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MATERNAL EMPLOYMENT DURING PREGNANCY AND BIRTH OUTCOMES - EVIDENCE FROM DANISH SIBLINGS

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Abstract

I use Danish survey and administrative data to examine the impact of maternal employment during pregnancy on birth outcomes. As healthier mothers are more likely to work and health shocks to mothers may impact employment and birth outcomes, I combine two strategies: First, I control extensively for time-varying factors that may correlate with employment and birth outcomes, such as pre-pregnancy family income and maternal occupation, pregnancy-related health shocks, maternal sick listing, and health behaviors (smoking and alcohol consumption). Second,

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to account for remaining time-invariant heterogeneity between mothers, I compare outcomes of mothers' consecutive children. Mothers who work during the first pregnancy trimester have a lower risk of preterm birth. I find no effect on the probability of having a baby of small size for gestational age (SGA). To rule out the possibility that health selection of mothers between pregnancies drives the results, I focus on mothers whose change in employment status is likely not to be driven by underlying health (unemployed mothers and students). Given generous welfare benefits and strict workplace regulations in Denmark, my findings support a residual explanation, namely, that exclusion from employment may stress mothers in countries with high-female employment rates.

keywords: maternal employment, birth outcomes

1 Introduction

Recent economic research suggests the existence of sensitive and critical periods for investment in children, with pregnancy likely to be among the critical ones (Almond and Currie, 2011; Cunha and Heckman, 2007). Given the evidence for substantial returns to health at birth for long-run outcomes such as education, labor market attachment, and health (Oreopoulos et al., 2008; Black et al., 2007; Currie et al., 2010), we need a better understanding of the effect of prenatal maternal behaviors on birth outcomes. While the economic literature has focused on a number of factors, including maternal age (Rosenzweig and Wolpin, 1995), maternal education (McCrary and Royer, 2011; Chou et al., 2007; Currie and Moretti, 2003; Lindeboom et al., 2009), family income (Case et al., 2002), maternal smoking (Abrevaya, 2006; Lien and Evans, 2005; Abrevaya and Dahl, 2008; Currie et al., 2009), and prenatal care (Rosenzweig and Schultz, 1983; Dave et al., 2008), maternal employment has not been extensively researched.

In most developed countries, the percentage of employed pregnant women has steadily increased over the decades since the 1970s. In the U.S., around 67 percent of first time mothers worked during their pregnancy in the period 2001-2003 (Johnson, 2007)—87 percent of whom worked during the last trimester and 64 percent of whom worked up to the ninth month. Knowing the effects of prenatal employment on birth outcomes is instrumental for policy makers designing policies such as prenatal maternity leave. This paper examines the effect of maternal employment during pregnancy—measured by mothers' survey report on holding a job at different stages of pregnancy—on birth outcomes.

Several mechanisms may link employment at different stages of pregnancy to birth outcomes: On the one hand, employed parents contribute to a higher family income, which in some settings may positively impact other prenatal inputs such as a healthy diet or adequate prenatal care (Lindo, 2011).[1] On the other hand, maternal exposure to hazardous workplaces may affect birth outcomes negatively (Gabbe and Turner, 1997). Finally, employment may induce stress in mothers. Consequently, this stress may be a channel for a negative effect of prenatal maternal employment on birth outcomes.[2]

Empirical evidence on the impact of maternal employment on birth outcomes is still scarce. Most existing economic research is on postnatal maternity leave and maternal employment (among many others, see Ruhm, 2000; Brooks–Gunn et al., 2002; Ruhm, 2004; Dustmann and Schönberg, 2008; Baker and Milligan, 2008; Rasmussen, 2010; Baker and Milligan, 2010; Liu and Skans, 2010; Greve, 2011; Carneiro et al., 2011).[3] Almost all studies on *prenatal* employment come from the field of occupational health, which has focused on risk factors such as the exposure to a hazardous workplace and, more recently, on stress and anxiety as channels for potential effects of prenatal maternal employment

[1]Lindo (2011) demonstrates that, in the U.S., paternal job loss impacts children's birth weight negatively. He shows that one dominant mechanism in the U.S. is higher spending on food for families with employed fathers. However, maternal employment rates also increase as a consequence of paternal job loss. Thus both an income and a maternal stress channel appear to connect paternal job loss to children's birth weight.

[2]While some studies relate general stress during pregnancy to lower birth weight and preterm birth, others have not found any correlation (Copper et al., 1996; Guendelman et al., 2009; Hedegaard et al., 1996).

