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PhD Thesis

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Investments in Universal Early Childhood Education

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Contents

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Dansk introduktion

Denne ph.d.-afhandling består af tre selvstændige kapitler, der alle omhandler universel børnepasning. De tre kapitler supplerer hinanden ved at belyse tre forskellige vinkler af en stor investering i introduktionen af børnehaver i Danmark i slutningen af 1960erne og 70erne. Før 1964 tilbød det offentlige kun hjælp til børnepasning til enlige forældre og forældre, hvor begge parter blev nødt til at arbejde for at kunne overleve. Det blev der lavet om på i 1966, da man ændrede reglerne, så der nu var tilskud til alle. Dermed blev børnehaven en universel institution, hvor den tidligere havde været målrettet børn med ringere socioøkonomisk baggrund.

Det første kapitel undersøger, hvordan introduktionen af universel børnepasning i midten af 1960erne har påvirket børnenes uddannelsesniveau og lønindkomst som voksne. Resultaterne i det første kapitel peger på, at effekterne er størst for børn af højtuddannede mødre. Særligt deres sønner har gavn af børnehave. Resultaterne i det første kapitel er et nyt bidrag til litteraturen om langsigtseffekterne af børnepasning, da de tidligere studier har fundet, at børnepasning uden for hjemmet kan have negative konsekvenser for børn af højtuddannede. Forskellen mellem effekterne for børn af henholdsvis lavt- og højtuddannede mødre kan i den danske kontekst forklares ud fra forskelle i mødrenes arbejdsudbud. Introduktion af universel børnepasning har i højere grad fået flere lavtuddannede mødre til at begynde at arbejde i stedet for at gå hjemme, mens de højtuddannede mødre skiftede fra at arbejde deltid til fuldtid. Dermed er det skift i pasningsforhold, som børn af henholdsvis lavt- og højtuddannede mødre har oplevet, forskelligt.

Det andet kapitel dykker dybere ned i forholdet mellem mødres uddannelse, deres børns uddannelse og børnepasning uden for hjemmet. I mange lande er der siden Anden Verdenskrig sket en simultan stigning i kvinders uddannelsesniveau, kvinders arbejdsmarkedstilknytning og andelen af børn, der går i børnehave, hvilket har ændret interaktionen mellem mor og barn. Imidlertid er det for den enkelte person et valg at tage mere uddannelse og et valg at sende sine børn

i institution, hvilket vanskeliggør en analyse af hvordan, sådanne valg påvirker barnet på lang sigt. Kapitlet bruger skolereformen i 1958 som en eksogen kilde til variation i morens uddannelse sammen med geografisk variation i adgang til børnehave for barnet til at undersøge, hvordan børnehave påvirker sammenhængen mellem morens og barnets uddannelse. Resultaterne peger på, at et ekstra års uddannelse for moren hæver barnets uddannelse med to måneder. Samtidig er denne intergenerationelle effekt større for de børn, der har adgang til børnehave. Dermed forstærker børnehave den effekt, mødres uddannelse har på deres børns uddannelse. Dette resultat er foreneligt med teorien om komplementaritet mellem forskellige inputs, hvilket i det her tilfælde vil sige at blive passet af en højt uddannet mor i starten af livet og senere begynde i børnehave.

Det tredje kapitel ser nærmere på, hvordan muligheden, for at få passet børn i en børnehave, påvirker mødres karriere gennem hele livet. I kapitlet sammenlignes mødre, der bor et sted, hvor der er en børnehave med mødre, der bor et sted, hvor der ikke er en børnehave. Mødre, der har lettere mulighed for at få deres børn passet i en børnehave arbejder mere både deltid og fuldtid. Ligeledes har de en højere lønindkomst også mere end 30 år efter, de får deres første barn. Det tredje kapitel viser også, at mødre, der har mulighed for at få passet deres børn i børnehave, har en højere sandsynlighed for ikke at bo sammen med barnets far 16 år efter barnets fødsel, at de får færre børn og, at de venter længere tid med at få barn nummer to. Resultaterne i det tredje kapitel viser endvidere, at særligt højtuddannede mødre, der har lettere mulighed for at få passet deres børn i børnehave, har en højere lønindkomst, når deres børn er blevet voksne sammenlignet med højtuddannede mødre, der ikke har samme mulighed for at få passet deres børn i børnehave på trods af, at de arbejder lige meget, når børnene er blevet voksne. Det tyder således på, at den erfaring, der opbygges gennem jobs i de år, hvor man også får børn, lønner sig i det lange løb.

English introduction

This dissertation consists of three self-contained chapters on universal early childhood education. They complement each other by investigating three different perspectives of a large-scale investment in the rollout of universal daycare in Denmark during the late 1960s and 70s. Prior to 1964, subsidized childcare was targeted at single parents and parents, where they both had to work to make ends meet. This was changed in 1965, as the childcare regulations were changed to include subsidies to all families. Thus, by 1965, the Danish childcare system was changed from a targeted to a universal daycare system.

The first chapter examines how the introduction of universal daycare in the mid-1960s has affected children's educational attainment and earnings at age 35. The results in the first chapter point out that the effects are greatest for children of high-educated mothers, especially their sons benefit from universal daycare. The results in the first chapter are a new contribution to the literature on the long-term effects of universal childcare, since previous studies have found that childcare outside the home can have negative consequences for children of high-educated mothers. In the Danish context, the difference between the effects for children of low- and high-educated mothers can be explained by differences in the mothers' labor market participation. The introduction of universal daycare increases the probability of employment for mothers with no post-secondary education, while college-educated mothers primarily shift from part-time to full-time employment. For both groups, the long-run effects of daycare on maternal earnings and family income are five percent. Combining all of the results suggest that the mechanisms behind the long-run child outcomes are a shift from informal to formal care rather than increased household resources for children of college-educated mothers, and a shift from maternal to formal care for children of low-educated mothers.

The second chapter deepens the relationship between maternal education, the education of their children, and public provision of daycare. Increased female schooling, greater labor market participation, and the wider availability of day-

care in many countries have changed the way mothers interact with their children. However, for each individual, it is a choice to attain more education and a choice to enroll their children in daycare. These choices complicate an analysis of how maternal education and daycare affects the schooling of the offspring generation. This chapter exploits a Danish schooling reform affecting the maternal generation alongside differential access to daycare affecting the offspring generation, and identify the causal chain from maternal schooling, via daycare availability, to child's schooling. The results indicate that one more year of maternal schooling increases offspring schooling by two months. At the same time, this intergenerational effect is greater for the children who had access to daycare. Thus, greater daycare availability increases schooling transmission – consistent with the complementarity of early years of highly schooled maternal care followed by later institutional care provision.

The third chapter looks at how the opportunity to enroll children in daycare affect mothers' career over a lifetime. Specifically, mothers with daycare access during their firstborn child's pre-school years are compared to mothers without daycare access. The results show that universal daycare access affects mothers' labor force participation, full-time employment, and long-run earnings. Mothers with daycare access work more, both part time and full time. Likewise, they have higher earnings, even more than 30 years after they had their first child. The third chapter also shows that mothers with daycare access are less likely to live with the father of the child 16 years after the child was born, that they have fewer children, and that they wait longer between childbirths. For high-educated mothers, participation effects diminish over time, while earnings effects prevail in the long run. This indicates that the experience gained through jobs during child-rearing years is important for earnings in the long run.

Chapter 1

One Size Fits All? - Long-run Effects of Universal Daycare on Child and Mother Outcomes

One Size Fits All? – Long-run Effects of Universal Daycare on Child and Mother Outcomes*

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Abstract

Using the rollout of universal daycare in Denmark during 1967-79, we challenge the conventional wisdom that childhood interventions only benefit the underprivileged. We find that daycare availability mainly increases educational attainment and earnings of children of college-educated mothers, primarily their sons (earnings increase by five percent). Daycare also increases the probability of working for basic-educated mothers, whereas college-educated mothers primarily shift from part-time to full-time work. For both groups, the long-run effects of daycare on maternal earnings and family income are five percent. Suggested mechanisms are a shift from informal to formal care for children of college-educated mothers, and a shift from maternal to formal care for children of basic-educated mothers, rather than increased household resources.

JEL codes: J13, J21, J22, H40

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1 Introduction

Today, universal or subsidized daycare for pre-school children (aged three through six) is the norm in many OECD countries (OECD, 2016). However, we know surprisingly little about the long-run effects of universal daycare programs. Support for maternal employment motivated the first subsidized daycare programs (e.g., the Lanham Act of 1940 in the U.S. (Herbst, 2017)), whereas child development has motivated recent changes in many countries (e.g., the UK (Blanden et al., 2016)). The two existing studies investigating the earnings effects of universal child care call both of these motivations into question. Maternal labor supply effects are mixed, and while both studies find that universal daycare benefits children from low-SES households in the long run, there is also evidence that daycare harms children from high-SES households (Havnes and Mogstad, 2011b, 2015; Herbst, 2017).[1]

Differences in treatment, as well as alternative modes of care, partly explain the mixed results. In general, observational studies compare the outcomes for the treatment group with the outcomes for the treatment-as-usual (i.e., the control) group, and while the documentation of the daycare program characterizes the treatment, the less documented counterfactual mode of care equally determines the impact of the program. Large variations in the existing evidence demonstrate the importance of the quality of the counterfactual mode of care for universal daycare program evaluations. For example, both for short- and medium-run outcomes, Baker et al. (2008, 2015) find strong negative effects of universal child care.

In this paper, we contribute to the literature by estimating the long-run earnings effects for both the mother and her children. Additionally, our analysis of mechanisms at work reconciles our findings with the quite different headline results from Canada, Norway, and the U.S. We find support for the original motivations for universal daycare, as maternal employment increases in both the short and long run. We also find that children's long-run earnings increase and that daycare do not harm children.

[1] For the U.S., Herbst (2017) exploits state-wise variation in the introduction of a heavily-subsidized and universal daycare program that took place during World War II (i.e., the Lanham Act of 1940). Herbst (2017) finds positive effects for maternal employment when the child enters daycare and for a summary index of child outcomes measured around age 40 (such as employment, schooling and earnings). He finds the largest effects for the most disadvantaged families. Havnes and Mogstad (2011b,a, 2015) utilize an expansion of daycare in Norway beginning in 1976. They define the treatment group as regions with daycare provision above the median as a consequence of the reform, and find positive long-run effects on education and labor market outcomes. However, they find no effects on maternal employment and conclude that this suggests a shift from informal and potentially lower quality care to formal daycare.

For identification, we exploit a gradual rollout of universal daycare provision in Denmark induced by a universal daycare reform implemented in 1965. The economic upturn for Denmark beginning in the late 1950s, combined with the rapid expansion of the welfare state in the 1960s, increased the demand for both unskilled and skilled female labor. In 1964, to meet this demand, the Danish government changed the daycare regulations from a targeted and privately run initiative to a universal and publicly run daycare system. However, public building restrictions during the period 1960-66 caused the daycare reform to first become effective after 1966. As a consequence, the number of daycare facilities increased rapidly after 1966, but the rollout varied across local child-care authorities (municipalities) and local neighborhoods. Thus, for the period 1967-79, we use differences in daycare availability across neighborhoods and time within the municipalities to estimate Intention-To-Treat (ITT) effects of daycare on child and mother outcomes.

Denmark is an ideal setting in which to conduct such a natural experiment. First, the increased demand for both skilled and unskilled female labor affected most families with children of pre-school age and not just low- or high-skilled women.[2] Second, daycare was heavily subsidized and under national quality regulations, which diminishes quality differences between different areas and selection into treatment because of insufficient family income. Third, because we combine historical records with administrative records for educational attainment, employment, and demographics for all mothers and their children in our period of observation, we have a direct link between the mother and the child across time for all mothers, regardless of e.g. maternal marital status.

To solve the issues of non-random implementation of the reform, we show that the initial correlation between municipality-level parental characteristics (such as educational attainment) diminishes when we include linear and quadratic time trends. Thus, conditional on municipal intercepts and linear or quadratic time trends, daycare openings are balanced on pre-determined parental background characteristics.[3] Also, for further validation of our results, we follow Chetty et al. (2009) and conduct

[2]The first type of child care institutions in Denmark, asylums, go back to 1824. The focus of these institutions was to teach the children to be disciplined, clean, and obedient (Ploug, 2012). Until 1964, the private-run child care institutions were aimed for children from low-income families and contained no regulations for the quality of care. Rossin-Slater and Wüst (2016) analyze the rollout of targeted child care for 140 municipalities that had rolled-out child care in 1960 (out of the total of approx. 1300 municipalities) and find positive impacts on educational attainment, earnings, and survival rate.

[3]See Holmlund (2008) for an application of the differential trends approach to regional implementation of compulsory schooling laws in Sweden.

a non-parametric permutation test for each of the main outcomes. We randomly allocate daycare openings to neighborhoods and re-estimate the results 10,000 times. The p-values of our permutation tests are close to zero, indicating that the placebo daycare openings have no effect on child and mother outcomes.

For children exposed to daycare availability, we have three main findings. First, conditional on linear and quadratic trend specifications, children increase their length of schooling by one month. Second, children increase earnings by 1.2 percent and are more likely to belong to the top income quartile at age 35 (by 1.2 percent). Third, child educational attainment and earnings increase by mothers' educational level and are mainly driven by boys.[4]

For the average effect of children's educational attainment, our results are very similar to those found in Havnes and Mogstad (2011b). However, Havnes and Mogstad (2015) find that daycare exposure leads to higher earnings for children of low-educated mothers and an income penalty for children of high-educated mothers, while we find the largest effect for children of high-educated mothers.

For the mothers, we have four main findings. First, maternal employment increases by 13 percent when the mother is exposed to daycare availability when the youngest child is four.[5] Second, in contrast to Havnes and Mogstad (2011a), the labor supply increased the most for basic-educated mothers, whereas college-educated mothers primarily moved from part-time to full-time work. Summary statistics show that mothers with different levels of educational attainment follow similar employment trends throughout the period, but mothers with higher educational attainment have a higher employment rate to begin with.[6] Given that we find that the labor response to formal daycare availability is lower for college-educated mothers, this result suggests that these mothers were previously using other forms of non-parental child care. In turn, this result also signals, that children of college-educated mothers benefit more from daycare because the counterfactual mode of care was more likely

[4]While several papers find gender differences (e.g., Datta Gupta and Simonsen, 2010, 2016; Havnes and Mogstad, 2011b, 2015, a recent review concludes that there are no consistent pattern of gender differences in the effects of universal daycare (Dietrichson et al., 2018).

[5]This result is similar to that of Baker et al. (2008), as they find that the introduction of daycare in Quebec increased maternal employment by 14 percent. However, the result is in contrast to the close to zero effects of daycare availability for Norway by Havnes and Mogstad (2011a) and the 27 percent maternal employment increase found in the U.S by Herbst (2017). However, in the U.S. case, program participation was conditional on maternal employment.

[6]Overall 40.1 percent of the mothers in our sample are employed, however, the employment rate is lower for basic-educated mothers (31 percent); and higher for mothers with high school/vocational training (47 percent) and college/university education (65 percent).

informal or private care rather than maternal care. Third, for basic-educated mothers, we find that daycare availability at age four still increases maternal employment, when the child is 17 (but not for college-educated mothers). This result supports the earlier finding that basic-educated mothers are in general more affected by the expansion of daycare because they do not have other alternative modes of care. Fourth, conditional on working, the earnings of both basic- and college-educated mothers increase by approximately 5 percent when the youngest child is 17, whereas there is no effect for medium-educated mothers. In addition, family income increases by 3.8 percent for families in which the mother has basic education, a modest 2.2 percent for families in which the mother has high school or vocational training, and 4.0 percent for families where the mother has a college or university degree. These results are in line with historical reports showing that mainly the lowest and highest income groups use formal daycare (Korremann, 1977). Importantly, we also find that maternal labor market responses do not vary by child gender.

Taken together, because our offspring results are mainly driven by boys, but we find no gender differences in the maternal income effects, our results suggest that increased household financial resources during childhood do not play an important independent role in the causal chain from universal daycare to offspring outcomes. Moreover, as maternal employment effects also do not vary by child gender suggests, that maternal labor market activity per se, whether at the intensive or extensive margin, in the short run or long run, does not play an important independent role in the causal chain from daycare to offspring outcomes either. However, because the availability of daycare causes maternal labor supply to increase and household financial resources to increase, we cannot identify their independent roles in the causal chain; countervailing effects might cancel out, as in Black et al. (2014). They isolate the income effects at age five and find positive effects on children's grade point average (GPA) in junior high school. Nevertheless, our thorough analysis of maternal labor force participation informs us about the daycare effects, because of the mothers' role in providing the alternative mode of care.

The remainder of this paper proceeds as follows. Section 2 describes the institutional background. Section 3 focuses on the identification and empirical strategy, whereas Section 4 describes the data, and presents the descriptive statistics. Section 5 shows and discusses the results; Section 6 shows our robustness checks, and Section 7 concludes.

2 Institutional Background

Dating back to 1919, subsidized daycare has a long tradition in Denmark. In this section, we motivate that the rollout of universal daycare in Denmark after the 1964 reform under certain conditions generates exogenous variation in access to daycare.

2.1 From Targeted to Universal Daycare

Before 1965, a targeted daycare system existed in Denmark; however, in 1963, this system covered less than half of the need for daycare (Børne- og ungdomsforsorgens pædagogiske nævn, 1963; Bingley and Westergaard-Nielsen, 2012; Korremann, 1977).[7] To meet this need for care, the Danish parliament transformed the existing targeted care system into a universal daycare system in 1965 (see Lunn (1971) for Act no. 193 of 1964, implemented April 1965). Before the reform, an institution was only eligible for the full subsidy if two-thirds of the children were from low-income households; otherwise, the institution received half the subsidy. Private initiatives provided childcare, and although there were some child development ideas at that time, the primary purpose was to provide a holding place for the children while the parents worked (Ploug, 2012).

The universal daycare reform mandated four main changes to daycare that both affected the number of daycare slots and the quality of care. First, the reform untied subsidies to institutions, regardless of the proportion of children from low-income families (Lunn, 1971).[8]

Second, the purpose of daycare changed from an entirely labor market oriented to a partly child development and a partly labor market oriented perspective. This change was a response to the political discussions surrounding quality of care and child development in daycare. For example, prior to the reform, the institutions were not defined as daycare institutions, but as social welfare or preventive care insti-

[7] In 1963, the number of children on the waiting list for a half-day daycare slot was 116 percent of the number of enrolled children. For full-time slots, the number of children on the waiting list was 70 percent of the number of enrolled children.

[8] After 1964, additional support for poor households remained, but the subsidy followed the income level of the individual families in contrast to the share of poor families in the institution. Parents with an annual family income below 52,700 USD (approximately 15 percent below the mean) could apply for a fully funded daycare slot. On average, parents paid annual running costs equivalent to 3 percent of the average family income (1,920 USD) (Horsten, 1969). Throughout this paper, all monetary figures are given in fixed 2016 prices.

tutions, primarily for single parents and low-income families who could not afford proper private care (Korremann, 1977).[9]

Third, housing costs changed from subsidized costs to fully publicly funded 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 daycare institutions. Thus, after 1965, daycare became an important instrument to boost local labor supply. Prior to the reform, the Ministry of Finance calculated that the product of 100 extra daycare slots was an 18 person net increase in female labor supply (Korremann, 1977).

From 1920 through 1990, figure 1 shows the development of the number of daycare institutions (solid line, left axis) and the female share of the labor force (dots, right axis). The two vertical dotted lines define our period of interest (1967-79), and the solid vertical line marks the change from targeted to universal daycare in 1965. The figure shows that the number of daycare institutions increased slowly until 1965 but increased rapidly after that. However, the reform did not reach its full potential before 1966 because the government prohibited local childcare authorities from funding public construction during 1960-66.[10] Thus from 1956 to 1966, the number of institutions increased by 274 (from 569 to 843 institutions), but from 1966 to 1979, this number more than tripled to 2,772 institutions.[11]

2.2 Female Labor Force Participation

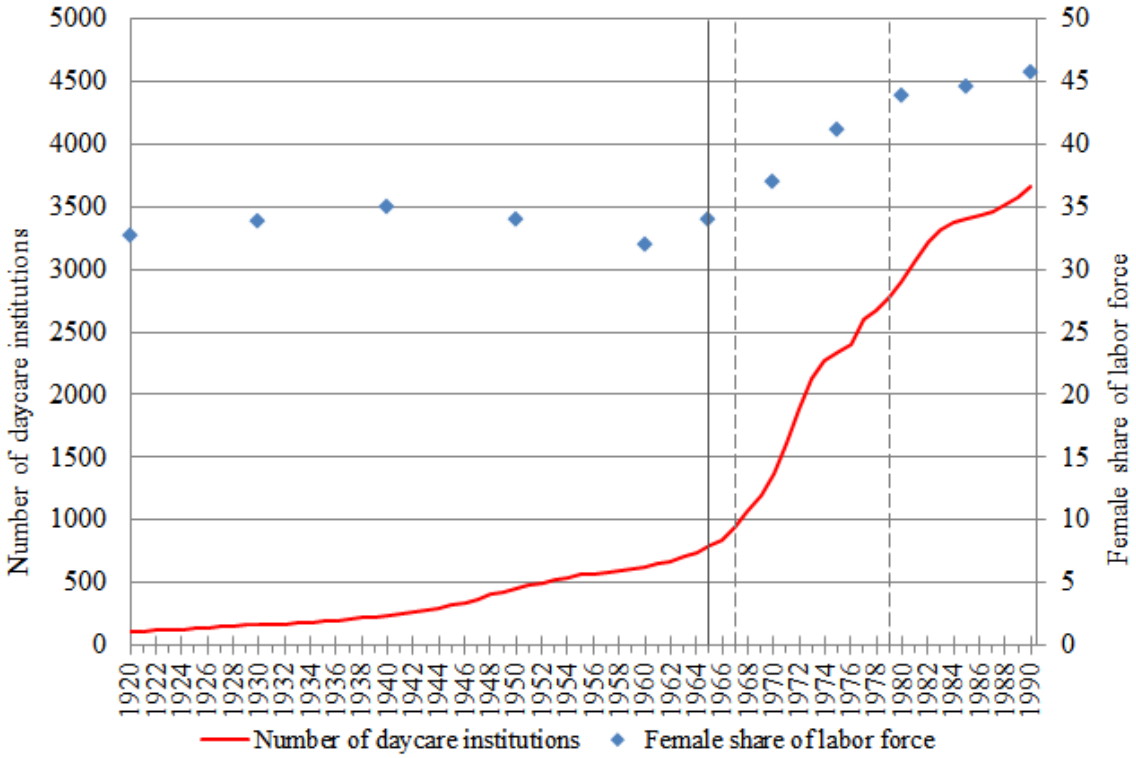
Daycare was a means to support women's entry into the labor market, and Figure 1 shows a similar trend in the female share of the labor force and in the development of daycare institutions. Until 1966, the female share of the labor force was about 34 percent, but this share increased rapidly in the following period. From 1965 to 1975, the female share of the labor force increased by seven percentage points.

[9] After 1953, an institution was only eligible for subsidies if the headmaster was a certified daycare teacher and formally approved by the Ministry. In 1970, the required pedagogical training changed from two to three years (Lunn, 1971). The child-teacher ratio changed from 8.3 in 1960 to 6.6 in 1972 and 7.7 in 1976 (Korremann, 1977).

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

[11] Likewise, the ratio of daycare slots to the number of 3-6-year-old children rose. In 1956 the ratio was 9 percent. This ratio rose only to 12 percent in 1967 but rose to 45 percent in 1979 (Johansen and Holten, 2015).

Figure 1: Formal daycare institutions and female share of labor force



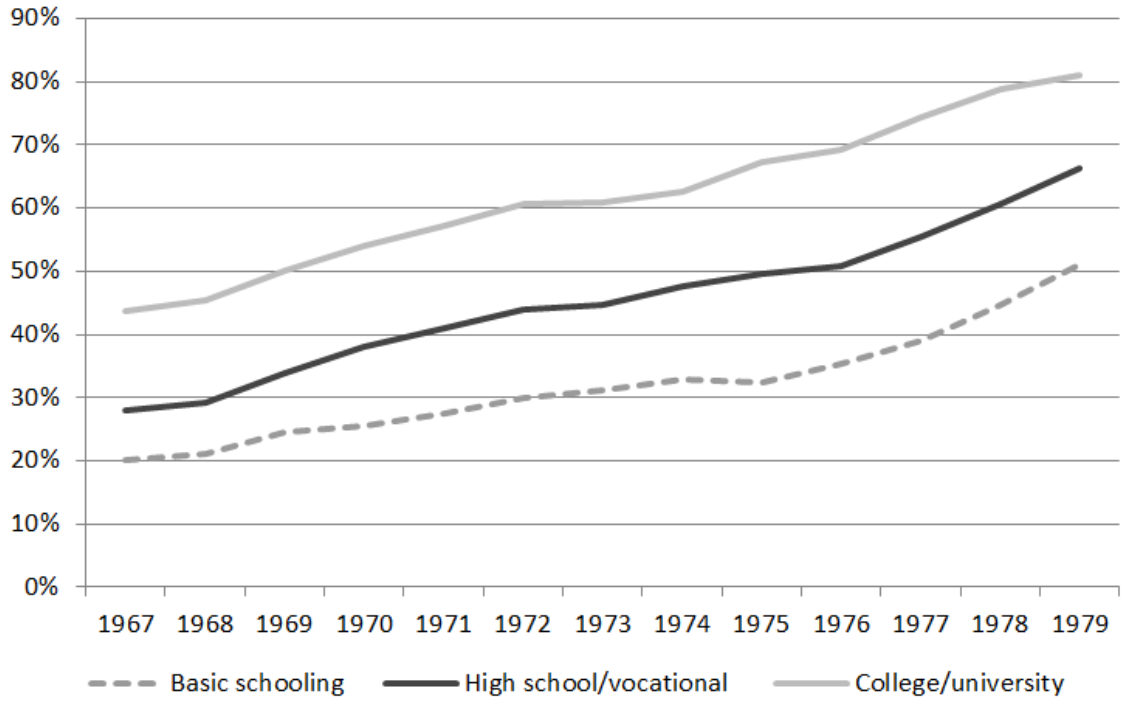
NOTE— The red line defines the number of daycare institutions for children 3-6 years old (left-hand axis), and the blue dots (right-hand axis) defines the female share of the labor force. The vertical solid line defines the 1965 daycare reform and the two vertical dotted lines define our period of interest (1967 to 1979). Data on the female share of the labor force are from Statistics Denmark, statistic yearbooks, various years. Data on daycare institutions are from various sources, see section 4 for more information.

Before 1960, the demand for female workers primarily targeted blue-collar workers, whereas the period after 1960 demanded female labor force participation from all skill levels (Korremann, 1977).[12] Figure 2 illustrates the demand for female labor by educational attainment (basic schooling, vocational training/high school degree, and college/university degree) for our period of interest (1967-79). Although the employment trends are similar for the three schooling levels, the employment level

[12]The demand for female labor force participation from all skill levels was partly a product of the economic upturn beginning in the late 1950s and partly a product of the rapid expansion of the welfare state from 1960. For example, the share of publicly employed workers increased by 2.3 percentage points from 1950 to 1960 but increased by 13.3 percentage points from 1960 to 1975 (Johansen and Holten, 2015).

is higher for the high-educated mothers throughout the period. Thus, because the increase in labor force participation affected women from all three levels of schooling, they were all potential users of public daycare.

Figure 2: Maternal employment by year and education



NOTE— The figure shows the development of maternal employment by maternal education. We use a sample of mothers, where employment is measured in the year when her youngest child turns four. Basic schooling equals 10 or less years of schooling (31 percent of the sample). High school/vocational equals 11-13 years of completed schooling, i.e. mostly mothers with some vocational training (47 percent of the sample). College/university equals 14 or more years of completed schooling (65 percent of the sample).

As a consequence of the reform and the high demand for female labor force participation, both the population of children that went into care and the quality of care likely changed. For example, daycare institutions no longer had any economic incentives to accept more children from low-income families. In addition, women from middle- and upper-income families, who were next in line to enter the public daycare system, had different quality standards and higher bargaining power towards the daycare institutions (Korremann, 1977).

2.3 Variation Across Local Daycare Authorities

Our identification strategy relies on the natural experiment of daycare openings across time (during 1967-79) and neighborhoods within the local daycare authorities (i.e., municipalities). Figure 3 maps the rollout of daycare institutions in two-year intervals. The lighter colors indicate neighborhoods where daycare institutions opened at the beginning of the period, and the darker colors indicate openings in later periods. Larger cities, such as Copenhagen (the capital), and Aarhus (the second largest city), where the expansion of production occurred first, had daycare institutions before 1966, whereas the rural areas containing farms and smaller businesses implemented daycare last.[13] However, because only a few municipalities had a daycare facility in every neighborhood or never implemented daycare within our period of observation, our identification strategy (neighborhood variation in daycare availability within the municipality) uses variation across most parts of the country.

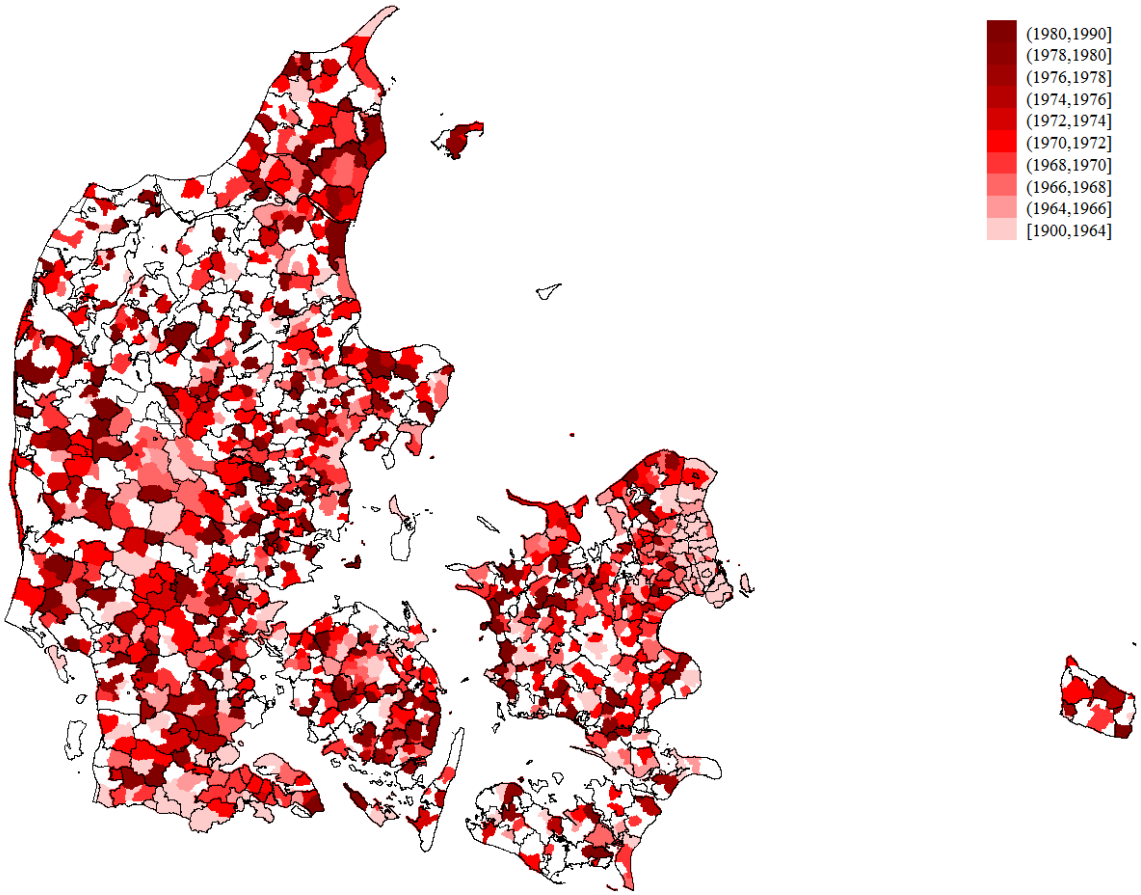
Nonetheless, as also suggested by Figure 3, the implementation of daycare was not fully random.[14] If parental demand for daycare affects the rollout of daycare, the staggered expansion is an invalid source of variation to estimate the effects of daycare on child and mother outcomes.[15] Our identification strategy includes municipality fixed effects, and therefore the availability of day- care can be related to time invariant municipality characteristics. However, changes to municipality characteristics might determine differences in the timing of daycare implementation and thus violate our identification strategy. Therefore, we follow previous research (e.g., Black et al. (2005); Holmlund (2008)) and regress year of implementation on parental ed-

[13] Municipalities dealt with the demands for daycare in various ways, and our identification strategy does not capture all types of daycare expansion. For example, in one municipality, the daycare authorities suggested enrolling eight percent more children per institution, because children went to care at different times during the day. We cannot capture such expansions directly, because we only capture daycare expansion by the opening of daycare institutions.

[14] Selective migration is also a potential problem when analyzing policy changes on a regional level. Consequently, we show that the distance between maternal place of birth and residence in 1970 is uncorrelated with daycare in Appendix Table A.1.

[15] Earlier in this section, we argue that the expansion of public daycare was mainly driven by the need to attract more women to work. From 1964 to 1970, the interests of pedagogues and parents worked hand in hand with this need for increased female labor participation. After 1970 when the government started to implement budget cuts, pedagogues and parents had little power in the general process of planning daycare. 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 the budget cuts that in the end meant fewer educated pedagogues per child a few years later. Thus this historical evidence suggests that labor market demands and not parental demands affected the overall changes in public daycare availability (Korremann, 1977).

