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Nobody Home: The Effect of Maternal Labor Force Participation on Long-Term Child Outcomes

Venke Furre Haaland
Mari Rege
Mark Votruba

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Abstract

We investigate how mother's employment during childhood affects long term child outcomes. We utilize rich longitudinal data from Norway covering the entire Norwegian population between the years 1970 to 2007. The data allows us to match all family members and to measure maternal labor force participation throughout the child's entire childhood. Our empirical approach exploits the variation in exposure to a working mother that exists across older and younger siblings in different family types. We compare sibling differences in families where the mother enters the labor force when the children are older and where the mother remains employed full time thereafter, to sibling differences in families where the mother remains out of the labor force during the entirety of her children's adolescent years. Our identification strategy is, therefore, in the spirit of traditional difference-in-differences, the first difference pertaining to the differences in children's ages within a family and the second pertaining to different family types. The analysis suggests that maternal labor force participation has significant and negative effects on years of education and labor market outcomes. However, the effects are small, which supports the notion that maternal labor force participation has, on average, a small effect on long-term outcomes for children.

JEL-Code: D130, J220.

Keywords: child development, household production, maternal labor force participation.

Venke Furre Haaland
University of Stavanger / Norway
venke.f.haaland@uis.no

Mari Rege
University of Stavanger / Norway
mari.rege@uis.no

Mark Votruba
Case Western Reserve University / USA
mark.votruba@case.edu

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1 Introduction

Dramatic increases in female labor force participation have changed the everyday life of children dramatically during the last decades. In the United States in 1970, 50 percent of women between 25-54 years were working. By 2008, this number had increased to 75 percent.[1] In Norway, the focus of our study, more than 85 percent of women between 25-54 years were working in 2009. In this paper we investigate how maternal employment during childhood ages of 1-16 affects long-term child outcomes.

Maternal employment can affect child development through at least four different channels. First, depending on the degree to which maternal care is substituted for by alternative care, it may affect the quality of care (Becker, 1991). The substitutability will be contingent on the quality of both the maternal care and the alternative care.[2] Second, to the extent that mother's employment increases family income, the increased financial resources could have a positive effect on child development (e.g. Becker and Tomes, 1993; Duncan and Brooks-Gunn, 1997; Blau, 1999; Baum and Charles, 2003; Dahl and Lochner, 2012). Third, if maternal employment leads to increased stress level and tiredness, higher levels of parent-child conflicts and lower levels of parental acceptance might develop (Crouter and Bumpus, 2001), which in turn could affect child development. Fourth, a mother's participation in the labor force could affect the attitudes and aspirations of her children, especially for daughters (see e.g. Fernandez and Fogli, 2009; Fogli and Veldkamp, 2011; Alesina et al., 2013).

To examine the relationship between maternal labor force participation and long-term child outcomes, we utilize rich longitudinal data from Norway, covering the entire Norwegian population between the years 1970 to 2007. Importantly, the data allow us to match all family members and to measure the maternal labor force participation throughout the child's entire childhood. Moreover, we measure long-term educational and labor markets outcomes, in addition to weight, height and IQ for boys at age 19.

Our empirical approach addresses problems of endogeneity by exploiting the

[1] OECD Labor Market Statistics: http://stats.oecd.org/

[2] For example, Brooks et al. (2001) show that in cases where the alternative to maternal care is unsupervised time at home, children of working mothers often have less discipline and less self-confidence. Yet, outcomes for some children may improve if working parents rely on high-quality day-care programs and after-school care (e.g. Blau and Currie, 2006).

variation in exposure to a working mother that exists across older and younger siblings in different types of families. Many mothers choose to stay home or work part time while they have young children at home but then permanently return to full-time employment when the children are older. We refer to these families as Enter Work (EW) families. In such families, older children systematically experience fewer years of exposure to a working mother than do their younger siblings. In families where the mother remains out of the labor force during the entirety of her children's adolescent years, these systematic differences in exposure to a working mother across older and younger siblings do not exist. We refer to these families as Never Work (NW) families. If longer exposure to a working mother affects children's outcomes, we would expect, ceteris paribus, that the difference in outcomes observed for older and younger siblings would vary across the two family types. Our identification strategy is, therefore, in the spirit of traditional difference-in-differences, the first difference pertaining to the differences in children's ages within a family and the second pertaining to the different family types. The crucial identifying assumption is that relative age has identical effects on children's outcomes in different types of families in the absence its effect on differential exposure to a working mom. Our rich data allow us to carefully investigate the plausibility of this assumption by interacting relative age with several observable family and event characteristics, as well as running placebo analyses.

Our analysis suggests that maternal labor force participation has a small but negative and significant effect on years of education for the child. Our estimate indicates that each additional year of exposure to a mother who chooses to stay at home instead of work outside the home is predictive of an additional 0.013 years of education. Linearly extrapolating from this result suggests that 5 years of a mother staying at home, as opposed to entering the labor force, is predictive of 0.065 years of additional education for her child, which amounts to about 4 percent of the standard deviation in years of education. While such extrapolations have to be interpreted with caution, this supports the notion that maternal labor force participation has, on average, a very small effect on a child's long-term education. We also find small effects of maternal labor force participation on a child's long-term labor market outcomes.

