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

Paul Bingley
Vibeke Myrup Jensen
Sarah Sander Nielsen

June 21, 2015

SFI - The Danish National Centre for Social Research, Herluf Trolles Gade 11, 1052 København K, Denmark

PRELIMINARY WORK - PLEASE DO NOT CITE.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) .

JEL no. J13, J21, J22, H40

*Acknowledgments: We acknowledges financial support from the Danish Agency for Science, Technology and innovation (grant DSF-09-070295). We are grateful for the comments of Thomas Dee, Bo Honore, Miriam Wüst, Marianne Simonsen, Rasmus Lentz and participants at the Copenhagen Education Network workshop, October 2014.

1 Introduction

Today, female labor participation is high even for women with smaller children but this high level of participation raises the question: in the long run, is this good or bad for the children? For policy makers (and parents), the 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 effects of maternal labor supply and child care on child outcomes. In particular, these studies investigate the direct effect of child care and parental leave on either child outcomes or maternal labor supply, e.g., 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 primarily on children's short- or medium-run outcomes. Thus, we know little about the long-run effects of maternal employment. We try to close this gap by estimating the effect of maternal employment while the children are four years old on children's schooling and earnings outcomes at age 35.

For identification, we exploit variation in the supply of child care across neighborhoods. In 1964, to meet the demand for female labor supply during the economic upturn, the Danish government began a program of daycare expansion.[1] Child care expanded rapidly but varied across the local child-care authorities and neighborhoods. Thus for the period 1966-1976, we use these differences in child care availability across neighborhoods and time as an instrument for maternal labor supply.

For two reasons, Denmark is ideally suited for exploiting such a natural experiment. First, concurrent with the economic upturn in the 1960s, Denmark introduced universally accessible child care. Thus we investigate child outcomes such as earnings 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

[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.

of all 3-6-year-old enrolled children were in private care.[2]

We identify maternal employment as those who are working more than 33 percent of full time (the mean) and find that maternal employment increases schooling by 0.5-1.3 years and log earnings by 8-25 percent.

To validate our results we deal with three issues. First, as the local child care authorities (municipalities) choose the timing of child care openings, child care availability is non-random and in fact highly correlated with parental education and parental household income. However, as suggested by Holmlund (2008) for the case of school expansions in Sweden, adding linear and quadratic trends to this correlation minimize the point estimates in the correlation between child care openings on the left-hand side and municipality-level maternal work frequency, parental education or equalized household incomes on the right-hand side. Thus to balance our sample of treated and untreated neighborhoods, we add these trend specifications to our main model.

Second, in the middle of our period, municipalities were collapsed from 1098 to 277 municipalities. Among other things, this collapse affected the priority of future child care openings in the municipalities.[3] As we use variation within the municipality, this 1970 municipality reform is less likely to directly affect our point estimates, but the reform changes our trend specification.We take this issue into account by grouping the municipalities according to early or late implementation of formal child care (into ten categories). Using this regrouping of the municipalities, we estimate municipality-level trends.

Third, we investigate a potential threat to the exclusion restriction. As we, in contrast to other studies, observe maternal working hours, we know that some mothers are working although there are no child care facilities in the neighborhood, i.e., the always takers. This evidence suggests that informal care existed, but we have no direct evidence of children changing

[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.

[3]The main reason for the 1970 municipality reform was that the units were simply to small to accomodate some of the required wellfare goals such as minimum nine years of schooling in both the rual and urban areas (implemented in 1957).

status from informal to formal care when formal child care becomes available. Nonetheless, such a potential change from informal to formal care once formal child care becomes available violates our exclusion restriction, and this would spuriously load onto hours effects. To accommodate this risk, we simulate what happens to our estimates if we either drop or randomly allocate children of working mothers in neighborhoods with no child care available into other neighborhoods in the munucipality where we observe child care. From this exercise we find that dropping the always takers reduces the effect of maternal employment to 8 percent on earning and 0.5 years of schooling.

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 section 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 takes into account both maternal working hours and child care availability.

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) sum-

maries the literature on parental leave on child outcomes for children below the age of two. They conclude that most studies focus on the effects of maternity leave reforms on female labor supply, fertility decisions and to some extent 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 in Norway. They find that mother and child spending more time together has a positive effect on child wages at age 30 and on children's high school graduation. 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 programs available for the general population. 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. As most of these programs target disadvantaged families, results are difficult to generalize to the general population.