Figure 3: Daycare openings



NOTE— The figure maps the variation in daycare availability within municipalities and across time. The lightest red color indicate neighborhoods where the first daycare institution opened prior to 1964, while the darkest red color indicate neighborhoods where the first daycare institution opened 1980-90. The white color indicate neighborhoods without daycare by 1990. The black lines define the municipality borders (1970-2007), but also smaller islands, which are not independent municipalities. The map is constructed using data from the Danish Geodata Agency.

ucational attainment and the paternal unemployment rate to check whether parental characteristics in the year before daycare availability correlate with the timing of daycare openings in the municipality.

Table 1 shows four model specifications for the correlation between a dummy for daycare availability and municipality-level parental characteristics. The first column includes year of birth dummies, the second column includes year of birth dummies and municipality fixed effects, and the third and fourth columns have the most comprehensive specifications (including year of birth dummies, municipality fixed effects,

Table 1: The correlation between municipality-level parental characteristics and daycare openings, by different model specifications

	(1)	(2)	(3)	(4)
Average maternal background				
Length of schooling	0.03296***	0.00909	0.00158	0.00399
	(0.01082)	(0.00567)	(0.00441)	(0.00462)
Average paternal background				
Length of schooling	0.07542***	0.00266	0.00482	0.00427
	(0.00973)	(0.00485)	(0.00446)	(0.00471)
Unemployed, age 3	-0.40134***	-0.02767	0.01352	0.01076
	(0.05696)	(0.02870)	(0.02330)	(0.02478)
Observations	5807	5807	5807	5807
Cohort dummies	Yes	Yes	Yes	Yes
Municipal fixed effects	No	Yes	Yes	Yes
Linear grouped trends	No	No	Yes	No
Quadratic grouped trends	No	No	No	Yes

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each column represents a model specification. Estimates are based on mean values from a year/municipality level aggregated data and a LPM model. We have also tried to use each of the three parental background variables as explanatory variables one at a time and results are robust. Column (1) only includes the explanatory variable and cohort dummies. Column (2) also includes municipality fixed effects. Columns (3) and (4) include linear or quadratic grouped trends, respectively. We group first year of institution into categories, before 1960, annually from 1960 to 1979, and after 1979. Second, we interact each of these municipality group indicators with a time variable and a squared time variable.

and linear or quadratic time trends). When we include year of birth dummies in the model (Column (1)), the correlation between daycare and parental characteristics is highly significant. For example, one extra year of maternal (paternal) education increases the probability of a daycare facility by 3.3 (7.5) percentage points. Nonetheless, when we control for year of birth and municipality fixed effects (Column (2)), the correlation between daycare openings and parental background is no longer statistically significant, and the point estimates are smaller than in Column (1). When we control for linear or quadratic time trends (Columns (3) and (4)), the point estimates are close to zero and still statistically insignificant. Thus, we argue that, conditional on trends specific to the timing of daycare implementation in the municipality, the reform causes a staggered introduction of subsidized daycare, and this staggered introduction is an exogenous source of variation in daycare availability. We assume that the year of birth dummies, (i.e., non-linear time trends) combined with

the linear or the quadratic time trend specifications account for potential variations in daycare quality across time within each municipality.

2.4 Outside Options

Other forms of child care existed during our period of interest, and after 1964 one type of informal care – family-based care – was also subsidized to meet the demand for child care. Family-based care is defined as care provided by an untrained (as a pedagogue) woman, who cares for one or two extra children besides her own or who cares for four to five children at her home. Although family-based care options also increased during our period, children aged three through six mainly used formal daycare centers. For example, in 1973, only six percent of all enrolled three to six-year-old children were in family-based care (Korremann, 1977), whereas 31 percent were in formal daycare (Johansen and Holten, 2015).

3 Identification

To examine the effects of daycare on child and mother outcomes, we exploit the variation in access to formal daycare induced by the reform. The exogenous variation stems from the progressive implementation of daycare opportunities in different neighborhoods and municipalities at different times across Denmark. We use a linear probability model for our binary outcomes, summarized in the following equation for child outcomes:

$$Y_{inmt} = \alpha DC_{nmt} + X'_{inmt}\beta + \gamma_t + \mu_m + T_t + u_{inmt}$$

Where Y_{inmt} is the adult outcome measured at age 35 for child i, who at age four lives in municipality m and neighborhood n. DC_{nmt} is an indicator taking the value one if daycare is available in the neighborhood at age four and zero otherwise. X'_{inmt} is a vector of covariates containing a dummy for boys, a dummy for urban neighborhoods, indicators for the number of siblings in the household, and maternal age at birth. γ_t is a full set of year-of-birth dummies, and μ_m refers to a full set of municipality fixed effects (i.e., local daycare authority intercepts). T_t accounts for potential differential trends between municipalities implementing daycare at different times. More specifically, we calculate the trends after we group the municipalities according

to the year in which they implement daycare. We define the first group of municipalities as those with daycare prior to 1960 and the last group as those with daycare later than 1979. We group the municipalities because a structural reform in the middle of our period (1970) merged the existing 1,098 municipalities to 276. This merger 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 the elderly). In the years prior to the reform, the city areas to some extent facilitated these services for the surrounding rural areas but did not get the 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 welfare beneficiaries. Although our variation of interest is institution openings within smaller units of the municipality (neighborhoods), the 1970 municipality reform has implications for our identification strategy, in particular for our specification of the municipality level time trends. To overcome this issue, we group the municipalities according to the year in which they implement daycare, and we use this ranking to generate the linear and quadratic time trends. u is the error term, which is allowed to be heteroskedastic and to cluster at the municipality level. Our final model identifies the effects of daycare availability on child and mother outcomes using the relative change in the timing of daycare availability between neighborhoods within the municipality and within neighborhoods across time.

As in Baker et al. (2008), Havnes and Mogstad (2011b), and Herbst (2017), our strategy produces ITT effects because we estimate the reduced-form effects for all children rather than for families that choose daycare. This method carries the advantage of potentially capturing the full impact of the program on both subsidized and unsubsidized care arrangements, as well as any peer externalities. In addition neighborhoods are not strict catchments areas; thus, parents can cross the border to another neighborhood for daycare. However, because neighborhood of residence, and not actual daycare utility, defines treated and untreated birth cohorts, our strategy minimizes the bias from selective parental responses to daycare availability. Assuming that daycare improves child outputs and all parents who really need daycare take their children to daycare next door, on average, children in the untreated areas are likely to do better than expected, and our estimates are potentially downward biased.

A concern for models using regional variation is that the estimates may reflect differential trends instead of a true policy effect. Daycare was implemented non-

randomly and related to the need for female labor market participation. Because regional demands vary, daycare implementation is likely to reflect municipality characteristics. Although municipality fixed effects allow for between-municipality variation, characteristics have most likely changed over time within the municipality. Therefore, to create further confidence in eliminating bias, we follow a similar method to that of e.g. Holmlund (2008) and include linear and quadratic time trends specific to municipalities, that capture differences in factors (e.g., parental education) correlated with daycare openings. We show in Table 1 that the implementation of daycare cannot be explained by municipality characteristics once we include municipality fixed effects and time trend specifications in the model.

4 Data

4.1 Data sources

The data set includes all children born between 1963 and 1975, who are residing in Denmark at age 35, and their parents. The data combine various administrative registers from Statistics Denmark, 1970 census track data, and indicators of daycare availability from historical records. Through the unique personal identifier in the registers, we match parental demographic characteristics, educational attainment, working hours, and income to child demographic characteristics and their adult outcomes.

From the National Board of Social Services, we collect information about daycare institutions from annual yearbooks for the periods 1966-71 (Socialstyrelsen, 1968-72) and 1974 (Tvenstrup, 1975). From 1976, we find information about daycare availability in the administrative registers. Together, these sources give us a panel of eligible institutions from 1966 through 1980.[16] For all families, we define daycare availability as a match between the neighborhood of daycare in the year the child

[16]To generate this data set of daycare institutions, we use the following three data steps. First, we digitize the historical records for 1966-71 and 1974. Annually these records contain a unique institution identifier, type of daycare, institution address and the number of slots per institution. Second, to bridge the period 1971-74, we use the additional information about date of establishment in the 1974 report. Thus for those institutions where the date of establishment is between 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 did not exist in the 1974 data but in the 1976 data, we assume they opened in 1976. The final daycare panel runs from 1966-80.

turns four and maternal neighborhood of residence in November 1970, according to the national census tract (Statistics Denmark, 1975).[17]

Our data set includes all children born in Denmark through 1963-75 and living in Denmark at age 35. From this data set, we make the following three exclusions; First, we exclude children without a maternal identifier (0.2 percent). Second, we exclude children for whom the maternal neighborhood of residence is unknown or invalid (2.5 percent). Third, because variation in daycare availability arises from neighborhood-level differences between children in the same birth cohorts, we exclude families from very small municipality-year cells (less than five children). Our final data set contains 889,392 children and their mothers, equivalent to 97.3 percent of the full population. In contrast to Havnes and Mogstad (2011b), we include all children and not just children of married mothers. At this time, 14 percent of children were born out of wedlock. A dummy for daycare availability in the neighborhood (parish) when the child turns four is our explanatory variable of interest. This measure is in contrast to previous studies that use differences in daycare coverage (e.g., Havnes and Mogstad (2011b,a); Baker et al. (2008, 2015)). We use this simplified measure of daycare because our historical data contains less noise in the daycare opening variable than in the daycare coverage variable. Therefore, using daycare openings allow us to exploit more variation across the country. For simplicity and to avoid doublets, we measure daycare availability for the child at one point in time (at age four).

4.2 Child and Maternal Dependent Variables of Interests

Our main child outcomes are log earnings and educational attainment. We measure educational attainment at age 35 for all children in the sample whereas log earnings are both measured at age 35 and for all available ages from 30 through 45.[18] To generate log earnings, we use annual earnings registered by the tax authorities adjusted to 2016 prices. This measure excludes income from unemployment insurance

[17]For matching children to daycare institutions, Haegeland et al. (2012) and Havnes and Mogstad (2015) suggest matching children's residence at birth to daycare institutions. This strategy minimizes a selection bias due to parents moving towards daycare availability. Our data does not include information about exact residence at birth but include parish at birth. Unfortunately, in our sample 50 percent of the children are born in a hospital parish. As hospitals serve several parishes and municipalities, we have a surplus of births in hospital areas and none in others. Thus we cannot match residence at birth to parish of daycare. Instead we match parish of daycare to maternal parish of residence in 1970.

[18]For the youngest birth cohort we observe them only at age 30-36, whereas we observe the oldest birth cohort from 30 through 45.

or other social benefits. In addition to log earnings, we measure whether the child is in the lowest or in the highest income quartile among all the children in our sample. We also investigate non-linearity in educational attainment. We investigate whether daycare availability increases the probability of high-school completion (12 or more years of schooling) and increases the probability of obtaining a college or a university degree (14 or more years of schooling).

Two dummies define our short-run maternal employment outcomes: maternal employment and full-time work. For both, we use information from taxpayer's contributions to a mandatory pension system (called ATP) in the year during which the child turns four. Contributions to ATP vary according to hours worked for employees working more than 9 hours a week. Specifically, persons working less than 9 hours a week, as well as unemployed, self-employed, and persons not participating in the labor force, were not a part of the ATP payment system during our period of observation (Hansen and Lassen, 2011). We define employed mothers as mothers working more than 33 percent full time of a year, equivalent to above the sample mean.[19] Thus, the dummy for maternal employment is a proxy for whether the mothers are away from home due to work. In addition, we define a dummy for working full time as equivalent to 30 hours of work per week or more all year.

We also use three measures of maternal long-run outcomes. First, we use a dummy variable for maternal employment, where the variable equals one if she holds any tax-based earnings. Second, we use a measure of maternal tax-based log earnings, and third, we use (log) family gross income. To account for differences in family size, we equalize family income by family size.[20]

To avoid doublets, we only measure maternal outcomes for the youngest child we observe in the family during our time window. We measure all three long run maternal outcomes in the year during which the child turns 17.

For covariates, we include indicators of maternal age at birth, year of birth dummies, number of children (household size), and a dummy for urban or rural areas. Both young and advanced maternal age captures any rearing quality effects correlated with maternal age. We define urban as neighborhoods (parishes) with a market town. Thus, by including an indicator for urban areas, we control for differences in

[19]The reference group of this dummy includes mothers working less than one third of a year. However, 81 percent of the mothers in this reference group consist of mothers not working at all. If we use a dummy for any work (mean 0.509), our estimate becomes 0.7 percentage points larger.

[20]We divide the family income by an equalized measure of family size, where the mother has weight 1, the father has weight 0.7 and children born by the same mother have weight 0.5.

job and earnings opportunities. We also control for gender in all specifications.

We examine heterogeneous effects by stratifying the sample on maternal education. We divide maternal educational attainment into three categories equivalent to basic schooling (ten years or less of schooling), high school or vocational training (11 through 13 years of schooling), and college/university degree (14 or more years of schooling).

4.3 Descriptive Statistics

Table 2 presents summary statistics for our explanatory variable of interest and outcomes. Column (1) reports means and standard deviations for the full sample, whereas Columns (2)-(5) report means and standard deviations for sub-samples, depending on the timing of daycare implementation in the municipalities. Column (2) shows the summary statistics for children and mothers in municipalities that have at least one daycare institution in each neighborhood before our period of investigation (i.e., before 1966). Columns (3) and (4) define summary statistics for the municipalities in which, daycare availability changes during our period of investigation (i.e., 1967 through 1979); these are the municipalities we use for identification. Specifically, Column (3) reports means and standard deviations for the untreated and Column (4) for the treated. Similarly, Column (5) reports summary statistics for those municipalities where daycare is not available throughout our period (i.e., daycare opens after 1979). In total, 645,453 or 73 percent of the children in our sample are located in a municipality that opens a daycare during our period of interest (Columns (3) and (4) compared to Column (1)).

Table 2 shows that the outcome log earnings are very similar in Columns (1)–(4), whereas log earnings are approximately 0.1 log points lower in Column (5). Table 2 also shows that educational attainment is 0.25 years higher in Column (4) than in Column (3).

Similar to Table 2, Table 3 shows summary statistics for background variables. We find that household size (as indicated by number of siblings) is smaller in areas with daycare than in areas where we do not observe daycare and that daycare is more prevalent in urban areas. On average, the women are around 23.2 years old at first birth and marginally older in the areas with early and later daycare implementation (Columns (2) and (5)).

We also find that women have higher educational attainment in the areas where

Table 2: Summary statistics for child and mother outcomes, by year of daycare implementation

Daycare opens:	All	<1966 Treated	1966-1979 Untreated	1966-1979 Treated	>1979 Untreated
	(1)	(2)	(3)	(4)	(5)
Panel A. Child outcomes					
Log earnings	10.634	10.652	10.616	10.653	10.554
	(0.863)	(0.875)	(0.846)	(0.864)	(0.859)
1^{st} quantile earnings	0.250	0.243	0.256	0.242	0.285
	(0.433)	(0.429)	(0.436)	(0.428)	(0.451)
4^{th} quantile earnings	0.250	0.272	0.227	0.268	0.181
	(0.433)	(0.445)	(0.419)	(0.443)	(0.385)
Observations	780957	88728	147192	420415	61638
Holding a job at age 35	0.878	0.878	0.881	0.879	0.878
	(0.327)	(0.328)	(0.324)	(0.326)	(0.327)
Length of schooling	12.916	12.947	12.816	13.063	12.471
	(2.355)	(2.382)	(2.286)	(2.372)	(2.252)
High school/vocational	0.774	0.778	0.769	0.789	0.728
	(0.418)	(0.415)	(0.422)	(0.408)	(0.445)
College/university degree	0.317	0.315	0.300	0.345	0.234
	(0.465)	(0.464)	(0.458)	(0.476)	(0.423)
Observations	889392	101104	167036	478417	70184
Panel B: Mother outcomes, child age 4					
Employment	0.401	0.408	0.316	0.461	0.170
	(0.490)	(0.492)	(0.465)	(0.499)	(0.376)
Full-time employment	0.124	0.131	0.090	0.145	0.051
	(0.329)	(0.338)	(0.286)	(0.352)	(0.219)
Panel C: Mother outcomes, child age 17					
Holding a job	0.760	0.791	0.707	0.791	0.608
	(0.427)	(0.407)	(0.455)	(0.407)	(0.488)
Log earnings	10.228	10.281	10.107	10.296	9.871
	(0.942)	(0.879)	(1.026)	(0.906)	(1.078)
Log family income	10.224	10.323	10.109	10.273	9.999
	(0.601)	(0.578)	(0.593)	(0.597)	(0.586)
Observations	403241	47995	73939	221892	27272

NOTE— Column (1) shows the mean coefficients for the total sample, and the corresponding standard deviations in parentheses below. Column (2) shows mean coefficients and standard deviations for individuals in municipalities, where all neighborhoods open a daycare institution at some point before 1966. Columns (3) and (4) show mean coefficients and standard deviations for individuals in municipalities, where a daycare institution opens between 1966 and 1979 for untreated and treated, respectively. Column (5) shows mean coefficients and standard deviations for individuals in municipalities, where daycare institution opens after 1979. Number of observations for mother's log earnings is 289,261 and for family income 374,317.

daycare had already been implemented (Column (2)) and in the municipalities where we observe daycare implementation during our period of investigation (Column (4))

Table 3: Summary statistics for the background variables, by year of daycare implementation

Daycare opens:	All	<1966 Treated	1966-1979 Untreated	1966-1979 Treated	>1979 Untreated
	(1)	(2)	(3)	(4)	(5)
Male	0.509	0.508	0.509	0.509	0.513
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Urban neighborhood	0.198	0.137	0.055	0.276	0.020
	(0.398)	(0.344)	(0.228)	(0.447)	(0.140)
Mother's age at first birth	23.211	23.500	23.002	23.185	23.605
	(4.103)	(4.374)	(3.986)	(3.931)	(4.622)
Mother's age at birth	26.196	26.250	26.497	26.037	26.886
	(5.174)	(5.249)	(5.400)	(4.906)	(5.811)
Birth order	1.920	1.840	2.095	1.844	2.180
	(1.056)	(0.976)	(1.161)	(0.991)	(1.239)
Number of siblings	1.598	1.492	1.788	1.506	1.946
	(0.949)	(0.911)	(0.986)	(0.903)	(1.051)
Maternal length of schooling	10.273	10.661	9.699	10.665	8.815
	(3.116)	(3.087)	(3.051)	(3.092)	(2.858)
Maternal basic schooling	0.539	0.478	0.627	0.483	0.747
	(0.498)	(0.500)	(0.484)	(0.500)	(0.435)
Maternal high school/voc.	0.340	0.391	0.274	0.379	0.177
	(0.474)	(0.488)	(0.446)	(0.485)	(0.381)
Maternal college/uni degree	0.121	0.131	0.099	0.137	0.076
	(0.326)	(0.337)	(0.299)	(0.344)	(0.266)
Paternal length of schooling	10.990	11.508	10.242	11.398	9.524
	(3.300)	(3.277)	(3.252)	(3.228)	(3.195)
Unemployed father, age 3	0.185	0.123	0.265	0.149	0.353
	(0.388)	(0.329)	(0.441)	(0.356)	(0.478)
Observations	889392	101104	167036	478417	70184

NOTE— Column (1) shows the mean coefficients for the total sample, and the corresponding standard deviations in parentheses below. Column (2) shows mean coefficients and standard deviations for individuals in municipalities, where all neighborhoods open a daycare institution at some point before 1966. Columns (3) and (4) shows mean coefficients and standard deviations for individuals in municipalities, where a daycare institution opens between 1966 and 1979 for untreated and treated, respectively. Column (5) shows mean coefficients and standard deviations for individuals in municipalities, where daycare institution opens after 1979.

than women in areas without daycare (Columns (3) and (5)). There is about a one-year difference in maternal educational attainment between the treated and untreated during the period 1967-79. We find a similar pattern for unemployed fathers and paternal educational attainment. In general, we find that the municipalities where daycare availability changes during 1967-79 are very similar to those municipalities, where daycare was already implemented before 1967. We also find that the

areas in Column (3) have a somewhat higher socioeconomic level than those (primarily rural) areas that implement daycare after our period of interest. Although our municipality fixed effects strategy allows for variation between municipalities, the external validity of our estimates is more sensitive to these differences between municipalities.

We test whether a random implementation of daycare can generate any effects of daycare availability on child and mother outcomes. We randomly allocate the fraction of neighborhoods with daycare each year, keeping the actual growth rate in daycare openings, and perform a non-parametric permutation test (cf. Figure 4).

5 Results

5.1 Child Outcomes

Table 4 shows the estimates for the effects of daycare availability on children's long-run outcomes. To target possible concerns about differential trends between local areas, we report estimates for five different model specifications.

Column (1) shows the OLS estimates with no covariates or trend specifications, and this estimate suggests that daycare availability correlates with a 5.2 percent increase in earnings at age 35. In Column (2) we include covariates and a nonlinear time trend (year of birth dummies), and we observe that daycare availability now increases earnings by 3.0 percent. In Columns (3)-(5) we add municipality fixed effects (3), municipality fixed effects and a grouped linear time trend (4), or municipality fixed effects and a grouped quadratic time trend (5).[21] In these three columns, the effects of daycare availability on child outcomes are fairly robust. For example, daycare availability increases children's earnings by 1.2 percent.

We measure daycare availability at the neighborhood level, and when we add covariates, year of birth dummies, and municipality fixed effects to the model, we compare the outcomes of treated and untreated neighborhoods within the municipality and within our period. Thus, we estimate the relative increase in child outcomes in the treated neighborhoods compared to that of the untreated neighborhoods within the municipalities. Our results are fairly robust to age differences; we find similar results when we estimate the earnings effects at ages 30-40 or 30-45 instead of at age

[21] In Section 3 we describe, how we define the time trends.

Table 4: The effects of daycare availability on child earnings, employment, and schooling at age 35, by different model specifications

	(1)	(2)	(3)	(4)	(5)
Dependent variable					
Log earnings	0.052***	0.030***	0.012***	0.012***	0.012***
	(0.006)	(0.006)	(0.004)	(0.004)	(0.004)
1^{st}quartile earnings	-0.022***	-0.011***	-0.000	-0.000	-0.000
	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
4^{th}quartile earnings	0.053***	0.037***	0.012***	0.012***	0.012***
	(0.005)	(0.004)	(0.003)	(0.003)	(0.003)
Observations	780960	780960	780960	780960	780960
Holding a job	0.002	0.005***	-0.002	-0.002	-0.002
	(0.002)	(0.002)	(0.002)	(0.001)	(0.001)
Lenght of schooling	0.302***	0.134***	0.090***	0.092***	0.092***
	(0.044)	(0.030)	(0.018)	(0.017)	(0.017)
High school/vocational	0.027***	0.007	0.009***	0.009***	0.009***
	(0.006)	(0.005)	(0.003)	(0.003)	(0.003)
College/university degree	0.055***	0.025***	0.016***	0.017***	0.017***
	(0.008)	(0.005)	(0.003)	(0.003)	(0.003)
Observations	889392	889392	889392	889392	889392
Covariates	No	Yes	Yes	Yes	Yes
Cohort dummies	No	Yes	Yes	Yes	Yes
Municipal fixed effects	No	No	Yes	Yes	Yes
Linear grouped trends	No	No	No	Yes	No
Quadratic grouped trends	No	No	No	No	Yes

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions, and each column represents a model specification. Column (1) only includes the explanatory variable. Columns (2)-(5) include covariates. The included covariates are indicators of gender of the focal child, urban area, mother's age at birth, and birth order. Columns (4)-(5) include grouped trends. We group first year of institution into categories, before 1960, annually from 1960 to 1979, and after 1979. Second, we interact each of these municipality group indicators with a time variable and a squared time variable.

35. For example, we find that daycare availability at age four increases earnings by 1.0 (1.2) percent at age 30-40 (30-45) (Panel A in Appendix Table A.2).

We also investigate the effect of daycare availability on holding a job (i.e., the effect on having tax-based earnings), the probability of being in the lowest earnings quartile, and the probability of being in the highest earnings quartile (hence, low versus high earner). We find no effect of daycare on the probability of being a low earner, small and insignificant effects of daycare on the probability of holding a job, whereas daycare availability increases the probability of being a high earner by 1.2

percentage points (4.8 percent).[22] Thus, we find that daycare availability has persistent effects on earnings when the children turn 35 and that children at the high end of the earnings distribution drive the average effect on earnings.

For the mediating role of schooling, we find that daycare availability increases the length of schooling by 0.092 years (approximately 5 weeks). Daycare availability also increases the probability of obtaining a college/university degree by 1.7 percentage points (5.4 percent from the base of 32 percent) and the probability of high school completion by 0.9 percentage point (1.16 percent from the base of 77 percent).

Our ITT results for both length of schooling and the probability of obtaining a college degree are similar to those found for Norway (Havnes and Mogstad, 2011b, 2015). However, in contrast to Havnes and Mogstad (2011b, 2015) we find positive effects on earnings in the top end of the earnings distribution and no effect on the probability of being in the lowest quartile of the earnings distribution. Havnes and Mogstad emphasize that the introduction of subsidized child care can potentially equalize the income distribution of children, partly because of an income penalty at the top. Our results suggest that universal daycare raises the income level only for children in the top of the income distribution, mainly through raising their level of educational attainment.[23]

5.1.1 Heterogeneous Effects of Universal Daycare

We investigate heterogeneity in the effects by gender and maternal educational attainment. Critics of universal child care programs argue that investments in daycare are better spent on children from poor socioeconomic background because the returns from daycare are likely to be greater for this group. Such conclusions are supported by e.g., Herbst (2017) and Havnes and Mogstad (2015). Both find that children of low-income families are the primary beneficiaries of universal daycare, and Havnes

[22] We compare our point estimates to the sample means presented in Table 2.

[23] Following the literature, we also contextualize the increase in daycare availability to the actual increase in daycare coverage. During our period, daycare coverage in Denmark increased from 12 percent in 1967 to 45 percent in 1979 (33 percentage points). If we scale our estimates according to the total increase in daycare coverage, our estimated treatment on the treated (TT) effects are ITT divided by the total increase in daycare coverage (ITT/0.33). We find the TT effect is 0.278 years of schooling per percentage point increase in daycare coverage and a 5.2 percentage points increase in receiving a college degree per percentage point increase in daycare coverage. Estimates, which are similar to the 0.35 years of schooling per percentage point increase in daycare coverage and the 6 percentage points increase in college completion per percentage point increase in daycare coverage found by Havnes and Mogstad (2011b).

and Mogstad (2015) even finds that daycare deteriorates earnings for children from high-income families.[24] Some studies also find differential effects for boys and girls, but Duncan and Magnuson (2013) conclude on the basis of 22 studies that there are no systematically stronger effects of daycare programs for girls than for boys (or vice versa). Havnes and Mogstad (2011b) find strongest earnings effects for girls, while previous findings for Denmark suggest that boys are the primary beneficiaries of formal center-based daycare (Datta Gupta and Simonsen, 2010).[25] To target heterogeneous effects in both parental background and child gender, we estimate the effect of daycare availability on child outcomes by three levels of maternal educational attainment and child gender.

We find strong heterogeneous effects over maternal educational attainment levels and child gender. Children of mothers with more than basic education drive the effects (Table 5, Panel A), and we find strongest effects for boys (table 5, panel B2). For example, for boys, we find that earnings increase by 4.9 percent when the mothers have a college/university degree, by 1.7 percent when the mothers have a high school degree/vocational training, and by 0.8 percent and not statistically significant when the mothers have basic schooling. For girls, the schooling and earnings effects are, with one exception, not statistically significant for any of the three maternal educational attainment levels and are in general much closer to zero. Only for girls of mothers with a high school degree/vocational training we find that daycare increases length of schooling by 0.05 years. In addition, we find that, for girls, daycare reduce the probability of holding a job by 0.5 percentage point in contrast to the effect for boys (Table A.2). The gender differences are persistent when we estimate earnings at age 30-40 or 30-45 (Table A.2). For example, for girls (Panel B1), the pooled earnings are small and statistically insignificant (0.02 percent at age 30-40 and 0.04 percent at age 30-45), whereas the effects for boys are 1.8 percent at age 30-40 and 2.1 percent at age 30-45 (Panel B2).

[24] Papers targeting shorter-term outcomes also investigate heterogeneous effects. For Canada, Kottelenberg and Lehrer (2017) extend the work of Baker et al. (2008) and find that the overall negative effect of universal daycare suggested by Baker et al. (2008) covers substantial heterogeneous effects. They find positive gains of daycare on developmental test scores at age 4/5 for children from single parent households and particular for children from disadvantaged single parents' households.

[25] More specifically, Datta Gupta and Simonsen (2010) find that boys born to mothers with basic schooling or a high school degree/vocational training experience an increase in non-cognitive skills (measured at age 7) when they are in formal daycare compared to family day care.

Table 5: Heterogeneous effects of daycare availability on child earnings and schooling at age 35, by maternal educational attainment and child gender

Maternal schooling level:	All (1)	Basic (2)	High/voc (3)	Col/uni (4)
Dependent variable				
Panel A: All				
Lenght of schooling	0.092***	0.021	0.064***	0.077***
	(0.017)	(0.016)	(0.017)	(0.028)
Observations	889392	479489	302531	107372
Log earnings	0.012***	0.000	0.011*	0.015*
	(0.004)	(0.005)	(0.006)	(0.009)
Observations	780960	412579	271286	97095
Panel B1: Girls				
Length of schooling	0.049***	-0.022	0.051***	-0.033
	(0.018)	(0.019)	(0.020)	(0.028)
Observations	436364	235617	148196	52551
Log earnings	0.001	-0.008	0.006	-0.018
	(0.006)	(0.006)	(0.009)	(0.012)
Observations	382566	201961	133006	47599
Panel B2: Boys				
Lenght of schooling	0.133***	0.065***	0.073***	0.180***
	(0.021)	(0.018)	(0.023)	(0.042)
Observations	453028	243872	154335	54821
Log earnings	0.022***	0.008	0.017**	0.049***
	(0.005)	(0.006)	(0.007)	(0.012)
Observations	398394	210618	138280	49496

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions. All columns include covariates, cohort dummies, municipality fixed effects and quadratic grouped trends. The included covariates are indicators of gender, urban area, mother's age at birth and birth order. Basic schooling equals 10 or less years of completed schooling. High school/vocational training equals 11-13 years of completed schooling. College/university degree equals 14 or more years of completed schooling.

5.2 Maternal Outcomes

The primary purpose of the daycare expansion in the mid-1960s was to support female labor market participation, and existing studies show that increasing household resources due to maternal employment is a potential mechanism for improving child outcomes in the long run. For example, Black et al. (2014) find that an 8 percent increase in annual disposable income at age five increases later academic performance in junior high school, when they isolate family income effects from employment and

formal daycare effects.

Table 6: The effects of daycare availability on maternal employment, earnings, and family income, by different model specifications

	(1)	(2)	(3)	(4)	(5)
Dependent variable					
Panel A: child age 4					
Employment	0.168***	0.100***	0.052***	0.052***	0.052***
	(0.011)	(0.006)	(0.004)	(0.004)	(0.004)
Full-time employment	0.061***	0.032***	0.016***	0.016***	0.016***
	(0.005)	(0.003)	(0.002)	(0.002)	(0.002)
Panel B: child age 17					
Log earnings	0.233***	0.173***	0.053***	0.053***	0.053***
	(0.016)	(0.011)	(0.007)	(0.007)	(0.007)
Log family income	0.195***	0.118***	0.050***	0.050***	0.050***
	(0.013)	(0.012)	(0.006)	(0.006)	(0.006)
Observations	400766	400766	400766	400766	400766
Covariates	No	Yes	Yes	Yes	Yes
Cohort dummies	No	Yes	Yes	Yes	Yes
Municipal fixed effects	No	No	Yes	Yes	Yes
Linear grouped trends	No	No	No	Yes	No
Quadratic grouped trends	No	No	No	No	Yes

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions, and each column represents a model specification. Column (1) only includes the explanatory variable. Columns (2)-(5) include covariates. The included covariates are indicators of gender of the focal child, urban area, mother's age at birth and number of children. Columns (4)-(5) include grouped trends. We group first year of institution into categories, before 1960, annually from 1960 to 1979, and after 1979. Second, we interact each of these municipality group indicators with a time variable and a squared time variable. Family income is equalized by family size, where the second parent count 0.7 and each child counts 0.5. Family income is measured in brutto amounts. Number of observations for family income is 374,317.

To investigate maternal labor market outcomes as amechanism, table 6 shows the effects of daycare availability on maternal outcomes. For this purpose, we focus on maternal employment measured when the youngest child is four, but we find similar results for the full sample of mothers (see Table 9). Panel A shows the effects on maternal employment when the child is four. Conditional on the covariates, year of birth dummies, and municipality fixed effects (Column (3)), we find that daycare availability increases maternal employment by 5.2 percentage points or 13 percent (from the mean employment rate of 0.40). We also find that full-time employment increases by

1.6 percentage points or 12.9 percent (from the mean full-time employment rate of 0.12). Once we include municipality fixed effects in the model, our results are robust across time trend specifications (Columns (3)-(5)).

If we relate these maternal employment results to the 33 percentage points increase in daycare coverage, a 10 percentage points increase in daycare coverage is equivalent to a 1.6 percentage points increase in maternal labor supply. In absolute terms, this result suggests that for every 100 extra daycare slots (a total cost of \$505,400 - \$692,500),[26] maternal labor supply increased by 12 workers, i.e., \$57,403 per worker, which is close to the back of the envelope calculations that the Ministry of Finance did at the time. They expected a net employment increase of 18 women per 100 additional daycare slots (Korremann, 1977).