There is a substantial body of literature investigating the effect of maternal employment on child outcomes. Recent studies investigating the effect of maternal employment during the child's first year of life have utilized parental leave reforms to deal with the non-random selection into maternal employment (e.g. Carneiro et al., 2011; Baker and Milligan, 2010; Dustmann and Schönberg, 2012). Most studies of older children lack the same sophistication (Ruhm, 2008). One important exception can be found in studies evaluating welfare-to-work programs, which provide consistent evidence that maternal labor force participation is positive for child development (Grogger et al., 2002). However, even if these studies provide compelling evidence for the population of welfare recipients, it is hard to generalize these results to the population at large. So far, the evidence for the broader population is mixed (Datcher-Loury, 1988; Muller, 1995; Waldfogel et al., 2002; Anderson et al., 2003; James-Burdumy, 2005; Blau and Currie, 2006; Ruhm, 2008). Similar to the approach in the present study, some of these studies investigate how maternal labor force participation affects children in family fixed effects models (e.g. Waldfogel et al., 2002; James-Burdumy, 2005; Ruhm, 2008). An important concern in these papers is that a mother's decision to enter or exit the labor market may depend on child characteristics (Ruhm, 2008). A second concern is that the effect of birth order on child development varies across different family types (Kalil et al., 2012a). The rich registry data allow us to carefully address these concerns in our fixed effects framework. Finally, a general concern in fixed effects models is that problems of attenuation bias—arising from measurement error in covariates—become amplified. By focusing our analysis on the two types of families, EW and NW, our approach eliminates the problem of measurement error. This is because of the zero difference in mother's labor force participation across siblings for the NW families, whereas for the EW families, the difference in mother's labor force participation is exactly equal to the children's difference in age.

This paper is particularly related to Bettinger et al. (2013), who investigate a causal relationship between maternal labor force participation at ages seven to eleven and on the grade point average in tenth grade. In stark contrast to the present study, Bettinger et al. (2013) demonstrate a very large, negative effect of maternal labor force participation on long-term child outcomes. In the concluding section we discuss how the different findings may be due to the fact that the fixed effects model in the present study estimates an average treatment effect, whereas the instrumental variable approach in Bettinger et al. (2013) estimates a local average treatment effect (LATE) (Imbens and Angrist, 1994).

The remainder of this paper proceeds as follows: Section 2 presents the empirical approach, Section 3 describes the data, and Section 4 presents the results. Conclusions are offered in Section 5.

2 Empirical Strategy

To fix ideas, we start with the following stylized model:

$$Y_i = \alpha + \beta X_i + \theta MWyrs_i + e_i, \tag{1}$$

where Y_i denotes the child's long-term outcome (e.g. educational level, earnings, weight), X_i is a vector of fixed child and parental characteristics and $MWyrs_i$ is a measure of mother's labor force participation during childhood. There are many reasons why a child's long-term outcomes may be correlated with the mother's labor force participation during childhood. For instance, if mothers who work have (on average) greater human capital than mothers who do not, then the inheritability of ability would lead us to expect a positive correlation between mother's work and child outcome, independent of any causal relationship. As such, the coefficient γ in equation 1 is likely biased by unobserved characteristics of the mother that affect child outcomes and are correlated with mother's labor force participation.

We control for time-invariant maternal, paternal and family characteristics by exploiting the differential variation in exposure to a working mother that exists across older and younger siblings in different family types. To implement our strategy, we define two types of families as follows:

1. Never Working (NW): Families with a mother who, for at least two of her children, does not work full time when her child is less than 19 years old. From these families, we drop any children who were less than 19 years old when their mother enters full-time work.

2. Enters Work (EW): Families with a mother who does not work full time between the birth of her first child and the birth of her youngest child, but enters full-time employment by the time her youngest child turns 16 years, and then remains fully employed at least until her youngest child is 16 years

old. From these families we drop children who were older than 18 when their mother enters full-time work.

Group one (NW) is the comparison group. The age difference across siblings in these families has no relationship with differences in exposure to a working mother. Group two (EW) is the treated group, in the sense that there is systematic variation in exposure to a working mom across older/younger siblings, determined by their difference in age.

If longer exposure to a working mother adversely affects child outcomes, the difference in outcomes across older and younger siblings should favor the older sibling more so in the EW families than in the NW families. As the existence of birth order effects is well-established, with earlier born siblings generally doing better than later-born ones (Black et al., 2011), we could also describe our hypothesis in terms of birth order gradients. Those gradients should be steeper among EW families if longer exposure to a working mother is harmful to children.

We implement this empirical strategy by estimating the following model for the outcome of child i in family s:

$$Y_{i,s} = \beta_s + \beta X_i + \rho RA_i + \gamma EW_s RA_i + e_i, \tag{2}$$

where β_s captures family fixed effects, X_i captures observed individual-level characteristics that vary across siblings (birth order interacted with family size, birth cohort interacted with gender, twin status, etc.), EW_s is an indicator of family type (defined above) and RA_i captures the age of child i relative to the mean age of his/her siblings (relative age). Note that the direct effects of family type are subsumed by the inclusion of family fixed effects, while the inclusion of family fixed effects leads to exact colinearity between relative age and mother's age at birth.[3] We are primarily interested in the coefficient γ , which captures the differential effect of relative age in EW families compared with NW families. If maternal labor force participation during childhood adversely affects child outcomes, then we should see that $\gamma > 0$, i.e. there should be a differential advantage of being an older sibling in EW families compared with NW families.

[3] In our estimated models, we include quadratic controls for mother's age at birth, and therefore omit explicit controls for relative age.

The crucial identifying assumption is that the relationship between relative age and child outcomes would be identical across EW and NW families if not for the differential exposure to a working mother that exists in EW families. This assumption is potentially undermined by the fact that family type is not assigned exogenously.