[3]In economics, maternal employment has been studied primarily in a post-natal context, focusing on the effect of maternal leave taking on early life health (Ruhm, 2000) and longer-run outcomes such as educational attainment (Rasmussen, 2010; Dustmann and Schönberg, 2008; Liu and Skans, 2010; Carneiro et al., 2011). Ruhm (2000), using cross-country variation in maternity leave programs, finds a positive association of maternity leave after birth with aggregated child health measures. While Rossin (2011) also includes birth outcomes in her analysis of maternity leave enactment, she points out that postnatal leave drives her main results for the U.S. She finds a positive effect of the introduction of unpaid maternity leave on birth outcomes and infant mortality for mothers who can take advantage of maternity leave, i.e. who can afford to leave the labor market. For longer-run benefits of maternity leave, Dustmann and Schönberg (2008) and Rasmussen (2010) find no significant long-run effect of mothers' leave taking. Likewise, Liu and Skans (2010) find, on average, no effects of maternal leave taking on the school performance of children but some positive effects on this outcome for children of well-educated mothers. For Norway Carneiro et al. (2011) demonstrate significant long-run returns to maternal leave expansions for children's completed education and young-adult wages. Studies examining the effects of maternal return to employment in the child's first years of life have yielded mixed results: Some studies find negative effects of employment in the first year after childbirth on child cognitive outcomes (e.g., Brooks–Gunn et al., 2002; Ruhm, 2004), and others find little evidence for negative effects (Baker and Milligan, 2010). Highlighting the importance of context, a recent study from Denmark finds no effects of maternal employment later in the child's life for measures of child obesity (Greve, 2011).

(Saurel-Cubizolles et al., 2004; Vrijkotte et al., 2009; Henriksen and Secher, 1994).

Given that these existing studies use mainly cross-sectional data, the causal effect of employment during pregnancy is still not well understood. Factors such as mothers' health status, working hours, and job characteristics may matter crucially not only for outcomes but also for their employment decisions. Consequently, a comparison of birth outcomes across mothers is likely to be confounded by mother-specific factors that the researcher cannot observe.

Two recent economic studies address these issues: Using data on siblings, Del Bono et al. (2012) find that not working or taking work interruptions up to two months before birth has positive effects on birth weight and fetal growth. However, 60 percent of the mothers in their UK sample either are non-working during pregnancy, i.e. they study a low-female employment setting, or there is no information on their employment status. Rossin (2011) finds that the enactment of unpaid maternity leave in the U.S. improved child health at birth and infancy for college-educated and married mothers. She finds that while disadvantaged mothers cannot afford to take maternity leave, mothers who are likely to take unpaid prenatal leave experience improvements in their children's birth weight and decreases in the probability of a preterm birth.

Virtually no other economics study has examined the impact of maternal employment during pregnancy on birth outcomes. Thus, a number of questions remain unresolved, three of which this paper examines. First, I explore the impact of prenatal employment with data on pregnant women in Denmark, a high-employment and generous-leave setting.

Second, while previous studies from Denmark focus on employed women and find no differences in birth outcomes across different levels of job strain—i.e., at the intensive margin (Henriksen and Secher, 1994; Larsen et al., 2013)—I focus on women who are employed or not employed during pregnancy, i.e., the extensive margin. While most other studies that focus on this extensive margin are based on cross-sectional data and account for a limited number of control variables (Saurel-Cubizolles et al., 2004; Vrijkotte et al., 2009), I exploit data on siblings and thus deal with heterogeneity between mothers. This strategy attempts to solve the problem of the "healthy worker phenomenon" (Gabbe and Turner, 1997), i.e., the selection of women (who are on average healthier) into employment. Given that mothers may select into and out of employment due to unobserved time-varying factors such as health shocks, I factor in unusually detailed information on maternal characteristics and behaviors.

Third, I focus on birth outcomes that may be especially responsive to the recently suggested stress channel: the probability of preterm birth and the probability of infants being small for gestational age (SGA). Given that the SGA measure evaluates the infant's size relative to expected infant size at birth, it is well-suited for measuring disturbances in fetal growth.

I use combined administrative and survey data from the Danish National Birth Cohort (DNBC). Comparing outcomes of siblings, I find that women who are employed in the first pregnancy trimester (around week 12 of their pregnancy) are less likely to experience a preterm birth. At this stage of the pregnancy, it is unlikely that women or health professionals can predict the probability of preterm birth and act on this knowledge. Focusing on employment during the third trimester (around week 30), I also find employed mothers to be less likely to have a preterm birth. A larger employment effect for employment measures collected late during pregnancy may suggest some selection of mothers in poorer health out of employment during pregnancy. Moreover, I find no effect of maternal employment at either week 12 or 30 of the prengancy on the SGA measure.

While most sibling studies leave the black box of behavioral changes *between* consecutive births unaddressed, I show that mothers who change employment status between pregnancies for well-defined reasons not related to health (mothers who are either unemployed or students during one of their pregnancies) see positive employment effects for the probability of preterm birth. To rule out the possibility that unobserved health shocks to mothers drive my results, I focus on mothers with closely spaced children (for whom the likelihood of similar maternal health circumstances in both pregnancies is more plausible) and on separate samples that focus on mothers who either start or stop working between pregnancies. Both analyses confirm my main findings.

