and the results of these studies are mixed, which to some extent is caused by differences in the counter-factual mode of care between studies. For example, Baker et al. (2008) find negative effects of child care on child outcomes, but positive effects on maternal labor supply, which is in line with other Canadian studies using the same natural experiment, (see for example Lefebvre and Merrigan (2008)). In a danish setting Datta Gupta and Simonsen (2010, 2012)exploit regional variation in the take-up of preschool to examine the effects of center-based preschool compared to family day care. They find that family day care has adverse effects for boys with low educated mothers at age seven and no evidence that one type of care outperform the other on outcomes at age 11. Dustmann and Schönberg (2012) find positive effects on language and motor skills measured at school starting age for immigrant children but no effects for native children. For identification, they rely on a staggered introduction of universal child care for children aged 3 to 6 across local child care authorities in Germany. Felfe et al. (2013) utilize an expansion of universal child care for 3-year-olds in Spain. They define the treatment groups as those states that experienced an increase in child-care enrollment above the median and finds positive effects on reading and math

skills at age 15 and a decreased likelihood of grade repetition. Effects are primarily driven by girls and children from disadvantaged families. This setup is much in line with Havnes and Mogstad (2011b,a), who exploit a child-care reform in Norway in 1976. They find positive long-run effects of education and labor market outcomes. However, they find no or small effects on maternal employment. Indicating a shift from informal to formal non-parental child care. Felfe et al. (2013) find no effect on expanding access to child care on maternal employment, however this merely reflects that formal child care has crowed out maternal care rather than informal care as in the Norwegian case.

3 Institutional Background

Public provision of child care has a long tradition in Denmark and is historically linked to the demand for female labor supply (Ploug, 2012). Figure 1 shows the number of formal child-care institutions from 1920 through 1990. The red line defines the number of child-care institutions for 3-6-year-olds (left-hand axis), and the blue dots (right-hand axis) defines the female share of the labor force. The two vertical dotted lines define our period of interest (1966 to 1976), and the vertical solid line defines the 1964 child-care reform.

Before 1919, private initiatives funded child care.[3] After 1919, to support these private initiatives, the government subsidizing existing institutions with small amounts (30 %). The purpose of these subsidies was to support the expansion of the labor market.[4] However, the economic downturn in the 1920s had the highest negative impact on female workers. Therefore,

[3]The first child-care facility opened in 1828, and many followed in the years during the industrial revolution. These first types of institutions were all asylums and a type of charity run and funded by the upper-class and held up to 150 children per 2 or 3 adults. The purpose of theses asylums was to keep the children away from the street while the parents were working. Slots were offered to poor families where the mother had to work. In 1904, the first folk child-care institution opened. These child-care institutions were also privately funded but run by groups that took a special interest in the parents and the children such as the working-class union and pedagogues (Korremann, 1977).

[4]In two ways, this initiative supported the demand for female labor supply. First, by increasing access for working-class women whom could not afford child care. Second, by preventing current institutions from closing due to very low budgets.

the 1919-reform had little effect on the level of child care.[5]

In 1933, institution openings were further motivated. Not only running cost, but also costs for building new institutions were now subsidized.

After 1945, the demand for female labor supply rose and in 1951, the publicly funded running costs for child care increased to 70 percent. As a consequence, the number of child-care institutions increased, despite the economic slowdown at the beginning of the 1950s (Bingley and Westergård-Nielsen, 2012; Korremann, 1977).

3.1 The 1964 reform and after

The economic upturn in the 1960s spurred the demand for child care and in 1963 more than 28000 children were on the waiting list for child care (Korremann, 1977). Thus in 1964 the government made four main changes to the current child-care regulations.

First, the reform untied subsidies to institutions from the proportion of children from low-income families.[6] Consequently, not only working-class women had better possibilities to enter the labor market.

Second, the purpose of child care changed from an entirely social to a partly pedagogical perspective adding discussions of quality and child development into the political debate about child care. For example, after 1964, an institution was eligible for subsidies if the headmaster was a certified child-care teacher and formally approved by the Ministry (BUPL, 2003). In addition, in 1970, the theoretical part of pedagogical training changed from one to two years (to a total of three years).

Third, the state covered all housing costs, whereas running costs

[5]The unemployment rate rose from 17 percent in 1920 to 32 percent in 1932, and the breadwinners were first in line to vacant jobs(Korremann, 1977)

[6]Untill 1964, an institution was eligible for full subsidy if two-thirds of the children were from low-income households, otherwise half the subsidy. Nonetheless, after 1964 additional support for poor households remained. Figures from 1969 suggest that parents paid an average annual running cost of 1920 USD per child (equivalent to 3 percent of the average family income), but parents with a family income below 52,700 USD (approximately 15 percent below the mean) could apply for a fully funded child-care slot (Horsten, 1969). Throughout this paper, all monetary figures a given in fixed 2011 prices. We use the following exchange rate: DKK/USD = 5.7

remained 70 percent publicly funded with a split between the state, 40 percent.; the local authorities (municipalities), 30 percent; and the parents, 30 percent.

Fourth, the municipalities and not private initiatives were responsible for providing child-care institutions.

After 1964, child care became an important instrument for regulating the labor supply for local and state-level administrators. For example, policy makers calculated that the product of a 100 slot institution was an 18 persons net increase in female labor supply (Korremann, 1977).

Figure 1 also shows that the 1964 reform led to a dramatic increase in the number of institutions. However, not until after 1966, the 1964 reform had full potential. A general building-stop in city areas from 1960 prevented municipalities to build new institutions.[7]

3.1.1 Changes in Quality

From 1966 to 1970 public spending on child care increased dramatically to meet the demand for female labor supply. An increasing tax-level financed the increasing expenditure on child care (and many other welfare services). As people reacted strongly to these total tax-increases, the government started regulating welfare costs in the 1970s.

For example, in 1971, the government transferred five percent of the state-funded costs to the parents and in 1974, the government put a cap on the state-funded running-costs. Thus forcing the local authorities to make budget cuts. Despite these changes, figure 1 shows that the number of institutions continued to grow throughout the 1970s, but the quality likely changed from the beginning to the end of our period.