The second branch consists of studies analyzing the effects of institutional child care that is made generally available. 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 Simon-

sen (2010, 2012)exploit regional variation in the take-up of preschool during the 1990's 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 outperforms 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 generally available child care for children aged 3 to 6 across local child care authorities in Germany. Felfe et al. (2013) utilize an expansion of 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 on education and labor market outcomes. However, they find no 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. In this section, we describe the development of child care and motivate that the roll-out of child care in Denmark during the 1960s and 1970s, under certain conditions, generates exogenous variation in access to child care and thus can be a relevant instrument for maternal labor supply.

3.1 The 1964 child care reform

In particular, the great economic upturn in the 1960s spurred the demand for access to publicly funded child care for all families in Denmark, but even before that time, Denmark had a history of subsidized child care to support the demand for female labor supply. Figure 1 shows the development of 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 axis), and the blue dots (right axis) defines the female share of the labor force. The two vertical dotted lines define our period of interest (1967 to 1979), and the vertical solid line defines the 1964 child-care reform.

Figure 1 shows that the number of child-care institutions slowly increased from about 100 to 300 institutions during 1920-1945. In 1919, the Danish government started subsidizing private child care initiatives (up to 30 percent of the running costs).[4] In two ways, this initiative supported the demand for female labor supply. First, by increasing access for working-class women whom could not otherwise afford child care. Second, by preventing current institutions from closing due to very low budgets. However, as the economic downturn in the 1920s had the highest negative impact on female workers, the 1919-reform had little effect on the level of child care.[5] After 1945, in the post-war period, 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 doubled from 1945 to 1960 (Bingley and Westergård-Nielsen, 2012; Korremann, 1977; Ploug, 2012).

[4]Only private child care facilities existed before 1919 and the first one opened in 1828. 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 off 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 (Korremann, 1977).

[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) In 1933, the expansion of child care was further motivated. Not only running cost, but also construction costs for establishing new institutions were subsidized.

After 1960, both the female share of the labor force and the number of child-care institutions increased rapidly 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, regardless of 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 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. Thus 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 would be an 18 persons net increase in female labor supply (Korremann, 1977).

The 1964 reform led to an increase in the number of institutions. However, not until after 1966, the 1964 reform had full potential as a general building-stop in city areas prevented municipalities to build new institu-

[6] Until 1964, an institution was eligible for full subsidy if two-thirds of the children were from low-income households, otherwise half the subsidy. After 1964, additional support for poor households remained but the subsidy followed the income of the individual families in contrast to the share of poor families in the institution. 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.

tions.[7] Totally from 1966 to 1976, the number of institutions tripled from 800 to 2400 institutions and the female share of the labor force increased from 34 percent to 42 percent during the same period (see Figure 1).

3.2 Variation across local child-care authorities

Our identification strategy relies on an as-good-as random implementation of child-care opportunities across time and municipalities during 1966-1976 and Figure 2 maps the roll-out of child-care institutions in two-year intervals. The lighter colors define the municipalities where child-care institutions opened at the beginning of the period, and the darker colors define later periods. Not surprisingly, municipalities with larger cities such as Copenhagen (the Capital), and Aarhus (2nd largest city) where the expansion of production happened first, had child-care institutions before 1966, whereas the rural areas containing mainly farming and smaller businesses implemented child care last. Nonetheless, between these extremes, there is variation where neighboring municipalities roll-out child care in different periods.[8]

As suggested by Figure 2, the implementation of child care across municipalities is non-random and thus likely correlated with parental characteristics.[9] Therefore, we follow Black et al. (2005); Holmlund (2008) and check in Table 1 whether municipality-level parental characteristics correlate with the timing of child-care openings in the municipality.

Table 1 shows four model specifications for the correlation between a dummy for formal child care and parental characteristics (maternal and paternal schooling, family income and maternal work frequency). The first column has the simplest specification (including year dummies) and the fourth

[7] The stop was implemented in 1960 to encourage production instead of construction.

[8] Municipalities dealt with the demands of providing child care in various ways. As children are in care at different times during the day, in 1973 the local authority in one municipality summoned that the existing child-care institutions used only 80 percent of their capacity. Consequently, the local authority suggested enrolling eight percent more children per institution.

[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.

has the most comprehensive specification (including year and municipal dummies and linear and quadratic trends).