Given generous maternity leave and accommodation of employment to the need of women, as well as limited evidence for heterogeneity of the estimated effects for mothers below and above the median income in the year before birth, my findings suggest that employment may impact birth outcomes through a residual channel. The stress mechanism described for other countries may work differently in Denmark, creating stress for those mothers who are not employed during their pregnancy. The result that job loss is bad for one's health is not new: Studies on workers displacement have found a negative effect of displacement on health outcomes, while others show no effects (Browning et al., 2006; Kuhn et al., 2009). Studies that have examined the societal costs of plant closures have found important negative health effects for displaced workers, attributing this finding to stress (Sullivan and von Wachter, 2009).[4]

[4]Additionally, given recent research on peer effects in the timing of pregnancy for women at the same workplace (Hensvik and Nilsson, 2010; Ciliberto et al., 2010), peer effects for working mothers may also translate to better birth outcomes, e.g., through better timing of fertility and social support for pregnant women.

2 Data and institutional background

The DNBC comprises information on pregnancies from 1998-2003. These data have three advantages: First, as the DNBC was collected for studying the determinants of health at birth, it contains information on many maternal health behaviors and characteristics. While focusing on employment, this paper is the first to examine the effect of maternal employment, smoking, and alcohol consumption simultaneously in a sibling framework.

Second, the DNBC surveys were administered to mothers during pregnancy. Therefore, the data does not suffer from recall and justification bias, both of which constitute a considerable problem in retrospective reports (Currie, 2000). Mothers answered two surveys, around week 12 (in the following first trimester interview) and around week 30 (in the following third trimester interview) of their pregnancy. Third, given a unique personal identifier, I do not encounter problems of mismatches of mothers and children, and I can merge the DNBC to reliable control and outcome variables from administrative data.

The DNBC contains information on birth outcomes for 100,309 pregnancies. Merging survey and administrative data, and considering only live births and mothers with non-missing adminstrative data, I have a sample of 84,970 children. Accounting for attrition in the survey (between the interviews in trimesters 1 and 3) and for item non-response, I have a sample of 78,851 children for whom mothers have answered both pregnancy interviews (see appendix table A.1 for further details). Of those children, 10,331 children have at least one sibling in the DNBC survey. This "sibling sample" results from the sampling window that allowed mothers to participate with all pregnancies discovered from 1998-2003.

2.1 Outcome measures

Given established knowledge from epidemiology, the SGA measure replaces the widely used birth weight measure, which is less suited for measuring disturbance in healthy fetal development (Marsall et al., 1996). SGA infants are defined as falling in the 10th percentile of the gestational age- and sex-specific birthweight distribution. I use administrative data to construct a sex- and gestational-age specific measure of SGA as a measure for disturbed fetal growth (ibid.). I define preterm birth as birth before completed 37 weeks of gestation.

Table 1 compares outcomes for the sibling sample, the full survey sample, and all singleton births in Denmark in 1998-2003. While comparable for gestational length, the survey samples have a lower percentage of preterm birth than the overall population. Sibling sample babies are also less likely to be SGA. These differences indicate that my results may not generalize to a general population of mothers as they are based on a positively selected sample.

2.2 Maternal employment in Denmark

I use mothers' reports in the DNBC to measure maternal employment. I define employment as reporting some paid part-time or full-time job in the first or third trimester of the pregnancy, respectively.

Denmark has several institutional features relevant for the analyses of this paper: Danish employers are legally obligated to accommodate work tasks to the needs of pregnant women. This feature of the employment legislation aims at keeping pregnant women in the labor market by accommodating their needs and without endangering their health or the health of the unborn child. Thus especially in manual jobs, tasks are most likely accommodated to the pregnant woman's needs.

In the period considered in this paper, all women are entitled to four weeks of prenatal maternity leave, and close to all women are entitled to either a maternity leave benefit on the level of the unemployment benefit with a benefit cap or even their full wage (under additional collective agreements) during prenatal leave. Thus the DNBC interviews are timed for almost all women before their access to prenatal maternity leave.[5]

Another feature of the Danish labor market is paid sick leave for pregnant women. General practitioners can sick-list pregnant women, if their health status requires doing so. Women who are sick-listed (and meet an employment criteria) are eligible for a benefit, either temporary or for the rest of their pregnancy. Recent figures show that pregnant women in Denmark on average are absent from the labor market for 48 days (including the four weeks of pre-birth leave) (Danish Ministry of Employment, 2010). This number conceals differences in absence for mothers with different characteristics (ibid).