Table 1 confirms this trend. The number of personnel working hours per child-care slot increased in the late 1960s and then decreased after 1972 to the end of our period. As our identification strategy relies on variation across time and neighborhoods, changes in quality leave a potential threat to our identification strategy. We take these non-linear trends into account

[7] The stop was implemented to encourage production instead of construction.

by adding linear and quadratic trends to our model specification.

3.1.2 Demanddriven supply?

From 1963 to 1970, the interests of pedagogues and parents worked hand in hand with the labor market: expanding child care (and quality) to attract more women to work. But after 1970, pedagogues and parents had little power in this process of planning child care.

In 1972, the union of pedagogues demanded better work conditions and higher teacher/child ratios. Despite local strikes and the threat of a national strike, the result was "further investigations". Simultaneously, the government negotiated and implemented the budget cuts that in the end meant fewer educated adults per child.

Thus, the overall changes in child-care availability most likely were more a labor market demand than a parental demand.

3.1.3 Outside options

Private child care existed during our period of interest and after 1964, they were also subsidized to meet the demand for child care. However, they were not as popular. In 1973, only 6 percent of all enrolled 3-6-year-olds were in private care.

At this time, private child care meant an uneducated (as a pedagogue) women, who cares for one or two extra children besides her own. In contrast, about 40 percent of all children less than three years old were in this type of care.

Figure 3 shows the development of private child care at the national level. The red solid line defines the number of public child-care slots for 3-6-year-olds, and the dashed blue line defines the private options (number of slots for 1-6-year-olds). The private options did not develop at the same rate as public child care and remained low at all times, indicating that they are no real concern for our strategy.

3.2 Variation across municipalities

Our identification strategy relies on a progressive implementation of child-care opportunities across time and neighborhoods within municipalities in the period 1966-1976. Figure 2 shows the roll-out of child-care institutions in two-year intervals in our period. The lighter colors define the municipalities with a child-care institution at the beginning of the period, and the darker colors define later periods. Not surprisingly, municipalities with larger cities such as Copenhagen (capital), and Aarhus (2nd largest city) where the expansion of production happened first, had child-care institutions before 1966, whereas the rural areas with most farmers implemented child care last. Nonetheless, between these extremes there is nice variation where neighboring municipalities roll-out child care in different periods.

Municipalities dealt with the demands of providing child care and later on the reduction in child-care expenditure in various ways. In 1973, the municipality of Aarhus summoned that because children were in care at different times during the day, the existing child-care institutions used only 80 percent of their capacity. As a result, they suggested enrolling eight percent more children per institution. Another municipality suggested fewer opening hours a day to reduce child-care costs.

A structural reform in 1970 merged the existing 1098 municipalities to 276. The merge was economically motivated. The smaller units did not have the economic foundation (or the manpower) to implement the increasing level of welfare services, such as nine years of mandatory schooling and homes for elderly. In the years prior the reform the city areas facilitated these services for the surrounding rural areas but did not get any tax payment from the people living in the rural areas. Consequently municipalities merged, to close this discontinuity between the pool of taxpayers and the well-fare beneficiaries.

Although our variation of interest is institution openings within smaller units of the municipality (neighborhoods), this change in municipality-borders has implication for our identification strategy. In our main specification, we group the municipalities according to the year they implement

child care to take this structural change into account.[8]

3.3 Political economy analysis

A concern in this analysis would be if the implementation of child-care institution is not (as good as) random. One could expect openings of child-care institutions to be correlated with parental characteristics, such as education or employment rate. As more resourceful parents, who use informal care, could create a political pressure for subsidized child-care expansions in their municipality. We follow Black et al. (2005); Holmlund (2008) and check whether parental background can predict when a child care is built in their municipality. Results from various model specifications are displayed in table 3. In order to demonstrate that formal child-care availability is an exogenous source for explaining maternal employment, we need that parental background does *not* predict child-care availability in our specifications.

In all columns of table 3 we include year fixed effects. However, in the first column municipality fixed effects are excluded. The second column includes municipality fixed effect, whereas the third and fourth column adds linear and quadratic trends specific to the year municipalities implement child care. When excluding municipality fixed effects, the DD framework is no longer valid, and we see that maternal employment, parental education, and household income seem to predict child-care openings. When controlling for municipality fixed effect, and thereby relying on a DD framework with many groups and many periods, maternal employment can no longer predict child-care openings. However, maternal education is still positively correlated with child-care opening. When controlling for trends (linear or quadratic), none of the parental characteristics are able to predict child-care openings. Not only does the significant correlation disappear, but the size of the estimates decrease substantially as well. Thereby is the reform causing a staggered introduction of subsidized child care and an exogenous source of variation in maternal employment when controlling for trends specific to the timing of child care implementation in the municipalities. Our

[8]We have also tried using the municipality borders after 1970 throughout the period, and results are robust to the structural change. (See section 6 for further details.)

preferred specification will thereby be those with quadratic grouped municipality trends.[9]

4 Identification

To examine the effects of maternal employment on child outcomes, we exploit the variation in access to formal child care induced by the Danish reform. The exogenous variation in maternal labor supply stems from progressive implementation of child-care opportunities in different places at different times across Denmark. We estimate the relationship between maternal employment, child care and long-run child outcomes using a two-stages-least-squares (2SLS) framework; summarized in the following two equations:

$$\begin{aligned} Y = \quad & \beta_0 + \beta_1 WORK^m + \beta_2 MALE + \beta_3 URBAN + \\ & \beta_4 COHORT + \beta_5 MUNICIPAL + \beta_6 TREND + \varepsilon \end{aligned} \tag{1}$$

$$\begin{aligned} WORK^m = \quad & \alpha_0 + \alpha_1 CHILDCARE + \alpha_2 MALE + \alpha_3 URBAN + \\ & \alpha_4 COHORT + \alpha_5 MUNICIPAL + \alpha_6 TREND + v \end{aligned} \tag{2}$$

Y is the adult outcome of the child and $WORK^m$ is a dummy indicating whether the mother worked or not when the child was four years old. $MALE$ is a dummy variable taken the value 1 if the child is male and 0 if the child is female. $URBAN$ is a dummy indicating if the child grew up in an urban area. $COHORT$ is a full set of year-of-birth indicators, $MUNICIPAL$ refers to a full set of municipality indicators. We include both linear and quadratic trends to control for the timing of child care implementation in the municipalities, which is summarized in equation (1) and

[9] When analyzing policy changes on a regional level, one needs to consider whether selective migration is an issue. Rhode and Strumpf (2003) uses around 150 years of data observation and find no evidence that people migrated between states due to more favorable political environment. Unfortunately, we do not have data allowing us to examine whether this is also the case for Danish citizens.