Our average point estimate for maternal employment is higher than the 4.5 percent increase found for Norway by Havnes and Mogstad (2011a), lower than the 27 percent increase found for the U.S. by Herbst (2017), but similar to the one found for Canada by Baker et al. (2008). For Canada, they find a 14.5 percent (7.7 percentage points) increase in maternal labor supply. However, because daycare coverage was somewhat higher in Denmark, we find that maternal labor supply increases by 0.16 percentage point per percentage point increase in coverage, whereas Baker et al. (2008) suggests a 0.55 percentage point increase in maternal labor supply per percentage point increase in coverage. Thus, our results also reflect the crowding out of informal care, as in the Norwegian case (Havnes and Mogstad, 2011a).

As a new contribution to the literature, we also investigate the effects of daycare availability on long-run maternal labor market attachment, log earnings, and family income. We find that daycare availability when the child is four increases the probability of maternal employment by 3.7 percentage points when the child is 17 (equivalent to a 4.9 percent increase).[27] Daycare availability also increases long-run maternal earnings by 5.3 percent. For earnings, we only compare mothers with earnings greater than zero, and our results suggest that early labor market entry, in contrast to later entry, has positive long-run consequences. Similarly, we find that daycare availability when the child is four increases family income at age 17 by 5.0 percent (Table 6, Panel B).

In summary, these long-run outcomes suggest that maternal labor market attach-

[26] Expenditure per daycare slot was \$5,054 in 1967 and \$6,925 in 1972 in 2016 prices (Korremann, 1977).

[27] Child age 17 is the earliest that we can investigate family income and earnings for all mothers.

ment, earnings, and family income are potential mechanisms through which daycare availability affects children's long-run outcomes. Unfortunately, we are not able to distinguish between these mechanisms.

5.2.1 Heterogeneous Employment Effects of Universal Daycare

The overall positive effects of daycare availability on maternal labor supply cover heterogeneous effects. Table 7, Panel A shows that daycare availability has the largest employment effect for basic-educated mothers and smallest effect for high-educated mothers (i.e., mothers with a college/university degree). In contrast, for full-time employment (i.e., 30 hours per week or more), the point estimates are larger for high-educated mothers. However, when we relate the point estimates to the sample means, the effect sizes are in line with those for employment. We find that daycare availability increases full-time employment by 15.0 percent for basic-educated mothers, by 6.3 percent for mothers with a high school or vocational degree, and by 4.7 percent for mothers with a college or university degree.[28]

Table 7, panel B shows the heterogeneous effects of daycare availability on maternal labor market attachment, log earnings, and family income when the child is 17. For basic-educated mothers, we find that daycare availability increases the probability of holding a paid job by 4.2 percentage points (5.3 percent), whereas we find closer to zero effects for mothers with high school/vocational training (0.8 percent) and a college/university degree (a statistical insignificant 0.7 percent increase). However, in terms of earnings, both mothers with basic schooling and mothers with a college/university degree are affected (earnings increase by 5.2 and 5.9 percent, respectively). For mothers with high school or vocational training, we find a statistically insignificant effect of 1.5 percent. For family income, we find a similar U-shape: Daycare availability mostly affects families in which the mothers have basic schooling and college/university degrees.

Together, these results suggest that the daycare expansion overall had the largest effects for basic-educated mothers. In terms of employment, earnings, and family income, basic-educated mothers are affected in both the short- and long-run. High-educated mothers, on the other hand, are less affected in terms of short- and long-run employment possibilities but equally affected in terms of long-run earnings. This

[28]The overall sample mean of full-time employment is 12.4 percent, for mothers with basic schooling the sample mean is 6 percent, for mothers with a high school or vocational degree the sample mean is 15.8 percent, and for mothers with a college or university degree the sample mean is 33.9 percent.

Table 7: Heterogeneous effects of daycare availability on maternal employment, earnings, and family income by maternal educational attainment

Maternal schooling level:	All (1)	Basic (2)	High/voc (3)	Col/uni (4)
Dependent variable				
Panel A: child age 4				
Employment	0.052***	0.049***	0.029***	0.028***
	(0.004)	(0.004)	(0.005)	(0.008)
Full-time employment	0.016***	0.009***	0.010***	0.016**
	(0.002)	(0.002)	(0.003)	(0.008)
Panel B: child age 17				
Holding a job	0.037***	0.042***	0.007**	0.006
	(0.003)	(0.004)	(0.003)	(0.005)
Observations	403241	222678	136540	44023
Log earnings	0.053***	0.052***	0.015*	0.059***
	(0.007)	(0.008)	(0.009)	(0.014)
Observations	306421	154334	112504	39583
Log family income	0.050***	0.038***	0.022***	0.040***
	(0.006)	(0.005)	(0.005)	(0.011)
Observations	400766	220890	136039	43837

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions. All columns include covariates, cohort dummies, municipality fixed effects and quadratic grouped trends. The included covariates are indicators of gender of the focal child, urban area, mother's age at birth and number of children. Basic schooling equals 10 or less years of completed schooling. High school/vocational training equals 11-13 years of completed schooling. College/university degree equals 14 or more years of completed schooling. Family income is equalized by family size, where the second parent count 0.7 and each child counts 0.5. Family income is measured in brutto amounts.

indicate, that high-educated mothers, who made a quicker return to the labor market after their child bearing years, earn more in the long run than high-educated mothers who did not return as quickly because of lack of daycare availability.

5.2.2 Mechanisms for Child Outcomes

For the child outcomes, these maternal results provide at least three potential interpretations. First, although the daycare expansion mostly affected basic-educated mothers, we find that their children are less affected in the long run. One potential explanation is that children of basic-educated mothers experience a transfer from home care to formal daycare, while children of high-educated mothers experience a

transfer from informal out-of-home care to formal care when a daycare opens. Figure 2 supports this interpretation. The figure shows that already in 1967 (i.e., the very early years of universal daycare), the employment rate was 24 percentage points higher for mothers with college or university degrees than for basic-educated mothers. Thus, we interpret this to mean that, higher-educated mothers were more likely to work regardless of whether they had access to formal daycare. This is also evident from the insignificant differences in the probability of holding a job when the youngest child is 17 between mothers, who had daycare available when the child was four and mothers who did not.

Previous research from Denmark by Datta Gupta and Simonsen (2010) supports the interpretation that potential quality differences in the counterfactual mode of care partly explain the heterogeneous effects of formal daycare on child outcomes. Comparing universal formal daycare, home care, and family-based daycare (i.e., the most commonly used informal care for children younger than three) at age three, they find that children in home care and formal daycare have similar non-cognitive skills at age seven. They also find that children in family-based daycare have lower non-cognitive skills than children in formal daycare. Thus, for basic-educated mothers, we are also more likely to compare daycare settings of similar quality, if children of basic-educated mothers were more likely to experience a transfer from home care to formal care.

Second, stratifying by child gender, we find that daycare affects only boys' schooling and earnings, whereas maternal labor market responses to daycare availability do not significantly vary by child gender (Appendix Table A.3). This contrast between generations suggests that increased household financial resources during childhood (which are similar by child gender) do not play an important independent role in the causal chain from universal daycare to offspring outcomes. Moreover, because maternal employment effects do not vary by child gender, these results suggest that maternal labor market activity per se, whether at the intensive or extensive margin, in the short run or long run, does not play an important independent role in the causal chain from daycare to offspring outcomes either. However, because the availability of daycare causes maternal labor supply to increase and household financial resources to increase, we cannot identify their independent roles in the causal chain; countervailing effects might cancel out. Nevertheless, maternal employment is relevant for the daycare effect because of mothers' role in providing the alternative mode of care.

Third, for mothers with a high school degree or vocational training, we find small

maternal earnings and employment effects, although we find positive child outcomes for this group. These results suggest that formal daycare has some direct effects on long-run child outcomes.

6 Robustness and Sensitivity Analysis

As shown in Figure 3, daycare availability did not rollout randomly across municipalities. Naturally, larger cities implemented daycare earlier because of the rapid industrialization in these areas, whereas rural areas implemented daycare later. In Section 2, we show that adding linear or quadratic trends to the model specification minimizes such heterogeneity between the timing of daycare openings and parental background information.[29] However, to increase the credibility of our findings, we conduct four additional specification checks.

First, we show that if we exclude up to the five largest cities, we find similar results for child and mother outcomes (Table 8).

Second, we follow Bertrand et al. (2004) and test whether serial correlation in the rollout of daycare institutions (i.e., the treatment variable) is upward biasing our results and causing a rejection of the null hypothesis of no effects of daycare availability on child and mother outcomes. As in previous studies, we test for serial correlation by performing a non-parametric permutation test (e.g., Chetty et al. (2009)). Specifically, we initiate the rollout of treated neighborhoods 1,000 times in randomly chosen year*neighborhood cells and calculate the point estimates of the placebo treatment indicator on the outcomes.[30] Figure 4 shows the cumulative distribution function (CDF) for the placebo estimates on four outcomes: children's log earnings and educational attainment, and maternal employment and full-time employment. For each outcome, the figure shows that the majority of placebo estimates are close to zero. In contrast, our original point estimates from Table 4 and Table 6 are in the right-hand tail of the graphs (indicated by red vertical lines). We use the fraction of placebo

[29] We use parental educational attainment and the paternal unemployment rate as indicators for parental background. We use paternal unemployment rate as a signal for the general unemployment rate for both parents, as mothers not working at this time also reflects the traditional family pattern where the father is the breadwinner, and the mother runs the household. However, farmers where not a part of the ATP and are therefore likely to count as unemployed in our data. We measure parental educational attainment and paternal unemployment rate when the child is three to capture any push factors before the child reaches daycare age.

[30] For each random rollout, we include grouped quadratic time trends. As in the main analysis, we estimate grouped time trends instead of using time trends for each municipality.

Table 8: The effects of daycare availability on maternal employment, children's earnings and schooling at age 35, largest municipalities excluded

Excluding cities:	None (1)	Largest (2)	Two largest (3)	Three largest (4)	Four largest (5)
Dependent variable					
Panel A: Child outcomes					
Length of schooling	0.092***	0.101***	0.100***	0.106***	0.105***
	(0.017)	(0.016)	(0.017)	(0.017)	(0.017)
Observations	889392	804144	755965	724406	695958
Log earnings	0.012***	0.013***	0.014***	0.016***	0.016***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	780960	708522	666281	638762	613963
Panel B: Mother outcomes, child age 4					
Employment	0.052***	0.054***	0.053***	0.053***	0.054***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
Observations	403241	362984	342157	327699	314892
Full-time employment	0.016***	0.017***	0.018***	0.018***	0.019***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Observations	403241	362984	342157	327699	314892

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions. All columns include covariates, cohort dummies, municipality fixed effects and quadratic grouped trends. The included covariates are indicators of gender, mother's age at birth, and number of children (mother outcomes) or birth order (child outcomes). In Column (2) Copenhagen is excluded, in Column (3) we further exclude Aarhus, in Column (4) Odense is further excluded, and in Column (5) we also exclude Aalborg.

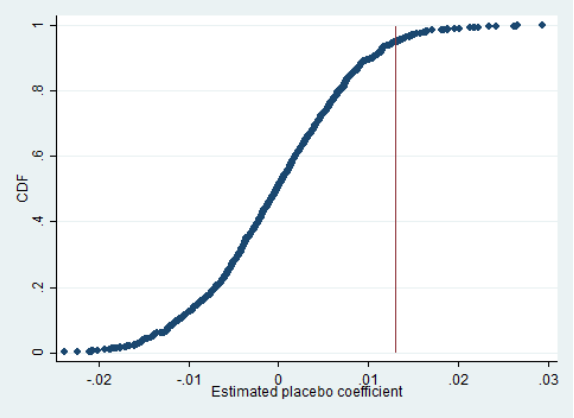
estimates above our original point estimates to calculate p-values for the permutation test. The majority of these p-values are below 0.05 (0.051 for log earnings) and indicate that our main results are not driven by serial correlations in the standard errors (Bertrand et al., 2004).

Third, for the sample of mothers, we report estimations for her youngest child to avoid any possible heterogeneous behavior arising from entering the labor market between child births. In Table 9, we substantiate that our results for maternal employment are not overly sensitive to such heterogeneous decisions processes. Not surprisingly, estimates are marginally lower when we use the full sample of children for each mother. For example, the effect of daycare availability on maternal employment reduces from 5.2 to 4.9 percentage points because, mothers are more likely to stay home, regardless of daycare availability, if they also have younger children.[31] If

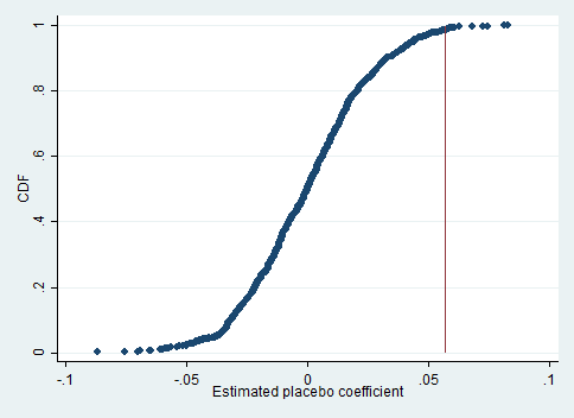
[31]Korremann (1977) shows that women are less likely to work if their youngest child is younger

Figure 4: Cumulative distribution of estimates from placebo rollout

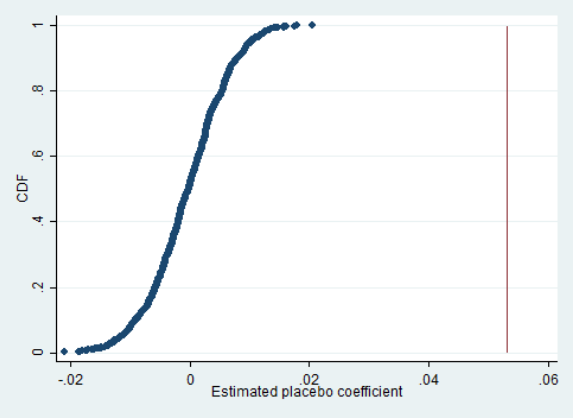
(a) Log earnings (p-value=0.051)



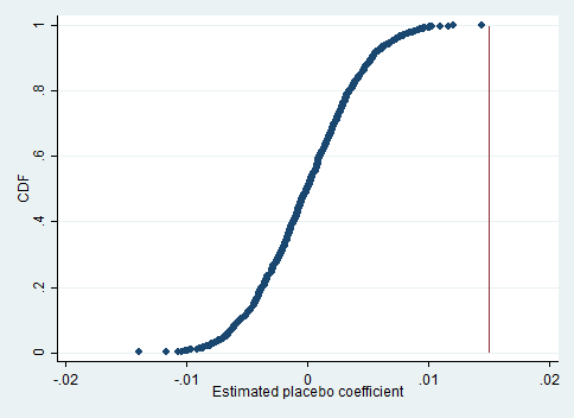
(b) Length of education (p-value=0.014)



(c) Employment (p-value=0.000)



(d) Full-time employment (p-value=0.000)



NOTE— For the four main outcomes, the figure shows the empirical cumulative distribution function (CDF) from the permutation test and a vertical line illustrating our point estimates from the main tables. For each CDF, we randomly assign daycare (the treatment indicator) to year*neighborhood-cells 1,000 times and estimate the result. To mimic the actual rollout, we fix the share of daycare openings to fit the actual distribution of yearly daycare openings.

we exclude families with younger siblings when the child is four (Columns (2) and (3)), the point estimates are in line with our main specification of last-born children (Column (4)). Our estimates for families with one child are a bit smaller but similar to those of our sample of last-born children (Column (5)).

Fourth, universal daycare, when available, during this period is for children from when they turn three until they start school. Because our treatment is a dummy for

than two, regardless of the number of children.

Table 9: The effects of daycare availability on maternal employment, by child spacing

Sibling space:	All (1)	Space≥4 (2)	Space≥5 (3)	Last born (4)	Only child (5)
Dependent variable					
Employment	0.049***	0.052***	0.054***	0.052***	0.047***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.008)
Full-time employment	0.017***	0.017***	0.018***	0.016***	0.024***
	(0.002)	(0.002)	(0.002)	(0.002)	(0.005)
Observations	889392	629190	552952	403241	70019

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. All columns include covariates, cohort dummies, municipality fixed effects and quadratic grouped trends. The included covariates are indicators of gender of the focal child, urban area, mother's age at birth, and number of children. Column (1) includes all children in our sample. Column (2) includes children, where the next sibling is born 4 or more years after the focal child. Column (3) includes children, where the next sibling is born 5 or more years after the focal child. Column (4) includes children, who have no younger siblings (results are equivalent to Column (5) in Table 6). Column (5) includes children with no siblings.

daycare availability in contrast to daycare coverage, we measure potential daycare exposure when the child is four instead of potential daycare exposure between the ages three through six as in other existing studies. However, to create confidence about the results, we also try measuring daycare availability when the child is three and five. We find that our results for both the mother and the child are quite robust to this specification, but the fixed year of births entails a mechanical increase in daycare from age three through five (Table 10).

To summarize, our estimates are robust to urbanicity (i.e., removing larger cities from the analysis), child birth order, and age of exposure to daycare. Furthermore, given that municipalities could open daycare facilities in any year of our 13-year observation period, a permutation test of counterfactual opening year and neighborhood supports our standard inference that actual openings had statistically significant effects.

Table 10: The effect of daycare on maternal employment, children's earnings and schooling at age 35, by age at daycare exposure

Exposure age:	Age 3 (1)	Age 4 (2)	Age 5 (3)
Dependent variable			
Mother outcomes, child age 4			
Employment	0.043***	0.052***	0.050***
	(0.004)	(0.004)	(0.004)
Full-time employment	0.014***	0.016***	0.016***
	(0.002)	(0.002)	(0.002)
Observations	403241	403241	403241
Child outcomes, age 35			
Length of schooling	0.081***	0.092***	0.096***
	(0.017)	(0.017)	(0.017)
High school/vocational	0.008***	0.009***	0.010***
	(0.003)	(0.003)	(0.003)
College/university degree	0.015***	0.017***	0.017***
	(0.003)	(0.003)	(0.003)
Observations	889392	889392	889392
Log earnings	0.009**	0.012***	0.011***
	(0.004)	(0.004)	(0.004)
1^{st}quartile earnings	0.000	-0.000	0.000
	(0.002)	(0.002)	(0.002)
4^{th}quartile earnings	0.011***	0.012***	0.012***
	(0.003)	(0.003)	(0.003)
Observations	780960	780960	780960

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Column (1) shows the effects of child care, where child care is measured when the focal child is three. Column (2) shows the effects of child care, where child care is measured when the focal child is four (main results). Column (3) shows the effects of child care, where child care is measured when the focal child is five. All columns include co-variates, cohort dummies, municipality fixed effects and quadratic grouped trends. The included covariates are indicators of gender of the focal child, urban area, mother's age at birth, and number of children (mother outcomes) or birth order (child outcomes).

7 Conclusion

Received wisdom is that both targeted and universal childhood interventions have beneficial long-run effects for the underprivileged. However, the existing literature suggests that outside underprivileged groups, long-run effects are at best non-existent. Exploiting the first Scandinavian transition from targeted to universal daycare fol-

lowing a reform in 1965, we investigate the long-run effects of universal daycare availability on child and mother outcomes. We find that universal daycare increased offspring schooling attainment and earnings through age 45. As intended with the reform, daycare availability also increased maternal employment and earnings in both the short- and long-run.

Overall, the rollout of universal daycare increased household resources, changed the mode of care for many children, and increased child long-run outcomes. However, heterogeneity in both child and maternal outcomes helps us understand some of the causal mechanisms at play. For mothers, the employment *level* was higher for mothers with higher educational attainment than for mothers with basic educational attainment throughout our period, and in line with these summary statistics, the effect of daycare availability on maternal employment decreases as mothers' educational attainment increases. Indeed, the effect on full-time employment for high-educated mothers is close to zero. Assuming that employed mothers are more likely to use informal out-of-home care than unemployed mothers, this heterogeneous pattern of effects is consistent with mothers using different alternative modes of care. Thus, similar to Havnes and Mogstad (2011a), we conclude that formal care crowded out informal care, at least for high-educated mothers, because they were already in the labor market prior to the reform and thus more likely to be using informal care.

We find strongest offspring earnings effects for children of mothers with more than basic educational attainment, whereas we find closer to zero offspring earnings effects for mothers with basic educational attainment. Assuming that informal non-maternal care is of lower quality than maternal care, this result is in line with the previous finding for maternal employment. Children of high-educated mothers, who were more likely to be in informal care pre-reform, received the largest gains from transferring from informal care to formal care compared to children of mothers with basic educational attainment, who more likely transferred from maternal care to formal care. Thus, from a policy perspective, our results suggest that supporting formal settings for daycare, in which a certain level of quality is guaranteed (e.g., through educated personnel) is more beneficial for children than providing subsidies to support informal forms of child care where quality can vary.

Dividing outcomes by child gender, we find that daycare availability affects only boys' educational attainment and earnings, whereas maternal employment and earnings do not vary by child gender. If we assume that child responses to household resources are similar for boys and girls, this contrast between generations suggests

that increased household financial resources during childhood (which we find are similar for boys and girls) do not play an independent role in the causal chain from universal daycare to offspring outcomes. Moreover, because maternal employment effects do not vary by child gender, this result suggests that maternal labor market activity per se, whether at the intensive or extensive margin, in the short run or long run, does not play an independent role in the causal chain from daycare to offspring outcomes either. However, because the availability of daycare causes maternal labor supply to increase *and* household financial resources to increase, we cannot identify their independent roles in the causal chain; countervailing effects might cancel out. Nevertheless, in our case, maternal employment is relevant for the daycare effect because of the mothers' role in providing the alternative mode of care.

The magnitude of the effects on children's earnings and educational attainment, with a plausible four percent earnings return to schooling, suggests that most of the effect of daycare on earnings works through higher educational attainment. Furthermore, one potential explanation for the gender differences in the educational attainment (and earnings) effects is that daycare improves boys' school preparedness (and acquisition of skills at school, which is marketable afterward). Thus, daycare is beneficial for reducing the achievement gap between boys and girls.

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A Appendix

A.1 Selective migration

We investigate if selective migration is affecting our results by using information about the latitude and longitude of the neighborhood in which the mother is born and her neighborhood of residence in 1970. We calculate the distance between the mother's place of birth and her residence in 1970 and use the distance as an explanatory variable in regressions where daycare availability is the outcome variable.

Table A.1 shows the results of the regressions, Column (1) only includes the distance and cohort dummies. The point estimate of 0.00044 suggest that mothers who move 100 km away from where they are born are 0.044 percent more likely to reside in a neighborhood with daycare. Adding municipality fixed effects in Column (2) suggest that mothers who move 100 km away from where they are born are 0.005 percent more likely to live in a neighborhood with daycare in 1970. In Columns (3) and (4) we add trends to the regression, which does not alter the results. While all four columns show a statistical significant relation between distance and daycare availability, the magnitude of the estimates are minor. For instance, mothers moving

Table A.1: The effect of distance between mother's place of birth and neighborhood of residence 1970 on daycare availability

	(1)	(2)	(3)	(4)
Distance mother-1970	0.00044***	0.00005***	0.00005***	0.00005***
	(0.00005)	(0.00001)	(0.00001)	(0.00001)
IHST distance mother-1970	0.00935***	0.00006	0.00005	0.00006
	(0.00255)	(0.00085)	(0.00085)	(0.00085)
Observations	349854	349854	349854	349854
Cohort dummies	Yes	Yes	Yes	Yes
Municipal fixed effects	No	Yes	Yes	Yes
Linear groupded trends	No	No	Yes	No
Quadratic grouped trends	No	No	No	Yes

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions. Column (1) only includes the explanatory variable and cohort dummies. Column (2) also includes municipality fixed effects. Columns (3) and (4) include linear or quadratic grouped trends, respectively. The mean distance between mother's place of birth and neighborhood of residence in 1970 is 51 km with a standard deviation of 68 and the 50th percentile is 19 km. IHST is short for Inverse Hyperbolic Sine Transformation.

from the most western point to the most eastern point in Denmark, approximately 450 km, are 0.02 percent more likely to have daycare available. In fact, 8 percent stay in the same neighborhood and only 19 percent of the mothers move more than 100 km away from where they are born. The mean distance is 51 km with a standard deviation of 68 and the distribution of distance has a long right-hand tail. Consequently, we convert the distances to a logarithmic scale. However, as 8 percent of the mothers stay in the neighborhood they are born in, we use the Inverse Hyperbolic Sine Transformation (IHST) in order to keep those with zero distance. Using IHST distance and including municipality fixed effects indicate that there is no correlation between the distance the mothers move and daycare availability. Consequently, the results presented in Table A.1 indicate that selective migration is not confounding our results.

A.2 Additional heterogeneity

Table A.2: Heterogeneous effects of daycare on children's pooled earnings at age 30-40 and 30-45

	Age 35 (1)	Age 30-40 (2)	Age 30-45 (3)
Dependent variable			
Panel A: All			
Log earnings	0.012***	0.010***	0.012***
	(0.004)	(0.004)	(0.004)
Observations	780960	7940800	9774308
Holding a job	0.002	0.002	0.002*
	(0.001)	(0.001)	(0.001)
Observations	889387	9068683	11198612
Panel B1: Girls			
Log earnings	0.001	0.002	0.004
	(0.006)	(0.004)	(0.004)
Observations	382566	3881049	4794021
Holding a job	-0.005***	-0.004***	-0.005***
	(0.002)	(0.002)	(0.002)
Observations	436362	4453686	5503186
Panel B2: Boys			
Log earnings	0.022***	0.018***	0.021***
	(0.005)	(0.005)	(0.005)
Observations	398394	4059751	4980287
Holding a job	0.008***	0.008***	0.008***
	(0.002)	(0.002)	(0.002)
Observations	453025	4614997	5695426

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. The table shows the child earnings effects at age 35 (Column (1)), pooled earnings effects at age 30 through 40 (Column (2)), and pooled earnings effects at age 30 through 45 (Column (3)). Each cell shows point estimates from separate regressions. All columns include covariates, cohort dummies, municipality fixed effects and quadratic grouped trends. The included covariates are indicators of gender, urban area, mother's age at birth, and birth order. In Columns (2)-(3) we include indicators of age in the year where earnings are measured.

Table A.3: Heterogeneous effects of daycare and child gender on maternal employment, earnings, and family income by maternal educational attainment

Maternal schooling level:	All (1)	Basic (2)	High/voc (3)	Col/uni (4)
Mother outcomes, child age 4				
Employment				
Daycare	0.050***	0.048***	0.030***	0.023**
	(0.004)	(0.004)	(0.006)	(0.010)
Boy	-0.004*	-0.006**	-0.003	-0.009
	(0.003)	(0.003)	(0.006)	(0.010)
Daycare × Boy	0.003	0.004	0.001	0.010
	(0.003)	(0.004)	(0.007)	(0.011)
Full-time employment				
Daycare	0.017***	0.009***	0.011***	0.016
	(0.002)	(0.002)	(0.004)	(0.011)
Boy	0.000	-0.001	0.001	0.001
	(0.002)	(0.001)	(0.004)	(0.012)
Daycare × Boy	-0.001	-0.001	-0.002	0.000
	(0.002)	(0.002)	(0.004)	(0.013)
Mother outcomes, child age 17				
Log earnings				
Daycare	0.051***	0.056***	0.007	0.053***
	(0.008)	(0.009)	(0.011)	(0.018)
Boy	-0.005	-0.004	-0.015	0.000
	(0.007)	(0.010)	(0.012)	(0.019)
Daycare × Boy	0.003	-0.009	0.020	0.014
	(0.008)	(0.011)	(0.013)	(0.022)
Observations	306447	154350	112515	39582
Log family income				
Daycare	0.045***	0.031***	0.022***	0.034***
	(0.006)	(0.005)	(0.006)	(0.013)
Boy	-0.006*	-0.008**	-0.001	-0.013
	(0.003)	(0.004)	(0.006)	(0.009)
Daycare × Boy	0.006*	0.008*	0.000	0.013
	(0.004)	(0.005)	(0.007)	(0.009)
Observations	400787	220897	136055	43835

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions. All columns include covariates, cohort dummies, municipality fixed effects and quadratic grouped trends. The included covariates are indicators of urban area, mother's age at birth and number of children. Basic schooling equals 10 or less years of completed schooling. High school/vocational training equals 11-13 years of completed schooling. College/university degree equals 14 or more years of completed schooling. Family income is equalized by family size, where the second parent count 0.7 and each child counts 0.5. Family income is measured in brutto amounts.

Chapter 2

How Daycare Affects Intergenerational Transmission of Schooling

How Daycare Affects Intergenerational Transmission of Schooling*

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2018

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Abstract

Increased female schooling, greater labor market participation, and the wider availability of daycare in many countries have changed the way mothers interact with their children. We exploit a Danish schooling reform affecting the maternal generation alongside differential access to daycare affecting the offspring generation, and identify the causal chain from maternal schooling, via daycare availability, to child's schooling. We find that one more year of maternal schooling increases maternal labor supply by five percentage point and offspring schooling by two months. Greater daycare availability increases schooling transmission – consistent with the complementarity of early years of highly schooled maternal care followed by later institutional care provision.

Keywords: Intergenerational Transmission, Schooling, Daycare
JEL codes: *I2, I21, J13*

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1 Introduction

Historical changes in schooling laws have raised the educational attainment of parents (Black et al., 2005). Female labor supply has increased, and greater maternal labor market participation was facilitated by the expansion of subsidized daycare (Baker et al., 2008). At the same time, daycare affects long-run schooling of exposed children (Herbst, 2017).

Schooling and daycare reforms have affected the skills of parents and the environments that children are exposed to both inside and outside the home. However, despite both policies often affecting the same families, economic analyses of the effects of daycare exposure on child outcomes have only been studied in isolation from the effects of parental schooling on child outcomes. While daycare has been found to be beneficial for children of parents with low schooling, unmeasured traits correlated with parental schooling and daycare availability might drive the association.

This paper combines the literature on intergenerational schooling transmission and the literature on long-run effects of universal daycare exposure in order to examine how the intensity of intergenerational schooling transmission varies with exposure to daycare. We exploit a schooling reform affecting the maternal generation, together with differential access to daycare for the child generation, in order to identify the effects of daycare, and the role of daycare as a link in the causal chain from maternal schooling to child schooling.

Denmark is especially informative for several reasons. First, a school reform and a daycare expansion affected subsequent generations. Second, the daycare coverage has a long and well-documented history and today, public provision of care for children younger than six years is the most comprehensive, expensive, and highly prioritized.[1] Third, early cohorts of children exposed to universal daycare have now completed their education.

Daycare is often cited as being one of the main engines of high intergenerational mobility. Carneiro and Heckman (2003) propose a theoretical framework of learning-begets-learning to interpret the observed high returns to targeted early childhood interventions. Esping-Andersen (2008) argues that maternal employment reduces child poverty risk and, in combination with high quality non-parental childcare, can

[1] In 2014, 65.2 percent of all children younger than two and 95.5 percent of all children between age three and five enrolled in daycare (OECD, 2016b). The average cost of daycare was USD 9,200 per child in 2013. Daycare is highly prioritized with 1.4 percent of GDP in 2013 (OECD, 2016a).

diminish reproduction of inequality. In Denmark, current daycare provision is now population-wide, but there has been an enormous variation in the development of provision over time across municipalities. We exploit the differential regional expansion of daycare and relate it to changes in intergenerational transmission for children growing up at different times in different parts of the country.

Using the school reform as an instrument for maternal schooling, we find that one more year of maternal schooling increases offspring schooling by two months and that children who had access to daycare gain more from their mothers' schooling. By using information about mother's work hours during the offspring's preschool years, we show that both schooling level and daycare access in themselves have positive effects on maternal work hours; however, maternal schooling does not have differential effects with respect to daycare on maternal work hours. This suggests that the mode of care shift is different across areas with high and low daycare density and that we identify a causal chain from maternal schooling, via mode of care, to child's schooling.

The literature on estimating the causal relationship between parental schooling and child's schooling has taken three different approaches: adoption studies, twin studies, and policy-induced variation, and is well summarized in the review by Holmlund et al. (2011). First, adoption studies show that, if adopted children are genetically unrelated and randomly matched to the families that raise them, then the effect of family environment and endowments at birth are perfectly separated. Plug (2004), using U.S. adoption data, finds a causal effect of father's schooling but not of mother's. Second, twin studies use parents who are themselves identical twins and their children to difference out heritable endowments transmitted from parent to child and parenting skills shared by the twin parents. Behrman and Rosenzweig (2002) is a leading example, finding father's schooling to increase the child's schooling but no effect of mother's schooling. Third, policy-induced variation in parental schooling is a widely used research approach. Black et al. (2005) and Oreopoulos et al. (2006) use changes in compulsory schooling laws for parents to estimate the effect on children's schooling. For Norway, Black et al. (2005), find that increasing maternal schooling by a year increases offspring schooling by 0.122 of a year among a sub-sample of mothers with less than 10 years of schooling. For Sweden, Holmlund et al. (2011) find similar results. For the U.S., Oreopoulos et al. (2006) find that one more year of parental schooling reduces the probability of offspring repeating a grade. In our paper, we follow the policy variation approach to provide exogenous

variations in maternal schooling.

There is a small but growing literature estimating the causal effects of universal daycare on long-run child outcomes. Herbst (2017) uses US state-wise variation in level of subsidy for a universal daycare and after school program operating for two years during World War II. He finds positive effects on child outcomes at age 40, especially for disadvantaged families. Havnes and Mogstad (2011, 2015) use an expansion of daycare in Norway from the late 1970's. They find that exposure to daycare increases child educational attainment for low-SES families. In contrast, Bingley et al. (2018) find that daycare exposure in Denmark increases child educational attainment most for high-SES families using an expansion of universal daycare during the mid-1960s and 70s.