There are at least three situations that could challenge our identifying assumption. First, there could be a selection into EW based on the characteristics of the youngest child relative to the characteristics of the older siblings.

For example, a mother may be more inclined to enter work if her younger children are "performing well" relative to her older children. If so, the coefficient for the interaction between EW and relative age would give a downward biased estimate for the effect of differential exposure to a working mother.

Second, an event could affect selection into EW, which could bias the analysis if the event affects older and younger siblings differently. For example, a marital conflict could be predictive of the mother's decision to enter the labor market. This could be problematic for our identifying assumption because marital conflict could affect siblings differently since they experience differential exposure to divorced parents during their childhood. Third, there could be a selection into EW based on parental characteristics. For example, it could be that more resourceful mothers are more likely to enter the labor force. This selection could bias our estimates if birth order effects (e.g. Black et al., 2011) differ across various family types. Our rich data permit us to investigate the plausibility of our identifying assumption by including interactions between relative age with characteristics predictive of family type, child ability and events. Moreover, we run placebo analyses to test whether the differential effect of relative age in EW families can be explained by differences in mothers' propensities for work.

3 Data

Our empirical analysis utilizes several registry databases provided by Statistics Norway. We have a rich longitudinal data set containing records for every Norwegian from 1970 to 2007. The variables captured in this dataset include individual demographic information (sex, age, marital status, number of children) and socioeconomic data (years of education, earnings). Importantly, the dataset includes personal identifiers for one's parents, allowing us to link children to their parents and siblings.

We focus our analysis on the 1970-1980 birth cohorts in order to ensure availability of outcome measures when the child reaches the age of 27. These cohorts amount to 590,312 native-born children who can be matched to both biological parents. In order to assure clean covariates for birth order and parity, we exclude 62,326 children whose mother had children by more than one man. Another 73,690 children are dropped to avoid capturing unusual living arrangements: We drop children who did not live in the same municipality as the father and the mother in the time period when the child is between 5 and 16 years of age. To focus on differential exposure to maternal employment we exclude an additional 1,495 children whose mother or father died before the child reached 16 years of age.

We drop 157,046 children of families where the mother enters and/or exits full-time employment in a pattern that is inconsistent with the "family type" definitions (NW and EW) described in Section 2. To facilitate the utilization of family fixed effects, we also drop 121,106 children who do not have any siblings represented in the sample. Finally, avoiding issues arising from differential exposure to divorced parents require us to drop from the sample 6,877 children of families where the mother is not married to the father when the youngest child is 16. These restrictions give us a sample of 165,957 children in 77,581 families. About 38 percent of these families are EW types.

Our main outcome of interest is the child's years of education at age 27. As secondary outcomes, we also investigate high-school completion rates ($\geq$12 years of education), college attendance rates ($\geq$15 years of education) and log earnings at age 29. For boys, we also estimate effects for IQ score, height and body mass index (BMI) available (at age 19) from military records, which facilitates additional placebo tests.

Our key explanatory variable is an interaction term between relative age and an indicator for EW status (with NW status as the omitted group) as defined in Section 2. Relative age refers to the child's age relative to the mean age for all included siblings from a given family. For measuring EW status we utilize information on annual earnings[4] to approximate full-time employment, as the data do not cover

[4]Annual earnings include wage, earnings from self-employment and work-related transfers, such as sickness benefits, parental leave benefits, disability pensions and unemployment benefits.

work hours for the relevant time period. We follow previous studies (See Havnes and Mogstad, 2011a,b) by referring to an individual as employed full time in a given year if he/she earns more than four "basic amounts" in that year. The "basic amount" is defined by the Norwegian Social Insurance Scheme.[5] In 2007, one "basic amount" corresponded to 72,000 NOK measured in 2009 prices (approximately 12,200 USD). Mean and median earnings (of persons with earnings) in our sample are 336,652 and 347,910 NOK, respectively, as measured in 2009 prices.

Our rich dataset allow us to construct several variables for capturing important child and family characteristics. Unless otherwise stated, we include the following set of control variables in all models: family fixed effects, indicators for birth cohort, child gender and gender/cohort interactions; indicators for birth order and birth order/family size interactions; an indicator for twin/triplet births; and quadratic covariates for mother's age-at-child's birth. Additional controls included to evaluate the robustness of our estimates will be described during presentation of our robustness results.

Table 1 presents summary statistics for key variables of interest. We separately report the means and standard deviations for the NW and EW families. Comparing children in the EW families with the NW families, we can see that educational attainments are higher for children from families where the mother enters work. We also see that in families where the mother enters work, parents' education is higher, family earnings are higher, families are smaller and IQs are higher. The large differences in parental and family characteristics strongly indicate that a mother's decision to enter or exit the labor market is not a random event. Notably, however, with the inclusion of family fixed effects, this will only bias our estimates if birth order effects (or, more precisely, if relative age effects) differ across various family types, which we will carefully investigate.

[5]The "basic amount" is used by the Norwegian Social Insurance Scheme to determine eligibility for and magnitude of benefits like old age pension, disability pension and unemployment compensation. The "basic amount" is adjusted annually by the Norwegian Parliament to account for inflation and general wage growth.