When we in column one just include year dummies in the model, the correlation between child care and parental characteristics is highly significant. For example, one extra year of maternal (paternal) education increases the probability of a child care facility in the neighbourhood by 12.2 (12.3) percentage points. Nonetheless, when we in column two control for year and municipality fixed effect, maternal and paternal education can no longer predict child-care openings. However, maternal education is still positively correlated with child-care opening.

When we control for trends (linear or quadratic), none of the parental characteristics are able to predict the dummy for child care in the municipality. For example, one additional year of maternal (paternal) education increases the probability of child-care by 0.6 (0.4) percentage points and is no longer statistically significant. Thus we argue that conditional on trends specific to the timing of child-care implementation in the municipality, the reform causes a staggered introduction of subsidized child care and this staggered introduction is an exogenous source of variation for maternal employment. [10]

In addition, we expect that the combined linear and quadratic trend specifications also takes into account the variation in child-care quality across

[10] More specifically we calculate the trends after we group the municipalities according to the year, they implement child care. We do this grouping, because a structural reform in the middle of our period (1970) merged the existing 1098 municipalities to 276. The merge was economically motivated, as 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 to some extend facilitated these services for the surrounding rural areas but did not get equivalent tax payment from the people living in these rural areas. Consequently, municipalities were 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), the 1970 municipality reform has implication for our identification strategy, and in particular, our specification of the municipality level trends. To overcome this issue, we group the municipalities according to the year they implement child care and and use this ranking to generate the linear and quadratic trends. As an additional robustness check, we run our main model using the post 1970-reform units throughout our period. The results are robust to this structural change.

time within each municipality. The rapid increase in the number of child-care institutions likely affected the quality of care in these institutions and Table 2 confirms these changes in quality. 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.

3.2.1 Parental demand driven child-care expansion?

The increasing expenditure on child care (as well as many other concurrent increases in welfare services) was financed by an increasing tax-level. 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 child-care 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 that likely affected the quality of care in these institutions.

If the roll-out of child care is largely driven by parental demands for child care, the staggered expansion is invalid as an instrument for maternal employment. Earlier in this section, we argue that the expansion of public child care was mainly driven by labor market demands as this was a mean to attract more women to work. Nonetheless, from 1963 to 1970, the interests of pedagogues and parents worked hand in hand with the demand for increased female labor force. But after 1970 where budget cuts started to kick in, pedagogues and parents had little power in the general process of planning child care.

For example, in 1972, the union of pedagogues demanded better working conditions and higher pedagogue/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 pedagogues per child. Thus, this evidence suggest that the overall changes in public child-care availability were mainly driven by the government (and thus labor market demands) than parental demands (Korremann (1977)).

3.2.2 Outside options?

Private child care[11] 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.

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 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:

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

[11] Also called family day care in other papers.

$$WORK^m = \alpha_0 + \alpha_1 CHILDCARE + \alpha_2 MALE + \alpha_3 URBAN + \alpha_4 COHORT + \alpha_5 MUNICIPAL + \alpha_6 TREND + v \quad (2)$$

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 (2) by $TREND$.[12] 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. 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

[12] 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.

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.

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. We show that parental characteristics do *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. The exclusion restriction states that there should be no direct effect of child care opening on adult outcomes other than that through maternal employment. This assumption is invalid if there is a substitution from informal care to formal care and no change in maternal hours, which is the Norwegian case. As we have no individual level data on enrollment in formal child care, we

can only render probable that in the Danish case the substitution from informal to formal care was only minor. In this analysis we identify the local average treatment effect (LATE) of maternal employment on child long-run outcomes for the group of compliers, e.i. mothers who are induced to change their work hours from below one-third to above one-third full time full year employment because of child-care availability.

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 for this time period. 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

The data set uses all children born between 1963 and 1975 residing in Denmark at age 35 and their parents. The data combine various administrative registers from Statistics Denmark and indicators of child-care availability

from historical records. Through the unique personal identifier in the registers, parental demographic characteristics, schooling, working hours and income is matched to child demographic characteristics and their adult outcomes (earnings and schooling).

From the historical records published by the National Board of Social Services (i.e., annual reports), we collect information about child-care institutions in Denmark for the period 1966-1971 and 1974. From 1976, we find information about child-care availability in the administrative registers. Together, these sources give us a panel of eligible institutions in Denmark from 1966 through 1980.