This paper adds to the novel empirical literature investigating potential interactions between various types of shocks inspired by the theory of capacity formation (Heckman, 2007). Almond et al. (2017) review some recent papers investigating interactions between a shock and an investment in human capital. Adhvaryu et al. (2016) and Gunnsteinsson et al. (2016) use a negative shock facilitated by a natural disaster in combination with variation in cash incentives for school enrollment and vitamin D supplements, respectively. Kalil et al. (2016) investigate how father presence affects intergenerational correlations in schooling using the event of the father's death and a sibling fixed effects design. Kalil et al. (2016) use within family variations of time with father, while accounting for birth order effects, and find that longer paternal presence increases the father-child association in education and decreases the mother-child association.

Two papers investigate how early childcare exposure interacts with others factors in childhood. Johnson and Jackson (2017) investigate interactions between preschool spending and public school spending for poor children. They find that preschool spending is more beneficial when followed by access to better funded public schools. Rossin-Slater and Wüst (2016) examine how a home visiting nurse program and a targeted childcare program interact. In isolation, both the home visiting program and targeted childcare have positive effects on educational attainment and income. However, their results suggest that children not exposed to the home visiting nurse program gain most from the targeted childcare. As highlighted by Almond et al. (2017) this result can reflect similarities between the programs and that the home visiting program trained parents to give better care at home reducing the benefit of care in non-parental childcare setting.

In the remainder of the paper, we relate the differential expansion of daycare to the pattern of intergenerational transmission of schooling. Section 2 presents the institutional background about a school reform affecting the maternal generation and the daycare expansion affecting their offspring. In Section 3 we describe the method and data on the school reform, daycare institution information, and individual background and outcome measures that are observed. Estimates of intergenerational transmission and its association with daycare are presented and discussed in Section 4, and finally we draw some conclusions in Section 5.

2 Institutional Background

We exploit two sets of policy-induced regional variation in our analysis, where the first set of regional variation affected the mothers and the second set of regional variation affected the children. In this section we describe both sets of policy-induced variation starting with the school reform in 1958, which affected the maternal generation.

2.1 The 1958 school reform

During the period we consider in this paper the mandatory schooling level in Denmark was seven years. A school reform in 1937 required 8th and 9th grade teaching to be provided in the market towns. At the time, there were 87 market towns (urban areas) with a status of historical and economical importance, which entailed a higher degree of administrative autonomy than the rural towns had. A key difference between the rural and urban areas was the access to educational opportunities after 7th grade. However, in 1958 the Danish government implemented a major school reform alleviating differences between rural and urban areas. Our 1958 reform required 8th and 9th grade teaching be provided in the rest of the country. The 8th and 9th grade was not compulsory before the 1972/3-73/4 school year.

There were four elements to the 1958 school reform. The first part of the reform removed a selection mechanism that separated children into an exam taking line and a more vocational line. Before 1958, the selection mechanism into the exam taking line took place at the end of 5th grade. Subsequently, selection happened at the end of 7th grade. The second part changed the school year from beginning in April to beginning in August. Implementation was such that the 1957/8 school year was

lengthened by four months (effectively by 10 weeks because summer vacation always spanned the six weeks through end of July). The third part changed the school start date of birth rules, which essentially narrowed the discretionary early start window. The fourth part of the reform abolished the differences between rural and urban areas. Parts one through three of the reform were once-and-for-all across the whole country, whereas only the fourth part of the reform differentiated between municipalities. Rural municipalities were obliged to provide 8th and 9th grade teaching for children born from April 1, 1946. Arendt (2005, 2008) used the 1958 school reform to look at schooling effects on health outcomes.

Figure 1: Distance to nearest 8th grade

NOTE— The figure shows the distance to the nearest market town for all parishes prior 1958. The lightest color represents the urban areas or market towns, and the darker colors represent the rural areas.

The 1958-reform dramatically changed the average distance pupils had to commute in order to obtain post-compulsory schooling. Figure 1 displays the distance from each rural parish to the nearest market town, which is equivalent to the min-

imum distance pupils from the rural areas, had to travel to attend 8th grade prior to the reform. The lightest color represents the urban area (market towns) and the darker colors are the rural areas. Prior to the reform more than half of the parishes had more than 10 km to the closest post-compulsory school option, in the year immediately after the reform this was only the case for 11 parishes. Thus before 1958, children growing up in rural and urban areas had different educational opportunities. The reform changed the educational opportunities for children growing up in the rural areas of Denmark, and we exploit this change of educational opportunities to account for maternal selection into schooling.

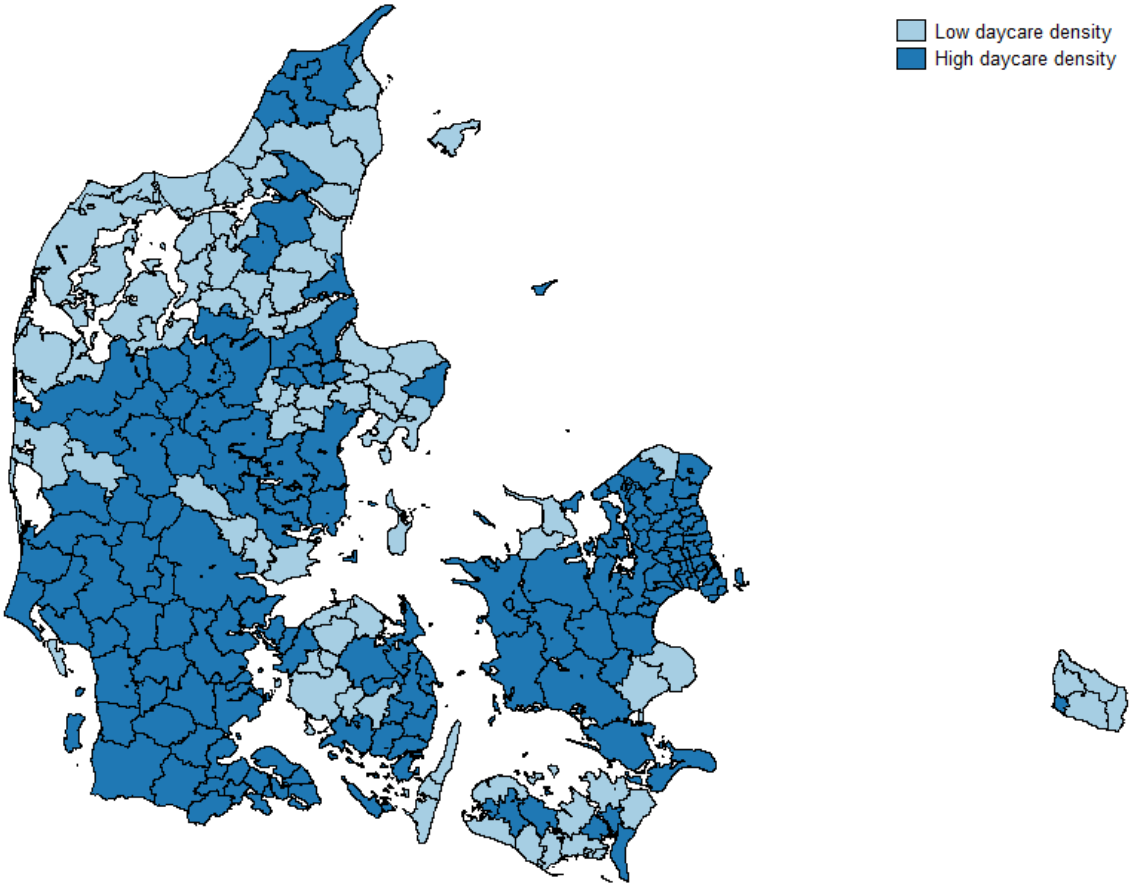
2.2 Daycare density

From 1933, childcare was targeted the poor with subsidies for institutions with at least two-thirds of the enrolled children living in poverty. By 1960, 9 percent of children aged three through six were enrolled in childcare. In 1964, a reform changed the purpose of daycare institutions from a social to a pedagogical perspective aimed at children from all types of families. After the reform, the State covered all housing costs and 40 percent of running costs. Municipalities covered 30 percent of the running costs and parents the remaining 30 percent. The Danish parliament passed the reform with broad political support. After the reform, there was a dramatic increase in the enrollment in daycare. By 1976, 30 percent of all children aged six or younger enrolled in subsidized daycare. This number has increased gradually since; and today, two-thirds of all children younger than three and 95.5 percent of all children aged three through six in Denmark attend daycare (OECD, 2016b). The Statistics Denmark daycare institution register begins in 1976, and we use this to capture daycare exposure for birth cohorts 1971-81.

The training and education of personnel in daycare institutions gradually improved throughout our period. Each institution should have a certain minimum proportion of educated personnel, and the duration of education was extended to three years. Besides the formal daycare institutions, there has always existed a private alternative in the form of licensed caregivers, where private persons opened their homes for up to five children. Most of those arrangements were organized and quality-assured by the municipality, and most licensed caregivers were municipality employees. There were no formal educational requirements for licensed caregivers, and they were mostly used by mothers with children under aged three.

To meet the general demand for higher welfare services a large reform led to a merge of municipalities in 1970, implying that the offspring generation grew up in larger municipalities than their mothers. Figure 2 shows the municipality borders that existed when the offspring generation grew up. The map shows the division of the municipalities according to the definition of high and low daycare coverage. Our analysis uses regional variation in the coverage of subsidized center-based daycare. We follow a similar strategy as Havnes and Mogstad (2011), who exploit variations in the supply shocks of daycare and divide the municipalities by the median according to the increase in daycare coverage.

Figure 2: High and low daycare density for post 1970 municipality borders.



NOTE— The figure maps the municipality borders that existed in Denmark 1970-2007. The dark (light) color indicates the municipalities defined by a high (low) daycare density.

3 Method and Data Description

3.1 Data sources

We combine historical geographic records with administrative data on a sample of mothers born between 1940 and 1950. From the demographic registers, we collect information on date, parish, and municipality of birth, in addition to identifiers of any children and the fathers of these children for each individual in our sample. We observe children born 1971-81. Using individual identifiers for mothers, fathers, and children, we merge the demographic information with the education register to collect information on the highest level of completed schooling. Ideally, we would like to measure the mother's education before the child is born; as the education register starts in 1973, we use this year to measure mother's highest level of schooling achieved. We measure the children's highest level of completed schooling in 2011, when the children are aged 30-40.

To account for selection into schooling, we exploit historical information on school opportunities in combination with individual locality information. We assign mothers to school districts according to their parish of birth, rather than assignment to home address at age 14 (8th grade). Assignment at birth is not subject to bias due to moving parish for later schooling opportunities.[2] We calculate the minimum distance as crow flies between the closest market town offering post-compulsory schooling and residence, measured as the location of the church of the parish, in which the mother was born. Prior to the reform, mothers living outside the market towns on average had to travel 17 km to attend post-compulsory schooling in the nearest market town.

To investigate whether mothers with more schooling are working more, and whether this varies by daycare availability, we collect information on individual work history from The Supplementary Pension Fund Register (called ATP). The ATP register begins in 1964 and contribution to the ATP is mandatory for all wage earners and salaried employees aged 18–66 years who are employed in Denmark. Contributions to the ATP vary according to hours worked. Persons working more than 29 hours a week pay the full ATP contribution, while persons working 20 to 29 (10 to 19) hours a week pay two-third (one-third) of the full ATP contribution. The ATP is not a perfect measure for hours worked, because it does not include overtime and hours for those

[2]Mobility between municipalities is not known over this time period, but for the first cohort (1970 births) for which we do have reliable mobility information, 15 percent are found to reside in a different municipality on their 12th birthday.

working less than 10 hours a week. However, the ATP is a good proxy for whether a mother spends the majority of her time away from the home. Especially, because self-employed such as farmers' wives are not included in the ATP. Consequently, the ATP is informative when identifying mode of care. The ATP contributions are available on an annual basis, and we use them to construct a measure ranging from zero to one, where zero indicate that the mother worked less than 10 hours a week while her child was preschool aged, and one indicate that the mother worked full time (at least 30 hours a week throughout the years) her child was preschool aged.

Although public provision of daycare for preschool children has a long history in Denmark, Statistics Denmark's administrative records of the number of daycare places at municipality level is only available from 1976. Consequently, we calculate the enrollment rate by using information on the number of daycare places scaled by the number of preschool children in each municipality in 1976. We then use a similar approach as Havnes and Mogstad (2011) and order the municipalities according to their enrollment rate. The municipalities with an enrollment rate above the median are defined as a high daycare density municipality, and the municipalities below the median are defined as a low daycare density municipality. We restrict our sample of children to ensure that offspring have completed their schooling by 2011. Consequently, we exclude offspring younger than 30 in 2011. To ensure that the sample of offspring is coherent with our measure of daycare availability, we exclude offspring older than 40 in 2011. Figure A.1, Panel (a) plots the densities of mother's age at childbirth in our sample (gray bars) and for all mother's born 1940-50 (white bars) along densities of mother's age at first childbirth in Panel (b). The restriction to only include children aged 30-40 in 2011 implies that our sample consist of mothers, who are 21-41 years at childbirth. To investigate the sensitivity to this selection criteria, Table A.2 plots the OLS and IV estimates of mother's schooling on child's schooling separately for mothers born 1940-50, who had children aged 30-50 and 30-40 in 2011.

3.2 Descriptive statistics

Tables 1 and 2 present descriptive statistics for the outcome variables and covariates we use in the analysis. Column (1) of Table 1 presents means and standard deviations of outcomes and covariates for the full sample, whereas Column (2) displays mean values for families, where the mother grew up in a market town (distance 0 km).

Columns (3) through (6) display the mean values for families in which the mother grew up in the rural areas in intervals of 5 km distance to nearest market town. The highest level of schooling is found for families in which the mother grew up just outside a market town, and this applies to both parents and children. However, parents and children in families in which the mother grew up in a market town are in general more schooled than those living in the rural areas. In particular, above the threshold of 5 km, there is a negative correlation between distance to nearest market town and level of schooling. Thus, the relation between distance and schooling is not linear, which we account for by including squared distances in the analysis.

Table 1: Descriptive statistics by mother's distance to nearest market town

	(1) All	(2) 0km	(3)]0,5]km	(4)]5,10]km	(5)]10,15]km	(6) >15km
Child's schooling	13.760	13.779	13.826	13.764	13.734	13.693
	(2.345)	(2.390)	(2.416)	(2.302)	(2.289)	(2.289)
Mother's schooling	11.451	11.891	12.009	11.270	10.998	10.897
	(3.126)	(2.959)	(2.977)	(3.166)	(3.200)	(3.195)
Father's schooling	12.095	12.528	12.707	11.885	11.606	11.531
	(3.255)	(3.084)	(3.099)	(3.308)	(3.299)	(3.332)
Male	0.512	0.512	0.508	0.514	0.519	0.512
	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)	(0.500)
Full-time equivalent	0.433	0.459	0.461	0.424	0.409	0.402
	(0.354)	(0.353)	(0.354)	(0.354)	(0.352)	(0.350)
Mother's age at birth	28.216	28.040	28.250	28.325	28.267	28.247
	(3.495)	(3.441)	(3.484)	(3.520)	(3.509)	(3.524)
Mother's age at first birth	24.504	24.513	24.716	24.541	24.387	24.339
	(3.657)	(3.631)	(3.740)	(3.684)	(3.603)	(3.604)
High daycare	0.845	0.873	0.921	0.851	0.805	0.768
	(0.362)	(0.333)	(0.269)	(0.356)	(0.396)	(0.422)
Observations	215418	47302	52349	30432	27823	57512

NOTE— Each cell represents the mean of the corresponding variable displayed in the row. Column (1) displays mean values for the total population. Column (2) shows mean values for the population in urban area. Columns (3)-(6) displays the mean values for the population in the rural areas for 5 km intervals of distance to nearest urban area. Standard deviations in parentheses.

Table 2 shows means and standard deviations for our population of 215,418 children distributed across the 275 municipalities existing in Denmark after 1970. In the first column, mean values are presented for the full sample and the subsequent

Table 2: Descriptive statistics by daycare density

	(1) All	(2) Low	(3) High
Child's schooling	13.760	13.657	13.778
	(2.345)	(2.261)	(2.360)
Mother's schooling	11.451	10.788	11.573
	(3.126)	(3.241)	(3.089)
Father's schooling	12.095	11.219	12.256
	(3.255)	(3.399)	(3.201)
Male	0.512	0.504	0.514
	(0.500)	(0.500)	(0.500)
Full-time equivalent	0.433	0.381	0.442
	(0.354)	(0.355)	(0.352)
Mother's age at birth	28.216	28.940	28.083
	(3.495)	(3.688)	(3.442)
Mother's age at first birth	24.504	24.282	24.544
	(3.657)	(3.787)	(3.632)
Distance to market town	10.305	14.694	9.498
	(12.356)	(14.454)	(11.753)
Observations	215418	33445	181973
Number of municipalities	275	137	138

NOTE— Each cell represents the mean of the corresponding variable displayed in the row. Column (1) displays mean values for the total population, and Columns (2) and (3) for municipalities with low and high day care provision, respectively. Standard deviations in parentheses.

two columns for low and high daycare density status, respectively. As previously explained, half of the municipalities are defined by a low daycare density and the other half of the municipalities by a high daycare density. Parents living in municipalities with high daycare density have about one year more schooling than those parents from low daycare municipalities. The positive correlation between schooling and daycare is also found for the children, as children from high daycare municipalities have about one and a half month more schooling than children from low daycare municipalities. Further, the mother's distance to the nearest market town is higher for children in municipalities with a low daycare density, which is consistent with the figures in Table 1. When comparing high and low daycare municipalities, we see a 5 km difference in how far the mothers themselves had to travel to attend 8th and 9th grade. Additionally, we see that mothers living in municipalities with a low daycare

density were on average almost a year older when giving birth.

These associations in the raw data suggest not only that mother's schooling is correlated with high daycare density, but also that this correlation might go through mother's distance to nearest market town. These correlations motivate the regression analysis presented in the next section.

3.3 Identification

We start the empirical analysis by considering Equation (1), where schooling of the child, S^c, is regressed on observable characteristics of the mother and the child.

$$S^c = \beta_1 S^m + \beta_2 S^m \cdot HDC^c + \beta_3 HDC^c + \beta_4 male^c + T^c\tau + X^m\delta + \epsilon \qquad (1)$$

Specifically, S^m indicates the schooling of the mother and HDC^c is an indicator taking the value one if the child grew up in a municipality with high daycare density. The interaction term between the high daycare density indicator and mothers schooling level is included to capture how daycare exposure of the child affects the intergenerational transmission of schooling from mother to child. Thus, β_2 captures the additional effect of maternal schooling for those children exposed to daycare compared to non-exposed children. Additionally, Equation (1) includes a dummy for the gender of the child to capture gender differences in schooling. Consequently, $male^c$ is an indicator taking the value one if the child is male. T^c is a full set of child year of birth dummies, included to capture flexible trends in schooling across birth cohorts. X^m is a matrix of observable characteristics of the mother; municipality fixed effects, a full set of year of birth dummies, and age at childbirth dummies. ϵ is allowed to be heteroskedastic and cluster at municipality level. OLS estimation of Equation (1) produces unbiased estimates of β_1 and β_2 if S^m conditional on the set of covariates is uncorrelated with ϵ. However, ϵ includes a number of unobservable characteristics such as inheritable ability and schooling preferences.

Consequently, we use policy-induced variation in maternal schooling stemming from a school reform. This approach is in line with Black et al. (2005) for Norway and Holmlund et al. (2011) for Sweden, who use changes in compulsory schooling laws. The 1958 school reform, we consider in this paper, is not an increase of the compulsory schooling level, instead we consider a reform, that increased the educational opportunities for some pupils.

The 1958 school reform essentially reduced the distance that rural pupils needed to travel in order to attend 8th and 9th grade. We use this discrepancy in a two stage least squares framework to account for maternal selection into schooling and estimate the intergenerational transmission of schooling.

$$S^c = \beta_1 \widehat{S^m} + \beta_2 S^m \widehat{\cdot HDC^c} + \beta_3 HDC^c + \beta_4 male^c + T^c\tau + X^m\delta + \epsilon \tag{2}$$

Equation (2) displays the second stage. We add an interaction term between maternal schooling and high daycare density as an endogenous explanatory variable in our main specification, to examine how daycare exposure of the child interacts with maternal schooling in the intergenerational transmission of schooling. This exercise results in two first stage equations.

$$S^m = f(dist^m)(HDC^c + LDC^c)\alpha + v \tag{3}$$

$$S^m * HDC^c = f(dist^m)(HDC^c + LDC^c)\omega + \varepsilon \tag{4}$$

Where $f(dist^m)$ is our instrument and $LDC^c = 1 - HDC^c$. As an instrument for maternal schooling we use distance and squared distance to nearest school offering post-compulsory schooling in combination with a difference-in-differences (DiD) framework. Pre-reform, rural pupils needed to commute to the nearest market town. Post-reform, all pupils could stay within their home municipality, just as pupils in the market towns could all along. We construct an interaction term between an indicator of pre-reform cohorts and distance and squared distance to the nearest market town and use this interaction as an instrument for maternal schooling. To distinguish between exposures to high and low daycare density we include an interaction term between the instrument (distance and squared distance for pre-reform cohorts) and a dummy for belonging in a municipality with high or low daycare density. In the second stage, we include the dummy for high daycare density, but to avoid violating the exclusion restriction high daycare density is not a part of the excluded instruments.

In order to obtain causal estimates of how daycare moderates the intergenerational transmission of schooling, our measure of daycare density must be uncorrelated with the instrument. Table 3 displays the results of a series of regressions where the high daycare density measure is the dependent variable, while the instruments are the main explanatory variables. In Column (1), we estimate the pure correlation be-

Table 3: School distance and daycare density

	(1) High daycare	(2) High daycare	(3) High daycare	(4) High daycare
Distance to market town	-0.00343***	0.00004	-0.00019	-0.00016
	(0.00095)	(0.00027)	(0.00036)	(0.00036)
Squared distance	0.00001	0.00000	0.00001	0.00001
	(0.00001)	(0.00001)	(0.00001)	(0.00001)
Municipality fixed effects	No	Yes	Yes	Yes
Year dummies	No	No	Yes	Yes
Mother's age at birth	No	No	No	Yes
Observations	215418	215418	215418	215418

NOTE— Each column represents a regression. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. Column (1) shows the pure correlation between high daycare density and the instrument (distance and squared distance to nearest market town). In Column (2) we include municipality fixed effects based on the mother's place of birth, Column (3) further includes year dummies for mother's year of birth, and Column (4) includes indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. * p<0.10, ** p<0.05, *** p<0.01.

tween high daycare density and the instruments without controlling for municipality- and year fixed effects. We find that there is a negative relation between the mother's distance to the nearest market town and the child's exposure to high daycare density. However, when we add municipality fixed effects to control for all time-invariant differences between municipalities, when the mother grew up, in Column (2) the instruments and the daycare measure are uncorrelated. Additionally, we add year fixed effects and indicators of mother's age at childbirth in Columns (3) and (4), which does not alter the result that the instruments we use for mother's schooling are uncorrelated with our measure of high daycare density for the children in our sample.

To investigate whether maternal schooling is driving the daycare density we perform a series of regressions, where we examine whether mother's schooling instrumented with distance and squared distance can explain daycare density. Without controlling for municipality of mother's own birth, her schooling appears to cause her to locate in a municipality with high daycare density for her child (cf. Column (1) in Table 4). Whereas, controlling for municipality of mother's own birth is enough to kill the relationship (cf. Column (2)). Hence we can treat daycare density and maternal schooling as conditionally (on mother municipality of birth) independent.

Table 4: The effect of mother's schooling on daycare density

	(1) High daycare	(2) High daycare	(3) High daycare	(4) High daycare
Mother's schooling	0.0728***	-0.0048	0.0001	-0.0018
	(0.0089)	(0.0054)	(0.0101)	(0.0099)
Municipality fixed effects	No	Yes	Yes	Yes
Year dummies	No	No	Yes	Yes
Mother's age at birth	No	No	No	Yes
First stage F-statistics	51.11	106.40	20.54	21.65
Observatons	215418	215418	215418	215418

NOTE— Each column represents a regression. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. Mother's schooling is instrumented with distance and squared distance to nearest market town. No futher control are included in Column (1). In Column (2) we include municipality fixed effects based on the mother's place of birth, Column (3) further includes year dummies for mother's year of birth, and Column (4) includes indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. * p<0.10, ** p<0.05, *** p<0.01.

4 Results

Table 5 presents the OLS estimates of maternal schooling and high daycare availability on child schooling. Each column in Table 5 corresponds to a regression, where the included explanatory variables vary. Column (1) displays estimates of the relationship between mother's schooling and child's schooling. There is a clear positive relationship between longer maternal schooling and longer offspring schooling. Our estimates suggest that increasing mother's schooling by one year increases the child's schooling by 0.25 of a year. Additionally, there is a negative relationship between being male and schooling, and this negative relationship is apparent in all columns of Table 5. Column (2) displays estimates of a regression where the indicator for high daycare density is added to the model. It is evident from these estimates that there is a positive correlation between growing up in a municipality with high daycare density and completed years of schooling for the children in our sample.

In Column (3) we exclude maternal schooling as an explanatory variable, this exclusion leads to an increase in the correlation between high daycare density and completed years of schooling compared to the estimate in Column (2). Specifically, the coefficient on the dummy variable for high daycare density triples in size if we do not control for maternal schooling. This difference in coefficients suggests that

maternal schooling is correlated with the alternative mode of childcare, and a simple DiD analysis that ignored this correlation would suffer from omitted variable bias. Typically, a large change in DiD coefficients suggests misspecification; in our case, it appears that the control state (alternative mode of care) differs according to maternal schooling and comparing differences in treatments (density of institutional daycare provision) only would lead to upward biased treatment effect estimates. This omitted variable bias might occur if mothers with more schooling had access to higher quality alternative modes of care for their children. Consequently, ignoring this difference in alternative modes of care by maternal schooling would spuriously load onto the coefficient for high daycare density. In Column (4) we include both maternal schooling and the high daycare density indicator as explanatory variables; in addition, we add an interaction term between them. In this model, the estimate of high daycare density turns negative, whereas the interaction term is positive. This suggests that greater daycare availability increases the association between maternal and offspring schooling. However, because the OLS results do not account for maternal selection into schooling, they cannot be interpreted as causal relations.

Table 5: OLS estimates of mother's schooling on child's schooling

	(1) Child's schooling	(2) Child's schooling	(3) Child's schooling	(4) Child's schooling
Mother's schooling	0.247***	0.247***		0.215***
	(0.004)	(0.004)		(0.004)
Male	-0.383***	-0.383***	-0.384***	-0.383***
	(0.011)	(0.011)	(0.010)	(0.011)
High daycare		0.056***	0.181***	-0.363***
		(0.016)	(0.021)	(0.063)
Mother's schooling*high daycare				0.038***
				(0.005)
Observations	215418	215418	215418	215418

NOTE— Each column represents a regression. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. * p<0.10, ** p<0.05, *** p<0.01.

To account for maternal selection into schooling we rely on a two stage least square framework, and use the 1958-reform to instrument maternal schooling. Table

6 presents the first stage estimates, where distance and squared distance to nearest market town are used to predict maternal schooling. As the schooling reform in 1958 equalized educational opportunities for children growing up in rural and urban areas, the instruments only affects pre-reform mothers from the rural areas. We code the instrument accordingly by setting distance equal to zero for post-reform cohorts and all mothers who grew up in a market town. Column (1) in Table 6 shows the first stage estimates of distance and squared distance, without including the high daycare density. For pre-reform mothers, living one km further away from a market town is correlated with 0.026 of a year (about 10 days) lower schooling. This suggests that there is an educational penalty of living in the rural areas prior to the reform. However, the sign of the first stage parameter estimate on squared distance is positive, suggesting that families residing very far from educational opportunities take the distance into account in a different way than families with shorter distances. This could be the case if the barrier for attaining more schooling is whether or not to travel (e.g., by bus) to the nearest market town offering 8th and 9th grade and not additional transportation time (once on the bus). Columns (2) and (4a) show the first stage estimates of distance and squared distance, separate for interactions with high and low daycare provision municipalities. The educational penalty of living in the rural areas is 0.24 of a year for mothers, who later reside in a high daycare provision municipality, and 0.34 of a year for mothers, who later reside in a low daycare provision municipality. In Column (4b) the endogenous variable is an interaction between maternal schooling and the high daycare indicator. Essentially, the first stage estimates in Columns (4a) and (4b) are combined in our main specification.

The reduced form estimates are presented in Table 7, as expected the instruments have a much smaller correlation with the child's schooling than the mother's schooling. However, it is still the case that there is a negative impact of mother's distance to post-compulsory schooling opportunities prior to the 1958-reform on child's schooling. In the first stage, we find that the educational penalty for mother's living one km further away from a market town is 0.026 of a year, Column (1) in Table 7 reveals that the educational penalty for those mother's children is 0.005 of a year. Column (2) displays the reduced form estimates of distance and squared distance, separate for interactions with high and low daycare density. Mother's distance to school affects child schooling only for children in high daycare density municipalities. Table 7 controls for municipality fixed effects and in Table 3 we show that mothers' distance to school is unrelated to child daycare density after controlling for municipality fixed

Table 6: First stage estimates of distance instruments on mother's schooling

	(1) Mother's schooling	(2) Mother's schooling	(4a) Mother's schooling	(4b) Mother's school*high
Distance	-0.0260***			
	(0.0046)			
Squared distance	0.0003***			
	(0.0001)			
Distance*high daycare		-0.0243***	-0.0243***	-0.0320***
		(0.0048)	(0.0048)	(0.0049)
Distance*low daycare		-0.0338***	-0.0338***	0.0230***
		(0.0059)	(0.0059)	(0.0024)
Squared distance*high daycare		0.0003***	0.0003***	0.0004***
		(0.0001)	(0.0001)	(0.0001)
Squared distance*low daycare		0.0002***	0.0002***	-0.0003***
		(0.0001)	(0.0001)	(0.0000)
High daycare		0.4440***	0.4440***	11.6074***
		(0.0472)	(0.0472)	(0.0545)
Observations	215418	215418	215418	215418

NOTE— Each column represents a first stage regression. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. The variable high daycare density is not a part of the excluded instruments. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. Columns (1) and (2) show the first stage estimates of the second stage estimates in Columns (2) and (3) in Table 8. Columns (4a) and (4b) show the first stage estimates of the second stage estimates in Column (4) in Table 8, where two endogenous variables are included. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

effects.

Table 8 presents the second stage estimates of the intergenerational transmission of schooling. Column (1) presents the results without taking daycare provision into account. There is a positive intergenerational transmission effect of schooling, in the sense that one more year of maternal schooling increases child's schooling by 0.175 of a year or equivalently 2 months. This result is in line with Black et al. (2005), who find that maternal schooling increases child's schooling by 0.122 of a year for children of parents with less than 10 years of schooling. Column (2) shows the estimates from a regression where the indicator for high daycare density is added to the model. The parameter estimate of mother's schooling on child's schooling is slightly smaller, when

Table 7: Reduced form estimates of mother's school distance on child's schooling

	(1) Child's schooling	(2) Child's schooling
Distance	-0.0049**	
	(0.0023)	
Squared distance	0.0001	
	(0.0000)	
Male	-0.3834***	-0.3843***
	(0.0104)	(0.0105)
Distance*high daycare		-0.0062**
		(0.0025)
Distance*low daycare		-0.0011
		(0.0029)
Squared distance*high daycare		0.0001**
		(0.0000)
Squared distance*low daycare		-0.0000
		(0.0001)
High daycare		0.1881***
		(0.0235)
Observations	215418	215418

NOTE— Each column represents a reduced form regression. Column (1) shows the estimates of the distance and squared distance instrument, while Column (2) shows the eatimates of distance and squared distance separately for high and low daycare. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. The variable high daycare is not a part of the excluded instruments. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

we control for high daycare provision. The intergenerational schooling coefficient in Column (2) suggest that one more year of maternal schooling increases child's schooling by 0.155 of a year controlling for high daycare provision. The parameter estimate of the high daycare indicator is 0.102. This suggests that children who grew up in a municipality with a high daycare density on average have 0.1 of a year more schooling than children from municipalities with low daycare provision. Column (3) presents the results of a specification where we exclude the endogenous variable mother's schooling. Thereby, the results in Column (3) are essentially the OLS results also presented in Column (3) of Table 5 showing the effect of daycare on child's schooling. The estimate in Column (3) suggest that children exposed to daycare

attain 0.181 of a year more schooling, which is a bit higher than the intention to treat estimates found in Bingley et al. (2018); Havnes and Mogstad (2011), but lower than the treatment of the treated estimates reported in Berlinski et al. (2008).

Table 8: IV estimates of mother's schooling on child's schooling

	(1) Child's schooling	(2) Child's schooling	(3) Child's schooling	(4) Child's schooling
Mother's schooling	0.175***	0.155***		0.144***
	(0.058)	(0.052)		(0.049)
Male	-0.383***	-0.383***	-0.384***	-0.383***
	(0.010)	(0.010)	(0.010)	(0.010)
High daycare		0.102***	0.181***	-0.695*
		(0.033)	(0.021)	(0.400)
Mother's schooling*high daycare				0.070**
				(0.035)
First stage F-statistics	21.62	16.73		12.81
Observations	215418	215418	215418	215418

NOTE— Each column represents a regression. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. In Column (1) mother's schooling is instrumented with distance and squared distance. In Column (2) mother's schooling is instrumented with interactions between the two distance measures and high versus low daycare density. In Column (3) mother's schooling is excluded. Column (4) include two endogenous explanatory variables as mother's schooling is interacted with high daycare density. * p<0.10, ** p<0.05, *** p<0.01.