(2) by $TREND$.[10] Finally, $CHILDCARE$ is an indicator taking the value 1 if the individual belongs to a cohort that was subject to the existence of a child-care institution in the neighborhood at age four, and 0 otherwise. Equation (1) is the second stage and equation (2) is the first stage, where $CHILDCARE$ serves as an instrument for $WORK^m$. The error terms ε and v capture all unobserved factors that affect the child outcomes Y and maternal employment $WORK^m$, respectively.

When including both year and municipality fixed effects this set-up corresponds to a difference in difference (DD) model with many time periods and many groups. The identifying assumption is similar to a classic DD with two periods and two groups, namely that . A concern with DD models is that the estimates reflect differential trends instead of a true policy effect. This could be caused by factors, such as general investments in schooling at municipality level, correlating with the child-care reform. Therefore, to account for potential differential trends between areas implementing child care at different times, we include linear and quadratic group-specific trends as seen in Meghir et al. (2012).

Factors such as unobserved ability shared between child and mother (or nature and nurture) will influence both maternal employment and adult outcomes directly. Using ordinary regression methods will thereby bias the estimate of β_1 in equation (1) due to correlation between the error term and maternal employment. To circumvent problems with correlation the literature on instrumental variables try to find a source of variation in maternal employment that is uncorrelated with unobserved factors entering the error term. Instruments earlier used to extract variation in maternal employment include changes in maternity leave and local female unemployment rate. However, unemployment rates are not truly uncorrelated with unobserved ability, as more skilled workers have a higher probability of holding a job than less skilled workers, even when you control for education. In this paper we instrument maternal employment with local child-care availability.

[10] First, we group the municipalities according to the year a child-care institution opens in the municipality for the first time. Second, we interact each of these municipality group indicators with a year-of-birth variable and a squared year-of-birth variable.

It is crucial that the implementation of child care is uncorrelated with unobserved factors influencing adult outcomes. If this is not the case the instrument it not valid. Although, we have not found any evidence that, some municipalities implemented child-care institutions with higher subsidies than others. It is very likely that parental characteristic could influence local politicians and thereby correlate with implementation of child care. In this case the reform would be correlated with specific municipality characteristics. However, this is taken care of by the differences-in-differences framework, where the municipality fixed effects control for all time-invariant factors specific to the municipality. To create further confidence in eliminating bias, we include linear and quadratic trends specific to municipalities, which capture trends in education and earnings that might correlate with the child-care reform. In section 3.3 we prove that parental characteristic can *not* predict child-care openings and conclude that the instrument is exogenous conditional on the controls. Our identification strategy relies on variation in access to child-care facilities for mothers. Conditional on available control variables the instrument is exogenous and serves a valid instrument. In this analysis we identify the local average treatment effect (LATE) of maternal employment on child long-run outcomes for the group of compliers.

4.1 Reduced form

The out-roll of child-care institutions did not only affect labor supply for mothers with eligible children it did also affect the enrolled children. Unfortunately, we do not have data on enrollment at this time. Instead we can examine the general impact on children's adult outcome estimating the reduced form. Previous research have used a similar reduced form to calculate so-called Intention-To-Treat (ITT) estimates (Baker et al., 2008; Havnes and Mogstad, 2011b). The reduced form in this analysis is given by the following specification:

$$
\begin{aligned}
Y = \quad & \alpha_0 + \alpha_1 CHILDCARE + \alpha_2 MALE + \alpha_3 URBAN + \\
& \alpha_4 COHORT + \alpha_5 MUNICIPAL + \alpha_6 TREND + u
\end{aligned} \tag{3}
$$

In the reduced form we regress the adult outcome, Y, on the childcare indicator, $CHILDCARE$, using the same setup at in the 2SLS framework. The error term u captures all unobserved factors that affect the outcome, Y. We report estimates from the reduced form in all headline tables in section 6.

5 Data

5.1 Data sources

Our primary data source is administrative registers from Statistics Denmark for cohorts 1963 through 1975. All registers contain a unique personal identifier which makes it possible to match children to their parents and to combine the different registers. We have information on parental characteristics such as educational attainment, parental age, number of children and income. For children, we have gender, date and place of birth and adult outcomes. Importantly, we also have data on formal child-care institutions from 1966 through 1979.

5.1.1 Child-care data

We have collected unique historical data about child-care institutions in Denmark. We use three different data sources to gather information about child-care availability. The first source is annual reports published by the National Board of Social Services from 1966 through 1972. The second source is also a report published by the National Board of Social Services in 1975. The third source is data from Statistics Denmark. Together, these three sources gives a panel of eligible institutions in Denmark from 1966 and onwards.