4 Empirical Results

4.1 Descriptive Analysis

Table 2 presents estimates for the partial correlation between a child's educational attainment and mother's employment during that child's childhood, employing equation (1) as our estimation model, with MWyrs measured as years of full-time maternal employment over child ages 1-18. For comparison purposes, we initially produce estimates over an unrestricted sample of Norwegian children born in 1970-1980 before focusing on our main analytic sample.[6] Conditional on the individual-level covariates described above, column 1 demonstrates a strong correlation between maternal employment in childhood and child outcomes. Each additional year of maternal employment is predictive of a 0.056 increase in child's years of education, suggesting a sizable, positive effect on child outcomes. However, in column 2 we see that the size of partial correlation can largely be explained by differences in other family characteristics which are correlated with MWyrs. In column 2, when we add controls for parental characteristics and family size, the coefficient on MWyrs decreases to 0.015. We generate similar results when we restrict the sample to children in NW and EW families (columns 3 and 4). Thus, the finding that mother's employment is positively correlated with child outcomes holds true in our analytic sample to a similar degree as it does in the broader population. Moreover, in our analytic sample, we again see that the apparent relationship between mother's employment and child outcomes is very sensitive to the inclusion of controls for family characteristics (see column 4).

The sensitivity of the MWyrs coefficient to inclusion of the family characteristics raises a strong concern about the role that family-level unobservables might play in biasing our results. Columns 5 and 6 demonstrate the legitimacy of such concerns. In column 5, we augment our family-level covariates with indicators for the age of the mother's youngest child when the mother returns to full-time work. The omitted category refers to NW families. The small, positive coefficient we had estimated for MWyrs switches signs with the inclusion of these additional controls. In column 6, when family fixed effects are controlled for, the estimated

[6] For the unrestricted sample in Table 2, we relax two of the sample restrictions defined in Section 3. First, we relax the restriction to children of families where the mother enters and/or exits full-time employment in a pattern that is inconsistent with the EW and NW family types. Second, we do not restrict the sample to children who have siblings represented in the sample.

coefficient on MWyrs becomes slightly more negative. Thus, as richer controls for parental characteristics are included, the correlation between mother's employment and child outcomes becomes increasingly negative. However, the estimated effect remains quite modest in size. From column 6, a one-year increase in exposure to a working mother is associated with a 0.025 increase in child education years, a small effect relative to the standard deviation in education outcomes (2.1 years).

4.2 Main Results

Table 3 presents our main results, employing equation (2) as our estimation model. All models in this table include family fixed effects, as well as indicators for birth order and birth order/family size interactions, an indicator for twin/triplet births, indicators for birth cohort and gender/birth cohort interactions and linear and quadratic terms for mother's age at child birth. With the inclusion of family fixed effects, our covariate of interest, namely *Relative age*EW*, is (almost) perfectly collinear with the *AWyrs* covariate utilized in Table 2.[7] Thus, it is unsurprising that the coefficient on *Relative age*EW* in column 1 of Table 3 is nearly identical in magnitude to the coefficient on *AWyrs* in model 6 of Table 2, but it takes the opposite sign. The coefficient on Relative age*EW can be interpreted as evidence for the causal effect of differential exposure to a working mother, under the assumption that the "relative age gradient" in outcomes for children in EW and NW families would have been equal if not for the differential exposure. The estimate in column 1 could be biased, however, if the size of the relative age gradient differs systematically across various sorts of families. Columns 2-4 investigate this by focusing on the systematic differences we documented across EW and NW families (see Table 1).

In column 2, we include additional covariates by interacting parents' education levels with child's relative age. Consistent with results reported in Kalil et al. (2012a), we find that the relative age gradient in child outcomes tends to be steeper in families with higher parental education. As a result, our coefficient of interest decreases somewhat in magnitude (to 0.013) in column 2. When we allow relative age to exhibit heterogeneous effects along other predetermined individual or fam-

[7]Conditional on family fixed effects, *AWyrs* and *Relative Age*AW* are not perfectly collinear because *AWyrs* is measured in discreet units of years (*Awyrs*=$\{0, 1, 2, \ldots , 18\}$), while relative age is a continuous variable. If not for this difference, Table 2/model 6 and Table 3/model 1 would have produced estimates identical in magnitude.

ily characteristics, this has virtually no effect on our estimate (see column 3). In column 4, we include an additional covariate by interacting father's mean earnings (measured over the years when the youngest child is 1-16 years old) with relative age. This provides another robustness check for whether our estimate is biased by the existence of steeper relative age gradients in families with higher socioeconomic status, but again it has no effect on our coefficient of interest.[8] As a father's earnings are likely endogenous with his wife's decision to work, we utilize the model represented in column 3 of Table 3 as our preferred specification.

Our preferred specification implies that each additional year of maternal full-time employment during one's childhood is associated with a 0.013 decrease in years of education. Extrapolating from this result, we would predict that 5 additional years of full-time employment by one's mother reduces a child's education by 0.065 years, which amounts to 4 percent of a standard deviation in our sample. While such extrapolations have to be interpreted with caution, this supports the notion that the mother's labor force participation has a statistically significant, but quite small negative effect on children's long-term educational attainment.

4.3 Robustness

The results in Table 3 allow us to be fairly confident that our estimate is not contaminated by fixed differences across families, which lead to differential relative age gradients across EW and NW families. However, as discussed in Section 2, our identifying assumption could also be violated if the selection of mothers who enter the labor force is affected by family events that affect younger and older siblings differently. Two particular concerns in this regard are father's job loss and marital conflict, as both are known to be detrimental to children (see, e.g., Oreopoulos et al., 2008; Amato, 2001; Rege et al., 2011) and could influence a mother's decision to work. We address these issues in Table 4, with column 1 replicating the result from our preferred specification for comparison.