In Column (4) of Table 8 we present the IV estimates of our most advanced specification, where we include the interaction term between maternal schooling and the high daycare density indicator. In this specification, the intergenerational transmission of schooling effect is 0.144 of a year. The parameter estimate on the high daycare indicator is negative and not statistically different from zero at the conventional significance level. However, the parameter estimate on the interaction term between maternal schooling and the high daycare provision indicator is significantly positive. The parameter estimate of the interaction term is 0.07 of a year, which suggests that children from municipalities with high daycare density achieves about 20 days more schooling by increasing mother's schooling by a year. The daycare effect in Column (4) of Table 8 is a LATE in the interaction term, i.e. the effect for children of mothers

who complied with the 1958 schooling reform.

The results point to the conclusion that daycare accentuates the causal effect of maternal schooling on child schooling. This is consistent with the theory of dynamic complementarity in human capital formation, where investment at one stage of child development make following investments more productive (Cunha and Heckman, 2007). Historical records show that enrollment in daycare for children younger than three are lower than the enrollment rate for child aged three through six (Johansen and Holten, 2015). Thus, in this context, the results suggest that early years of highly schooled maternal care followed by later institutional daycare provision has a positive effect on child's schooling.

We find IV estimates that are smaller than the OLS estimates. Larger OLS estimates suggest that the OLS estimates of mother's schooling on child's schooling are upward biased, because we expect mother's educational choice to be positively correlated with unobserved ability. This is in consonance with what others in the literature find using school reforms to identify the causal effects of parents' schooling on child's schooling. Black et al. (2005) find an OLS estimate of mother's schooling on child's schooling of 0.237, using a school reform implemented during the sixties and early seventies on Norwegian data they find an insignificant IV estimate of 0.076. Restricting their sample to only include mothers with less than 10 years of schooling, they find an OLS estimate of 0.211 and an IV estimate of 0.122 years of schooling. Using data from Sweden in combination with a school reform implemented during the fifties and early sixties, Holmlund et al. (2011) find an OLS estimate of 0.28 and an IV estimate of 0.11 for mother's schooling on child's schooling. These scholars analyze school reforms that lengthened the *mandatory* level of schooling; contrary, we analyze a school reform that increased the educational *opportunities* on a voluntary basis. This difference in school reforms may explain the somewhat larger IV estimates we estimate in this paper (Belzil and Hansen, 2010).

4.1 Mothers' work hours as a mechanism

Table 8 shows that children exposed to daycare benefit more from their mothers schooling. While children enrolled in daycare spend a considerable amount of time away from their parents, it is not clear that children not enrolled in daycare spend more time with their parents. Children not enrolled in daycare can be cared for in informal settings, such as grandparents or neighbors. While both informal and mater-

nal care are unobserved, we can use variations in mothers' hours of work as a proxy for variations in maternal care.[3] Assuming that a child is cared for in either a formal, informal, or maternal care setting informs us about how the mode of care varies by maternal schooling.

We continue our analysis by examining whether there is a causal effect of schooling on work hours for the mothers in our sample. Table 9 shows the effect of mother's own schooling on mother's work hours during the years when her child is of preschool age.

Table 9: IV estimates of mother's schooling on mother's work hours

	(1) Hours	(2) Hours	(3) Hours	(4) Hours
Mother's schooling	0.049***	0.050***		0.050***
	(0.009)	(0.008)		(0.008)
Male	-0.001	-0.001	-0.001	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
High daycare		0.038***	0.063***	0.052
		(0.005)	(0.003)	(0.063)
Mother's schooling*high daycare				-0.001
				(0.006)
First stage F-statistics	21.62	16.73		12.81
Observations	215418	215418	215418	215418

NOTE— Each column represents a regression. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. In Column (1) mother's schooling is instrumented with distance and squared distance. In Column (2) mother's schooling is instrumented with interactions between the two distance measures and high versus low daycare density. In Column (3) mother's schooling is excluded. Column (4) include two endogenous explanatory variables as mother's schooling is interacted with high daycare density. * p<0.10, ** p<0.05, *** p<0.01.

Increasing mother's schooling by a year increases mother's work hours by 4.9 percent (cf. Column (1)). Reassuringly, the gender of the child has no effect on maternal work hours (in any of our specifications). The effect of mother's schooling on mother's work hours increases to 5.0 percent when the dummy for high daycare den-

[3]Although, the objective of this paper is to use maternal work hours to inform about the mode of care, the amount and quality of time parents invest in their children have also been found to affect the cognitive development of children (Hsin and Felfe, 2014; Del Bono et al., 2016).

sity is added to the model in Column (2). The results in Column (2) further suggest that mothers in a high daycare density municipality work 3.8 percent more hours, when we account for maternal schooling. Column (3) shows the association between the dummy for high daycare density and mother's work hours measured when her child is in preschool age. In this specification we leave out maternal schooling, which loads on to the high daycare density estimate of 6.3 percent. In Column (4) we include the interaction term between maternal schooling and the dummy for high daycare density. In this specification, the effect of mother's schooling on mother's work hours remain 5.0 percent; however, there is no effect of the interaction term between maternal schooling and high daycare density on mother's work hours. This suggests that there is *no differential effect* of mother's schooling on mother's work hours during her child's preschool years for mothers residing in a municipality with a high daycare density compared to mothers residing in a municipality with a low daycare density. In contrast, if the interaction term between maternal schooling and high daycare density had been positive, high schooled mothers would be working more and consequently looking less after their children; thus, we would infer that daycare are better than high schooled maternal care (in terms of child schooling). Because the effect of maternal schooling on maternal labor supply does not differ by daycare density, the differential effect of maternal schooling on child schooling must be due to changes in the alternative mode of care. Assuming that the mother looks after the child the first years after the birth of the child, the mode of care shift is from mother to formal care in high daycare areas, but mother to informal care in low daycare areas. Thus, we identify a link in the causal chain from mother's schooling, via changing mode of care, to child schooling.

4.2 Robustness analysis

In this paper, we investigate the intergenerational transmission of schooling from mothers to children; thereby, we disregard the transmission from fathers to children and any effects of assortative mating there may exist. Consequently, our estimate of mother's schooling on child's schooling includes both the direct effect of mother's schooling and the indirect effect of father's schooling due to assortative mating. Oreopoulos et al. (2006) estimate the effect of the sum of parents' schooling on the probability of the child repeating a grade; however, because the main objective of this paper is how maternal schooling in combination with daycare affect child's school-

ing, our sample does not include schooling information on 0.8 percent of the fathers. To ease comparison with the estimates of mother's schooling on child schooling in Table 8 and account for missing information on fathers' schooling, we use a measure of average parental schooling, in which missing paternal schooling is equal to maternal schooling. Table 10 shows the effects of average parental schooling on child schooling. The results in Table 8 and Table 10 are very similar indicating that assortative mating is not a major issue.

Table 10: IV estimates of average parental schooling on child's schooling

	(1) Child's schooling	(2) Child's schooling	(3) Child's schooling	(4) Child's schooling
Parental schooling	0.178***	0.154***		0.138***
	(0.058)	(0.052)		(0.049)
Male	-0.383***	-0.384***	-0.384***	-0.384***
	(0.010)	(0.010)	(0.010)	(0.010)
High daycare		0.083**	0.181***	-0.853*
		(0.039)	(0.021)	(0.452)
Parental schooling*high daycare				0.080**
				(0.039)
First stage F-statistics	21.47	17.94		13.32
Observations	215418	215418	215418	215418

NOTE— Each column represents a regression. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. Parental schooling is the sum of schooling divided by number of parents (mothers' schooling is divided by one for the 0.8 percent with no information on fathers' schooling). In Column (1) parental schooling is instrumented with (mother's) distance and squared distance. In Column (2) parental schooling is instrumented with interactions between the two distance measures and high versus low daycare density. In Column (3) parental schooling is excluded. Column (4) include two endogenous explanatory variables as parental schooling is interacted with high daycare density. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

The main analysis is based on a sample of mothers born 1940-50 and their offspring born 1971-81. Consequently, mothers who have children between the age of 25 and 37 are over-sampled in our data, which is evident from Figure A.1, Panel (a). However, Figure A.1, Panel (b) shows that the distribution of mother's age at first birth in our sample of mothers is closer to the distribution of mother's age at first birth in

the population. One potential concern is that mother's schooling affects mother's age at birth and; consequently, whether the mother is included in our sample. Table A.1, Column (1) shows that increasing mother's schooling by a year is correlated with a increased probability of the mother having children aged 30-40 in 2011 (i.e., included in our sample). However, when instrumenting mother's schooling with distance and squared distance there is no longer any correlation between mother's schooling and the probability of having children aged 30-40 in 2011.

Nevertheless, to investigate how sensitive the intergenerational schooling estimates are to the exclusion of younger mothers (earlier born children), we expand the sample to include children born 1961-81 by mothers born 1940-50 and compare the intergenerational transmission of schooling across samples. Columns (1)-(2) of Table A.2 shows the OLS and IV results for children born 1961-81 (denoted A), while Columns (3)-(4) repeats the OLS and IV estimates from Column (1) in Tables 5 and 8 for children born 1971-81 (denoted B). Restricting the sample to children born 1971-81 increases the intergenerational transmission of schooling by approximately 22 days compared to the intergenerational transmission estimate for children born 1961-81. Thus, excluding earlier born children from the sample might upward bias our results. However, while the IV estimates are LATE it is reassuring that the IV estimates are smaller than the OLS estimates. Nevertheless, to account for the sample selection, we present estimates in Columns (5)-(6), that are weighted by the sampling probabilities (denoted C). The intergenerational transmission of schooling estimate presented in Column (6) is closer to the estimate presented in Column (2) for children born 1961-81 than the estimate in Column (4). However, the weighted IV estimate of the intergenerational transmission of schooling is still larger than the estimate presented in Column (2).

Because mothers who have children between age 25 and 37 are over-sampled in our data, the estimates in C put more weight on mothers younger than 25 and older than 37. While it is evident from Figure A.1, Panel (a) that the weights (relative size of the bars) are large for mothers younger than 25, the weights are equally large for mothers older than 37. Table A.3 excludes mothers who were older than 37 at childbirth (i.e., the right tail of the distribution in Figure A.1, Panel (a)) to test the sensitivity of the weights. The results are not sensitive to the exclusion of mothers older than 37, although the weighted IV estimate increases when excluding mothers older than 37 at childbirth.

Having established that weighting the estimates reduces the bias we potentially

infer by including only children born 1971-81, we continue the robustness analysis by presenting weighted IV estimates of the effect of mother's schooling via daycare density on child's schooling and mother's work hours, equivalent to those presented in Table 8 and Table 9. Table 11 presents the weighted IV estimates of mother's schooling on child's schooling. Comparing the intergenerational transmission of schooling estimate presented in Column (1) of Tables 8 and 11 suggest that weighting the estimates reduces the transmission of schooling from mother to child by 0.015 of a year or 5.5 days. The intergenerational schooling estimate in Column (1) of Table 11 suggests that increasing mother's schooling by a year increases child's schooling by 0.16 of a year. The interaction term between mother's schooling and high daycare density in both Tables 8 and 11 point to the conclusion that daycare provision accentuates the intergenerational transmission of schooling from mother to child.

Table 11: Weighted IV estimates of mother's schooling on child's schooling

	(1) Child's schooling	(2) Child's schooling	(3) Child's schooling	(4) Child's schooling
Mother's schooling	0.160***	0.134***		0.081
	(0.054)	(0.051)		(0.055)
Male	-0.378***	-0.378***	-0.376***	-0.379***
	(0.014)	(0.014)	(0.014)	(0.014)
High daycare		0.128***	0.200***	-1.182**
		(0.037)	(0.023)	(0.574)
Mother's schooling*high daycare				0.119**
				(0.053)
First stage F-statistics	25.44	19.64		14.31
Observations	215418	215418	215418	215418

NOTE— Each column represents a regression, in which the estimates are weighted to account for the skew sample as shown in Figure A.1, Panel (a). Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. In Column (1) mother's schooling is instrumented with distance and squared distance. In Column (2) mother's schooling is instrumented with interactions between the two distance measures and high versus low daycare density. In Column (3) mother's schooling is excluded. Column (4) include two endogenous explanatory variables as mother's schooling is interacted with high daycare density. * p<0.10, ** p<0.05, *** p<0.01.

Table 12 shows the weighted IV estimates of mothers' schooling on mothers' hours

of work. Specifically, the interaction term between mother's schooling and high daycare density is not statistically different from zero as in our main specification. Thus, maternal schooling positively affects maternal work hours but not differentially so by daycare density.

In summary, the weighted estimates in Tables 11 and 12 support our interpretation of the main results. Children exposed to daycare gain more from their mothers' schooling; and, because the effect of maternal schooling on maternal work hours do not differ by daycare density, we identify an effect of maternal schooling on child schooling, via changing mode of care, from mother to formal care in high daycare areas and from mother to informal care in low daycare areas.

Table 12: Weighted IV estimates of mother's schooling on mother's work hours

	(1) Hours	(2) Hours	(3) Hours	(4) Hours
Mother's schooling	0.046***	0.049***		0.054***
	(0.009)	(0.008)		(0.009)
Male	-0.001	-0.001	-0.000	-0.001
	(0.002)	(0.002)	(0.002)	(0.002)
High daycare		0.037***	0.064***	0.156
		(0.006)	(0.004)	(0.097)
Mother's schooling*high daycare				-0.011
				(0.009)
First stage F-statistics	25.44	19.64		14.31
Observations	215418	215418	215418	215418

NOTE— Each column represents a regression, in which the estimates are weighted to account for the skew sample as shown in Figure A.1, Panel (a). Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. The sample includes all mothers born 1940-50, who had children aged 30-40 in 2011. In Column (1) mother's schooling is instrumented with distance and squared distance. In Column (2) mother's schooling is instrumented with interactions between the two distance measures and high versus low daycare density. In Column (3) mother's schooling is excluded. Column (4) include two endogenous explanatory variables as mother's schooling is interacted with high daycare density. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

5 Conclusion

This paper provides new evidence to the intergenerational transmission of schooling literature by examining whether daycare moderates the intergenerational transmission of schooling from mother to child. Since WWII female schooling attainment and labor market participation have increased in many countries and thus the way that mothers interact with their children has changed. By using policy-induced variations in post-compulsory schooling to instrument for mother's educational attainment, we are able to estimate a positive causal relationship from mother's schooling to child's schooling.

Further, by including a measure of daycare density, we are able to examine how this schooling effect is moderated by daycare exposure of the child generations. We find that one more year of maternal schooling increases offspring schooling by two months. Greater daycare availability increases the effect of maternal schooling. Suggesting that daycare accentuates the causal effect of maternal schooling. This result is consistent with the complementarity of early years of highly schooled maternal care followed by later institutional daycare provision.

We investigate whether mothers work hours is a mediating factor, as both increased schooling and daycare access can affect mothers labor force participation. Our results suggest that the increased effect of maternal schooling via daycare is not driven by mothers work hours. We show that both schooling level and daycare access have positive effects in themselves on maternal work hours, however, maternal schooling does not have differential effects with respect to daycare on maternal work hours. This suggest that the mode of care shift is from mother to formal care in high daycare areas, but that the shift in mode of care is mother to informal care in low daycare areas. Thereby we are able to identify a mode of care story by mother's schooling effect.

Women across the world are pursuing more education and in many countries women today attain more education than men. However, in most countries women persist being the main caregiver in the early years of a child's life and take substantially more leave from work than men. Thus investing in mother's education may have dynamic effects on the educational level of the offspring generation. Enrollment rates in non-parental daycare are increasing and research has shown that childcare can have persistent effects at least for *some* children, thus focus on the marginal child enrolling in non-parental daycare is of importance. While our results suggest comple-

mentarity between early years of highly schooled maternal care and later institutional daycare provision, we are not able to identify which factors of high schooled maternal or institutional care are productive inputs in the production of offspring schooling. Thus further research is needed on this important topic.

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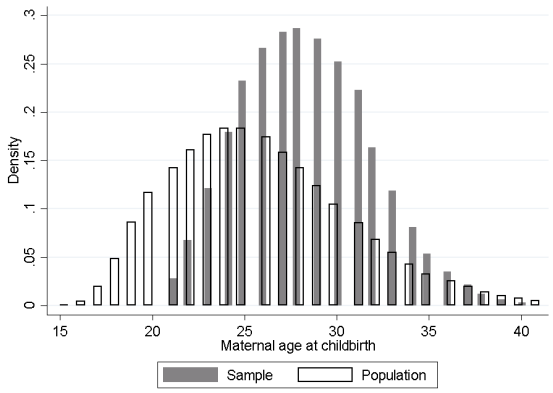
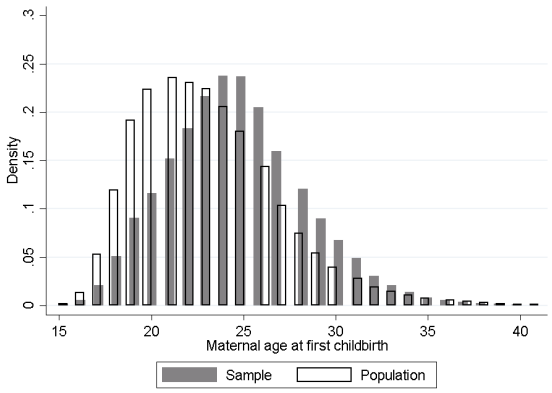
A Appendix: Sample selection

Figure A.1: Maternal age at childbirth in sample and population

(a) Age at childbirth



(b) Age at first childbirth



NOTE— The figure shows the densities of mother's age at childbirth in our sample (gray bars) and for all mothers born 1940-50 (white bars) in Panel (a). Densities of mother's age at first childbirth in our sample (gray bars) and for all mothers born 1940-50 (white bars) are shown in Panel (b). Our sample consists of all mothers born 1940-50, who have children aged 30-40 in 2011.

Table A.1: OLS and IV estimates of mother's schooling on the probability of having children aged 30-40 in 2011

	(1) OLS	(2) IV
Mother's schooling	0.001***	0.008
	(0.000)	(0.006)
Male	-0.000	-0.000
	(0.001)	(0.001)
First stage F-statistics		18.10
Observations	557390	557390

NOTE— Each column represents a regression, in which the outcome is a dummy variable taking the value one if the mother has any children aged 30-40 in 2011. Column (1) shows OLS estimates of mother's schooling on the probability of having children aged 30-40 in 2011. Column (2) shows the corresponding IV estimates, where mother's schooling is instrumented with distance and squared distance. All regressions include municipality fixed effects based on mother's place of birth, year dummies for mother's year of birth, and indicators of mother's age at first birth. * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.2: OLS and IV estimates of mother's schooling on child's schooling

	A		B		C	
	(1) OLS Child's schooling	(2) IV Child's schooling	(3) OLS Child's schooling	(4) IV Child's schooling	(5) W. OLS Child's schooling	(6) W. IV Child's schooling
Mother's schooling	0.233***	0.115***	0.247***	0.175***	0.247***	0.160***
	(0.003)	(0.042)	(0.004)	(0.058)	(0.004)	(0.054)
Male	-0.286***	-0.287***	-0.383***	-0.383***	-0.380***	-0.378***
	(0.007)	(0.007)	(0.011)	(0.010)	(0.014)	(0.014)
First stage F-stat		17.94		21.62		25.44
Observations	557390	557390	215418	215418	215418	215418

NOTE— Each column represents a regression. Estimates in (1), (3), and (5) are from OLS regressions, while estimates in Columns (2), (4), and (6) are from IV regressions. In Columns (2),(4), and (6) mother's schooling is instrumented with distance and squared distance. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. Sample A includes all mothers born 1940-50, who had children aged 30-50 in 2011, while the sample used in the analysis includes all mothers born 1940-50, who had children aged 30-40 in 2011 (B and C). The estimates indicated by C are weighted to account for the skew sample as shown in Figure A.1, Panel (a). * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Table A.3: OLS and IV estimates of mother's schooling on child's schooling excluding mothers older than 37 at childbirth

	A		B		C	
	(1) OLS Child's schooling	(2) IV Child's schooling	(3) OLS Child's schooling	(4) IV Child's schooling	(5) W. OLS Child's schooling	(6) W. IV Child's schooling
Mother's schooling	0.233***	0.115***	0.247***	0.178***	0.248***	0.176***
	(0.003)	(0.042)	(0.004)	(0.061)	(0.004)	(0.056)
Male	-0.286***	-0.287***	-0.379***	-0.380***	-0.372***	-0.371***
	(0.007)	(0.007)	(0.011)	(0.010)	(0.014)	(0.014)
First stage F-stat		18.41		21.36		25.97
Observations	557390	557390	212347	212347	212347	212347

NOTE— Each column represents a regression. Estimates in (1), (3), and (5) are from OLS regressions, while estimates in Columns (2), (4), and (6) are from IV regressions. In Columns (2),(4), and (6) mother's schooling is instrumented with distance and squared distance. Standard errors in parentheses are clustered at municipality level based on the mother's place of birth. All regressions include municipality fixed effects based on the mother's place of birth, year dummies for mother's and child's year of birth, and indicators of mother's age at childbirth. Sample A includes all mothers born 1940-50, who had children aged 30-50 in 2011, while the sample used in the analysis includes all mothers born 1940-50, who had children aged 30-40 in 2011 (B and C). The estimates indicated by C are weighted to account for the skew sample as shown in Figure A.1, Panel (a). * p<0.10, ** p<0.05, *** p<0.01.

Chapter 3

Daycare and Mothers' Working Lifetime

Daycare and Mothers' Working Lifetime*

Sarah Sander

2018

Abstract

This paper examines the effects of universal daycare on mothers' labor force participation, full-time employment, and earnings in the long run. I exploit differential access to daycare caused by a rollout of daycare across Denmark in combination with rich administrative data. Daycare availability has persistent effects on labor force participation and increases long-run earnings. Reduced fertility and parental separation are potential mechanisms behind the participation effects. For higher-educated mothers, participation effects diminish over time, whereas earnings effects prevail in the long run. These results suggest that labor market attachment during child-rearing years has important long-run economic consequences.

Keywords: Maternal labor supply, Daycare, Earnings
JEL codes: J13, J16, J21, J22

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1 Introduction

Children have consequences for women's career paths (Goldin, 1994; Waldfogel, 1998; Kleven et al., 2018), and policymakers across the world have initiated different strategies to facilitate labor force participation for parents during their child-rearing years. One strategy that policymakers use is subsidization of non-parental childcare. Enrollment in daycare institutions has been increasing in many countries (OECD, 2017), representing a move towards a universal childcare system and thereby lowering one potential barrier of female labor force participation.

Although there is a growing body of literature evaluating the effects of daycare on mothers' labor force participation, the results from these studies are inconclusive, with results varying between no or small effects (e.g., Havnes and Mogstad, 2011; Lundin et al., 2008) and positive employment effects (e.g., Lefebvre and Merrigan, 2008). Additionally, only a few studies evaluate long-run effects. Haeck et al. (2015) evaluate the medium-run effects of an introduction of daycare 11 years after implementation, and Herbst (2017) estimates the effects of a temporary wartime daycare program on maternal employment 17 years after the implementation of the program.

In this paper, I analyze the causal effects of daycare access on women's labor market participation and earnings through 34 years after the birth of their first child. This paper contributes to the literature by evaluating the life-long consequences of an expansion of universal childcare on mothers' family- and working lives. Access to daycare has the potential to enable mothers to obtain a higher attachment to the labor market and enhance their job skills. Thus, daycare access can affect mothers beyond the years in which, they have children of pre-school age.

In order to identify casual effects, I utilize the Danish transition from targeted to universal care. The transition followed after a reform in the mid-1960s. This reform mandated that all approved daycare institutions were eligible for subsidies; previously, only institutions in which two-thirds of the enrolled children lived in poverty were subsidized. After the reform, daycare operating costs were partly subsidized, and the state funded the building costs of new institutions. Together, this led to an increase in the number of daycare institutions. I exploit time and regional variations in access to daycare to estimate the long-run effects on mothers' labor market outcomes, and I provide evidence that daycare implementation is uncorrelated with pre-reform local employment trends.

The results suggest that access to universal daycare has lasting effects on maternal

employment and earnings for 34 years after the birth of the first child. The effects are larger for mothers with no post-secondary education. For low-educated mothers, the effect of daycare access on labor market participation is five percent 17 years after the birth of the first child, whereas the corresponding effect for higher-educated mothers is less than one percent. However, daycare access has a positive earnings effect 30 years after childbirth for higher-educated mothers, even though the employment effects are insignificant after 23 years. This suggests that accumulated labor market experience during the child-rearing years has important economic consequences in the long run.

I include both married and single mothers in the analysis. Although single mothers had access to targeted care prior to the daycare reform, universal daycare can affect their labor supply on the intensive margin (e.g., because of a higher quality of care). The rich administrative data allows me to investigate whether daycare availability affects completed fertility and parental separation. I find that mothers with daycare access have fewer children, wait longer before they get a second child, and are more likely to live apart from the first born child's father 16 years after childbirth. The results are robust to a series of sensitivity checks.

The rest of the paper is structured as follows: Section 2 summarizes the related literature about maternal labor supply. Section 3 outlines the pre-reform settings and the mid-1960s universal daycare reform in Denmark. Section 4 presents the unique daycare panel and the rich individual data used in this paper. Section 5 explains how the transition to universal daycare is used to identify the effects on maternal employment and earnings. Finally, the results are presented in Section 6, and Section 7 concludes.

2 Literature

In this section, I review the literature on how subsidized childcare affects maternal employment. More comprehensive reviews are given in Blau and Currie (2006) and Morrissey (2016).[1] The results in this literature range from no or small effects to large positive effects of universal daycare on mothers' labor supply. I structure this section according to the findings of the reviewed papers.

[1] A related literature examines how family leave and job protection after childbirth affect maternal labor supply, and evidence from the OECD countries are reviewed in Hegewisch and Gornick (2011).

Examples of studies finding modest or no effects include Havnes and Mogstad (2011) for Norway and Lundin et al. (2008) for Sweden. Lundin et al. (2008) study a recent reduction of daycare prices using a difference-in-differences regression matching estimator and find no effect on maternal employment. The Swedish reform introduced a cap on daycare prices and was implemented at a time when the maternal employment rate was already high at 70 percent. In contrast, Havnes and Mogstad (2011) examine a daycare reform in 1976, which introduced universal subsidized daycare in Norway at a time when 24 percent of the treated mothers were employed. They divide the Norwegian municipalities into a control group and a treatment group, in which the treatment group is defined as the municipalities with the highest post-reform growth in the enrollment rate. They find a 0.06 percentage point increase in married mothers' employment per percentage point increase in daycare coverage, and they argue that the introduction of universal daycare mostly crowds out informal care arrangements instead of increasing maternal employment.

Cascio (2009) and Fitzpatrick (2012) only find significant positive employment effects for single mothers. Cascio (2009) exploits a reform that led to an increase in the number of slots in U.S. public kindergartens. She exploits five waves of census data in the years 1950-90 and analyzes the effects on mothers with children aged five using a difference-in-differences design. The results suggest that for most mothers, there is no effect. However, Cascio (2009) find a large positive effect of 11.8 percent for single mothers with a youngest child aged five. Fitzpatrick (2012) uses a regression discontinuity instrumental variable framework to examine the effects of public kindergarten for children aged five and find that public kindergarten increases the labor supply of single mothers with no younger children.

Nollenberger and Rodríguez-Planas (2015) and Berlinski and Galiani (2007) find large effects that are imprecisely estimated. Nollenberger and Rodríguez-Planas (2015) investigate the effects of a reform in Spain that extended daycare to children aged three using a triple difference design. They find that maternal employment on average increases by 9.6 percent, a result that is only statistically significant at the 10-percent level. Similarly, Berlinski and Galiani (2007) use a difference-in-differences design to examine the impact of a large expansion of pre-primary school facilities in Argentina on maternal labor supply. They find large but imprecisely estimated effects on maternal employment ranging between between seven and 14 percentage points, depending on the specification and household type.

The literature also includes studies that evaluate daycare programs that success-

fully increase maternal employment. Carta and Rizzica (2018) exploit an eligibility cutoff for mothers with children aged two in a regression discontinuity design. They use data from Italy and find that daycare increases mothers' labor force participation by 7.1 percentage points, which corresponds to a 12.5 percent increase from a baseline participation rate of 57 percent. For the Netherlands, Bettendorf et al. (2015) find a modest increase in mothers' labor force participation of 3.6 percent and a 6.2 percent increase in hours worked using a reform that cut the childcare fee by 50 percent, along with offering increased tax credits for the same group of parents. Hardoy and Schøne (2015) examine the effects of a reform that combined price reductions and increased availability of daycare in Norway and find a five percent increase in the participation rate of mothers. The Norwegian price reductions were introduced at a time when maternal participation was 79 percent. Hardoy and Schøne (2015) argue that one potential reason why they find different results than Havnes and Mogstad (2011) is that the reform in the 1970s did not provide sufficient daycare availability, whereas the more recent reform studied by Hardoy and Schøne (2015) combined a price reduction with increased availability of daycare. The only other Danish study uses more recent data for a 10 percent sample of the population. Using Danish data, Simonsen (2010) finds that mothers reduce their employment when the price of daycare increases and that guaranteed access to daycare increases mothers' labor force participation in the year following childbirth. Several studies examine the effects of universal daycare in Quebec by comparing it with the rest of Canada and find positive maternal employment effects (Baker et al., 2008; Lefebvre and Merrigan, 2008; Lefebvre et al., 2009; Haeck et al., 2015). The Quebec program was gradually introduced between 1997 and 2000 and offered daycare places at a cost of $5 per day. Baker et al. (2008) use biannual data and find a positive effect on mothers' labor market outcomes for mothers in two-parent households with children up to age four. Using a slightly different strategy and annual data, Lefebvre and Merrigan (2008) also find that the subsidies significantly increased mothers' participation rate by 8.1 percentage points, from an average participation rate of 67 percent for mothers with children between one and five.

Finally, only a few papers examine whether there are lasting effects of universal daycare on maternal employment. Lefebvre et al. (2009) examine the dynamic labor supply effects of the Quebec program. Their results demonstrate that the policy has persistent effects on mothers labor market outcomes seven years later. The effects are driven by changes in the labor supply of less educated mothers. Haeck et al. (2015)

also compare the province of Quebec to the rest of Canada before and after the introduction of universal daycare in 1997. They find that universal daycare has lasting effects on mothers' participation rate and weeks worked 11 years after the reform. Additionally, Haeck et al. (2015) find the strongest effects for higher-educated mothers. Herbst (2017) exploits a temporary provision of universal daycare in the U.S. during World War II to examine the effect on maternal labor supply. The program was intended to boost war production by freeing mothers to work, and all children up to age 12 of working mothers, regardless of family income, were eligible for the daycare program. Herbst (2017) apply census data from before and after the war and compares treated to untreated mothers in states with high and low funding of the daycare program. Using the census data, Herbst (2017) evaluate the effects of the temporary wartime childcare program on maternal employment both seven and 17 years after the program. One caveat with the use of census data is that treated and untreated mothers are defined based on whether there are children in the household, which might be problematic 17 years after the termination of the program. Herbst (2017) finds that moving a state from the bottom quartile of childcare funding to the top quartile increases participation by 4.4 percentage points seven years after the program, which is equivalent to a 25 percent increase. This is a considerable effect, although not surprising; enrollment in the wartime childcare program was likely contingent on a parental work requirement. In summary, there is a wealth of short-run mixed findings, whereas there are only a couple of long-run studies.

3 Institutional background

Although subsidized childcare in Denmark dates back to 1919, daycare with universal access was first implemented in 1965.[2] A series of laws was implemented between 1919 and 1951 to improve the quality and availability of childcare. However, the Danish childcare system remained targeted at children from low-income families because only institutions in which two-thirds of the children came from families in which both parents had to work to make a living got the maximum subsidy. Through the 1960s and 70s, women increasingly entered the labor market, and many mothers could no longer care for their own children during work hours. By 1963, the

[2]The first childcare institutions were run by philanthropists with an aim to teach children from poor households to be disciplined, clean, and obedient. These institutions was considered a charity and thereby discouraged working class families from using them (Ploug, 2012).