The first source regarding child-care institutions is six annual reports (Economic overview of eligible child-care institutions, 1966/67 - 1971/72). Each report contains a list of existing day care institutions at that time in

Denmark. For each entry is given a 6-digit unique institution-number, name, address, type, and how many children in each institution. We have digitized these annual reports, and they provide the basis for a panel data set of institutions in Denmark from 1966 through 1972, where the National Board of Social Services stopped publication of these annual reports. The second source (Day and Residential institutions for children and adolescents, 1975) contains a list of existing institutions by the end of 1974. In this source, there is likewise for each institution listed a unique institution number and type - and the institution's name, address and number of places. In addition, there is for each institution listed a date of establishment, which we utilize to expand the panel for the years 1973 and 1974. Unfortunately, it was not possible to scan information from this source. Therefore, we have manually keyed in entries from this source, which gave us the opportunity to validate each entry in the dataset. Combining the first two data sources gives a full panel of child-care institutions from 1966 through 1974. The third source is data from Statistics Denmark's registers on child-care institutions beginning in 1976. A new 5-digit institution number is introduced in 1976, thereby it has not been possible to use the institution number to connect the third source with the two former sources. Instead, we have relied on the address information, which is available in all three sources with information on street name and number, in addition to zip code and municipality for each institution. We do not have any data source about existing institutions in 1975. However, we assume that institutions existing in 1974 and 1976, also existed in 1975, thereby institutions established in 1975 will first be in the panel from 1976. We merge all three sources using address information on each institution. This exercise gives a panel of all existing child-care institutions beginning in 1966.

To identify which children were exposed to child care or an alternative mode of care than the mothers care, we need to link each family's home to the data on child-care institutions. Unfortunately, we do not have exact address information on families and individuals in 1966 as we have on child-care institutions. Instead, we rely on individual birth registration places, which correspond to the parish in which the child is born. At this

time, most mothers gave birth at home or near their home. We thereby assume that the birth registration place corresponds to the place where the mother lived at time of birth.

5.1.2 Outcome and explanatory variables

Our preferred outcome is log earnings. We have access to annual earnings reports from register on taxed income from labor [11]. As we observe date-of-birth for each child, we can construct a birth cohort indicator allowing us to measure earnings at age 35 for all cohorts in our sample, which is useful as average earnings may differ substantially over a ten year period for example between age 30 and 40. We also use educational outcomes, which are constructed using the norm length of highest completed education.

The main variable of interest, maternal employment, is constructed on the basis of contributions to a universal supplementary occupational pension system (ATP) implemented in Denmark in 1964. Contributions to the ATP system are split between employer and employee. Importantly, contributions to the ATP system vary according to hours worked. Full contribution are paid when full time employed, allowing us to infer each mothers work status.

Unemployed, self-employed, and persons out of the labor force are not a part of the ATP-system at this time. Thereby the ATP contributions can be used as a proxy for whether the mother spend the primary part of her time on the labor market. If a mother spends the primary part of her time working, it is safe to assume that the child is exposed to a different mode of care than the mothers care. We define employed mothers as those mothers working above the sample mean.

Female employment and child care availability are both higher in urban areas of the country, we take account of this by controlling for a indicator of urban area in all regressions. Further, we control for gender in all regressions.

To examine for heterogeneous effects we stratify the sample with

[11] Not including unemployment insurance and sick-payment

respect to parental education and household income.

We use the unique personal identifier to match each child to its parents and their income when the child was 17. We construct a measure of equalized household income, to take account for varying household sizes. We define equalized household income as the total amount of income divided by persons in the household. Where the first adult person has weight 1, the second adult has weight 0.7, and each child in the household has weight 0.5.

5.1.3 Sample selection

Statistics Denmark's registers contains all individuals living in Denmark. Our sample consist of the entire population of children, born in Denmark between 1963 and 1975 and their mothers. However, due to the identification strategy, we restrict the sample to persons born in the main part of Denmark (excluding Greenland and the Faroe Islands), alive and living in Denmark at age 35. Further, we exclude individuals with an invalid birth registration code, individuals without an identifier of the mother, and individuals from small (<5) municipality-year cells as identification of child care availability arises from relative differences between individuals in the same cohorts. We arrive at a data set of 761,412 children and their mothers.

5.2 Descriptive statistics

Table 2 presents summary statistics of important variables in this analysis. The first column reports mean and standard deviation for the full sample while columns (2)-(5) reports mean and standard deviation for sub-samples depending on child care and maternal employment status. We see that 72 percent of the children in the sample grew up nearby a child-care institution and that 39 percent had an employed mother when four years old. 31 percent of the sample had both a childcare nearby and an employed mother when aged 4.

It is evident from table 2 that outcome variables such as earnings and education on average are higher for those individuals growing up near a child care with a working mother (column (2)) compared to outcomes for the

rest of the sample. For instance, for children growing up near a child care; Children with a working mother on average receives 13.4 years of education, whereas a child without a working mother on average possess 13.0 years of education at age 35. For children without a child care nearby; Children with a working mother on average has 13.3 years of education and children with a non-working mother has 12.8 years of education at age 35. The summary statistics also draws a picture that suggest that household income and parental education systematically correlates with child-care availability and maternal employment. At first hand, one might suspect that it calls our identification strategy into question as more resourceful parents will be able to create a political pressure for expanding universal child care in their neighborhood. Consequently, to verify that the reform is exogenous with respect to maternal employment conditional on observables; we prove that parental characteristics can not predict child-care openings, when we control for municipality fixed effect and linear grouped trends.

6 Results

6.1 Main results

In this section, we present the main result of our analysis. The results seem to indicate a quite robust positive effect of subsidized, universal child care on maternal labor supply and adult outcomes of the children. For comparison reasons, we present ordinary least squares (OLS), first stage, reduced form, and IV estimates for each outcome. To address possible concern about selection bias or other bias due to policy changes correlated with the implementation of child care, we report estimates with and without municipality fixed effect in addition to linear and quadratic trends. Estimates are quite similar and qualitative the same across all specifications. The specification with quadratic trends is our preferred specification, hence in the following we focus on those results. Standard errors are clustered at the level of treatment (municipalities) and reported in parentheses.

Table 4 shows the OLS results, the first stage, the reduced form,

and the second stage estimates. The outcome used is log-earnings measured at age 35.