Column 2 investigates potential bias arising from paternal job loss. In the absence of an explicit measure for paternal job loss, we construct a proxy for job loss (*DropIncomeF*) based on whether the father experienced a large drop in income

[8] We have also interacted relative age with family income and fathers earnings at the time each of his children is between 1-16 years. None of these inclusions affect our coefficient of interest.

after the birth of his youngest child.[9] We then include as an additional covariate the interaction between this indicator and relative age. We can see that the coefficient on this additional covariate is insignificant and our coefficient of interest is unaffected.

In columns 3 and 4 we investigate potential bias arising from marital conflict. Again, we have no direct measure of marital conflict. So, to investigate the role that marital conflict could play, we modify our sample to include families with divorced parents. Column 3 reveals that our estimate is only slightly larger in this broader sample (0.015). When we interact divorce with relative age, the coefficient on this interaction term is near zero and our coefficient of interest in unchanged (see column 4). This suggests that our preferred estimate is unlikely to be biased by unobserved variation in marital conflict across families.

As a final test of potential bias, column 5 investigates the possibility that the magnitude of the relative age gradients in outcomes varies systematically with unobserved propensities for maternal employment. We do so by dividing the NW sample into three subgroups based on the number of years the mother spent in full-time employment over the years her youngest child is aged 20-35 (i.e. beyond the age where we would expect differential exposure to a working mother to influence child outcome). In particular, *NW1* identifies NW families where the mother had 6-16 years of employment over this period, and *NW2* identifies NW families where the mother had 1-6 years of employment.[10] We then interact these indicators with relative age and include them as controls in column 5. A finding that NW families exhibit steeper relative age gradients when the mother had greater post-childhood employment would present a serious challenge for the causal interpretation of our model, since this would suggest that the gradient varies positively with mothers' propensity for work. However, we find no evidence of this. The relative age gradients in NW families appear unrelated to variation in (post-childhood) maternal employment.

[9] The data do not cover job loss for the relevant time period. Hence we utilize information on annual earnings to approximate paternal job loss. Job loss is an indicator variable taking the value 1 if the father experienced at least a 20 percent drop in earnings (compared to the mean of father earnings in the five years before the youngest child is born) in any of the years after the youngest child is born and until the youngest child is 16 years of age.

[10] A third omitted category of NW families are those where the mother had no years of full-time employment over the years her youngest child was 20-35 years old.

4.4 Alternative Outcome Measures

In Table 5 we estimate the effect of maternal employment on other outcomes of interest: high-school completion rates, college attendance rates and log earnings. For each, we report estimates under our preferred specification (Table 3, column 3). For both high-school completion and college attendance, we estimate effects that are statistically significant but small in magnitude, in line with our finding for years of education. For instance, the result in column 1 implies that 5 additional years of full-time employment by one's mother reduces the probability of the child completing high school by 1.5 percentage points, while reducing the probability of the child attending college by 2 percentage points (see column 2). The estimated effect of maternal employment on earning is somewhat larger, implying a 2.5 percentage point decrease in earnings from 5 additional years of full-time employment for one's mother.

4.5 Subsample Analysis, by Mother's Education

In Table 6 we explore whether these estimated effects differ based on the educational level of the mother. One motivation for this analysis is that mothers might differ in their ability to provide a rich and stimulating environment for their children. For example, studies suggest that highly educated parents produce more cognitively stimulating home learning environments and more verbal and supportive teaching styles (Harris et al., 1999). Moreover, highly educated parents spend more time in activities believed to be more "developmentally effective" (Kalil et al., 2012b). If so, the detrimental effect of maternal employment on children's outcomes might be expected to be larger for the children of more educated mothers. On the other hand, highly educated mothers, versus those with less education, may be able to secure more effective and stimulating child care when they work, leaving us without a clear prediction.

Each cell in Table 6 presents the coefficient on the *Relative age*EW* covariate under different regressions conducted separately for children of mothers with low and high education. The outcome variable in these regressions is presented in the left-hand column of the table. The estimated effect on years of education and on log earnings is somewhat larger in magnitude for children of less educated mothers, although the differences are not statistically significant. The effect on high-school completion rates appear somewhat larger for children of less educated mothers, while the opposite is true for college attendance. This likely reflects differences in the underlying distribution of educational outcomes in the two subsamples. Regardless, only for the high-school completion outcome do we estimate a significantly different result for the two subsamples. For the children of less educated mothers, this estimate suggests that 5 additional years of maternal employment is predictive of a 3 percentage point decrease in the likelihood of completing high school.

4.6 Subsample, Gender

As suggested, previous maternal employment may provide a positive role model of a working mother, which may especially affect the aspirations of her daughter(s). Hence, we might expect the benefits of having a stay-at-home mother to be more muted (or even possibly negative) for girls. Alternatively, several studies suggest that girls benefit more from early child care than boys (see e.g. Melhuish et al., 2008). If the alternative care arrangement is formal child care, then the positive effect of having a stay-at-home mother could be smaller for girls than for boys.

Table 7 explores the existence of differential effects across boys and girls. Our first set of estimates (model 1) reflect models where gender-specific estimates are produced for the *Relative age*EW* covariates. This model also includes gender-specific controls for all the observed individual-level characteristics that vary across siblings X_i (see Section 2) as well as parental education, but it maintains the assumption that unobserved family characteristics have similar effects for boys and girls in a given family. The estimated effect on years of education and college attendance appears a bit larger for boys, although the difference is not statistically significant. However, the effect of maternal employment appears to be more detrimental to the subsequent earnings of girls than boys. The coefficient for girls implies that 5 additional years of maternal employment is predictive of a 4 percentage point decrease in the log earnings of girls.