To ensure that I capture actual maternal employment and not a woman's formally holding a job but being on sick leave, I account for mother-reported sick-listing during pregnancy (sick-listing periods of three days or more at the respective interview). Table 1 shows that at both pregnancy interviews an identical percentage of mothers in both the sibling and the full survey sample report that they formally hold a job, 81 and 77 percent respectively.[6] At the interviews in the first and third trimester, about 6 and 11 percent, respectively, of employed mothers report some sick listing (of at least 3 days).[7]

[5]Not only previously employed women but also women on welfare benefits are entitled to paid prenatal maternity leave. The very few exceptions from paid prenatal maternity leave include students (who in turn receive a higher student benefit after childbirth) and women who are not entitled to any welfare benefits, i.e., home-makers or uninsured self-employed women. Those two groups are very small. Post-birth parental leave programs are also generous, granting all parents access to 14 weeks of maternity leave and up to 32 weeks of parental leave. The duration of paid postnatal leave is also subject to collective bargaining (for an overview, see, Datta Gupta et al., 2006).

[6]Table 1 shows that 88 percent of surveyed women are registered as employed in November of the year before birth. This percentage is higher than for the source population of all mothers (around 80 percent). Furthermore, the administrative data reveals that white-collar mothers (ISCO groups 1 to 4 in the year preceding birth) are overrepresented in the DNBC.

[7]Given that I lack detailed information on working hours in interview 2, I do not examine in detail the importance of working hours for birth outcomes. The average working time for working mothers

2.3 Additional analysis variables

I include a rich set of control variables. From the administrative data I use indicators for child's sex and parity (first-born), maternal age at birth and maternal age squared, mothers' educational level (in five categories and missing education), and cohabitation status. Additionally, I control for maternal occupation (or missing occupational code) in the year before the child's birth as a proxy for job characteristics, for the family's disposable income in the year before the child's birth, and for an indicator for poor maternal pregnancy-related health. The indicator summarizes a set of conditions, among them diabetes, preeclampsia, edema, and placenta praevia.[8]

From the survey, I include variables on maternal smoking status and maternal consumption of alcohol. At the first trimester inverview, around 20 and 26 percent of the mothers in the sibling and full survey sample report they have smoked during their pregnancy or are still smoking.[9] At the third trimester interview, these figures have decreased to 11 and 16 percent, respectively. To validate these survey reports, I compare survey and register data: The percentage of women reporting smoking to their midwives at birth is identical to the percentage of mothers who report smoking at the second DNBC interview (see table 1).

Alcohol consumption of survey mothers increases between the two pregnancy interviews: The percentage of mothers who drink at least one unit of alcohol per week rises from 26 percent to 33 percent for the sibling sample.[10] Mothers who drink consume on av-

in the DNBC at interview one is around 35.5 hours, somewhat lower than full-time but slightly higher than the mean reported in the National Labor Force Survey (33 hours) (Danish Ministry of Employment, 2011). This figure indicates that most women continue in their job on similar terms at least early in their pregnancy.

[8]ICD 10 codes DO11-16, DO24, DO30-48.

[9]In the 1980s 38 and 41 percent of pregnant women in two major Danish towns smoked during pregnancy (Olsen et al., 1989). The figures in the first DNBC interview is close to UK smoking figures and considerably higher than those reported for the U.S.(Del Bono et al., 2012). At the second interview, the Danish figure is closer to those reported from the U.S. (Abrevaya, 2006).

[10]One unit equals 1 bottle of beer, 1 glass of wine, or 1 glass of liquor.

erage around two units of alcohol per week. The increase in alcohol consumption between interviews may reflect women's expectations about critical periods.[11]

3 Empirical Methods

The estimation equation that describes the relationship of birth outcomes and maternal inputs—among them maternal employment—is

$$Y_{i,s} = \alpha \times EMP_{i,s,t} + \beta \times X_{i,s,t} + c_s + \epsilon_{i,s,t} \tag{1}$$

for child i in family i at interview $t = 1, 2$ where $Y_{i,s}$ is the child health outcome of interest. $EMP_{i,s,t}$ is an indicator for maternal employment. $X_{i,s,t}$ is a vector of (time-varying) observable characteristics of mothers. An estimation of equation (1) by OLS is likely biased by heterogeneity between mothers, described by c_s.[12] Thus I compare outcomes of consecutive births i and j as in Abrevaya (2006); Del Bono et al. (2012); Rosenzweig and Wolpin (1995); Currie et al. (2009).

$$(Y_{i,s} - Y_{j,s}) = \alpha \times (EMP_{i,s,t} - EMP_{j,s,t}) + \beta \times (X_{i,s,t} - X_{j,s,t}) + (\epsilon_{i,s,t} - \epsilon_{j,s,t}) \tag{2}$$

Given that identification relies on within-family variation, I face at least three remaining concerns: unobserved time-varying factors, measurement error, and dynamic parental response. First, mothers who change their health behaviors between births may also

[11]Among researchers no consensus exists about the importance of timing (see e.g., Henderson et al., 2007), although the kind of damage to the fetus may vary between trimesters (for an overview see Nilsson, 2008). Variation in national guidelines also reflects uncertainty about the impact of low but regular doses of alcohol during pregnancy (O'Brien, 2007; Nathanson et al., 2007).