The OLS results suggest that having a working mother is associated with 4.8 percent higher earnings compared to children growing up with a mother, who does not work. The first stage estimation results, on the hand, reflect the effect of child care availability on maternal labor supply. The first stage estimation suggests that mothers living with a child care in their neighborhood are 5.4 percentage points more likely to work, than those living in a neighborhood without a child-care institution. F-statistics are well above the rule of thumb suggested in Staiger and Stock (1997), indicating that child-care availability is a valid instrument for maternal labor supply. A 5.4 percentage points effect might seem like a relatively small effect. However, in the light of only xx percent of all mothers in the first year of our period was working - this is a substantial increase. (Corresponding to "14" percent of first-year-mean)

The reduced form estimates reflect the direct association between the instrument (child care availability) and the long-run outcome. The reduced form suggest that children growing up in a neighborhood with a child-care institution in their neighborhood on average has 1.4 percent higher earnings than those growing up without a child care in their neighborhood. Combining the first stage estimation and the reduced form estimation gives us the 2SLS estimation results, which is to be interpreted as LATE effects. The group of compliers consist of children of those mothers changing their labor supply in the same year as a child care institution opens in their neighborhood. As the group of compliers is relatively small, the LATE effects are quite large. We find that maternal employment, when the child is four years old, has a positive effect of 25.4 percent on earnings at age 35.

We now turn to different measures of educational attainment as outcomes variables. Table 5 displays the estimation results of the analysis, where length of education is the outcome variable. In this analysis, the OLS estimate suggests that children with an employed mother has 0.3 years of education (or equivalently 3.8 months of education) more than children with an unemployed mother. The IV estimate, on the other hand, suggests

that having an employed mother increases education length by 1.3 years for the group of compliers, i.e. mothers, who change their labor supply as a consequence of extended access to formal child care. In table 6 we examine the effect of maternal employment on the child's probability of completing high school. The IV estimate shows that there is a LATE effect of maternal employment on high school completion of 18.3 percentage points. Similar from table 7 it is evident that maternal employment reduces the probability of only having the basic amount of schooling (9 years for the cohorts examined in this paper) by 11.8 percentage points.

Taken together the results suggest that maternal employment, when the child is four-years-old, has lasting effects on the child's adult earnings and education.

6.2 Robustness and sensitivity analysis

We use the model with grouped municipality quadratic trends as our baseline specification in a variety of specification checks.

Maternal labor supply is likely to correlate with the number of siblings. For example, a child with a younger sibling, born when the child is about to start in child care, might start, even though, the mother is staying home. On the other hand, the mother might choose to keep both children at home to save some money. To remove noise from this possible heterogeneous behavior, we exclude children who have a younger sibling in the first four years of life in the first column of table 8. In the next column, we are tightening the restriction and exclude all children how have a younger sibling in the first five years of life. In the third column, we look at children who are the last born, i.e. a sub-sample of only children and the youngest of all siblings. Finally, in the last column we look at a subsample of only children. The first stage estimate changes from 5.4 percentage points in the original sample to 5.8 percentage points when we exclude children, who have a sibling within the first four year of life. It is evident from table 8 that the first stage estimate is increasing as we remove observations; when we exclude all children how have a younger sibling in the first five years of life

the first stage estimate is 6.1 percentage points. The first stage estimate for the subsample of last born and only children are both 6.0 percentage points. Thus, the first stage estimates of child care availability on maternal labor supply are quite sensitive to the number of children a mother has. However, all first stage estimates are positive and in the range between 5.4 and 6.1 percentage points.[12] The reduced form and the IV estimates are also sensitive to the number of children in the family. Indeed, both the reduced form and the IV estimates are not significant in the subsamples of children, who are the last born child or an only child.

Formal child care is available for children between three and school starting age. We measure child-care exposure and maternal employment when the child is four year old, however to create confidence about the results we also try measuring when the child is three and five. The results are reported in table 8. Results are quite robust. However, the fixed cohorts entail a mechanical increase in child-care exposure from age three to five.

7 Conclusion

This paper exploits a historical change in access to child care to estimate the effect of maternal labor supply on long-run child outcomes.

To meet the demand for female labor supply, in 1964 universally accessible child care was implemented in Denmark. This implementation led to a dramatic increase in the roll-out of child care across the country. In 1960 only 10 percent of all children aged 3 through 6 were in child care whereas 35 percent of all children age 3 through 6 were in child care in 1975.

We use variation across local child care authorities and time as an instrument for maternal labor supply. We use this instrument to estimate the effect of maternal labor supply on child outcomes.

Using women who work more than 33 percent of full time as an indicator for a working mom (equivalent to the mean at that time), we find

[12]When dealing with potentially heterogeneous treatment effects, we need to assume monotonicity in the first stage. In the sense that the instrument may have no effect on some observations, but those affected are affected in the same direction (Angrist and Pischke, 2009).

that maternal employment has a positive effect on both length of schooling and earnings at age 35.

As we exploit variation across time and municipalities–the local child care authorities– parts of this variation is likely driven by differences in parental background in these municipalities. We follow the literature, and take care of such selection issue. First by proving that such a correlation between the opening of a child care institution and parental income and education isn't statistical significant and economically relevant once we control for linear and quadratic trends. Second, we add linear and quadratic trends to our model specification.

Our results are in contrast to many recent studies that find negative effects of maternal labor supply on short-run outcomes. Nonetheless, our results supports the existing results of positive effects of maternal labor supply on medium-run outcomes such as GPA at age 15.