The second set of results in Table 7 (model 2) estimates our preferred specification,restricting our sample to boys and to families where at least two boys are present. With respect to the educational and earnings outcomes, the subsample of boys produces generally smaller estimates than those derived in the pooled model.

As mentioned earlier, our estimates for the effect of maternal employment could be biased if maternal employment decisions are affected by unobservables predictive of differential outcomes for older versus younger siblings. For instance, if a mother is more likely to return to work when the prospects of success for her younger children are (relatively) better, this would mute the estimate detriment associated with maternal employment. Limiting our sample to boys allows us to test outcomes related to this hypothesis, using measures of IQ and height available from Norwegian military records. Both IQ and height are strong predictors of subsequent economic outcomes (Cawley et al., 2001; Case and Paxson, 2008). If the selection process described above was operational, we should expect positive coefficients on *Relative age*EW* for the IQ and height outcomes. Instead, we estimate small and insignificant coefficients (of opposite sign) for these outcomes. For completeness, we also estimate a model employing BMI as our outcome, and again estimate a small, insignificant coefficient on our covariate of interest. While these results are only specifically applicable to part of our analytic sample, they do not provide any evidence of bias arising from endogenous employment choices of mothers.

4.7 Nonlinear Effects

In Table 8 we explore potential non-linearities in the effect of mother's labor force participation. Specifically, we explore whether differential exposure to a working mother matters more at younger child ages, as the literature on early childhood development suggests (Carneiro and Heckman, 2003; Heckman, 2006). To investigate this, we interact relative age with indicators for three subgroups of EW families, defined based on age of the youngest child when the mother enters full-time employment. Estimates are reported for each of the four outcomes (as Panels A-D), with estimates under our preferred specification reported in column 1 and gender-specific estimates reported in columns 2 and 3 (as in Table 7, model 1).

Notably, these estimates need to be interpreted with caution, as the timing of the mother's work entry decision is likely not random and is potentially affected by the development and maturity of the youngest child. If so, our effect estimates will be biased downwards for families in which the mother enters the labor force early and upwards for families in which the mother enters the labor force late.

Perhaps because of this, we find that estimates are generally no larger for EW mothers who enter work at an earlier stage in the life of their youngest child. In fact, for boys it appears that maternal employment is perhaps most detrimental to educational outcomes during mid-adolescence rather than at earlier ages, although the same does not appear true for earnings. Interestingly, for girls there is some indication that the detrimental effect of maternal employment (on college attendance and earnings) is larger at earlier ages. Thus, while we refrain from drawing strong conclusions from this analysis regarding the effect of maternal employment at different ages, our results do appear consistent with the existing childhood development literature in one respect: Girls, as compared to boys, appear to be more influenced by differences in the early childhood environment (Melhuish et al., 2008).

5 Conclusion

Understanding how maternal work affects child development is important for countries considering policies that either encourage or discourage maternal employment. The Scandinavian countries, for example, provide paid parental leave with job protection and high-quality, publicly subsidized day care as ways to encourage females to maintain a close attachment to the labor force while their children are young. Many other OECD countries are now adopting similar policies, as increased female labor force participation is considered important for maintaining economic growth and sustainable pension systems in an aging population (Burniaux et al., 2003).

This paper seeks to strengthen this understanding by analyzing how child development is affected by working mothers. The analysis exploits the variation in exposure to a working mother that exists across older and younger siblings in different family types. We compare sibling differences in families where the mother enters the labor force when the children are older and where the mother remains fully employed thereafter, to sibling differences in families where the mother remains out of the workforce during the entirety of her children's adolescent years. The analysis suggests negative and significant effects of maternal labor force participation on years of education and labor market outcomes. However, the effects are small, which supports the notion that maternal labor force participation has, on average, a small effect on children's long-term outcomes.

Our small estimated effects of maternal labor force participation on long-term child outcomes differ from those found by Bettinger et al. (2013), who investigate a causal relationship between maternal labor force participation at ages 7 to 11 and the grade point average (GPA) in tenth grade. For identification the study utilizes a family reform in Norway which increased parents' incentives to stay home with their children, in an instrumental variable (IV) approach. The IV results demonstrate that maternal labor force participation has a very large, negative effect on tenth grade GPA, which contrasts with the small, negative effect demonstrated in the present study. One plausible explanation for this is that the current paper estimates an average effect of an increase in maternal labor force participation, whereas Bettinger et al. (2013) estimate a local average treatment effect (LATE) (Imbens and Angrist, 1994). It may be that parents who expect to see the largest gains are the ones most likely to change their behavior in response to the family reform. For example, if a child was struggling in school, the family reform may have presented an opportunity for a parent to stay at home and help the student. Norway's educational system is characterized by short school days and extensive homework assignments and an after-school care program with little scholastic focus, so opportunities for helping a child with homework are significant.

Even if not conclusive, the present study indicates that mother's labor force participation has a small effect on the long-term outcome of the average child. The small estimates are striking, as alternative care at the time of our study was informal child care or unsupervised time at home, alongside the rather short supply of public day care for the younger children. However, even if maternal employment has a small effect on the average child, it is important to recognize that for some children parental care is not easily substituted (Bettinger et al., 2013). As such, in a world with historically high and still increasing female labor force participation, policies that provide high-quality care options for school children during parents' working hours could be positive for child development.