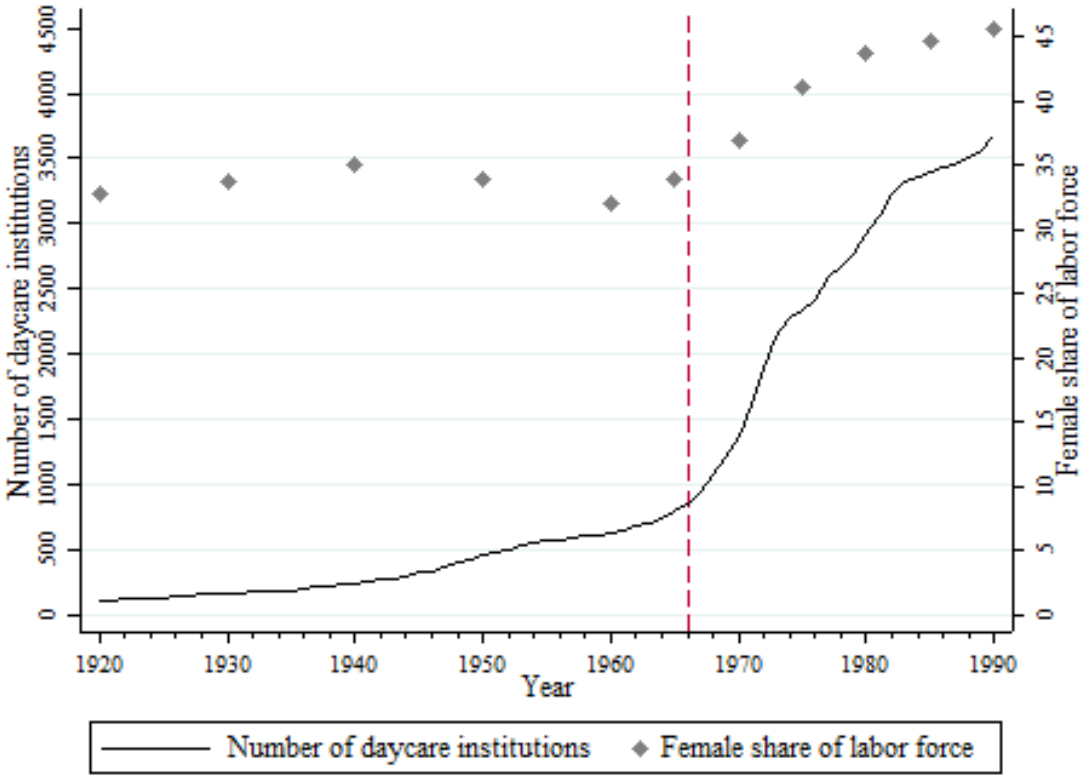
number of children on waiting lists for childcare was twice as large as the number of children enrolled (Horsten, 1963; Korremann, 1977). Consequently, the Danish parliament transformed the existing targeted care system into a universal daycare system in 1965 (See Lunn (1971) for Act no. 193, 1964). The reform mandated four main changes that both affected the number of daycare slots and the quality of care in the daycare institutions.

First, as the daycare institutions became available to children from all socioeconomic backgrounds, the focus of the institutions changed from being a place where children could stay during work hours to institutions with a focus on child development. Second, after the reform, building costs were fully publicly funded, and operating costs were split between the state, the municipality, and the parents, with parents paying 30 percent of the operating costs. Third, the reform untied subsidies to institutions, regardless of the proportion of children from low-income families (Lunn, 1971). Thus, after the reform, there was universal access to daycare for all children, irrespective of their socioeconomic background. Fourth, the municipalities were now given the responsibility to provide sufficient daycare institutions. For the purposes of this paper, the last two elements of the reform are especially important in the sense that daycare became an important instrument facilitating female labor supply, regardless of one's socioeconomic background.

In Denmark, the late 1950s were characterized by an economic upturn and the 1960s-70s by a rapid expansion of the welfare state. Consequently, the share of publicly employed workers increased by 2.5 percentage points from 1948 to 1960, whereas the share increased by 13 percentage points from 1960 to 1975 (Statistics Denmark, 2008). Figure 1 shows the development of the number of daycare institutions for children aged three through six (solid line, left axis) and the female share of the labor force (diamonds, right axis) from 1920 through 1990. The dashed vertical line marks the change from targeted to universal daycare in 1966. The figure shows that the number of daycare institutions increased slowly until 1965. The reform did not reach its full potential before 1966 because the government prohibited local daycare authorities from funding construction during 1960-66. Thus from 1956 to 1966, the number of institutions increased from 569 to 843, whereas from 1966 to 1994, the number of institutions increased by more than a factor four, to 4,000 institutions.

The geographical variation in the openings of daycare institutions is illustrated in Figure 2. The figure shows a map of the Danish municipalities (local daycare authorities) and the daycare availability within the municipalities. The municipalities had

Figure 1: Number of daycare institutions and female labor supply

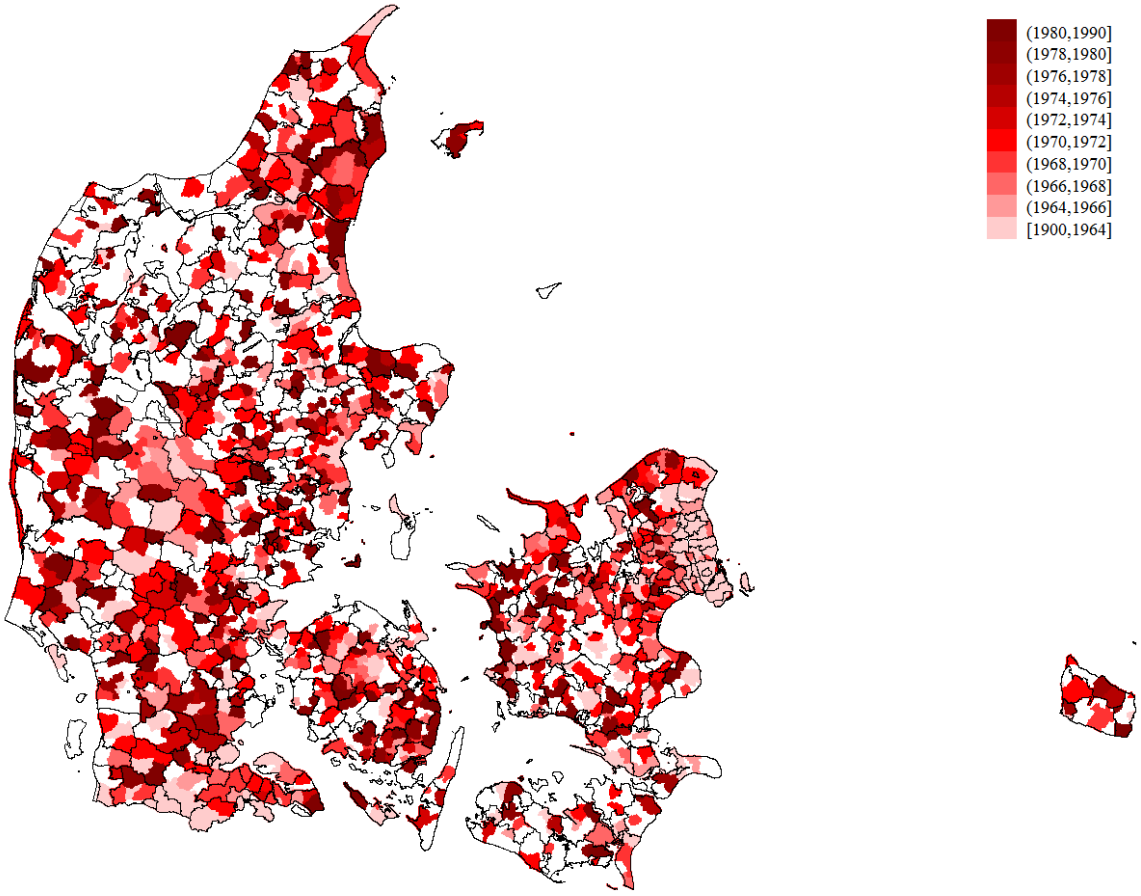


NOTE— The black line indicates the number of daycare institutions for children aged three to six (left axis), and the gray diamonds (right axis) indicate the female share of the labor force. The vertical dashed line indicates the implementation of the daycare reform in 1966. Data on the female share of the labor force are from Statistics Denmark, Statistical Yearbooks (various years). Data on daycare institutions are from the National Board of Social Services (various years), Tvenstrup (1975), and Statistics Denmark.

the responsibility of providing sufficient non-parental childcare, and the black lines on the map indicate the municipality borders. The lightest red color on the map indicate neighborhoods where daycare was available prior to 1964, and the darkest red color indicates neighborhoods where daycare became available after 1980. Importantly, the map shows that daycare became available gradually across Denmark, but also that the larger cities such, as Copenhagen and Aarhus, had at least one childcare institution before the universal daycare reform was implemented.[3] In Section 6.5, I

[3]Although it would be interesting to examine the effect of targeted versus universal childcare, it is not possible as there is no data on maternal employment for the targeted childcare period.

Figure 2: Daycare rollout



NOTE— The figure maps the variation in daycare availability within municipalities and across time. The lightest red color indicates neighborhoods where the first daycare institution opened prior to 1964, whereas the darkest red color indicates neighborhoods where the first daycare institution opened between 1980 and 1990. The black lines indicate the municipality borders (1970-2007) but also smaller islands, which are not independent municipalities. The map is constructed using data from the Danish Geodata Agency.

show that the results are robust to the exclusion of the largest cities and the exclusion of the suburbs.

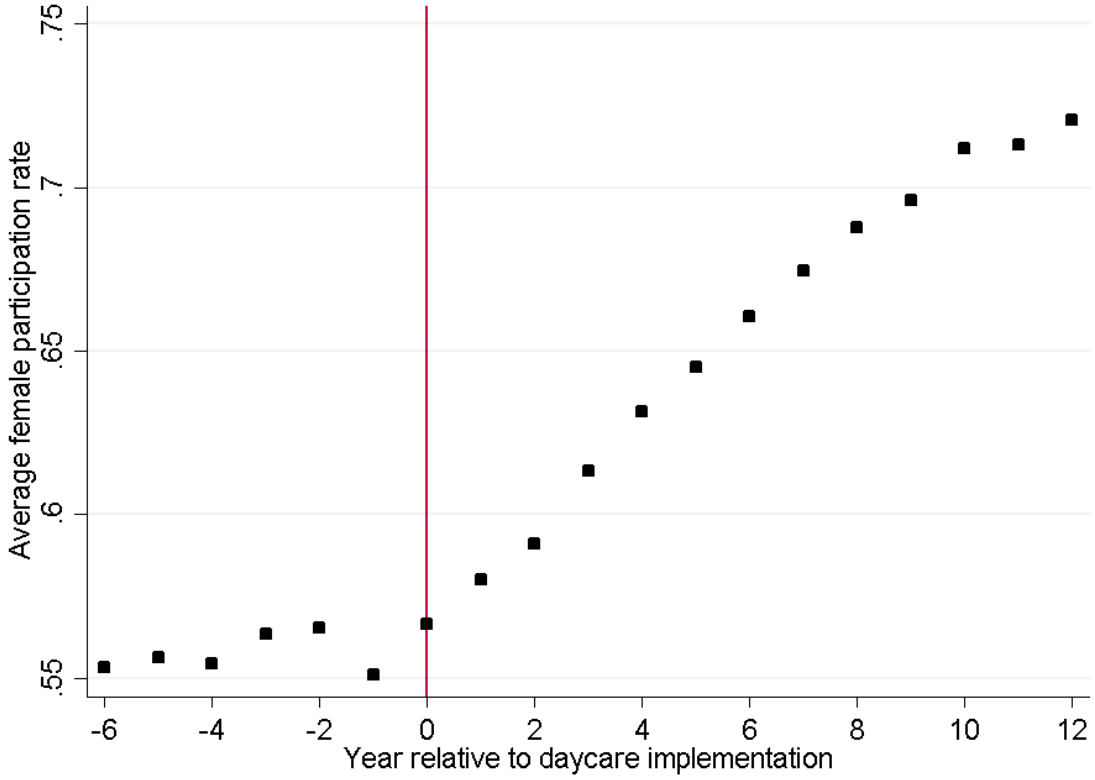
The universal daycare reform not only affected the care options for children aged three through six, but also opened up for non-parental care for children younger than three. However, daycare for children younger than three (nurseries) expanded more slowly than daycare for children aged three through six. Figure A.1 shows the number of places for the two age groups and indicates that the take-up in nurseries occurred during the 1980s and 90s. All employed women were entitled to 14 weeks of paid birth-related leave from 1960 (Borchorst, 2003).[4] Thus, during this time, there was limited support for mothers with children younger than three.

Figure 1 also reveals an increase in the female share of the labor force during this period. From 1920 through 1965, the female share of the labor force varied between 30 and 35 percent, whereas it increased to 46 percent in 1990. These numbers reflect not only an increase in the female participation rate but also a decrease in the participation rate of men. In 1960, 94 percent of all men aged 15-69 participated in the labor market, whereas only 44 percent of all women aged 15-69 had a job. By 1990, the participation rate among men had dropped to 84 percent, whereas the female participation rate had increased to 73 percent. The overall participation rate for both men and women aged 15-69 increased from 68 percent in 1960 to 79 percent in 1990 (Statistics Denmark, 2008).

Essential to the identification of causal effects in this study is that the openings of daycare institutions did not happen in neighborhoods where female labor force participation was already high or was anyway increasing. Figure 3 shows the average female participation rate for women aged 20 through 45 for neighborhoods that opened a daycare institution from six years prior through 12 years after daycare implementation. Importantly, Figure 3 reveals that the female participation rate was stable around 56 percent prior to the implementation of daycare. After the first opening of a daycare institution the average female participation rate increased gradually, reaching a level around 70 percent nine years after daycare implementation.

[4]The first law on birth-related leave was implemented in 1901 and gave women working at factories four weeks of leave after birth. Birth-related leave was gradually expanded to women working in others sectors, and in 1960 a law was passed that gave all women with a job eight weeks of paid leave before birth and 14 weeks of paid leave after birth. Women out of the labor force gradually also became entitled to some economic support following childbirth. In 1984, the birth-related leave period was extended to 20 weeks, with the option that the father could take the last six weeks of leave (Borchorst, 2003).

Figure 3: Event study graph of female participation rate



NOTE— The figure shows the average female participation rate for women aged 20-45 for neighborhoods that implemented a daycare institution plotted over years relative to the year of daycare implementation. The averages are weighted by the female population size in each neighborhood-year cell.

4 Data

I combine administrative registers from Statistics Denmark, 1970 census tract data, and information on daycare availability digitized from historical records. The Danish registers contain a unique identifier for each individual and a link between children and parents, which makes it possible to combine several registers and families. Through the personal identifier in the registers, I match demographic characteristics, educational attainment, labor market participation rates, earnings, and work hours of each of the mothers to the age of their first child. The data includes all women in the 1970 census tract (i.e., all women resident in Denmark in 1970) who have their first child between 1964 and 1975. From this sample, I drop women with an immigrant

background (2.2 percent) and women who move out of Denmark or pass away before their first child turns 16 (0.6 percent). The final sample consists of 370,602 mothers.

I use daycare yearbooks to collect information about daycare institutions from 1964 through 1975 (National Board of Social Services, 1968-72; Tvenstrup, 1975). From 1976, I find information about daycare institutions in the administrative registers. Together, these sources give a panel of neighborhoods with daycare availability from 1964 through 1990. I define mothers with daycare access as a match between the neighborhood of daycare in the years after the birth of the first child and the mothers neighborhood of residence in 1970 from the national census tract.[5] This approach is also used in Bingley et al. (2018). However, this match between neighborhood of daycare and the mother's neighborhood of residence in the 1970 census is problematic if parents select into a given neighborhood based on daycare availability. Another approach used in the literature is to match daycare availability based on the child's place of birth (e.g., Havnes and Mogstad, 2011). This is not possible in this setting because the birth registration variable in the Danish registers is inconsistently measured during the period I consider and has a data break in 1978. More specifically, the birth registration was changed in 1978 to reflect the mother's neighborhood of residence at the time of birth and not the actual place of birth. The authorities changed the registration practice because the number of children born in hospitals increased dramatically during the 1960s and 70s, causing the annual number of births in neighborhoods with a hospital to increase disproportionately. To validate that parents are not selecting into neighborhoods with daycare availability, I calculate the distance between the birth place of the mother and her address in the 1970 census tract and examine whether mothers who move a longer distance are more likely to live in a neighborhood with daycare. Additionally, I show that the results are robust to the exclusion of mothers who had their first child prior to 1970 in Section 6.5.

From the registers, I collect information about earnings from 1980 through 2015. Earnings are registered annually by the tax authorities. I adjust all earnings to 2016 U.S.-prices and convert earnings to log points. An advantage of the earnings measure is that it excludes income from unemployment insurance and other social benefits. Thus, labor market earnings are only available for those who have a paid job, and consequently, log earnings are undefined for those without a job. The estimates on

[5]The census tract reports neighborhood and municipality of residence November 4th, 1970. A neighborhood(parish) is a smaller administrative unit than a municipality. There are 2,033 neighborhoods nested in 277 municipalities in the data set.

log earnings are therefore conditional on employment.

I use three measures of employment in this paper. These are constructed on the basis of annual information about mandatory pension contributions to the Supplementary Pension Fund Register (in Danish: ATP). The ATP was introduced in 1964, and contributions to ATP vary with hours worked. Persons working between 10 and 19 hours a week pay one-third of the full ATP contribution, persons working between 20 and 29 hours a week pay two-thirds of the full ATP contribution, and persons working 30 hours or more a week pay the full ATP contribution. The ATP contributions are available on a yearly basis; thus, a one-third ATP contribution can correspond to a person working 10 hours a week throughout the year or full-time for one-third of the year. During this period, the unemployed, the self-employed, and persons out of the labor force were not part of the ATP (Hansen and Lassen, 2011). I use the ATP to construct a measure of average weekly hours worked throughout the year. Additionally, I construct a dummy for participation in a given year prior to 1980 if any contributions where made within that year. After 1980, I code the participation as one if the mother had any positive earnings within that year. Coding participation based on positive earnings has the advantage of including mothers who work less than 10 hours a week; Figure C.1 shows that the results are not sensitive to the different definitions of participation. Full-time employment is defined as making ATP contributions of the full amount and corresponds to a minimum of 30 hours of work a week throughout the year.[6]

The education registers contain information about the educational attainment of all individuals in Denmark on a yearly basis, and I use the registers from 1980. Information about educational attainment is reported directly from the educational institutions to Statistics Denmark. For individuals who achieved their education prior to 1980, the registers contain information about educational attainment and the date of achievement from the census data. I use this data to define each mother's educational attainment prior to the birth of her first child, because daycare availability might affect educational attainment. The majority (82 percent) of the mothers in the sample did not achieve more education after 1970; thus, for those who have their first child after 1970, I can easily define their pre-child educational attainment. However, for those who have their first child before 1970 (50 percent of the sample), I assume the mother's education had been obtained prior to the birth of her first child if the length

[6]Because work hours are not observed for those working less than 10 and above 30 hours a week, I do not include the wage rate as an outcomes.

of her education plus the normal starting age for obtaining that level of education is less than her age at the birth of her first child. Alternatively, I define their educational attainment as the mandatory level of education (0.02 percent). I define 2 groups of educational attainment of an equal size. Low-educated mothers I define as mothers with no post-secondary education and higher-educated mothers I define as mothers with some post-secondary schooling (i.e., vocational training, college, or university). Additionally, I use the education registers to construct a dummy outcome variable, which takes the value one if the mother attains more education after the birth of her first child.

All regressions include an indicator of urban neighborhood, defined as a market town. Market towns were larger cities with a greater degree of historical and economic status. Thus, by including an indicator for such urban areas, I control for differences in job and earnings opportunities.

The Danish administrative data contains information about complete fertility histories. Thereby, I am able to examine whether there are any mechanisms in play through fertility decisions. The sample includes mothers who have their first child between 1964 and 1975; therefore, I examine the effects of daycare on total fertility on the intensive margin. Specifically, I define four fertility outcomes: number of children, a dummy for more than one child, a dummy for more than two children, and a dummy for more than three children. Additionally, the personal identifiers linking parents and children in the registers allows me to calculate the spacing between children. I use cohabitation information to examine whether daycare affects the probability of the mother living together with the child's father in the year the first born child turns 16.

Table 1 presents summary statistics for mothers' characteristics and time invariant outcomes. Column (1) reports means and standards deviation for the full sample, and Columns (2) and (3) report means and standard deviations for sub-samples, depending on access to daycare. On average, mothers in the sample are 23.7 years old at first child and have 10.8 years of education. Specifically, 50 percent of the mothers in the sample have no post-secondary education. The mean year of birth of the mothers in the sample is 1945; thus, the low educational level and age at first birth are not surprising. Daycare availability is correlated with fewer children, larger spacing between children, more parents living apart when the first child is 16, additional educational attainment after first childbirth, and more years in the labor market after the birth of the first child. Summary statistics of the four fertility

Table 1: Summary statistics

	(1) All	(2) Daycare year 4 No	(3) Daycare year 4 Yes
Mother's age at first birth	23.70	23.46	23.77
	(4.35)	(4.55)	(4.29)
Year of birth	1945.57	1944.94	1945.77
	(5.42)	(5.80)	(5.27)
Years of education	10.78	10.11	10.99
	(2.94)	(2.99)	(2.89)
Basic schooling	0.50	0.59	0.47
	(0.50)	(0.49)	(0.50)
Urban neighborhood	0.22	0.06	0.27
	(0.41)	(0.25)	(0.44)
Missing father id	0.01	0.01	0.01
	(0.11)	(0.09)	(0.12)
Number of children	2.20	2.35	2.15
	(0.85)	(0.91)	(0.82)
Years between first and next child	3.76	3.55	3.83
	(2.44)	(2.29)	(2.49)
Not living with child's father	0.20	0.15	0.22
	(0.40)	(0.36)	(0.41)
Additional education after birth of first child	0.12	0.10	0.13
	(0.33)	(0.31)	(0.34)
Lenght of working life (years) after first child	25.19	22.97	25.89
	(10.26)	(10.94)	(9.93)
Observations	370602	89284	281318

NOTE— The table shows the means and standard deviations of the background variables of mothers who had their first child between 1964 and 1975 and the means and standard deviations of some of the dependent variables. Length of working life is a sum of the participation dummies through 34 years after the birth of the first child.

outcomes are presented in Table B.1 separately for those with and without daycare access measured at the time of birth of the first child. The pattern is the same; daycare availability is correlated with fewer children.

Table B.2 shows the summary statistics of the participation rate, full-time employment, and hours of work separately for each year through 6 years after the first childbirth and for mothers with and without daycare access in the given year. Overall, Table B.2 reveals that the participation rates and hours of work are lowest in year 1 (i.e., during the full calendar year after the year the first child was born), but they are increasing over the years, and daycare is correlated with higher levels of participation, full-time employment, and hours worked. The means of the long-run

outcomes are presented in Table B.3. The participation rate increases over the first 16 years after the first childbirth, then it flattens at 81 percent, and 23 years after the first childbirth, the participation rate begins to decrease, such that the participation rate is 66 percent 34 years after the first childbirth. The full-time employment rate is highest 29 years after the first birth, during which time 46 percent of the mothers in the sample work full-time. The weekly hours of work is highest 21-23 years after the first childbirth, and the earnings are highest 29 years after the first childbirth. All the outcomes follow a concave pattern over the years after first childbirth.

5 Empirical strategies

I follow two strategies in the empirical analysis. The first strategy is based on a difference-in-differences approach comparing the outcome of mothers with differential access to daycare in a given municipality. I begin with the contemporaneous effects of daycare availability:

$$Y_{inmt} = \alpha DC_{nmt} + X_{inm}^{'}\beta + \tau_i + \gamma_t + \mu_m + \varepsilon_{inmt} \quad (1)$$

where Y_{inmt} is the outcome for mother i in neighborhood n in municipality m at time t, and DC_{nmt} is an indicator taking the value one if municipality m has implemented daycare in neighborhood n at time t and zero otherwise. $X_{inm}^{'}$ is a vector of observed characteristics, such as age at first birth, month of birth of the first child, and an indicator of living in an urban neighborhood. τ_i indicates the birth year of the mother and controls for life-cycle events. γ_t is a full set of year dummies indicating the year the mother gives birth to her first child. In Equation (1), the outcome variables are measured zero through six years after the first child is born; thus, γ_t is a non-linear time trend controlling for macro shocks. I add a full set of dummies for the first child's month of birth in the analysis of contemporaneous effects because mothers who have their first child in the same year but in a different month (e.g., January and December) will have different participation rates in the year during which they have their first child. μ_m refers to a full set of municipality fixed effects (intercepts for local daycare authorities). The error term ε is allowed to be heteroskedastic and to cluster at the municipality level. I estimate Equation (1) for a set of contemporaneous labor market outcomes: participation, full-time employment, and hours worked.

To evaluate the long-run effects of daycare availability on mothers' labor force

participation and earnings, I estimate the following equation:

$$Y_{inmt+b} = \alpha DC_{nmt,t=4} + X'_{inm}\beta + \tau_i + \gamma_t + \mu_m + \epsilon_{inmt} \quad (2)$$

Equation (2) follows the same setup as Equation (1), with two exceptions. First, Y is measured b years after year t. Specifically, I evaluate mothers' labor market outcomes on a yearly basis from when her first born child is four through 34. Second, I use daycare availability in the year the mother's first born child turns four as the explanatory variable in all regressions of Equation (2)[7].

The models identify the effects of daycare availability on mothers' employment status using the difference in the timing of daycare availability between neighborhoods within the municipality and across time. Whereas the year of first birth dummies control non-linearly for a general time trend, job opportunities might be better in larger neighborhoods. Consequently, I test whether the results are robust to including an extra set of non-linear time trends for the largest (in terms of population) neighborhood within the municipality in Section 6.5. I continue the empirical analysis by investigating the effects of daycare availability on mothers' earnings following the setup of Equation (2). The earnings analysis is limited to the long run, because the first records of earnings are from 1980.

Equations (1) and (2) produce reduced-form effects for all mothers with a daycare in the neighborhood when their child is aged zero through six and four, respectively. Thus, the estimates of α are Intention-To-Treat (ITT) effects, and by estimating ITT effects, I avoid the issue that at the individual level, enrollment in non-parental childcare is most likely endogenous.[8]

The key identifying assumption in this setup is that trends in labor market outcomes for mothers without access to daycare would have been the same as those for mothers with access to daycare in the absence of daycare availability. This assumption is inherently untestable and impeded by the fact that getting a child has consequences for women's labor force participation from the time the child is born. Therefore, I continue the analysis by providing indirect evidence of the parallel trend assumption, employing an event study strategy using the year before the birth of the

[7]Appendix table B.4 shows the correlations of daycare availability across years after first childbirth. The correlations between daycare availability when the first born child is three through six are high, e.g. for mothers who had access to daycare when their first born child was 4, 87 percent also had access when the child was three.

[8]I do not observe individual daycare attendance during this period.

first child as event time zero.

I begin the event study analysis by comparing mothers and fathers labor market outcomes around the birth of their first child using the following approach:

$$Y^p_{im\tau} = \sum_{j \neq -1} \Theta^p_j \cdot \mathbf{I}[j = \tau] + \omega^p_i + \gamma^p_t + \mu^p_m + v^p_{im\tau} \tag{3}$$

Where $Y^p_{im\tau}$ is the labor market participation of individual i in municipality m at event time τ, and p indicates whether the parent is a mother or a father. In addition to a full set of event time dummies, I include age dummies (ω^p_i), year dummies (γ^p_t), and municipality fixed effects μ^p_m. The error term $v^p_{im\tau}$ is allowed to be heteroskedastic and to cluster at the municipality level. By allowing for a full set of age dummies, I control for underlying life cycle trends, which eases the comparison of mothers to fathers, since women on average are younger than men when they have their first child. Similarly, by including a full set of year dummies, I control non-linearly for factors such as business cycles and wage inflation. Because there is variation in how old parents are when they have their first child, there is variation in event times conditional on calendar year and parents' age, which makes it possible to identify all three sets of dummies.

I base the event study analysis on a balanced panel of parents observed each year between five years prior to the birth of their first child and 20 years after, indicated by the event time τ spanning from -5 to 20. I omit the event time $\tau = -1$, and consequently the event time estimates Θ^p_τ measure the impact of having a child relative to the year just before the first child was born. If the estimated event time coefficients on labor force participation are negative for $\tau >= 0$, it suggests that some parents opt out of the labor market after the birth of their first child. Similarly, if $\Theta^m_\tau \neq \Theta^f_\tau$ for $\tau >= 0$, it suggests that mothers and fathers are differentially affected by having a child.

To investigate whether daycare availability moderates the impact of children on mothers' labor market outcomes, I estimate event time coefficients separately for mothers with differential access to daycare:

$$Y^{DC}_{im\tau} = \sum_{j \neq -1} \Theta^{DC}_j \cdot \mathbf{I}[j = \tau] + \gamma^{DC}_t + \omega^{DC}_i + \mu^{DC}_m + v^{DC}_{im\tau} \tag{4}$$

Equation (4) follows the exact setup of Equation (3), but I estimate Equation (4) separately for mothers with no daycare access, some daycare access, and full daycare

access.[9] For the parallel trend assumption to hold, I should find that $\forall j < 0\, \hat{\Theta}_j^{DC} \approxeq 0$. Systematically negative event time coefficients prior to the birth of the first child for mothers with some or full daycare access would indicate that the daycare availability variable is spuriously capturing a secular trend in maternal labor force participation. In a similar vein, systematically positive event time coefficients prior to the birth of the first child for mothers with some or full daycare access would indicate that the daycare institutions opened in neighborhoods with higher levels of maternal employment.

6 Results

In this section, I present the empirical results based on the methods outlined in the previous section. I start by providing evidence on the contemporaneous effects of access to universal daycare on maternal labor force participation, full-time employment, and hours worked in Section 6.1, whereas Section 6.2 presents the long-run effects of daycare availability. In Section 6.3, I continue the empirical analysis by providing evidence from the event study approach. Finally, Section 6.4 examines the effects of daycare on fertility, education, and parental separation as potential mechanisms for mothers' labor force participation effects, and Section 6.5 presents a series of robustness and specification checks.

6.1 Contemporaneous effects of daycare availability

Table 2 shows the reduced form estimates of equation (1). Specifically, Table 2 shows the effect of daycare availability in year t on mothers' labor force participation in year t separately for values of t from zero through six, where t indicates the number of years after the birth of the first child. Each row indicates the year (t) in which daycare availability and the mothers' labor force participation are measured and each column presents a different model specification. Column (1) shows the relations between daycare availability and mothers' labor force participation, disregarding any further

[9] Mothers with no daycare access are defined as mothers who live in a neighborhood where there is no daycare during the first six years after the birth of her first child. Mothers with some daycare access are defined as mothers with access to daycare at least one of the first six years after the birth of her first child. Finally, mothers with full daycare access are defined as mothers with access to daycare in all the first six years after the birth of her first child. Appendix Table B.4 shows the correlation of daycare access across years after first childbirth.

controls. The point estimates are positive and statistically significant for all years. In Column (2) I add a full set of year dummies and in Column (3) I also include municipality fixed effects. The point estimates in Column (3) suggest that daycare availability has a positive effect on participation for all years apart from year zero, which is the year the first child is born. Specifically, mothers with daycare access in the year their first child turns one (two) are 1.2 (2.2) percentage points more likely to work than mothers without daycare access. For years three through six, the point estimates are higher, indicating e.g. that mothers with daycare access three years after first childbirth are 6.0 percentage points more likely to work three years after first childbirth.

The results of the full model are shown in Column (4). Specifically, I add covariates to the model in Column (4) to control non-linearly for mother's age at first birth, mother's year of birth, and month of birth of the first child. Additionally, I control for differences in job opportunities by including a dummy for living in an urban area. Overall, the results are robust to the inclusion of covariates. The point estimates are close to zero or not statistically different from zero the first couple of years after the birth of the first child. Mothers with daycare access are 1.5 percentage points more likely to work in the year their first child turns two compared to mothers without access to daycare. The point estimates in Column (4) are above 5 percentage points for years three through six. To get a sense of the magnitude, I relate the point estimates to the sample means reported in Table B.2. This exercise suggests that mothers with daycare access are 9.1, 9.7, 9.3, and 9.0 percent more likely to participate in the labor market in the year their first born child turns three, four, five, and six, respectively.

Table 3 shows the effects of daycare availability on mothers' full-time employment status separately for zero through six years after the birth of the first child. Overall the parameter estimates on full-time employment are smaller than those for any employment in Table 2; however, during the period I consider in this paper, the percentage of mothers working full-time was low. Table B.2 shows that 59 percent of the mothers in the sample participate in the labor market in the year their first born child turns four, whereas only 17 percent work full-time in that year. Relating the point estimates in Table 3 to the mean values in Table B.2, the results suggest that mothers with daycare access are 5.3 percent more likely to be full-time employed in the year they have their first child than to mothers without access to daycare. Similarly, mothers with daycare access are 3.8 percent more likely to work full-time the year their first born child turns one. Mothers with daycare access are 9.4 percent more likely

Table 2: The contemporaneous effects of daycare on mothers' labor market participation zero through six years after the birth of the first child

	(1)	(2)	(3)	(4)
Year 0	0.147***	0.068***	0.000	-0.007*
	(0.012)	(0.010)	(0.004)	(0.004)
Year 1	0.138***	0.064***	0.012***	0.005
	(0.012)	(0.007)	(0.004)	(0.003)
Year 2	0.124***	0.073***	0.022***	0.015***
	(0.011)	(0.007)	(0.004)	(0.003)
Year 3	0.146***	0.105***	0.060***	0.053***
	(0.007)	(0.008)	(0.005)	(0.004)
Year 4	0.145***	0.111***	0.063***	0.057***
	(0.008)	(0.008)	(0.005)	(0.004)
Year 5	0.139***	0.112***	0.063***	0.057***
	(0.007)	(0.008)	(0.005)	(0.004)
Year 6	0.134***	0.111***	0.062***	0.056***
	(0.008)	(0.008)	(0.005)	(0.004)
Observations	370602	370602	370602	370602
Year dummies	No	Yes	Yes	Yes
Municipal FE	No	No	Yes	Yes
Covariates	No	No	No	Yes

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions, each row indicates the year in which daycare availability and the outcome variable are measured, and each column represents a model specification. Column (1) only includes the explanatory variable. Columns (2)-(4) include a full set of year of first childbirth dummies, and Columns (3)-(4) include municipality fixed effects. Additionally, covariates are added in Column (4). The included covariates are a full set of dummies for mother's age at first birth, mother's year of birth, month of birth of the first child, and an indicator of urban area.

to be full-time employed in the year their first child turns three. Effect sizes remain at this level through years four to six, with effect sizes of 11.2 percent, 10.5 percent, and 11.7 percent in the year the first child turns four, five, and six, respectively.