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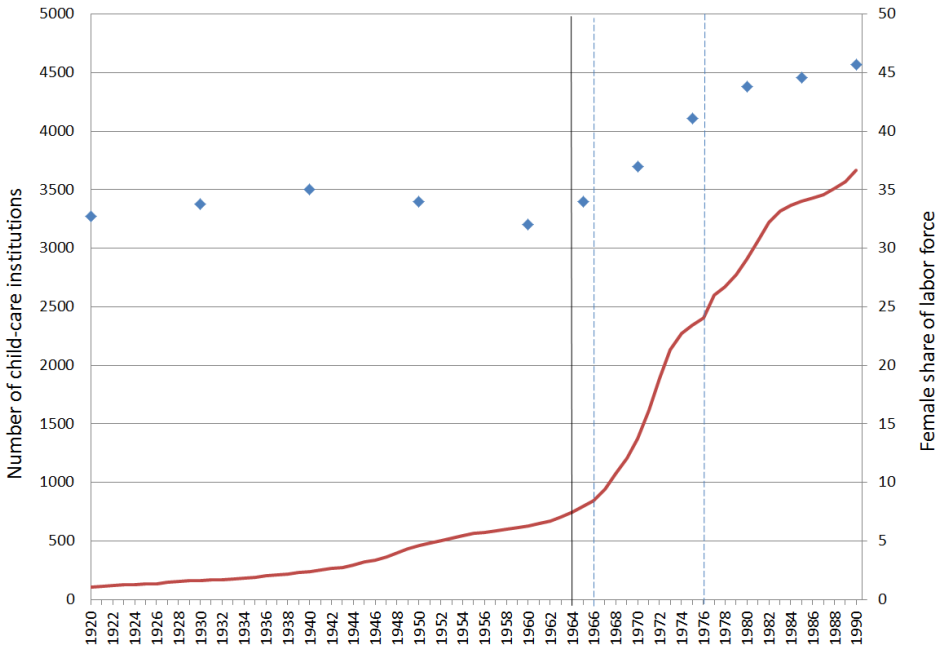
8 Tables and Figures

Table 1: Personnel working hours per child-care slot

1960	1972	1973	1975	1976
5,4	6,3	6,1	5,8	5,2

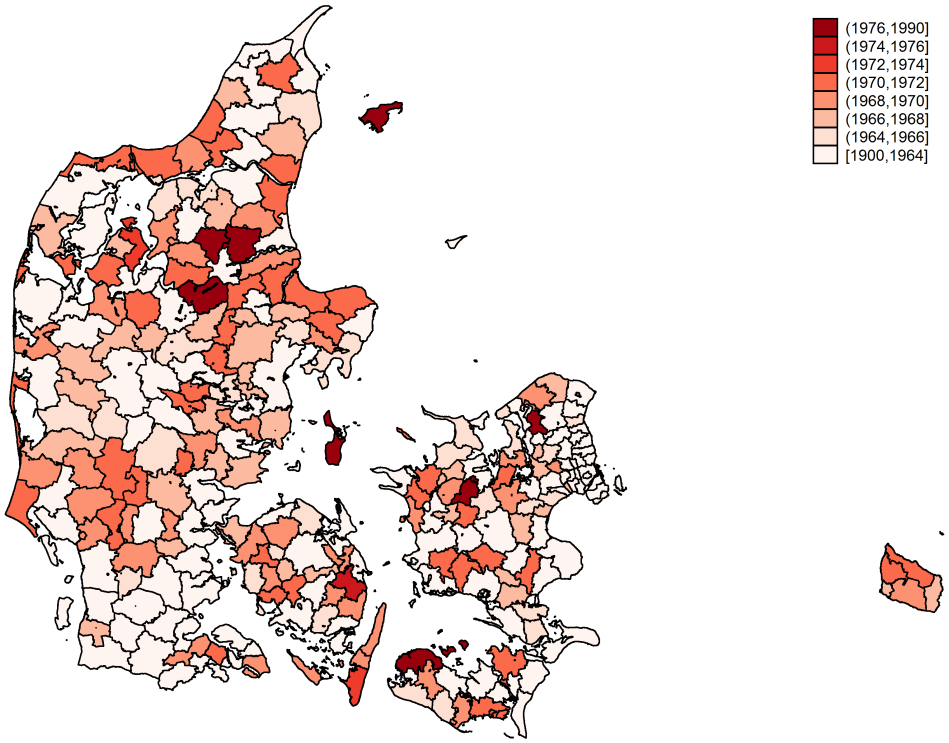
NOTE— The table shows the average weekly workning hours for personnel in child-care institutions in various year from 1960 to 1976 per child-care slot. Numbers are found in Korremann (1977) and serves as an indirect measure of quality.

Figure 1: Formal child-care institutions and female share of labor force



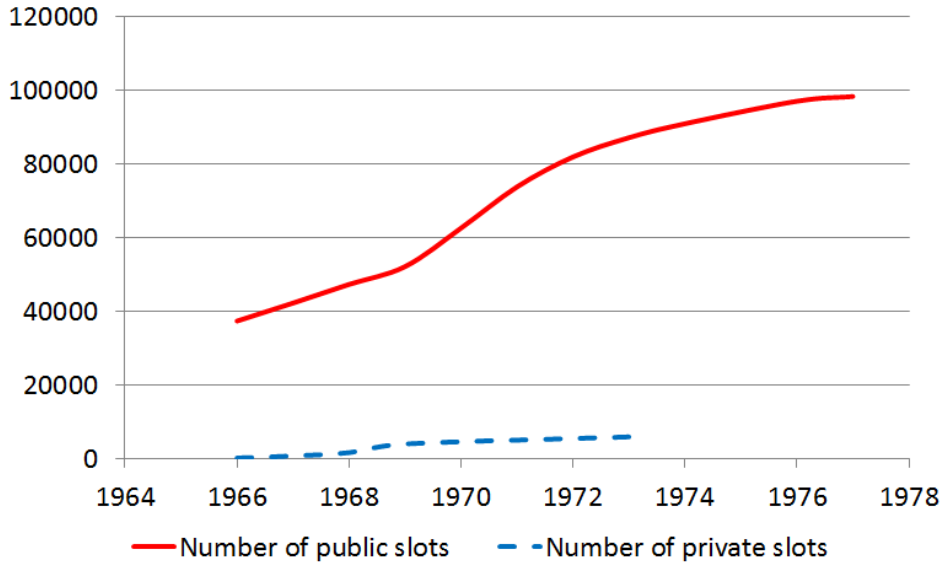
NOTE— The red line defines the number of child-care institutions for 3-6-year-olds (left-hand axis), and the blue dots (right-hand axis) defines the female share of the labor force. The two vertical dotted lines define our period of interest (1966 to 1976), and the vertical solid line defines the 1964 child-care reform. Data on the female share of the labor force are from Denmarks statistics, statistic yearbooks, varios years. Data on child-care institutions are from varios sources, see section 4.1.1 for more information.