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Table 1: Summary statistics

	NW		EW		p-value
Education years	13.40	(2.133)	13.96	(2.153)	0.000
Completed high school	0.764	(0.425)	0.837	(0.370)	0.000
College attendance	0.284	(0.451)	0.390	(0.488)	0.000
Earnings	338.6	(166.8)	358.4	(173.0)	0.000
Parental characteristic					
Father, education years	11.38	(2.080)	12.14	(2.403)	0.000
Mother, education years	10.68	(1.382)	11.74	(1.948)	0.000
Father, earnings (Child's age 1-16)	426.8	(148.6)	456.3	(147.2)	0.000
Mother, earnings (Child's age 1-16)	145.5	(37.82)	272.5	(64.88)	0.000
Father, age at birth	28.78	(5.530)	28.35	(4.825)	0.000
Mother, age at birth	25.94	(4.742)	25.74	(4.196)	0.000
Mother, age at birth (1 borne)					
Mother, birth cohort (year)	1948.5	(4.868)	1949.2	(4.374)	0.000
Child characteristics					
Birth cohort (year)	1974.4	(2.965)	1974.9	(2.919)	0.000
Birth order	2.008	(1.046)	1.866	(0.864)	0.000
Family size	3.049	(1.062)	2.669	(0.840)	0.000
IQ	5.044	(1.733)	5.493	(1.735)	0.000
Height in cm	179.6	(6.654)	180.1	(6.634)	0.000
Body mass index	22.40	(3.176)	22.37	(2.998)	0.142
Sample size					
N families	48,313		29,268		
N children	107,730		58,227		

Notes: Standard deviation in parentheses for mean statistics. Mother's and father's earnings reflect mean earnings from the period when the child is 1-16 years of age, measured in NOK (2009)/1000. Family size reflects number of children in the family. Sample size varies for IQ and height due to missing observations; sample counts of boys with non-missing values for IQ and height are 53,005 for NW and 28,730 for EW.

Table 2: The child's education and mother's employment status

	(1)	(2)	(3)	(4)	(5)	(6)
MWyrs	0.056**	0.015**	0.064**	0.020**	-0.018**	-0.025**
	(0.0007)	(0.0008)	(0.0015)	(0.0015)	(0.0049)	(0.0051)
Additional covars:						
Individual characteristics	Y	Y	Y	Y	Y	Y
Family characteristics		Y		Y	Y	
Indicator for EWay					Y	
Family fixed effects						Y
Sample restricted to:	All	All	EW/NW	EW/NW	EW/NW	EW/NW
R_squared2	0.077	0.153	0.075	0.152	0.152	0.375
Observations	428,197	428,197	165,957	165,957	165,957	165,957

Notes: OLS regressions with robust standard errors in parentheses. Dependent variable is years of education at child age 27. *Individual characteristics:* Indicator covariates for birth year and male/birth cohort interactions, birth order indicator covariates representing (2, 3, 4, 5, 6+) and birth order family size interactions, indicator for children born in twin or triple births and linear covariates for relative age, the father's and mother's age at birth. *Family characteristics:* Mother's and father's education; years of education ($<$10, 10–11, 12–15, $\geq$16) in 4x2 categories, family size indicator covariates representing (2, 3, 4, 5, 6+) and "Mothers age at birth of first born" (linear). For the unrestricted sample (all), we relax two of the sample restrictions defined in Section 3. First, we drop the restriction to children of families where the mother enters and/or exits full-time employment in a pattern that is inconsistent with the EW and NW family types. EW/NW consist of our main analytic sample defined in Section 3. Second, we do not restrict the sample to children who have siblings represented in the sample. MWyrs is a variable indicating mother's work years over child ages 1-18. EWay are 16 indicator variables for the age of the youngest child when mother enters full-time employment. The omitted category refers to NW families.

Table 3: Main results—effect on years of education

	(1)	(2)	(3)	(4)
Relative age*EW	0.024**	0.013*	0.013*	0.013*
	(0.0050)	(0.0052)	(0.0052)	(0.0052)
Additional covars:				
Parents' educ		Y	Y	Y
Birth order*relative age			Y	Y
Family size*relative age			Y	Y
M age at birth*relative age			Y	Y
M age at birthfirst*relative age			Y	Y
F earnings*relative age				Y
R_squared2	0.375	0.376	0.376	0.376
Observations	165,957	165,957	165,957	165,957

Notes: OLS regressions with robust standard errors in parentheses. Dependent variable is years of education at child age 27. Relative age refers to the child's age relative to the mean for their included siblings. All models include family fixed effects, indicators for birth order and birth order/family size interactions, an indicator for twin/triplets, indicators for birth cohorts and male/birth cohort interactions linear and quadratic terms for mother's age at child birth. In columns 2-4 we include additional controls for relative age interacted with the additional covars mentioned in the table. In particular, parents' education represents mothers education and fathers education; years of education (<10, 10–11, 12–15, $\geq$16) in 4x2 categories. Birth order and family size are indicator covariates representing (2, 3, 4, 5, 6+). "mother's age at birth" and "mother's age at birth of first born" are linear terms. "Father's earnings" is the log of father's mean earnings from the period when the youngest child is 1-16 years of age. MWyrs is a variable indicating mother's work years over child ages 1-16.