Table 4 shows the effects of daycare availability on mothers' hours of work separately for zero through six years after the birth of their first child. In line with the results on participation and full-time employment, the results for hours worked are small or not statistically significant in the first couple of years. When the first born child turns three the point estimates are significantly different from zero at conventional significance level, and they increase as the child gets older. Mothers with

Table 3: The contemporaneous effects of daycare on mothers' full-time employment zero through six years after the birth of the first child

	(1)	(2)	(3)	(4)
Year 0	0.084***	0.031***	0.011**	0.008***
	(0.006)	(0.006)	(0.004)	(0.003)
Year 1	0.059***	0.027***	0.007*	0.006**
	(0.006)	(0.006)	(0.004)	(0.003)
Year 2	0.052***	0.031***	0.007*	0.005*
	(0.006)	(0.005)	(0.004)	(0.003)
Year 3	0.058***	0.042***	0.019***	0.016***
	(0.004)	(0.004)	(0.003)	(0.003)
Year 4	0.061***	0.048***	0.021***	0.019***
	(0.004)	(0.004)	(0.004)	(0.003)
Year 5	0.064***	0.051***	0.022***	0.019***
	(0.004)	(0.004)	(0.003)	(0.003)
Year 6	0.065***	0.053***	0.023***	0.021***
	(0.005)	(0.005)	(0.003)	(0.003)
Observations	370602	370602	370602	370602
Year dummies	No	Yes	Yes	Yes
Municipal FE	No	No	Yes	Yes
Covariates	No	No	No	Yes

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions, each row indicates the year in which daycare availability and the outcome variable are measured, and each column represents a model specification. Column (1) only includes the explanatory variable. Columns (2)-(4) include a full set of year of first childbirth dummies, and Columns (3)-(4) include municipality fixed effects. Additionally, covariates are added in Column (4). The included covariates are a full set of dummies for mother's age at first birth, mother's year of birth, month of birth of the first child, and an indicator of urban area.

daycare access work 0.27 hours more per week (1.8 percent) than mothers without daycare access in the year their first child turns four.

Overall, daycare availability increases mothers' participation rate, full-time employment, and hours worked in the short run. The results in this section suggest that daycare availability affects mothers labor market attachment on both the intensive and extensive margin. The results further suggest that mothers primarily respond to daycare availability when their first child has turned three and not during the first couple of years after they become a mother. This may seem in contrast to the results of Simonsen (2010), who finds that mothers react to both daycare price changes and

Table 4: The contemporaneous effects of daycare on mothers' hours of work zero through six years after the birth of the first child

	(1)	(2)	(3)	(4)
Year 0	1.309***	0.460***	0.015	0.049
	(0.151)	(0.158)	(0.102)	(0.079)
Year 1	0.655***	0.209*	0.076	0.126*
	(0.116)	(0.122)	(0.084)	(0.065)
Year 2	0.490***	0.201*	0.005	0.059
	(0.122)	(0.117)	(0.091)	(0.079)
Year 3	0.611***	0.403***	0.221***	0.174**
	(0.111)	(0.119)	(0.083)	(0.071)
Year 4	0.737***	0.569***	0.331***	0.271***
	(0.105)	(0.110)	(0.090)	(0.083)
Year 5	0.879***	0.709***	0.372***	0.307***
	(0.107)	(0.113)	(0.076)	(0.067)
Year 6	0.944***	0.795***	0.419***	0.351***
	(0.111)	(0.118)	(0.077)	(0.070)
Observations	370602	370602	370602	370602
Year dummies	No	Yes	Yes	Yes
Municipal FE	No	No	Yes	Yes
Covariates	No	No	No	Yes

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions, each row indicates the year in which daycare availability and the outcome variable are measured, and each column represents a model specification. Column (1) only includes the explanatory variable. Columns (2)-(4) include a full set of year of first childbirth dummies, and Columns (3)-(4) include municipality fixed effects. Additionally, covariates are added in Column (4). The included covariates are a full set of dummies for mother's age at first birth, mother's year of birth, month of birth of the first child, and an indicator of urban area.

availability during the first year after childbirth. However, Simonsen (2010) use more recent data from 2001. The lack of statistically significant point estimates during the first couple of years after the birth of the first child in this paper suggests that the relevant margin has changed from mothers with children aged three through six in the 1960s-70s to mothers with children younger than two in recent times. This could reflect different cultural norms or that mothers have more than one child before they return to work during the period I consider in this paper. The mothers in the sample on average have 2.3 children, and I investigate whether daycare access affects fertility in Section 6.4.

6.2 Long-run effects of daycare availability

I now turn to the estimation of Equation (2), where for each year from four through 34 years after the first childbirth, I estimate the effect of daycare availability in year four on mothers' employment and labor market earnings. To ease comparison of the estimates over years and across outcomes, I plot the effect sizes in percent rather than percentage points by scaling the point estimates with the mean of the dependent variable in the corresponding year. The means of the dependent variables are presented in Table B.3.

Figure 4 shows the effects of daycare in year four on (a) participation rate, (b) full-time employment, (c) log earnings, and (d) hours of work per week. Mothers with daycare access are 9.7 percent more likely to participate in the labor market during the year their first child turns four. The effect sizes fall gradually as the first child ages e.g., when their first child is 14, mothers with daycare access are 5.7 percent more likely to participate in the labor market, and when their first child is 22, mothers with daycare access are 3.1 percent more likely to participate in the labor market. Daycare access has persistent effects on the participation rate of mothers 34 years after the birth of their first child. Mothers who had access to daycare are 1.2 percent more likely to participate in the labor market 34 years after they have their first child.

Figure 4 (b) shows the effects on full-time employment, and the pattern of decreasing effect sizes aligns with that of the participation rate. Mothers with daycare access are 11 percent more likely to be full-time employed in the year their first born child turns four, 8.2 percent more like to be full-time employed when their first child is 14, and 4.4 percent more likely to be full-time employed when their first child is 34.

Daycare availability when the first child is four also has persistent positive effects on earnings through 34 years after the birth of the first child, although the earnings estimates are noisier ranging between 3 and 6 percent. Mothers with daycare access when the first born child is four on average earn 5.3 percent more in the year the first child turns 16 and 4.2 percent more in the year the first child turns 34 than mothers who did not have access to daycare when the child was four. On average, the mothers in the sample are 23.7 years old when they have their first child, which corresponds to an average age of 57.7 years in the last year I evaluate the long-run effects of daycare availability.

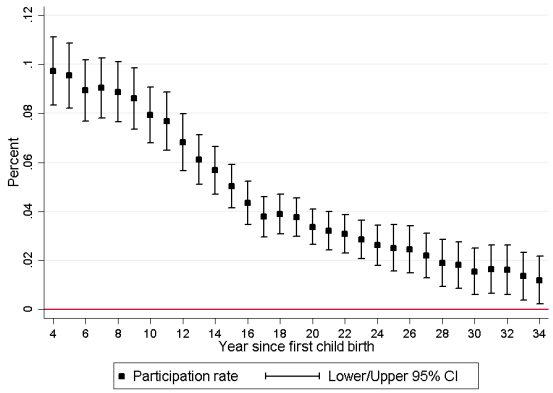
Thus, the results show that daycare availability during the child-rearing years has

life-long consequences on both labor market participation and earnings. As highlighted by Lefebvre et al. (2009), the existence of effects after the child(ren)'s preschool years is closely related to human capital. Participation in the labor market during child-rearing years yields lower levels of depreciation of human capital attained through education and in previous jobs, in addition to new human capital attained through the current job.

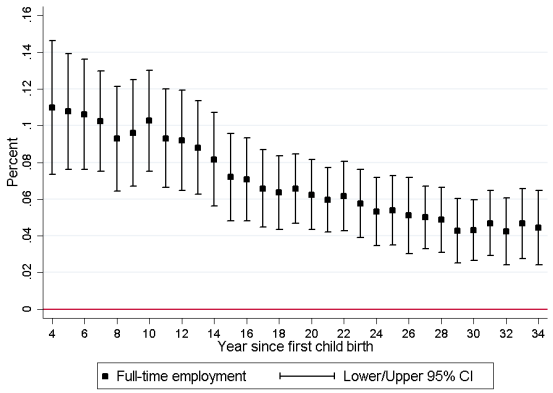
The results presented in Figure 4 are in line with the results in Herbst (2017); he finds that a $1 increase in the wartime daycare program increases mothers' full-time employment by 0.099 percentage points, up from a full-time employment rate of 12 percent for mothers in the treatment group. This corresponds to an 8 percent increase in full-time employment resulting from a $10 increase in daycare spending. Herbst (2017) uses census data in his analysis; consequently, he is only able to evaluate the impact of the temporary daycare program every tenth year. Thus, 17 years after the abolition of the program, he finds that a $10 increase in daycare spending implies that 6 percent of the mothers are more likely to work full-time. He also finds small but positive effects on earnings. Along the same lines, Lefebvre et al. (2009); Haeck et al. (2015) find that the introduction of universal daycare in Quebec affects mothers' labor market attachment beyond the years during which they have children of preschool age.

Figure 4: The short- and long-run effects of daycare availability when the first child is four on mothers' participation rate, full-time employment, earnings, and hours of work

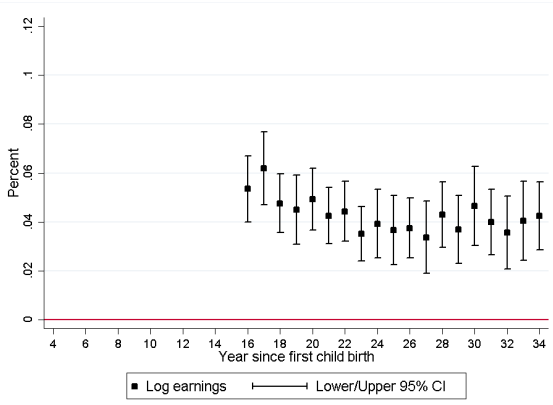
(a) Participation rate



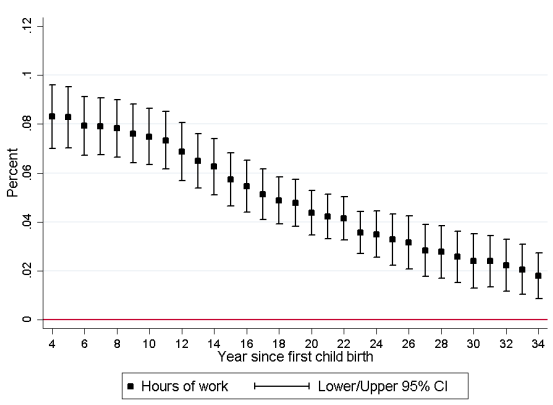
(b) Full-time employment



(c) Log earnings



(d) Hours of work



NOTE— The figure shows the effects of daycare in the year the first born child turns four on (a) the participation rate four through 34 years after the first childbirth relative to the sample mean, (b) full-time employment four through 34 years after the first childbirth relative to the sample mean, (c) log earnings (conditional on employment) 16 through 34 years after the first childbirth, and (d) hours of work per week four through 34 years after the first childbirth relative to the sample mean. Table B.3 shows the mean of the outcome variables. Each square corresponds to a point estimate from a separate estimation divided by the mean of the dependent variable in the corresponding year. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

6.2.1 Heterogeneous effects by mothers' education

Mothers who have invested (time and foregone labor market earnings) in education will likely have different incentives to enter and participate in the labor force. Additionally, lower-educated mothers might face different barriers to enter the labor market. For example, Baum (2002) finds that daycare costs are a larger employment barrier for low-income mothers. To examine such differences, I stratify the sample with respect to the mothers' education. Figure 5 shows the effects of daycare in year four on (a) participation rate, (b) full-time employment, (c) log earnings, and (d) hours of work per week separately for low- and higher-educated mothers. There are long-run positive effects on participation for both groups of mothers; however, the effect sizes are smaller for higher-educated mothers. Specifically, for higher-educated mothers, the effect of daycare availability on participation is 3.9 percent 10 years after the birth of the first child, less than 1 percent 17 years after the birth of the first child, and no longer significantly different from zero when the first born child turns 23. For low-educated mothers, the effect of daycare availability on participation remains significantly different from zero through 32 years after the birth of the first child. For low-educated mothers, the effect of daycare availability on participation is 10.1 percent 10 years after the birth of their first child, 5.1 percent 17 years after the birth of their first child, and 1.5 percent 32 years after the birth of their first child. Whereas low-educated mothers on average have their first child when they are 22.7, higher-educated mothers are on average two years older (cf. Table B.5). This difference in age at first childbirth between low- and higher-educated mothers can to some extend explain why the effects on the participation rate are longer lived for low-educated mothers.

The effects of daycare on full-time employment and hours of work follow the same pattern as the results for the participation rate. The effects are much larger for low-educated mothers than for higher-educated mothers, which is in line with the results in Lefebvre et al. (2009). There are especially larger differences in full-time employment. For example, when their first child is 6, low-educated mothers are 14.5 percent more likely to work full-time, whereas higher-educated mothers are 3.9 percent more likely to work full-time. The effects on full-time employment remain larger for low-educated mothers; however, by the time the first born child turns 32, the effect sizes are closer, with 3.8 for low- and 1.7 for higher-educated mothers, although the effects for higher-educated mothers are not significantly different from

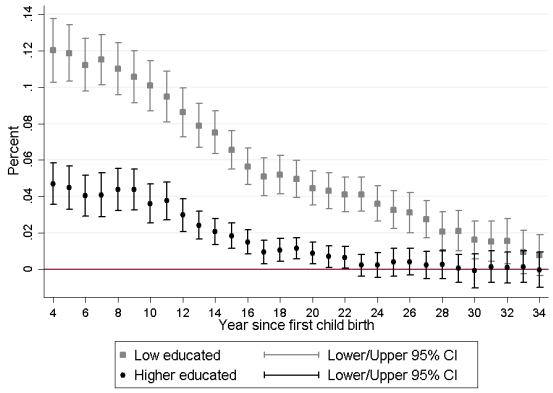
zero. Figure 5 (d) shows the effects of daycare on hours of work, and the effect sizes are 8.7 percent for low- and 3.9 percent for higher-educated mothers 10 years after the birth of their first child.

There are substantial differences in the effect sizes between low- and higher-educated mothers for the three different employment outcomes. However, the effects of daycare on earnings are more similar between low- and higher-educated mothers. Low-educated mothers with access to daycare earn 6.5 percent more 17 years after the birth of their first child than low-educated mothers without daycare access, whereas higher-educated mothers with daycare access earn 3.7 percent more 17 years after the birth of their first child. Thirty years after the birth of their first child, low-educated mothers with daycare access earn 4.5 percent more and higher-educated mothers earn 2.5 percent more than mothers without daycare access. For higher-educated mothers, the effects of daycare access on earnings are not statistically significant for 25-27 years and 31-34 years after the birth of their first child. On the other hand, the effects remain significantly positive for low-educated mothers through 34 years after the birth of their first child.

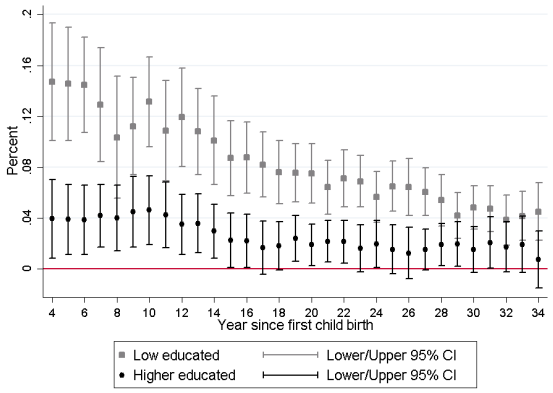
In summary, the effects of daycare on maternal employment are primarily driven by low-educated mothers, whereas the employment effects are smaller and less persistent for higher-educated mothers, indicating that the lack of daycare availability is a larger employment barrier for low-educated women during their child-rearing years than for higher-educated women. The effects on earnings for low- and higher-educated mothers are more closely aligned than the employment effects. However, for higher-educated mothers, the effects of daycare on earnings are longer lived than the effects on employment, suggesting that job attachment during child-rearing years is especially important for higher-educated mothers in terms of economic output.

Figure 5: Heterogeneous effects of daycare availability when the first child is four on mothers' participation rate, full-time employment, earnings, and hours of work by mothers' education

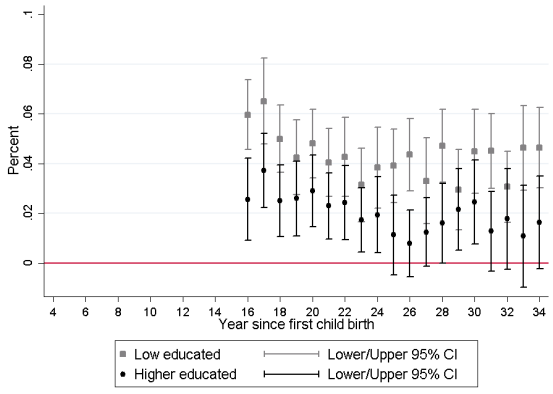
(a) Participation rate



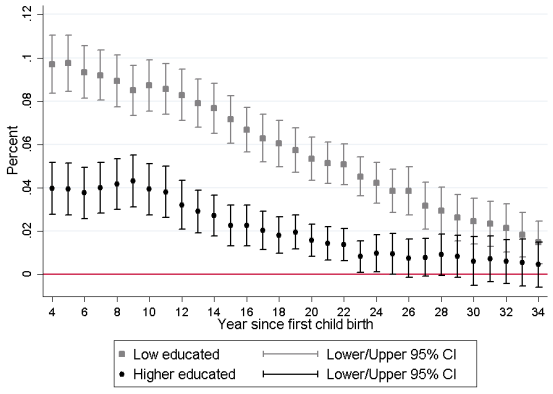
(b) Full-time employment



(c) Log earnings



(d) Hours of work



NOTE— The figure shows the effects of daycare in the year the first born child turns four on (a) the participation rate four through 34 years after the first childbirth relative to the sample mean for low- and higher-educated mothers, (b) full-time employment four through 34 years after the first childbirth relative to the sample mean for low- and higher-educated mothers, (c) log earnings (conditional on employment) 16 through 34 years after the first childbirth, and (d) hours of work per week four through 34 years after the first childbirth relative to the sample mean for low- and higher-educated mothers. Each square corresponds to a point estimate from a separate estimation. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

6.3 Event study analysis

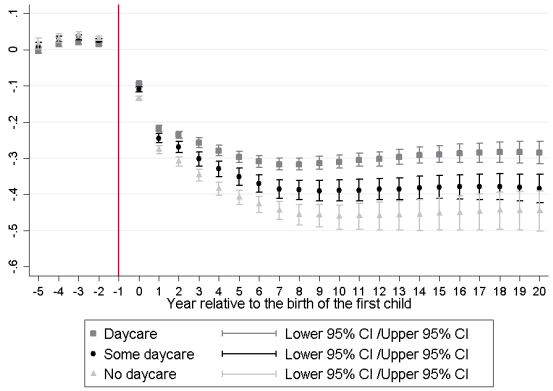
Prior research shows that women's participation rate drops after childbirth, whereas men's participation is unaffected. Using Danish data, Kleven et al. (2018) find a family gap in earnings of 20 percent; they attribute the gap to three different margins: labor force participation, hours worked, and wage rates. Specifically, Kleven et al. (2018) find a 13 percent drop in the participation rate of mothers who have their first child between 1985 and 2003.

For parents who have their first child between 1969 and 1975, I have data on their labor force participation five years prior to the year they become parents through 20 years after. I use this data to investigate whether daycare availability moderates the drop in labor force participation women experience after the birth of their first child. Figure 6 shows the event time coefficients estimated separately for three groups of parents. The first group is parents with access to daycare in all years after the birth of their first child, the second group is parents with access to daycare in some of the years, and the third group is parents with no access to daycare through the first six years after the birth of their first child. The event time coefficients are plotted separately for mothers (a) and fathers (b).

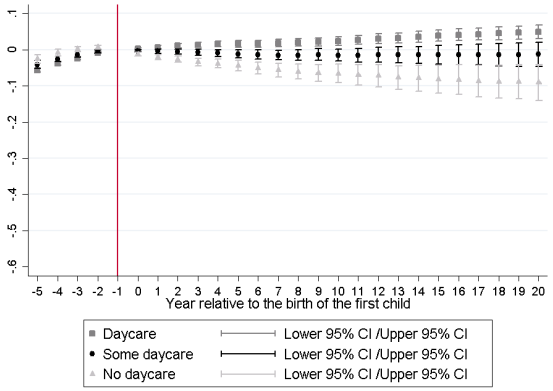
For all three groups of mothers, the event time coefficients are close to zero prior to the birth of their first child, supporting the identifying assumption, that the daycare institutions did not open in neighborhoods where female labor force participation was already increasing. From event time zero to seven, Figure 6 shows a gradual drop for all three groups of mothers; however, for mothers with daycare access in all years, the drop is smaller than for the two other groups of mothers. The gradual drop likely reflects additional childbirth, but given the size of the drop, it is also probable that some women drop out of the labor force when they have children. For the group of mothers who had daycare access in all pre-school years, the participation rate remains around 30 percentage points lower than the participation rate in the year prior to the birth of their first child from the time the child is seven to 20 years old. Although this is a considerable drop, it is a smaller drop than that exhibited by mothers who did not have access to daycare during the first years after they had their first child. The participation rate of mothers without access to daycare during the pre-school years of their first child drops to a level 45 percentage points lower than the participation rate in the year prior to the first childbirth. For fathers, the event time coefficients are much closer to zero, although fathers with daycare access in all years display a

Figure 6: Event study of parents' labor force participation

(a) Mothers with diff. daycare access



(b) Fathers with diff. daycare access



NOTE— The graph shows event time coefficients estimated separately for three groups of mothers (a) and fathers (b). The three groups are defined as parents with access to daycare in all years after the birth of their first child, parents with access to daycare in some years, and parents with no access to daycare through the first child's first six years. Estimations are based on a balanced sample of mothers and fathers who have their first child between 1969 and 1975 and are observed in the data during the entire period between five years prior and 20 years after they have their first child.

positive trend in participation rates, whereas fathers without daycare access display a negative trend compared to the year before the birth of the first child.[10]

6.4 Mechanisms

The results presented in the previous sections indicate that mothers with access to daycare after the birth of their first child are more likely to participate in the labor market. To investigate one possible mechanism for this result, I examine whether daycare availability affects fertility decisions. From a theoretical perspective, daycare availability can affect fertility through both an income effect and a substitution effect. Daycare increases a mother's earnings opportunity, which increases the demand for children if children are not an inferior good, and at the same time, it raises the opportunity cost of children (Becker, 2009).[11]

I estimate the effect of daycare availability on the number of children and indica-

[10]The ATP is not a perfect measure for labor force participation because it does not include self-employed (e.g., farmers).

[11]See also Gauthier (2007) for a review of the literature investigating the impact of different family policies on fertility.

tors for more than one, two, or three children. Because the sample consists of women who have their first child between 1964 and 1975, I consider fertility responses to daycare on the intensive margin. I construct two summary measures of daycare availability as explanatory variables. The first is an indicator for daycare availability for children up to age two, and the second is an indicator for daycare availability for children between age three and six. Among the mothers who had daycare available for children between age three and six, 53 percent also had daycare for children up to age two available. To avoid issues with reverse causality, I measure both types of daycare availability in the year the mother has her first child.

Table 5 presents the results for the four fertility outcomes. Daycare availability for children between age three and six negatively affects the number of children, whereas there is no effect of daycare availability for children up to age two or of having access to both types of daycare. One-third of the mothers had access to daycare for children up to age two; however, the enrollment in this type of childcare was lower than for children between age three and six (see Figure A.1). Thereby, it is not surprising that any fertility effects load onto the dummy for daycare access for children between age three and six. Access to daycare for children between age three and six reduces the number of children by 0.036 compared to mothers without access to daycare. Mothers in this sample on average have 2.2 children; thereby, the result indicates that daycare access reduces the number of children by 1.6 percent.

Table 5 also shows the effects of daycare access on the probability of having more than one, two, or three children. For all three fertility outcome dummies, there is no statistically significant effect of access to daycare for children up to age two, negative effects of access to daycare for children between age three and six, and no additional effect of having access to both types of daycare. Daycare for children between age three and six reduces the probability of having more than one child by 1.3 percentage points (1.6 percent), the probability of having more than two children by 1.8 percentage points (6.0 percent), and the probability of having more than three children by 0.5 percentage points (7.1 percent). Overall, daycare availability has negative effects on total fertility, and the effects are larger for mothers on the margin of having more than two or three children. The negative effects on fertility are in contrast to previous empirical findings from quasi-experimental variations in daycare access or costs. Bauernschuster et al. (2016) examines how the introduction of daycare for children aged three affects fertility in Germany, and they find that daycare increases fertility. However, Bauernschuster et al. (2016) examine a period during which the total fer-

Table 5: The effects of daycare on mothers' fertility decisions

	(1) # children	(2) More than 1	(3) More than 2	(4) More than 3
DC for 0-2 year olds	-0.020	-0.006	-0.012	-0.001
	(0.029)	(0.013)	(0.014)	(0.004)
DC for 3-6 year olds	-0.049***	-0.013***	-0.024***	-0.009***
	(0.005)	(0.002)	(0.003)	(0.002)
Interaction term	0.027	0.000	0.019	0.005
	(0.033)	(0.014)	(0.016)	(0.005)
Observations	370602	370602	370602	370602
Year dummies	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each column presents estimates from separate regressions, where the outcome variable is number of children, a dummy for more than one child, a dummy for more than two children, and a dummy for more than three children, respectively. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

tility in Germany was low, with 1.4 children per women; in contrast, the mothers in this sample on average have 2.2 children. In a similar vein, Rindfuss et al. (2010) find a positive impact of daycare availability on fertility in Norway during the 1970s, when total fertility was 1.85. Mörk et al. (2013) examine the effects of childcare costs on fertility in Sweden and find limited effects of changes in the childcare costs on fertility behavior. Combining the labor force participation and fertility effects in this paper suggests that daycare increases the opportunity cost of children.

Although daycare availability reduces the number of children, it can potentially encourage mothers to have their children over a shorter period of time in order to return to the labor market faster. On the other hand, daycare availability can encourage mothers to have longer intervals between childbirth in order to attain longer job spells and more job-specific human capital. Consequently, I examine whether daycare availability affects the spacing between children for mothers with more than one child. The results are presented in Table 6. The outcome variables are number of years to the next child and five indicator variables taking the value one if the mother has her second child within one through five years after the first child. The explanatory variables are an indicator for daycare for children up to age two and an indicator for daycare for children between age three and six, both measured in the year the mother has her first child. Again, only the indicator for daycare for children between

Table 6: The effects of daycare on spacing between first and second child

	(1) # years	(2) $\leq$ 1 year	(3) $\leq$ 2 years	(4) $\leq$ 3 years	(5) $\leq$ 4 years	(6) $\leq$ 5 years
DC for 0-2 year olds	0.058	0.001	0.003	-0.000	-0.005	-0.006
	(0.083)	(0.011)	(0.008)	(0.020)	(0.015)	(0.013)
DC for 3-6 year olds	0.137***	-0.004**	-0.022***	-0.028***	-0.023***	-0.017***
	(0.017)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Interaction term	-0.026	-0.000	-0.003	-0.005	-0.001	0.001
	(0.086)	(0.011)	(0.008)	(0.021)	(0.015)	(0.013)
Observations	305504	305504	305504	305504	305504	305504
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes
Municipal FE	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each panel presents estimates from separate regressions, with different outcomes in each column. The outcomes are number of years to the next child, a dummy for one year or less, a dummy for two years or less, and so forth. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

age three and six is statistically significant indicating that mothers with daycare access wait 0.137 years more to have their next child than mothers without daycare access. The results for the dummy outcomes are in line with the result of the number of years to next child. Mothers with daycare access are 2.2 percentage points less likely to have their next child within two years. Thus, daycare availability increases spacing between children, allowing mothers to work longer spells between childbirth.

As mothers entered the labor market and started to generate income they became less economically dependent on the child's father. Thus, greater female labor force participation may correlate with higher divorce rates. However, the causality between female labor force participation and divorce rates can run in both directions. On one the hand, married women might work more because they anticipate a divorce. On the other hand, women who work more might be more likely to divorce because their opportunity cost of marriage is higher (Becker et al., 1977; Johnson and Skinner, 1986). Consequently, I continue the analysis by investigating whether daycare availability has any effect on parental separation. I define separations based on the mother's and father's address in the year during which the first child turns 16, which is the first year I can observe address information for the parents from the earliest cohort.[12] Table 7 shows the effect of daycare availability on household separation

[12]I do not observe if the parents are married before they have children nor do I observe if they have

Table 7: The effects of daycare on parents not living together at child age 16 and additional educational achievement after the birth of the first child

	(1) Separation	(2) More education
Panel A		
DC for 0-2 year olds	-0.000	0.001
	(0.013)	(0.015)
DC for 3-6 year olds	0.013***	0.002
	(0.003)	(0.002)
Interaction term	0.010	0.003
	(0.013)	(0.016)
Panel B		
Daycare year 4	0.020***	0.001
	(0.003)	(0.002)
Observations	365949	370602
Year dummies	Yes	Yes
Municipal FE	Yes	Yes
Covariates	Yes	Yes

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each panel presents estimates from separate regressions. In Column (1) the outcome variable is a dummy taking the value one if the parents do not live together when the first born child turns 16. In Column (2) the outcome variable is a dummy taking the value one if the mother achieves more education after the birth of her first child. In Panel A, daycare availability is measured prior to the birth of the first child, and in Panel B, daycare availability is measured in the year the first born child turns four. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

at child age 16. In Panel A, the explanatory variables are daycare availability for children up to age two, daycare availability for children between age three and six, and the interaction term between the two daycare availability measures, which are all measured in the year of the first childbirth. In Panel B, the explanatory variable is daycare availability in the year the first born child turns four. Using daycare availability in the year the child turns four as the explanatory variable, Column (1) of Table 7 shows that mothers who had access to daycare are 2 percentage points more likely to live apart from the child's father in the year the child turns 16 compared to mothers who did not have access to daycare when the child was four.

As a final mechanism, I investigate whether daycare availability has any effects on additional educational achievement after the birth of the first child. The summary

ever lived together.

statistics in Table 1 show that 12 percent of the mothers in the sample continue their education after they have their first child; however, Column (2) of Table 7 shows that daycare availability does not affect additional education attainment after first childbirth. In summary, the results show that daycare availability, fertility, and female labor supply are associated.

6.5 Robustness and sensitivity checks

In this section, I present the results from a series of robustness and sensitivity checks. Specifically, I examine whether the results are driven by selective movement, the pre-1970 child cohorts, urbanicity, and differential time evolvement. First, the results in this paper would be biased if parents choose to live in a neighborhood based on daycare availability. To validate that parents are not selecting into neighborhoods with daycare availability, I calculate the distance between the birth place of the mother and her address in the 1970 census tract and examine whether mothers moving a greater distance are more likely to live in a neighborhood with daycare. I exploit information about the latitude and longitude of the neighborhood the mother is born in and the neighborhood in which she resides in 1970 to calculate the distance the mother moves. Variations of the distance measure are used as explanatory variables in regressions where daycare availability in year four is the dependent variable.

Table 8 shows the relationship between distance moved and daycare availability. The estimates in Column (1) are from regressions in which no other explanatory variables are included, whereas the estimates in Column (4) are from regressions with the full set of covariates and fixed effects. In Panel A, distance in terms of 100 km is the main explanatory variable, and the estimate in Column (1) suggests that mothers who move 100 km away from their birth place are 0.032 percentage points more likely to reside in a neighborhood with daycare access in 1970. Including the full conditioning set reduces the estimate, such that mothers who move 100 km are 0.004 percentage points more likely to live in a neighborhood with daycare access in 1970. Denmark is a small country, measuring only about 455 km from the most eastern point to the most western point. On average, mothers move 49 km away from their birth place, but there is a large variation in the distance mothers move (e.g., 12 percent of the mothers do not move away from their birth place (cf. Table8, Panel D)). In Panel B, both distance and squared distance are included as explanatory variables, and there is no longer any significant relationship between the distance the mothers

Table 8: Selective migration

	(1) Daycare	(2) Daycare	(3) Daycare	(4) Daycare
Panel A				
Distance	0.03238***	0.03651***	0.00400**	0.00435***
	(0.00553)	(0.00592)	(0.00170)	(0.00133)
Panel B				
Distance	0.00382	0.01786	0.00414	0.00739
	(0.03430)	(0.03331)	(0.00635)	(0.00514)
Squared distance	0.01347	0.00878	-0.00007	-0.00143
	(0.01478)	(0.01409)	(0.00250)	(0.00209)
Panel C				
IHST distance	0.00474	0.00785**	-0.00116	0.00062
	(0.00295)	(0.00304)	(0.00112)	(0.00086)
Observations	328330	328330	328330	328330
Year dummies	No	Yes	Yes	Yes
Municipal FE	No	No	Yes	Yes
Covariates	No	No	No	Yes
Panel D				
Means of the distance measures:	Distance (100km)	Squared distance	IHST distance	Zero distance
	0.49	0.69	3.34	0.12
	(0.67)	(1.52)	(1.87)	(0.32)

NOTE— * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Standard errors clustered at the municipality level in parentheses. Column (1) only includes the explanatory variable(s). Columns (2)-(4) include a full set of year of first childbirth dummies and Columns (3)-(4) include municipality fixed effects. Additionally, covariates are added in Column (4). The included covariates are a full set of dummies for mother's age at first birth, mother's year of birth, month of birth of the first child, and an indicator of urban area. There is no information about the birth place for 11.4 percent of the mothers in the sample.

move away from their birth place and daycare availability. In Panel C, I convert the distance to log values using the Inverse Hyperbolic Sine Transformation (IHST) in order to keep those with a distance of zero. The results of the IHST distance are not statistically significant, which suggests that mothers are not selectively moving a longer distance in order to have access to daycare. In summary, selective migration does not seem to be an issue.