Figure 2: Child care openings



NOTE— The figure shows the roll-out of child-care institutions in two-year intervals in our period. The lighter colors define the municipalities with a child-care institution at the beginning of the period, and the darker colors define later periods. Pre-1970 municipality borders. Data from the Danish Geodata Agency, Digdag (Kommunal), downloaded December 2013.

Figure 3: Number of public and private child-care slots



NOTE— The figure shows the development of private child care at the national level. The red solid line defines the number of public child-care slots for 3-6-year-olds, and the dashed blue line defines the private options (number of slots for 1-6-year-olds).

Table 2: Summary statistics

		Daycare=1		Daycare=0	
	All	Work=1	Work=0	Work=1	Work=0
	(1)	(2)	(3)	(4)	(5)
Childcare access	0.723				
	(0.447)				
Working mother	0.391				
	(0.488)				
Mothers hours at age 4	0.300	0.725	0.031	0.719	0.020
	(0.376)	(0.242)	(0.072)	(0.243)	(0.059)
Maternal education	10.626	11.737	10.286	11.447	9.247
	(3.218)	(3.109)	(3.084)	(3.321)	(2.925)
Paternal education	11.334	12.052	11.362	11.465	10.106
	(3.418)	(3.263)	(3.378)	(3.437)	(3.388)
Household income	78.606	96.455	74.151	86.490	56.484
	(52.100)	(50.797)	(52.117)	(46.180)	(45.796)
Equalized hh-income	31.752	39.439	29.892	34.560	22.339
	(21.605)	(21.232)	(21.558)	(19.055)	(18.590)
Urban	0.169	0.197	0.224	0.067	0.051
	(0.375)	(0.398)	(0.417)	(0.251)	(0.219)
Number of siblings	1.590	1.313	1.647	1.537	1.931
	(1.021)	(0.859)	(1.023)	(0.934)	(1.157)
Male	0.509	0.506	0.512	0.506	0.510
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Log earnings, USD	10.820	10.871	10.804	10.840	10.767
	(0.860)	(0.840)	(0.880)	(0.835)	(0.853)
Education, years	13.121	13.436	13.006	13.291	12.791
	(2.280)	(2.297)	(2.268)	(2.285)	(2.205)
High school	0.808	0.841	0.794	0.828	0.775
	(0.394)	(0.366)	(0.404)	(0.378)	(0.418)
Only basic school	0.073	0.055	0.078	0.067	0.095
	(0.260)	(0.228)	(0.268)	(0.250)	(0.293)
Observations	761412	237587	313203	60444	150178

NOTE— Mean coefficients, standard deviations in parentheses. Column (1) presents mean and standard deviation for the full sample, while columns (2) through (5) reports mean and standard deviation for sub-samples depending on child care and maternal employment status. Household income is measured in 1000 USD, and equalized by household size, where the second parent has weight 0.7 and each child has weight 0.5.

Table 3: Parental characteristics regressed on child-care openings

	(1)	(2)	(3)	(4)
Maternal work frequency	1.185***	0.068	0.053	0.053
	(0.098)	(0.049)	(0.046)	(0.046)
Maternal education	0.122***	0.007*	0.006	0.006
	(0.008)	(0.004)	(0.004)	(0.004)
Paternal education	0.123***	0.006	0.004	0.004
	(0.007)	(0.003)	(0.003)	(0.003)
Equalized household income	0.020***	0.001*	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Year dummies	Yes	Yes	Yes	Yes
Municipal FE	No	Yes	Yes	Yes
Linear grouped trends	No	No	Yes	No
Quadratic grouped trends	No	No	No	Yes
R squared (within)		0.08	0.12	0.12
Observations	6244	6244	6244	6244

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. Average municipality characteristics are calculated for each municipality-year cell. These characteristics are regresed on an daycare indicator taking the value one if a daycare exist in the specific municipality in the given year and zero otherwise. Each row represents a dependent variable and each column represents a model specification.

Table 4: Outcome: Log-earning at age 35 measured in 2011-US$

	(1)	(2)	(3)	(4)
OLS	0.054***	0.048***	0.048***	0.048***
	(0.003)	(0.003)	(0.003)	(0.003)
First stage	0.101***	0.054***	0.053***	0.054***
	(0.005)	(0.004)	(0.004)	(0.004)
Reduced form	0.036***	0.014***	0.014***	0.014***
	(0.006)	(0.005)	(0.005)	(0.005)
IV	0.357***	0.260***	0.259***	0.254***
	(0.058)	(0.082)	(0.082)	(0.082)
Year dummies	Yes	Yes	Yes	Yes
Municipal FE	No	Yes	Yes	Yes
Linear grouped trends	No	No	Yes	Yes
Quadratic grouped trends	No	No	No	Yes
F-stat excluded instrument	345.22	173.09	170.21	170.08
Observations	761412	761412	761412	761412

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. Clustered at municipality level. Each column represents a model specification.

Table 5: Outcome: Years of education at age 35

	(1)	(2)	(3)	(4)
OLS	0.333***	0.320***	0.320***	0.320***
	(0.017)	(0.014)	(0.014)	(0.014)
First stage	0.101***	0.054***	0.053***	0.054***
	(0.005)	(0.004)	(0.004)	(0.004)
Reduced form	0.159***	0.070***	0.071***	0.071***
	(0.036)	(0.025)	(0.025)	(0.025)
IV	1.569***	1.299***	1.324***	1.323***
	(0.370)	(0.431)	(0.426)	(0.426)
Year dummies	Yes	Yes	Yes	Yes
Municipal FE	No	Yes	Yes	Yes
Linear grouped trends	No	No	Yes	Yes
Quadratic grouped trends	No	No	No	Yes
F-stat excluded instrument	345.22	173.09	170.21	170.08
Observations	761412	761412	761412	761412

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. Clustered at municipality level. Each column represents a model specification.