To generate this child-care institution data set, we use the following data steps. First, we digitize these historical records for 1966-1971 and 1974. At an annual basis, these records contain a unique institution identifier, type of child care, institution address and number of slots per institution.

Second, to bridge the period 1971-1974, we use the additional information about date of establishment in the 1974 report. Thus for those institutions where date of establishment lies in 1972 and 1973, we assume that all the information we have about the institution in 1974 is consistent for 1972 and 1973.

Third, we bridge the 1974 records and the administrative records after 1975 by assuming that all institutions existing in 1974 and 1976 also existed in 1975. If these institutions didn't exist in the 1974 data but in the 1976 data, we assume they opened in 1975.[13] The final child-care panel data set runs from 1966-1980.

We link this child-care data set to children's parish of birth using the institutional address.[14] At this time, most mothers gave birth at home or near their home. We thereby assume that the birth registration place corresponds to the neighborhood where the mother lived at time of birth.

Our original data set includes all children born in Denmark between

[13]For this final bridge between the historical records and the institution register, we match institutions on their address, because the institution identifier changes format from a 6-digit to a 5-digit code in 1976.

[14]Unfortunately, we do not have exact address information on families and individuals in 1966-1980 as we have on child-care institutions.

1963-1975 and living in Denmark at age 35. From this data set, we make the following three exclusions. First, we exclude individuals with an invalid birth registration code (xx percent).[15] Second, we exclude children without maternal identifier at age 35? (xx percent). Third, as variation in child-care availability arises from parish-level differences between children in the same cohorts, we exclude families from very small municipality-year cells (less than five children). Our final data set contains 761,412 children and their mothers.

Our outcomes are log earnings and length of schooling measured at age 35 for all children in the sample. To generate log earnings, we use annual earning according to the tax authorities. Thus we exclude income from unemployment insurance or other social benefits. For length of schooling we use the expected length of schooling for highest completed level of education (measured in years).

Maternal employment is our explanatory variable of interest. To define maternal employment back to 1964, we use information from tax-payers' contributions to a mandatory pension system (ATP). Contributions to the ATP system vary according to hours worked.[16] Thus individuals pay full contribution for full time employed equivalent to 30 hours of work or more and one-third payment for only working 33 percent full time, full year.

For our period, unemployed, self-employed, and persons out of the labor force were not a part of the ATP-system and thus the ATP contributions is a proxy for whether the mother is away from home. If a mother spends the primary part of her time working in the labour market, we 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, equivalent to working more than 33 percent full time.

For co-variates, we include number of siblings to take into account household size and a dummy for urban or rural area as both maternal employment and child-care availability vary across the country. We also control

[15] This could be just a municipality-identifier instead of parish of birth, an unknown parish of birth.

[16] We can also distinguish between employer and employee payments thus we

for gender for differences in job and earning opportunities as adults.

In the robustness checks we also examine heterogeneous effects and we stratify the sample with respect to parental education and equivalized household income.

5.2 Descriptive statistics

Table 3 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 local daycare availability 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 3 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 attaining 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 show 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 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 effects of maternal labor supply on children's adult outcomes. For the effect on log earnings, Table 4 shows the OLS results, the first stage, the reduced form, and the second stage estimates. To address possible concerns about selection bias or other bias due to policy changes concurrent with the implementation of child care, we report estimates for four different model specifications. In the first specification (column 1) we include covariates and year dummies in the model, in column (2), we include year dummies and municipality FE, whereas we in column (3) and (4) include linear grouped and quadratic grouped municipality trends, respectively.

The OLS results suggest that maternal employment when the child is four correlates with 4.8 percent higher earnings compared to that of children where the mothers to do not work outside the household. The result is quite robust across specifications.

The first stage estimation results reflect the effect of child care availability on maternal labor supply. The first stage estimation suggests that mothers having 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 relevant instrument for maternal labor supply. A 5.4 percentage points effect might seem like a relatively small effect. However, in the light of only x 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 at age four increases children's earnings by 25.4 percent at age 35.

Similar to Table 4, Table 5 shows the effect on length of schooling. For schooling, the OLS results suggest that maternal employment at age four correlates with 0.3 additional years of schooling (or equivalently 3.8 months of education) compared to that of children where the mothers to do not work outside the household. The reduced form estimate shows that the effect of child-care availability increases length of schooling by 0.7 years once we add the full set of trend specifications (column 4). The IV estimate, on the other hand, suggests that maternal employment increases length of schooling by 1.3 years.