Second, in another set of robustness checks, I exclude mothers who have their first child prior to 1970. The assignment of daycare availability is based on the year the mothers have their first child and the mothers' address in 1970. Thus, for mothers who have their first child prior to 1970, their address is not predetermined. However, Table 9 shows that the results are robust to the exclusion of mothers who have their

Table 9: Robust to the exclusion of the pre-1970 cohort

	(1) Participation	(2) Full time	(3) Hours of work
Year 0	0.006	0.010***	0.180
	(0.003)	(0.004)	(0.132)
Year 1	0.013***	0.008*	0.376**
	(0.005)	(0.005)	(0.164)
Year 2	0.015***	0.006	0.393**
	(0.005)	(0.004)	(0.167)
Year 3	0.041***	0.017***	1.158***
	(0.006)	(0.004)	(0.167)
Year 4	0.042***	0.020***	1.246***
	(0.005)	(0.004)	(0.155)
Year 5	0.036***	0.019***	1.143***
	(0.005)	(0.004)	(0.138)
Year 6	0.036***	0.020***	1.062***
	(0.004)	(0.004)	(0.139)
Observations	173360	173360	173360

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. Standard errors clustered at the municipality level in parentheses. Each cell shows point estimates from separate regressions, each row indicates the year in which daycare availability and the outcome variable are measured. Column (1) shows the contemporaneous effects of daycare availability on mothers' labor force participation. Column (2) shows the contemporaneous effects of daycare availability on mothers' full-time employment. Column (3) shows the contemporaneous effects of daycare availability on mothers' hours of work. All regressions include municipality fixed effects, a full set of dummies for year of first childbirth, mother's age at first birth, mother's year of birth, month of birth of her first child, and an indicator of urban area.

first child prior to 1970 for the three contemporaneous outcomes.

I further test whether exclusion of these mothers affects the long-run estimates; Figure 7 shows the effect sizes from Figure 4 along the corresponding effect sizes from a robustness analysis excluding mothers, who have their first child prior to 1970. The results are qualitatively robust to the exclusion of the pre-1970 cohort, although the effect sizes are smaller for the participation rate. For the three other dependent variables, the main effects are close to the effects based on a sub-sample of mothers who have their first child after 1970.

Third, I consider whether the results are sensitive to differences between rural and urban areas. Denmark experienced an economic upturn during the late 1950s, but production was concentrated in the city areas. Consequently, job opportunities were more numerous in the cities, and the larger cities already had daycare prior to the universal daycare reform. Figure 2 shows a map of the rollout, and it is evident

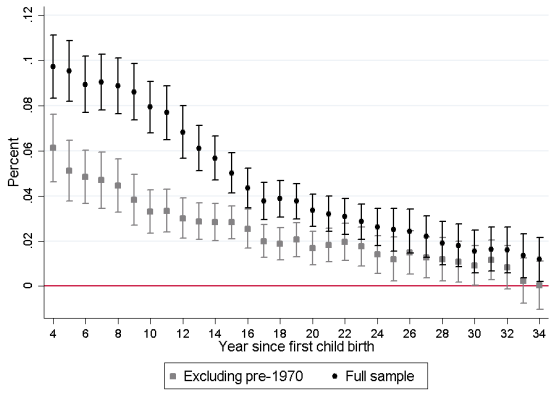
that the capital Copenhagen, the second largest city, Aarhus, and the third largest city, Odense, all had daycare before the universal daycare reform. I test whether the results are driven by daycare access and better job opportunities in the larger cities by re-estimating the effects while excluding the three largest municipalities. Figure 8 shows that the results are robust to the exclusion of the three largest municipalities in Denmark.

During the 1960s and 70s, the suburbs of Copenhagen and Aarhus expanded, and it is also evident from the map in Figure 2 that a daycare institution opened in many of these areas. I test whether the results are robust to the exclusion of the suburbs of the two largest cities. Specifically, I exclude the neighboring municipalities and re-estimate the effects. Figure 9 shows that the exclusion of the suburbs of the two largest cities does not change the results.

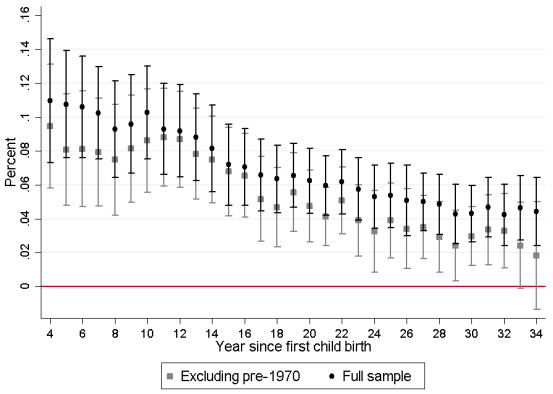
As a final set of checks, I test whether the results are robust to differential time evolvement. In the main specification I allow for flexible time trends by including a full set of year dummies for the year the mother has her first child. In figure 10 I examine if the results are sensitive to the inclusion of an additional set of time dummies by giving mothers living in the most populous neighborhood within the municipality a separate set of time dummies. Figure 10 shows that the inclusion of these time dummies does not alter the result that daycare availability during child-rearing years has lasting effects of labor market participation and earnings more than 30 years after the birth of the first child.

Figure 7: Robust to the exclusion of the pre-1970 cohort

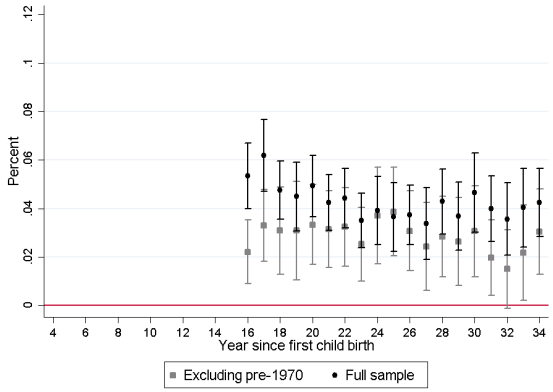
(a) Participation rate



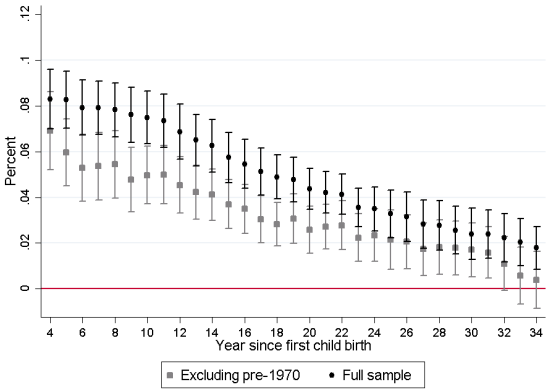
(b) Full-time employment



(c) Log earnings



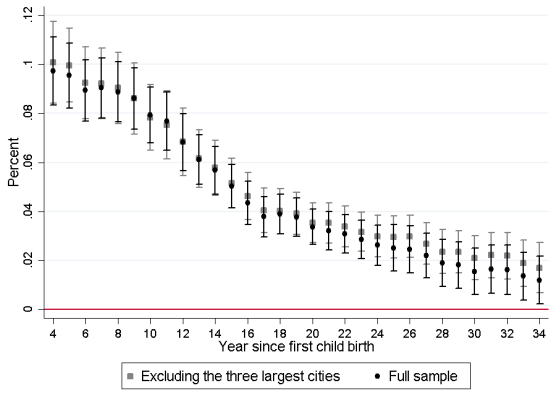
(d) Hours of work



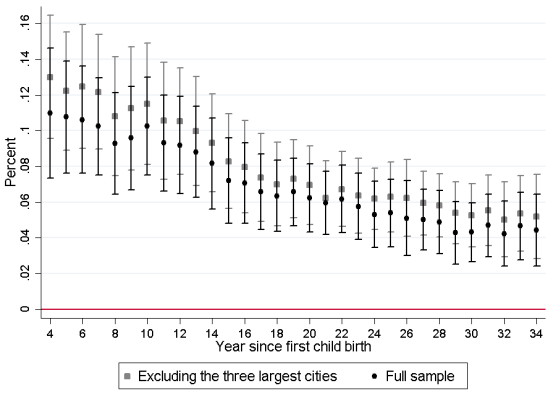
NOTE— The figure shows the effects of daycare in the year the first born child turns four on (a) the participation rate, (b) full-time employment, (c) log earnings (conditional on employment), and (d) hours of work per week for the full sample and for a subsample excluding the mothers who had their first child before 1970. Each square corresponds to a point estimate from a separate estimation. The point estimates in (a), (b), and (d) are scaled by the sample means for the full sample and for the subsample, respectively. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

Figure 8: Robust to the exclusion of the three largest cities

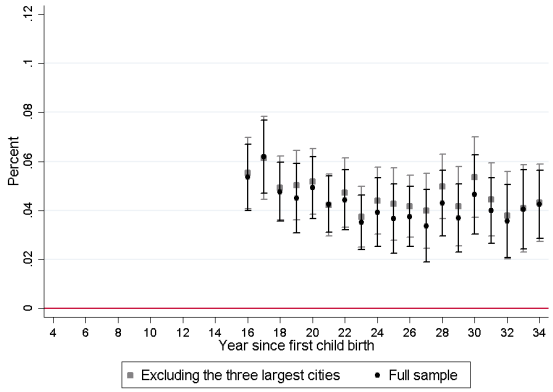
(a) Participation rate



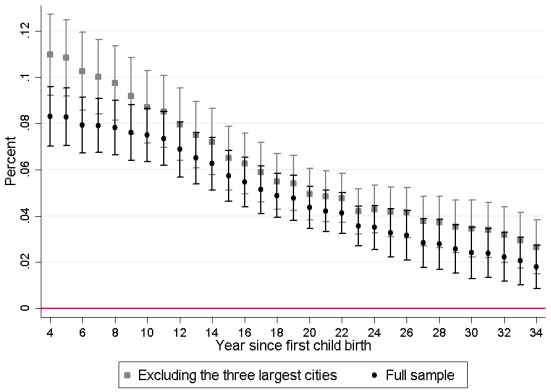
(b) Full-time employment



(c) Log earnings



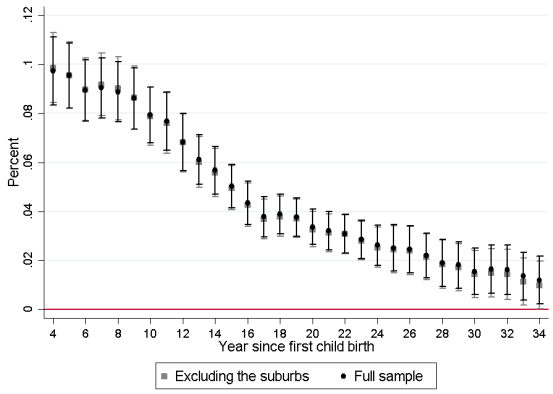
(d) Hours of work



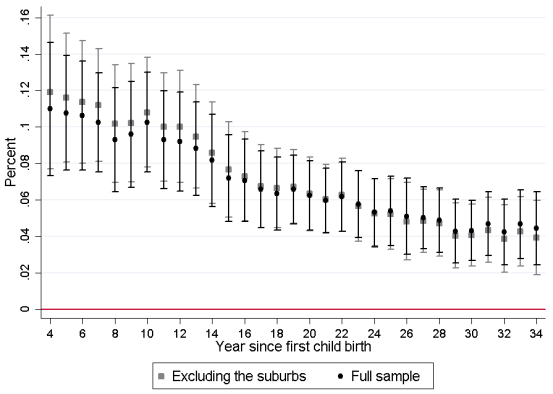
NOTE— The figure shows the effects of daycare in the year the first born child turns four on (a) the participation rate, (b) full-time employment, (c) log earnings (conditional on employment), and (d) hours of work per week for the full sample and for a subsample excluding the three largest cities (Copenhagen, Frederiksberg, Aarhus, and Odense municipality). Each square corresponds to a point estimate from a separate estimation. The point estimates in (a), (b), and (d) are scaled by the sample means for the full sample and for the subsample, respectively. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

Figure 9: Robust to the exclusion of suburbs

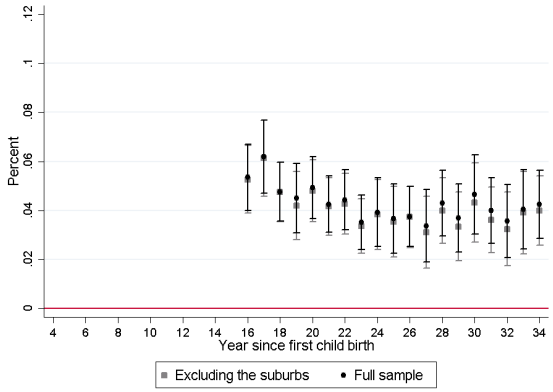
(a) Participation rate



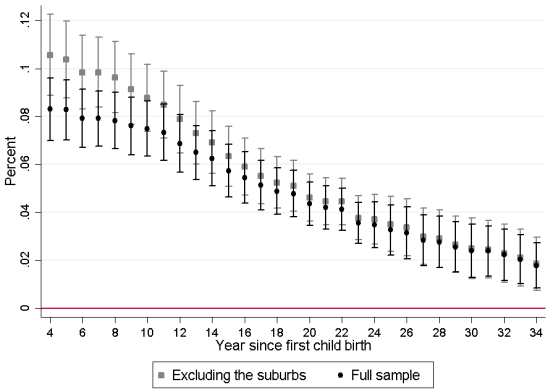
(b) Full-time employment



(c) Log earnings



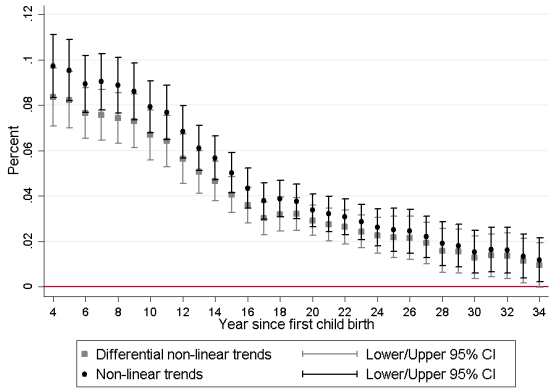
(d) Hours of work



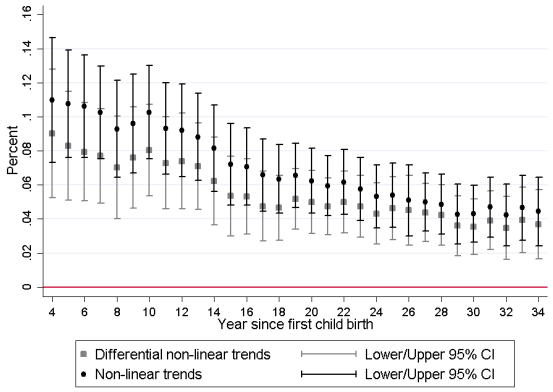
NOTE— The figure shows the effects of daycare in the year the first born child turns four on (a) the participation rate, (b) full-time employment, (c) log earnings (conditional on employment), and (d) hours of work per week for the full sample and for a subsample excluding the suburbs of the two largest municipalities; Copenhagen and Aarhus. Each square corresponds to a point estimate from a separate estimation. The point estimates in (a), (b), and (d) are scaled by the sample means for the full sample and for the subsample, respectively. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.

Figure 10: Robust to differential non-linear time evolvement

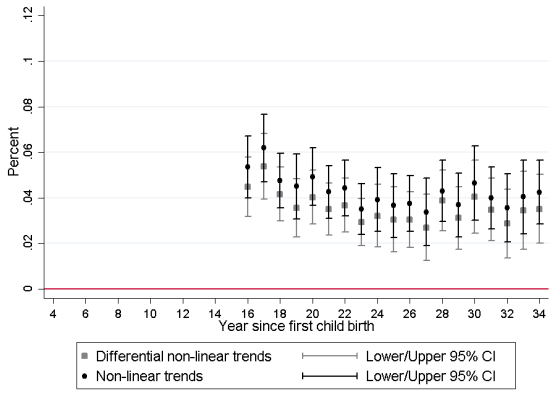
(a) Participation rate



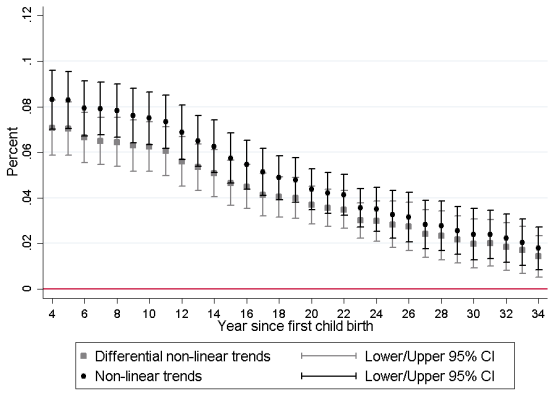
(b) Full-time employment



(c) Log earnings



(d) Hours of work



NOTE— The figure shows the effects of daycare in the year the first born child turns four on (a) the participation rate, (b) full-time employment, (c) log earnings (conditional on employment), and (d) hours of work per week separately for the main specification with non-linear time dummies and for a specification allowing for differential non-linear time dummies. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth (time dummies), and the year she has her first child. Additionally, all regressions include municipality fixed effects.

7 Conclusion

This paper analyzes the short- and long-run effects of subsidized daycare availability on mothers' employment and earnings through 34 years after the birth of their first child using the Danish rollout of universal daycare. The results show that the relevant margin of daycare provision during the mid-1960s and 70s was daycare for children between age three and six. The results further show that daycare access in the year the first born child turns four has lasting effects on labor force participation, full-time employment, hours worked, and labor market earnings. The daycare effects are larger the younger the child; however, the effects remain positive 34 years after the birth of the first child.

The rich administrative data allows me to investigate the effects of the rollout of universal daycare separately for low- and higher-educated mothers. The effects of daycare on maternal employment are primarily driven by low-educated mothers, whereas the employment effects are smaller and less persistent for higher-educated mothers, indicating that the lack of daycare availability is a larger employment barrier for low-educated women when they have children than for higher-educated women. The earnings estimates for low- and higher-educated mothers are more closely aligned than the employment effects. However, for higher-educated mothers, the effects of daycare on earnings are longer lived than the effects on employment, suggesting that job attachment during child-rearing years is especially important for higher-educated mothers.

Although access to daycare during the child-rearing years has a positive impact on labor market outcomes in the long-run, daycare access also negatively affects total fertility and increases the spacing between the first and the second child. Furthermore, daycare availability during child-rearing years increases the probability of living apart from the first born child's father 16 years after the birth of the child.

The results of this paper suggest that subsidized daycare can increase mothers' employment beyond the preschool years and that access to daycare can be an important policy tool for economic output. This suggests that increased labor market attachment during the child-rearing years has important economic consequences in the long run.

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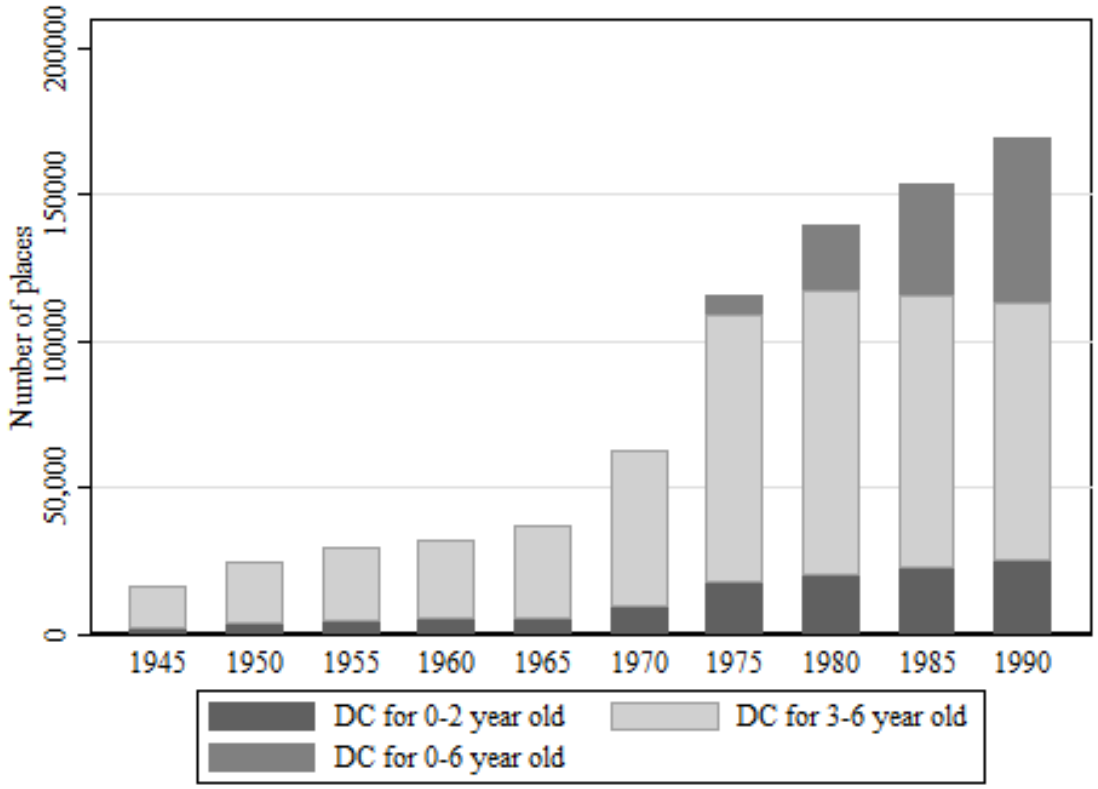
A Number of daycare places

Figure A.1: Number of places in daycare institutions for children aged 0-2, 3-6, and 0-6



NOTE— The figure shows the number of places in daycare institutions for children up to age two, children between age three and six, and children up to age six. Daycare for children up to age six (integrated institutions) was implemented in 1975.

B Additional descriptive statistics

Table B.1: Summary statistics of fertility and separation outcomes

	(1) All	(2) Daycare, year 0-2 No	(3) Daycare, year 0-2 Yes	(4) Daycare, year 3-6 No	(5) Daycare, year 3-6 Yes
Panel A					
Number of children	2.20	2.25	2.09	2.30	2.12
	(0.85)	(0.87)	(0.79)	(0.89)	(0.81)
More than 1 child	0.82	0.84	0.80	0.85	0.81
	(0.38)	(0.37)	(0.40)	(0.36)	(0.39)
More than 2 children	0.29	0.32	0.23	0.35	0.25
	(0.45)	(0.47)	(0.42)	(0.48)	(0.43)
More than 3 children	0.06	0.07	0.04	0.08	0.05
	(0.24)	(0.26)	(0.21)	(0.27)	(0.22)
Panel B					
Parents not living together, year 16	0.20	0.18	0.23	0.18	0.22
	(0.40)	(0.39)	(0.42)	(0.38)	(0.41)
Observations	370602	254550	116052	154662	215940

NOTE— Panel A shows the means and standard deviations of the four fertility outcomes for all mothers in the sample and separately for mothers with and without daycare access for children up to age two and for children between age three and six measured in the year of birth of their first child. Panel B shows the mean and standard deviation of the dummy for parents not living together when the first child is 16. The number of observations are for the full sample although the separation outcome is undefined for 1.26 percent of the sample because of missing father id.

Table B.2: Summary statistics of participation rate, full-time employment, and hours worked 0 through 6 years after first childbirth.

	(1)	(2)	(3)	(4)	(5)	(6)
	All		No Daycare		Daycare	
	Mean	Std. dev.	Mean	Std. dev.	Mean	Std. dev.
Year 0						
Participation	0.67	(0.47)	0.62	(0.49)	0.77	(0.42)
Full-time employment	0.15	(0.35)	0.12	(0.33)	0.20	(0.40)
Hours worked	16.34	(13.47)	14.92	(13.43)	19.47	(13.03)
Observations	370602		254550		116052	
Year 1						
Participation	0.56	(0.50)	0.51	(0.50)	0.65	(0.48)
Full-time employment	0.16	(0.37)	0.14	(0.35)	0.20	(0.40)
Hours worked	14.00	(14.07)	12.64	(13.90)	16.48	(14.03)
Observations	370602		239086		131516	
Year 2						
Participation	0.57	(0.49)	0.53	(0.50)	0.65	(0.48)
Full-time employment	0.17	(0.38)	0.15	(0.36)	0.20	(0.40)
Hours worked	14.54	(14.10)	13.18	(14.00)	16.61	(14.00)
Observations	370602		223720		146882	
Year 3						
Participation	0.58	(0.49)	0.47	(0.50)	0.62	(0.49)
Full-time employment	0.17	(0.38)	0.13	(0.33)	0.18	(0.39)
Hours worked	14.68	(14.06)	11.71	(13.73)	15.71	(14.03)
Observations	370602		95679		274923	
Year 4						
Participation	0.59	(0.49)	0.48	(0.50)	0.63	(0.48)
Full-time employment	0.17	(0.38)	0.12	(0.33)	0.19	(0.39)
Hours worked	15.00	(14.06)	11.91	(13.72)	15.98	(14.02)
Observations	370602		89284		281318	
Year 5						
Participation	0.61	(0.49)	0.50	(0.50)	0.64	(0.48)
Full-time employment	0.18	(0.38)	0.13	(0.33)	0.19	(0.39)
Hours worked	15.46	(14.08)	12.34	(13.78)	16.37	(14.04)
Observations	370602		83855		286747	
Year 6						
Participation	0.62	(0.49)	0.51	(0.50)	0.65	(0.48)
Full-time employment	0.18	(0.38)	0.13	(0.34)	0.19	(0.40)
Hours worked	15.78	(14.08)	12.68	(13.81)	16.62	(14.03)
Observations	370602		78754		291848	

NOTE— The table shows the means and standard deviations of the participation rate, full-time employment, and hours worked zero through six years after the first childbirth for all mothers in the sample and separately for mothers with and without daycare in the year the outcomes are measured

Table B.3: Mean of the outcome variables zero through 34 years after first childbirth

	(1) Participation	(2) Full-time	(3) Hours of work	(4) Earnings
Year 0	0.67	0.15	16.34	
Year 1	0.56	0.16	14.00	
Year 2	0.57	0.17	14.54	
Year 3	0.58	0.17	14.68	
Year 4	0.59	0.17	15.00	
Year 5	0.61	0.18	15.46	
Year 6	0.62	0.18	15.78	
Year 7	0.63	0.18	16.09	
Year 8	0.65	0.19	16.62	
Year 9	0.67	0.20	17.32	
Year 10	0.70	0.21	18.07	
Year 11	0.72	0.22	18.86	
Year 12	0.75	0.24	19.69	
Year 13	0.77	0.25	20.49	
Year 14	0.78	0.27	21.16	
Year 15	0.80	0.29	21.69	
Year 16	0.81	0.30	22.21	26330
Year 17	0.81	0.32	22.54	27078
Year 18	0.81	0.34	22.87	27770
Year 19	0.81	0.36	23.14	28347
Year 20	0.81	0.37	23.34	28809
Year 21	0.81	0.39	23.50	29268
Year 22	0.81	0.40	23.53	29613
Year 23	0.80	0.41	23.50	29886
Year 24	0.79	0.42	23.43	30008
Year 25	0.79	0.43	23.33	30043
Year 26	0.78	0.44	23.21	30081
Year 27	0.77	0.45	23.04	30010
Year 28	0.76	0.45	22.82	29854
Year 29	0.75	0.46	22.52	29608
Year 30	0.73	0.45	22.08	29281
Year 31	0.72	0.42	21.60	28915
Year 32	0.70	0.39	21.08	28481
Year 33	0.68	0.35	20.45	27877
Year 34	0.66	0.30	19.64	27013

NOTE— The table shows the means of the outcomes used in the analysis of long-run effects. The participation rate and full-time employment are dummy variables. The means of hours worked include those who work zero hours a week. The earnings are adjusted to 2016 USD and those with zero earnings are included in the mean values.

Table B.4: Correlation of daycare availability across years after first childbirth

	Year 0	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6
Year 0	1						
Year 1	0.916***	1					
Year 2	0.844***	0.922***	1				
Year 3	0.410***	0.446***	0.483***	1			
Year 4	0.358***	0.389***	0.421***	0.871***	1		
Year 5	0.342***	0.373***	0.402***	0.829***	0.951***	1	
Year 6	0.328***	0.357***	0.386***	0.793***	0.908***	0.954***	1

NOTE— $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$. The table shows the correlations of daycare availability across years after the birth of the first child. The correlations are high across all years, but highest between the years 0-2 and 3-6 (e.g., for those with daycare availability in year 1, 91.6 percent had daycare access in year 0, while for those with daycare access year 6, only 32.8 percent had access in year 0). Year 0 corresponds to the year the mother gives birth to her first child, year 1 corresponds to the year the mothers first born child turns one and so forth.

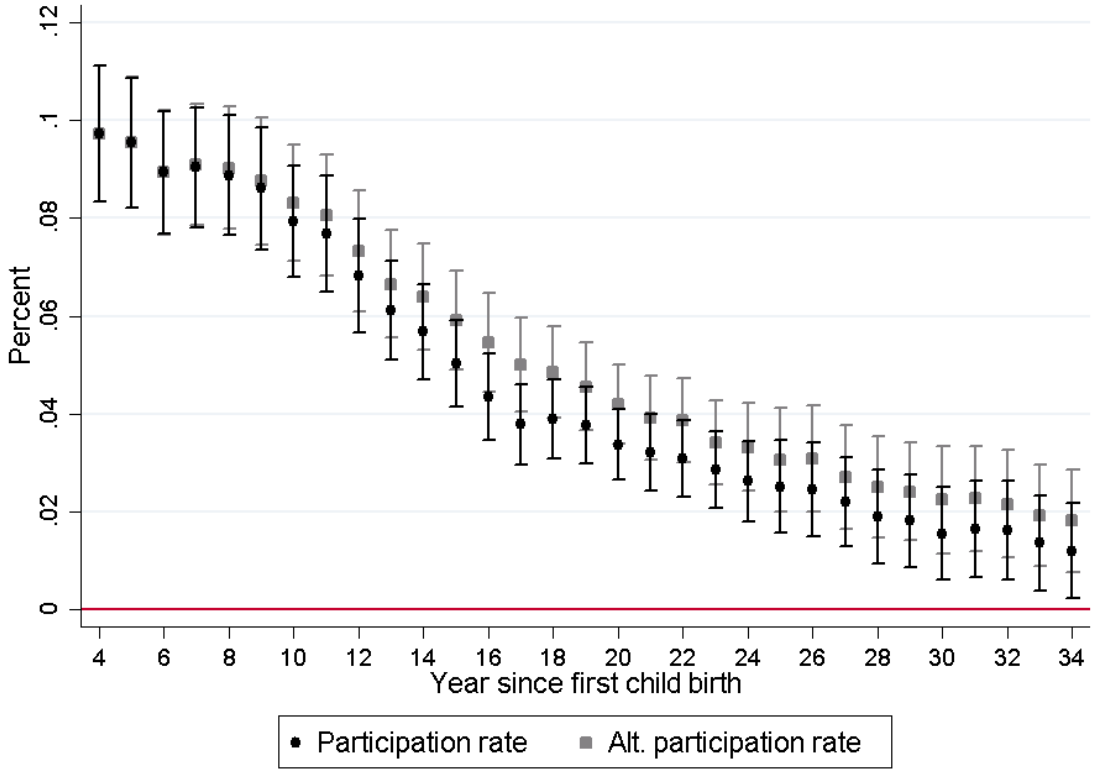
Table B.5: Summary statistics separately for low- and higher-educated mothers

	(1) All	(2) Low	(3) High
Mother's age at first birth	23.70	22.76	24.65
	(4.35)	(4.67)	(3.78)
Year of birth	1945.57	1946.35	1944.79
	(5.42)	(5.93)	(4.72)
Years of education	10.78	8.22	13.37
	(2.94)	(1.53)	(1.28)
Urban neighborhood	0.22	0.22	0.22
	(0.41)	(0.41)	(0.42)
Missing father id	0.01	0.02	0.01
	(0.11)	(0.12)	(0.10)
Number of children	2.20	2.28	2.12
	(0.85)	(0.93)	(0.75)
Years between first and next child	3.76	3.80	3.73
	(2.44)	(2.62)	(2.26)
Not living with child's father	0.20	0.24	0.16
	(0.40)	(0.43)	(0.37)
Additional education after birth of first child	0.12	0.20	0.04
	(0.33)	(0.40)	(0.21)
Lenght of working life (years) after first child	25.19	23.12	27.27
	(10.26)	(10.59)	(9.47)
Observations	370602	186127	184475

NOTE— The table shows the means and standard deviations of the same variables as in table 1 separately for low- and higher-educated mothers. Higher-educated mothers are defined as mothers with post-secondary education.

C Different participation definitions

Figure C.1: The effects of daycare on alternative definition of the participation rate



NOTE— The figure shows the effects of daycare in the year the first born child turns four on two different definitions of the participation rate. The black dots are the estimates presented in figure 4 based on a dummy for positive earnings in years from 1980 or contributions to the ATP in the years prior to 1980. The gray squares are estimates based on a dummy for contributions to the ATP for all of the years. The point estimates are scaled by the means of the dependent variable in the corresponding year. All regressions include an indicator of urban area and a full sets of dummies for mother's age at first birth, mother's year of birth, and the year she has her first child. Additionally, all regressions include municipality fixed effects.