[12]Prime candidates for omitted time-invariant variables are maternal preferences, abilities or underlying health.

change other unobserved behaviors—“turning healthy” or “substituting one evil with another”. Although Abrevaya (2006) finds that mothers who change smoking behavior also change other behaviors “in the same direction,” his set of health behavior variables is very restricted. I find no evidence for changes in employment during pregnancy resulting in changes of other health behaviors. As Appendix Table A.2 shows, first trimester employment status does not correlate with third trimester health behaviors in my sample (both when not accounting and accounting for a mother FE). Additionally, given that I control for a rich set of maternal behaviors, I am confident that omitted time-varying maternal behaviors are less important.

To deal with the concern that unobserved health shocks to the mothers between pregnancies drive my results, I focus my analysis on subgroups of mothers who are changing employment status for well-defined reasons (likely unrelated, e.g., to their health). I look at mothers who are registered as unemployed during one pregnancy and at mothers who change educational level between pregnancies.[13]

Second, the impact of classical measurement error is bigger in FE models than in OLS (Griliches, 1979). However, recall and justification bias can be induced when mothers systematically misreport their health behaviors. The Danish norm of the “working mother” could induce some women to over-report employment during pregnancy. If mothers misreport in consecutive pregnancies, the FE approach minimizes the impact of systematic misreporting. My finding that estimates for the effect of employment are bigger in the

[13]While a refinement of this mother FE strategy based on subgroups would be to instrument for maternal employment changes in the mother FE model (similar to Currie and Rossin-Slater (2013)), my first stage estimates using local unemployment rates during pregnancy one as instrument for mothers' changes in employment are very weak (with an F-statistic of only around 2). This weakness may be due to a combination of little regional variation in unemployment rates during 1997-2003 and the modest sample size of my sibling sample. Given that the alternative cross-sectional IV models (which use variation in unemployment across regions to instrument for maternal employment) do not account for maternal sorting due to unobservables, the mother FE estimates remain my preferred specification.

FE estimation may partly reflect mothers' over-reporting employment.[14]

Third, for the FE estimation to identify the effect of maternal employment, employment in the second observed pregnancy should not depend on the outcomes of the first pregnancy. Del Bono et al. (2012) show that their sibling-based estimates for the effect of maternal employment are very similar across specifications that account for this dynamic response. In my sample of mothers, earlier pregnancies' health outcomes do not predict mothers' employment probability in consecutive pregnancies.[15] This informal test indicates that dynamic response may not be important in this setting.

4 Results

Table 2 presents baseline OLS estimates for the probability of preterm birth and SGA. I estimate all models on the sibling sample and use measures from the first trimester interview (top panel) and the third trimester interview (bottom panel).[16] These specifications can shed light on dose-response relationships for health behaviors such as maternal smoking. Additionally, a comparison of estimates based on data from early and later measurements can give some indication of the importance of selection of mothers out of employment during pregnancy.

The narrow control model in columns 1 and 3 includes information on maternal educational level (and missing educational level), the mother's age and age squared,[17] mother's cohabitation status at birth, the family's disposable income in the year prior to birth,[18]

[14]Furthermore, the data generation process with timely interviews and the additional controls from the administrative registries makes me confident that I face less measurement error than earlier studies.

[15]Results for logistic regressions regression second-pregnancy employment on first-pregnancy characteristics and outcomes are available on request.

[16]Results based on the the full DNBC survey sample are very similar.

[17]I have tested various specifications for the age variable and find our main coefficients of interest unchanged.

[18]This variable pools the gross income of all members of the household.

child sex, whether the child is the first-born, and a set of year indicators. Furthermore, I include an indicator that is one if the mother is employed, an indicator for sick-listing, and an indicator for maternal smoking.[19] In columns 2 and 4 (extended control models) I add variables rarely available in other studies: indicators for pregnancy-related health problems that potentially lead women to stop working and introduce reversed causality to the analysis; indicators for the mother being in a white-collar occupational group in the year prior to birth or having missing information on occupation (blue-collar is the reference group); and the weekly units of alcohol the mother consumed. All columns report standard errors that are clustered at the mother level.

Table 2 shows that employed mothers have a significantly lower probability of preterm birth. The coefficient is larger in absolute size and more precisely estimated in the estimations with data from the third trimester interview. This finding may partly reflect selection of mothers out of employment between the two interviews. As expected, mothers who get sick-listed from work experience no positive employment effect—given that they leave work due to health problems these problems, are also likely to impact their children's health. Similar conclusions hold for an increased risk of adverse birth outcomes for women with pregnancy complications. The narrow and extended control models suggest a significant increase of the risk of both preterm birth and SGA for smoking mothers. Larger estimates in the regressions using measures from interview two suggest a dose-response relationship.