In table 6, we examine the effect of maternal employment on children's probability of completing high school. The IV estimate shows that maternal employment increases the probability of high school completion by 18.3 percentage points. Similarly, table 7 shows that maternal employment reduces the probability of only having the basic amount of schooling (9 years) 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 length of schooling.

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 who 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.[17] 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.

Previous literature finds that children of low socioeconomic background benefit most from attending institutionalized child care while their parents are working. We examine whether such heterogeneous effects are

[17]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).

present by stratifying the sample by parental education.

6.3 Imposing the exclusion restriction

According to the definition of maternal employment and child care availability we use, there are 60K children with an employed mother and no child care available. This suggests that somewhere between 0 and 20 % of the children has a working mother even though there is no childcare in the neighborhood. Thus, a child-care opening that changes the mode of care from informal non-parental child care to formal institutionalized child care will violate the exclusion restriction. We use two approaches to examine how much this is biasing the IV estimates. First, we remove the potentially contaminated from the sample in groups of 3000 (10 % of potentially contaminated). Second, we re-sample the potentially contaminated controls to the treatment group. We do this by moving 3000 at the time to a neighborhood within the same municipality, where child care was available when the child was four years old. By doing this exercise the exclusion restriction becomes less and less binding. Thus, the child care effect is to a lesser extent spuriously loaded on maternal employment.

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 generally 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 that maternal employment has a positive effect on both offspring 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.

References

Angrist, J. D. and J.-S. Pischke: 2009, *Mostly Harmless Econometrics. An Empiricist's Companion*. Princeton University Press.

Baker, M., J. Gruber, and K. Milligan: 2008, 'Universal child care, maternal labor supply, and family well-being'. *Journal of Political Economy* **116**(4), 709–745.

Bingley, P. and N. Westergård-Nielsen: 2012, *Intergenerational Transmission and Day Care*, Chapt. 8. Russell Sage Foundation.

Black, S. E., P. J. Devereux, and K. G. Salvanes: 2005, 'Why the apple doesn't fall far: Understanding intergenerational transmission of human capital'. *The American economic review* **95**(1), 437–449.

BUPL: 2003, 'Paedagogprofessionens historie [Pedagogical history]'. http://www.bupl.dk/paedagogik/paedagogprofessionen/paedagogprofessionens_historie? OpenDocument.

Carneiro, P., K. V. Løken, and K. G. Salvanes: 2015, 'A Flying Start? Maternity Leave Benefits and Long-Run Outcomes of Children'. *Journal of Politi* **123**(2), 365–412.

Carneiro, P., K. V. Loken, and K. G. Salvanes: 2011, 'A flying start? Maternity leave benefits and long run outcomes of children'. Discussion paper series 5793, Bonn.

Currie, J. and D. Almond: 2011, 'Human capital development before age five'. *Handbook of labor economics* **4**, 1315–1486.

Datta Gupta, N. and M. Simonsen: 2010, 'Non-cognitive child outcomes and universal high quality child care'. *Journal of Public Economics* **94**(1), 30–43.

Datta Gupta, N. and M. Simonsen: 2012, 'The effects of type of non-parental child care on pre-teen skills and risky behavior'. *Economics Letters* **116**(3), 622–625.

Dustmann, C., A. Raute, and U. Schönberg: 2012, 'Does universal child care matter? evidence from a large expansion in pre-school education'. Technical report, mimeo, UCL.

Dustmann, C. and U. Schönberg: 2012, 'Expansions in maternity leave coverage and children's long-term outcomes'. *American Economic Journal: Applied Economics* **4**(3), 190–224.

Felfe, C., N. Nollenberger, and N. Rodríguez-Planas: 2013, 'Can't buy mommy's love? Universal childcare and children's long-term cognitive development'. Technical report, CESifo Working Paper: Economics of Education.

Havnes, T. and M. Mogstad: 2011a, 'Money for nothing? Universal child care and maternal employment'. *Journal of Public Economics* **95**(11), 1455–1465.

Havnes, T. and M. Mogstad: 2011b, 'No child left behind: Subsidized child care and children's long-run outcomes'. *American Economic Journal: Economic Policy* **3**(2), 97–129.