Table 4: Robustness

	(1)	(2)	(3)	(4)	(5)
Relative age*EW	0.013*	0.013*	0.015**	0.015**	0.014*
	(0.0052)	(0.0052)	(0.0051)	(0.0051)	(0.0055)
Relative age*DropIncomeF		-0.007			
		(0.0048)			
Relative age*Divorce				0.001	
				(0.0121)	
Relative age*NW1					0.001
					(0.0095)
Relative age*NW2					0.001
					(0.0075)
Observations	165,957	165,957	172,834	172,834	165,957

Notes: OLS regressions with robust standard errors in parentheses. Dependent variable is years of education at child age 27. All models are identical to model 3 in Table 3, but column 2 includes an additional interaction between relative age and divorce, and column 4 includes an additional interaction between relative age and an indicator variable that takes the value 1 if the father experiences a large drop in income. Divorce is an indicator variable taking the value 1 if the parents are not married or cohabiting when the youngest child is 16 years of age. DropIncomeF is an indicator variable for fathers experiencing a large drop in income after his youngest child is born. The sample in column 3-4 is restricted to families where parents are married or cohabiting when the youngest child is 16 years of age. In column 5 we additionally includes indicator variables for "returning to work" (NW1, NW2), which are defined as having a mother that is full-time employed for 6-16 and 1-5 years, respectively, when her youngest child is between 20-35, but not employed when her youngest is between 1-16 years.

Table 5: Alternative outcomes

	(1) Completed H	(2) CollegeAt	(3) Log inc 29
Relative age*EW	0.003**	0.004**	0.005**
	(0.0010)	(0.0012)	(0.0015)
Observations	165,957	165,957	165,957

Notes: OLS regressions with robust standard errors in parentheses. All models are identical to model 3 in Table 3, but with different outcome variables. Completed high school (Completed H) is an indicator variable for the child having at least 13 years of education. College attendance (College At) is an indicator variable for having at least 15 years of education. Log income (log inc 29) refers to log of annual income at age 29.

Table 6: Subsample, mothers education

	(1)	(3)
Sample restriction, mothers education	<11 years	≥11 years
*Relative age*EW*		
Outcome variables:		
Education years	0.018+	0.011+
	(0.0102)	(0.0061)
Completed H	0.006*	0.002+
	(0.0024)	(0.0011)
College attendance	0.002	0.004**
	(0.0021)	(0.0015)
Log income at age 29	0.007*	0.005*
	(0.0029)	(0.0018)
N	51,012	114,945

Notes: OLS regressions with robust standard errors in parentheses. Each cell presents the coefficient for the interaction between relative age and EW from different regressions. All models are identical to model 3 in Table 3, but with different outcome variables.

Table 7: Subsample gender

	(1)		(2)
Sample restricted to:	Pooled sample		Boys subsample
*Relative age*EW:*	Girl coeffs	Boy coeffs	
Outcome variables:			
Education years	0.010	0.016+	0.004
	(0.0090)	(0.0084)	(0.0096)
Completed H	0.003+	0.003+	0.002
	(0.0017)	(0.0017)	(0.0020)
College attendance	0.002	0.005*	0.001
	(0.0021)	(0.0019)	(0.0022)
Log income at age 29	0.008**	0.002	0.001
	(0.0025)	(0.0025)	(0.0029)
IQ			0.007
			(0.0082)
Height			-0.007
			(0.0263)
BMI			0.004
			(0.0153)
N	165,957		49,551

Notes: OLS regressions, with robust standard errors in parentheses. Each cell presents the coefficient for the interaction between relative age and EW from different regressions. All models are identical to model 3 Table 3, but with different outcome variables, and in the pooled model we include gender specific controls for all the observed individual-level characteristics that vary across siblings (X_i) (see Section 2) as well as parental education. Sample size varies for IQ, height and BMI due to missing observations; sample counts of boys with non-missing values for IQ is 47,469.

Table 8: Nonlinear effects on education and earnings

	(1)	(2)	(3)
		Pooled sample	
	All	Girl coeffs	Boy coeffs
Panel A: Years of education			
Relative age*EW age 0-9	0.009	0.012	0.005
	(0.0076)	(0.0129)	(0.0120)
Relative age*EW age 10-12	0.020**	0.011	0.030*
	(0.0075)	(0.0127)	(0.0120)
Relative age*EW age 13-16	0.010	0.009	0.011
	(0.0091)	(0.0149)	(0.0141)
Panel B: Completed high school			
Relative age*EW age 0-9	0.003*	0.003	0.002
	(0.0014)	(0.0024)	(0.0023)
Relative age*EW age 10-12	0.004**	0.004+	0.004+
	(0.0015)	(0.0025)	(0.0025)
Relative age*EW age 13-16	0.002	0.002	0.003
	(0.0018)	(0.0029)	(0.0028)
Panel C: College attendance			
Relative age*EW age 0-9	0.003+	0.005+	0.001
	(0.0018)	(0.0031)	(0.0028)
Relative age*EW age 10-12	0.004*	0.001	0.008**
	(0.0018)	(0.0030)	(0.0028)
Relative age*EW age 13-16	0.002	0.000	0.004
	(0.0022)	(0.0035)	(0.0033)
Panel D: Log income at age 29			
Relative age*EW age 0-9	0.006**	0.012**	0.002
	(0.0023)	(0.0037)	(0.0036)
Relative age*EW age 10-12	0.004+	0.005	0.002
	(0.0023)	(0.0036)	(0.0037)
Relative age*EW age 13-16	0.006*	0.008*	0.003
	(0.0027)	(0.0043)	(0.0042)
Observations	165,957	165,957	165,957

Notes: OLS regressions with robust standard errors in parentheses. All models are identical to model 3 in Table 3, but with different outcome variables. EW category variables are constructed based on the age of the youngest child when the mother enters work