Given that heterogeneity between mothers is likely to bias the estimates, Table 3 presents results based on a comparisons between siblings. The table is structured like Table 2. The FE estimates for maternal employment are bigger in absolute size than the OLS estimates, i.e., the protective effect of employment is underestimated in the cross-sectional

[19] I tested models that include the number of daily cigarettes, the results remain unchanged.

model. A common finding in related studies is that OLS models overestimate the harmful the effect of behaviors such as maternal smoking. While it appears intuitive to assume a positive correlation for "high quality mothers"-unobservables with both employment and favorable birth outcomes, the changing mothers who identify the employment effect in the FE model may be the ones who benefit most from factors such as reduced stress at work. If "high quality" mothers are most stressed by being unemployed, their benefits from being at work may be underestimated in a regression that compares outcomes across mothers.[20] Furthermore, the FE models correct for the impact of potential systematic measurement error.[21]

The results in Table 3 confirm a protective effect for maternal employment at both pregnancy interviews with respect to preterm birth (i.e., a lower probability of preterm birth for employed mothers) and no impact on the probability of SGA. Similarly to the OLS results, the models using data from the first trimester interview show smaller employment effects than the models using data from interview two. The estimates indicate that mothers who are employed around week 12 of their pregnancy face a 1.8 percentage points lower probability of preterm birth. At the mean of the dependent variable this translates to a 60 percent decrease in the probability of preterm birth.

Table 3 shows that additional controls do not change the impact of the employment effect in the FE model. A pregnancy-related health condition increases the risk of of preterm birth and having a SGA child; the coefficients are similar to those in the OLS models. I find no significant impact of maternal smoking for the outcomes considered

[20] Currie et al. (2009) illustrate similarly by using pollution and residential choices that "high quality" mothers could be either more likely to move to low-pollution areas (e.g., because they have higher income and preferences for less pollution) or to move to high-pollution areas (e.g., because they value inner-city residence).

[21] For example, Smith (2009) finds bigger sibling-based estimates for the effect of early life health on later life socio-economic outcomes. He argues that systematic misreporting between individuals is smaller for siblings (who share a family environment).

here. However, for a rarely available measure of maternal behaviors during pregnancy—maternal alcohol consumption—I find in the FE models significant negative effects not apparent in the OLS results: At the mean consumption at interview two (2 units/week), mothers are around 1.6 percentage points more likely to experience a preterm birth.[22] I interpret this finding in two ways: First, given that I see an effect only for alcohol consumption on the given outcomes when using measures from interview two (and given that, on average, mothers increase alcohol consumption during pregnancy, as Table 1 shows), the estimates indicate a dose-response relationship. Second, I see no effect of alcohol consumption in the OLS models. This finding indicates that the FE models handle systematic measurement error, i.e., that mothers tend to misreport alcohol consumption.

Finally, an analysis of the effect of maternal employment across subgroups of mothers can shed light on the groups of mothers that drive the effect and on the potential mechanisms. Thus I estimate regressions both on subgroups defined by their occupational status prior to the year of their first birth (white vs. blue-collar), their educational level (above high school), and the family income prior to the year of the first observed birth (above median income for cohabiting mothers).[23] Although not precisely estimated in all regressions, subgroup estimates for the effects of maternal employment in the first trimester on the probability preterm birth are in general similar to the main results (appendix table A.3). The effects are somewhat bigger for white-collar mothers and mothers with a higher level of education (evaluated at the relevant mean prevalence of preterm birth). Limited

[22]In 99 percent of all pregnancies in the sample the mother has consumed fewer than four units of alcohol per week and in 70 percent of all pregnancies fewer than two. Excluding the mothers who have had more that ten weekly units, the alcohol result remains unchanged—an indication that it is not driven by high-consumption mothers. Estimating the models with indicators for more than one, more than two, and more than three weekly units, respectively, confirms the negative alcohol effect.

[23]As I have very few non-cohabiting mothers in my sample (and thus too few single mothers who change employment status between births), I cannot divide my sample along this dimension. Furthermore, to not confuse family composition changes and the impact of family income, I concentrate on cohabiting mothers in my subgroup analysis, which divides the sample at the median family income.

evidence for differential effects for mothers across the income distribution (if anything higher income mothers benefit more) indicates that the effect of maternal prenatal employment may not operate through the income channel. Given generous unemployment benefits, this mechanism may be less relevant in Denmark.

4.1 Robustness tests

Although I control for maternal pregnancy-related health and sick-listing, effects for maternal employment identified by a sibling comparisons may partly reflect unobserved health shocks to mothers between pregnancies. Given this concern, Table 4 isolates groups of mothers who change employment status between births for reasons most likely unrelated to underlying health. Table 4 focuses on mothers who are listed as unemployed during one of their pregnancies and on mothers who are students during one of their pregnancies, respectively.[24] Thirty-nine percent of changing mothers in my sample increase their education between pregnancies. Table 4 shows also a lower probability of preterm birth for these subgroups of changing mothers. The effects for mothers who experience unemployment support the stress mechanism outlined earlier.[25]

The larger effects for maternal employment when using measure from the third trimester interview may be due not only to attrition of working mothers out of employment but also out of the survey (i.e. not answering pregnancy interview two) due to health shocks.[26] However, employment around week 12 does not predict very preterm birth (before week

[24]Given that being registered as unemployed in Denmark means that women have to be available for the labor market, these women are not permanently excluded from the labor market, e.g. for health reasons.