Holmlund, H.: 2008, 'A researcher's guide to the Swedish compulsory school reform'. LSE Research Online Documents on Economics 19382, London School of Economics and Political Science, LSE Library.

Horsten, H.: 1969, *Børne-og ungdomsforsorgen i Danmark [Child and Youth Services in Denmark].* Denmark: Nyt Nordisk Forlag Arnold Busck.

Korremann, G.: 1977, *Børnehave og socialisering: om kvinders erhvervsomfang og børnehavens udvikling efter 1960. [Kindergarten and Socialization: Women's Labor Supply and the Development of Kindergarten after 1960].* Denmark: Gyldendal.

Lefebvre, P. and P. Merrigan: 2008, 'Child Care Policy and the Labor Supply of Mothers with Young Children: A Natural Experiment from Canada'. *Journal of Labor Economics* **26**(3), pp. 519–548.

Liu, Q. and O. N. Skans: 2010, 'The duration of paid parental leave and children's scholastic performance'. *The BE Journal of Economic Analysis & Policy* **10**(1).

Meghir, C., M. Palme, and E. Simeonova: 2012, 'Education, health and mortality: Evidence from a social experiment'. Technical report, National Bureau of Economic Research.

Ploug, N.: 2012, 'The Nordic child care regime - History, development and challenges'. *Children and Youth Services Review* **34**(3), 517–522.

Rasmussen, A. W.: 2010, 'Increasing the length of parents' birth-related leave: The effect on children's long-term educational outcomes'. *Labour Economics* **17**(1), 91–100.

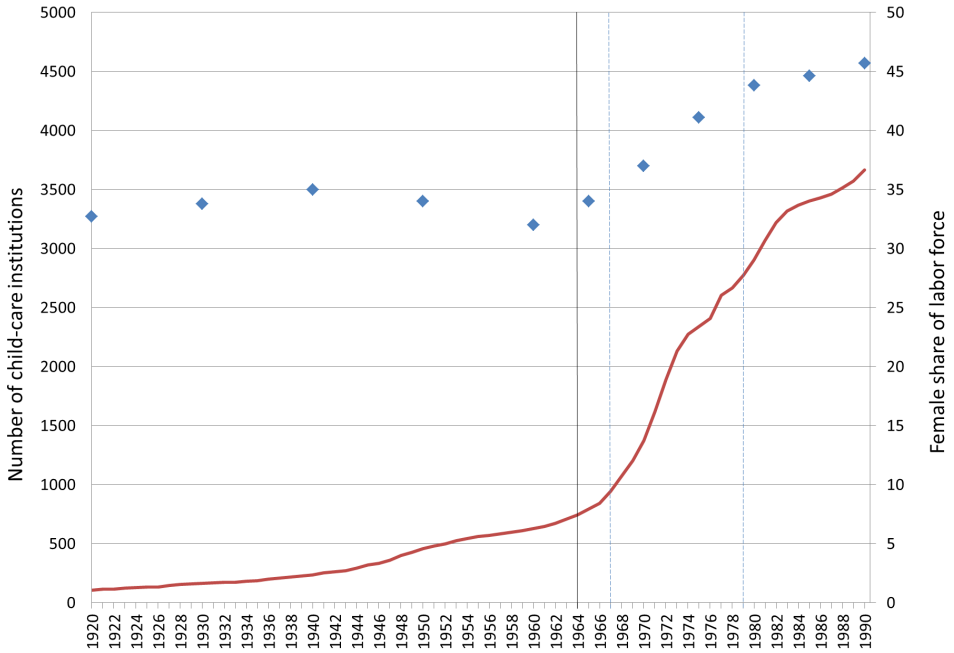
Rhode, P. W. and K. S. Strumpf: 2003, 'Assessing the importance of Tiebout sorting: Local heterogeneity from 1850 to 1990'. *The American Economic Review* **93**(5), 1648–1677.

Ruhm, C. and J. Waldfogel: 2012, 'Long-term effects of early childhood care and education'. *Nordic Economic Policy Review* **1**(1), 23–51.

Staiger, D. and J. H. Stock: 1997, 'Instrumental Variables Regression with Weak Instruments'. *Econometrica* **65**(3), 557–586.