Table 6: Outcome: High school completion

	(1)	(2)	(3)	(4)
OLS	0.036***	0.036***	0.036***	0.036***
	(0.002)	(0.002)	(0.002)	(0.002)
First	0.101***	0.054***	0.053***	0.054***
	(0.005)	(0.004)	(0.004)	(0.004)
Reduced	0.016***	0.010***	0.010***	0.010***
	(0.005)	(0.003)	(0.003)	(0.003)
IV	0.154***	0.185***	0.182***	0.183***
	(0.048)	(0.057)	(0.057)	(0.057)
Year dummies	Yes	Yes	Yes	Yes
Municipal FE	No	Yes	Yes	Yes
Linear grouped trends	No	No	Yes	Yes
Quadratic grouped trends	No	No	No	Yes
F-stat excluded instrument	345.22	173.09	170.21	170.08
Observation	761412	761412	761412	761412

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses

Table 7: Outcome: No more than basic schooling

	(1)	(2)	(3)	(4)
OLS	-0.018***	-0.016***	-0.016***	-0.016***
	(0.001)	(0.001)	(0.001)	(0.001)
First	0.101***	0.054***	0.053***	0.054***
	(0.005)	(0.004)	(0.004)	(0.004)
Reduced	-0.013***	-0.007***	-0.006**	-0.006***
	(0.002)	(0.002)	(0.002)	(0.002)
IV	-0.133***	-0.121***	-0.117***	-0.118***
	(0.024)	(0.031)	(0.031)	(0.031)
Year dummies	Yes	Yes	Yes	Yes
Municipal FE	No	Yes	Yes	Yes
Linear grouped trends	No	No	Yes	Yes
Quadratic grouped trends	No	No	No	Yes
F-stat excluded instrument	345.22	173.09	170.21	170.08
Observation	761412	761412	761412	761412

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses

Table 8: Log-earnings. Sample is split at different years between siblings.

	Total pop	Space>4	Space>5	Last born
OLS	0.048***	0.039***	0.033***	0.028***
	(0.003)	(0.003)	(0.003)	(0.004)
First	0.054***	0.058***	0.061***	0.060***
	(0.004)	(0.004)	(0.004)	(0.004)
Reduced	0.014***	0.013***	0.011**	0.008
	(0.005)	(0.005)	(0.005)	(0.005)
IV	0.254***	0.225***	0.179**	0.135
	(0.082)	(0.077)	(0.077)	(0.089)
Year dummies	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes
Linear grouped trends	Yes	Yes	Yes	Yes
Quadratic grouped trends	Yes	Yes	Yes	Yes
F-stat excluded instrument	170.08	187.23	207.71	230.35
Observation	761412	542083	475177	346509

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. Clustered at municipality level.

Table 9: Log-earnings: Child-care exposure and maternal employment measured at age three, four and five

	Age 3	Age 4	Age 5
OLS	0.049***	0.048***	0.052***
	(0.003)	(0.003)	(0.003)
First	0.047***	0.054***	0.054***
	(0.004)	(0.004)	(0.004)
Reduced	0.010**	0.014***	0.017***
	(0.005)	(0.005)	(0.005)
IV	0.218**	0.254***	0.309***
	(0.093)	(0.082)	(0.084)
Year dummies	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes
Linear grouped trends	Yes	Yes	Yes
Quadratic grouped trends	Yes	Yes	Yes
F-stat excluded instrument	127.69	170.08	171.26
Observation	761412	761412	761412

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. Clustered at municipality level.

Table 10: Log-earnings. Sample is split by mothers education.

	Low	Medium	High
OLS	0.019***	0.010**	0.045***
	(0.005)	(0.003)	(0.005)
First	0.053***	0.047***	0.016***
	(0.004)	(0.005)	(0.005)
Reduced	-0.004	0.016**	0.018**
	(0.006)	(0.007)	(0.007)
IV	-0.071	0.347**	1.096**
	(0.108)	(0.136)	(0.522)
Year dummies	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes
Linear grouped trends	Yes	Yes	Yes
Quadratic grouped trends	Yes	Yes	Yes
F-stat excluded instrument	202.91	95.60	10.91
Observation	332389	285903	142959

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. Low: Up to (not including) 10 years of education, e.i. basic education. Medium: 10 to 13 years of education (both included), e.i. high school and vocational education. High: Above 13 years of education, e.i. college or university.

Table 11: Log-earnings. Sample is split by fathers education.

	Low	Medium	High
OLS	0.043***	0.030***	0.042***
	(0.005)	(0.005)	(0.003)
First	0.061***	0.039***	0.030***
	(0.005)	(0.005)	(0.004)
Reduced	-0.012*	0.014*	0.014**
	(0.006)	(0.008)	(0.006)
IV	-0.190*	0.371*	0.483**
	(0.104)	(0.203)	(0.203)
Year dummies	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes
Linear grouped trends	Yes	Yes	Yes
Quadratic grouped trends	Yes	Yes	Yes
F-stat excluded instrument	176.35	60.04	49.36
Observation	273842	160517	313670

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. Low: Up to (not including) 10 years of education, e.i. basic education. Medium: 10 to 13 years of education (both included), e.i. high school and vocational education. High: Above 13 years of education, e.i. college or university. 1.8 percent of the sample has an unknown father.

Table 12: Log-earnings. Sample is split by equalized household income measured at age 17.

	Low	Medium	High
OLS	0.026***	0.015***	0.020***
	(0.006)	(0.003)	(0.003)
First	0.048***	0.039***	0.027***
	(0.004)	(0.004)	(0.004)
Reduced	-0.006	0.014**	0.007
	(0.007)	(0.006)	(0.006)
IV	-0.143	0.355***	0.250
	(0.140)	(0.136)	(0.242)
Year dummies	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes
Linear grouped trends	Yes	Yes	Yes
Quadratic grouped trends	Yes	Yes	Yes
F-stat excluded instrument	142.84	76.24	39.82
Observation	253804	253780	253701

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Clustered standard errors in parentheses. The sample is split by the 33rd and 66th percentile of equalized household income. The household income is equalized by household size, where the second parent has weight 0.7 and each child has weight 0.5.