[25]As suggested in section 3, maternal employment in the first trimester may also impact other maternal behaviors (maternal smoking and alcohol consumption) in the third trimester. As also detailed in section 3 regression models with these maternal behaviors in the third trimester as outcomes do not show indication for mothers' employment status in the first trimester correlating with these behaviors later in their pregnancy (appendix table A.2).

[26]In the main analysis, I use data for mothers who complete both pregnancy interviews of the DNBC.

30). Thus a higher probability of very preterm birth for working mothers does not drive the results. [27]

Similarly, to examine the importance of attrition between pregnancy interviews, Table 5 includes women with missing information on employment at interview two in the analysis. I assign all women with missing data at interview two to being either employed or non-employed, and control for maternal characteristics from interview one. Table 5 shows that assigning all missing mothers to employment shrinks the employment effect, while the opposite holds when assigning all mothers with missing employment to non-employment. This finding further indicates that some mothers change employment status during pregnancy due to underlying health. Thus the size of the employment estimates based on measures from interview 2 has to be interpreted with caution.

Table 6 shows two final informal tests that also support the main results: First, I find a smaller but consistently negative effect for maternal employment on the likelihood of preterm birth in a closely spaced sibling sample (siblings less than 3 years apart). The coefficients for other controls remain very similar.[28] Second, I present estimates that use only variation in employment from women who start or stop working, respectively (and non-changing women with respect to employment). In the sibling sample 451 women are in the "starter" group and 574 women are in the "quitter" group. Confirming my main results, the effects for starters and quitters are of opposite signs and similar size.

[27]Results of regressions of very preterm birth on maternal employment at interview one are available on request.

[28]The percentage of changing mothers (with respect to employment) is the same as in the sibling sample without the spacing condition.

5 Conclusion

As employment rates for pregnant women are increasing, our understanding of the effects of employment during pregnancy on birth outcomes is critical. While maternal employment after childbirth and during the early years of a child's life has been studied extensively, we still lack knowledge on the impact of maternal employment during pregnancy. This analysis is complicated by the lack of adequate data and unobserved differences between women, making a simple comparison between employed and non-employed women problematic.

In this paper, I examine the effects of maternal employment in the first and third trimesters of pregnancy with data from Denmark. Denmark constitutes an important case because employment rates for mothers are high by international standards both before, during, and after pregnancy. Furthermore, Danish workplaces are obligated to accommodate tasks to the needs of pregnant women, and a generous maternity leave takes pressures and stress factors off the mother's shoulders. Finally, the financially compensated sick-listing of mothers with poor health during pregnancy is an important factor in Denmark. Thus selection into and out of employment is likely to be different from countries such as the UK or the U.S.

Taking an extensive set of observables into account and comparing siblings born to the same mother, I find that employment measured in the first trimester does not harm the birth outcomes that I consider—to the contrary, employed women face a lower risk of preterm birth (an event that is most likely not predictable at this stage of pregnancy). As I control for sick leave from the job, this employment effect reflects the impact of actually being on the job. A larger coefficient for employment measured in the third trimester suggests some selection of mothers according to underlying health *during* pregnancy. However, given that the largest percentage of women remains employed until one month before their due date, and given a set of informal tests that focus on mothers who change employment for well-defined and not health-related reasons *between* pregnancies, I conclude that for the vast majority of Danish women, employment during pregnancy does not affect the considered birth outcomes negatively.

As medical care is free in Denmark and unemployment benefits are generous, the income channel may be less important in Denmark than in other settings. Accounting for family income and dividing my sample into income subgroups, I still find that being employed matters for birth outcomes. Thus alternative mechanisms may be important: Employment is often considered a stress factor for mothers, and the risk of preterm birth has been linked to maternal stress levels during pregnancy. Furthermore, recent economic research suggests that in utero stress exposure may have important long-run consequences (e.g., Aizer et al., 2012; Currie and Rossin-Slater, 2013). However, as indicated by my estimates based on mothers who change employment status between pregnancies, mothers in Denmark are potentially exposed to more stress when not employed—potentially because of anxiety over future employment prospects. My findings may also point to the importance of mothers' peers and networks. Future work should examine whether these features of the workplace or legal rules and institutions, such as workplace accommodation and generous prenatal leave, are the dominant mechanisms.