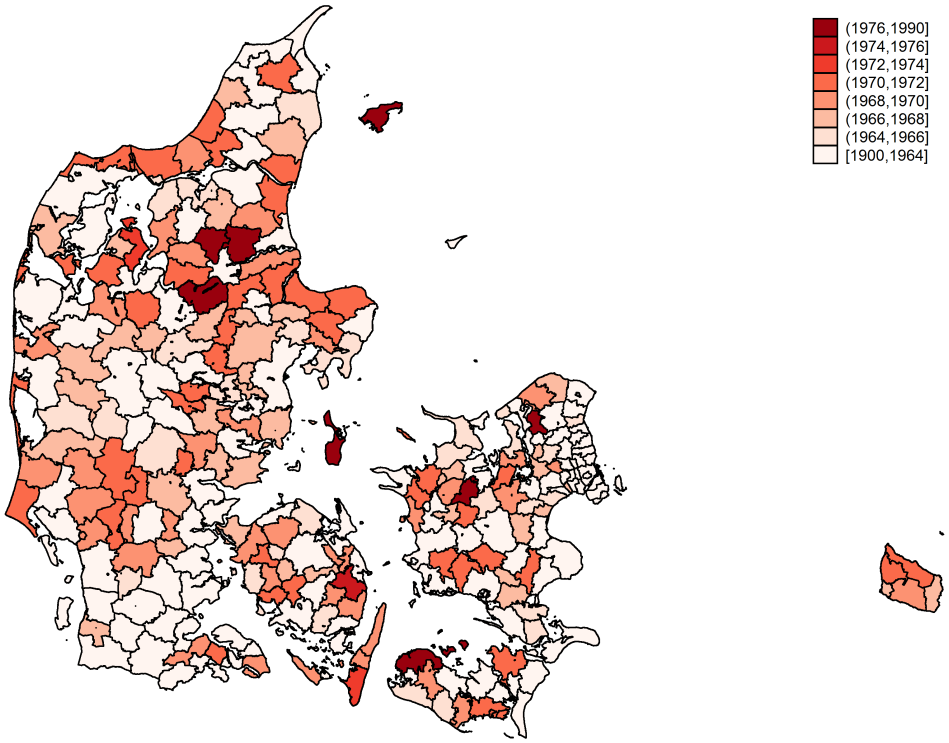
8 Tables and Figures

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



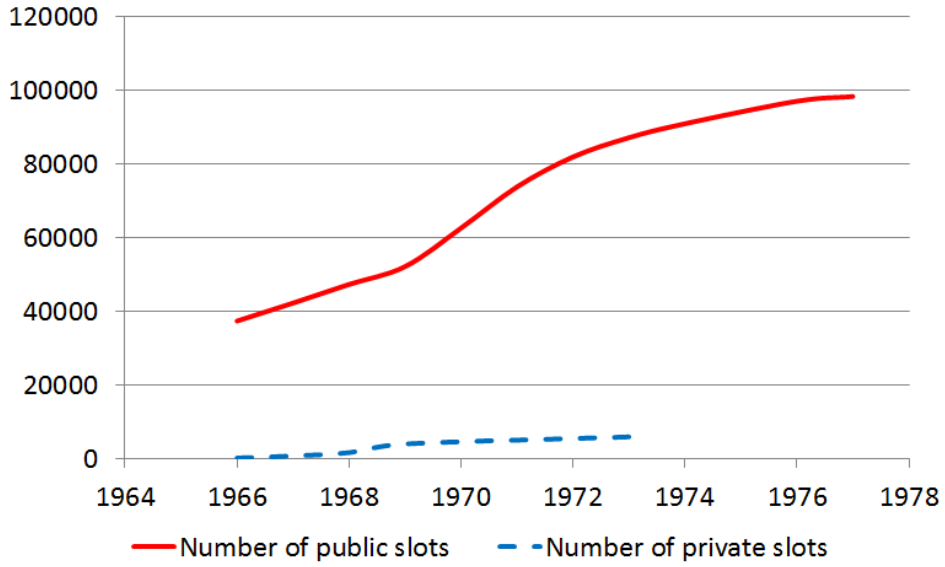
NOTE— The red line defines the number of child-care institutions for 3-6-year-olds (left-hand axis), and the blue dots (right-hand axis) defines the female share of the labor force. The two vertical dotted lines define our period of interest (1966 to 1980), and the vertical solid line defines the 1964 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 1: The correlation between child-care openings and parental characteristics

	(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 2: 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.

Table 3: 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 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.