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Table 1: Summary statistics, means and standard deviations, Singleton births 1998-2003

	Survey sibling sample	*Full survey sample*	*All singletons*
No. of obs.	10331	78851	366367
Administrative data			
Birth weight	3645.661	3604.822	3538.513
	(522.524)	(534.918)	(576.451)
Preterm birth	0.030	0.035	0.049
Gestational age (weeks)	40.089	40.045	39.867
	(1.514)	(1.568)	(1.841)
Small for gest. age (SGA)	0.085	0.096	0.115
Employment, yr before birth	0.887	0.884	0.802
Occupation, yr before birth			
Manager	0.015	0.016	0.019
Professional	0.202	0.173	0.147
Technicians and associate professionals	0.320	0.298	0.256
Clerical support workers	0.198	0.205	0.202
Service and sales	0.165	0.193	0.222
Skilled agriculture, forestry, fishery	0.006	0.006	0.006
Craft and related trades workers	0.021	0.025	0.027
Plant and machine operators, assemblers	0.028	0.035	0.045
Elementary occupations	0.042	0.047	0.074
White-collar occupation	0.658	0.617	0.496
Mom smoked	0.113	0.168	0.214
Family's disposable income, yr before birth	125256.190	124217.429	119002.364
	(44758.026)	(44634.472)	(62395.099)
Pregnancy complication	0.205	0.221	0.226
Mom's age	29.556	29.932	29.518
	(3.827)	(4.272)	(4.762)
Mom college educated	0.384	0.329	0.261
Mom cohabiting	0.941	0.910	0.875
Child parity	1.773	1.730	1.842
	(0.780)	(0.827)	(0.956)
Child sex (female)	0.488	0.488	0.487
Firstborn	0.397	0.466	0.429
Survey data			
Employment, first trimester	0.813	0.806	
Sick-listed, first trimester	0.060	0.067	
Employment, third trimester	0.771	0.771	
Sick-listed, third trimester	0.103	0.119	
Mom smoked, first trimester	0.195	0.257	
Mom smoked, third trimester	0.113	0.164	
Mom cunsumed alcohol, first trimester	0.260	0.243	
Mom consumed alcohol, third trimester	0.326	0.309	
Mom excercised, third trimester	0.317	0.303	

Continued on the next page.

Table 1 *continued.*		
Number of cigs, first trimester	7.595	8.263
	(5.601)	(5.654)
N	2014	20239
Number of cigs, third trimester	8.490	8.696
	(5.845)	(5.380)
N	1168	12929
Number of glasses/week	1.734	1.828
	(1.118)	(1.282)
N	2686	19141
Number of glasses/week, third trimester	1.890	1.934
	(1.301)	(1.341)
N	3367	24385
Hours worked, first trimester	35.555	35.391
	(5.310)	(5.821)

Table 2: OLS estimates for the effect of maternal inputs on preterm birth and SGA measure, measures from trimesters 1 and 3 (around week 12 and week 30 of the pregnancy)

	Preterm birth		*SGA*	
First trimester:				
Employment, first trimester	-0.009*	-0.010*	-0.002	0.001
	(0.005)	(0.005)	(0.008)	(0.008)
Sick-listed, first trimester	0.015*	0.012	-0.011	-0.012
	(0.008)	(0.008)	(0.011)	(0.011)
White-collar occupation		-0.008		-0.002
		(0.005)		(0.008)
Missing occupation		-0.009		0.018
		(0.007)		(0.011)
Pregnancy complication		0.035***		0.049***
		(0.005)		(0.008)
Mother smoked, first trimester	0.013**	0.012**	0.042***	0.041***
	(0.005)	(0.005)	(0.009)	(0.009)
Number of glasses/week, first trimester		-0.001		0.004
		(0.002)		(0.003)
N	10330	10330	10330	10330
R^2	0.005	0.013	0.022	0.028
Third trimester:				
Employment, third trimester	-0.016***	-0.016***	0.004	0.007
	(0.005)	(0.005)	(0.008)	(0.008)
Sick-listed, third trimester	0.022***	0.020***	-0.011	-0.013
	(0.007)	(0.007)	(0.009)	(0.009)
White-collar occupation		-0.007		-0.000
		(0.005)		(0.008)
Missing occupation		-0.009		0.021*
		(0.007)		(0.011)
Pregnancy complication		0.034***		0.049***
		(0.005)		(0.008)
Mother smoked, third trimester	0.019***	0.018**	0.086***	0.085***
	(0.007)	(0.007)	(0.013)	(0.013)
Number of glasses/week, third trimester		0.001		0.002
		(0.002)		(0.003)
N	10331	10331	10331	10331
R^2	0.007	0.014	0.028	0.033

Notes: Additional controls in all models: maternal age and age squared, family's disposable income in year before birth, indicators for maternal educational level and missing education, cohabitation status at birth, child sex and whether the child is the first-born, and a set of year indicators. Clustered std. errors (mother level) in parentheses; ***significant at the 1 percent level, **significant at the 5 percent level *significant at the 10 percent level

Table 3: FE estimates for the effect of maternal inputs on preterm birth and SGA, measures from trimesters 1 and 3 (around week 12 and week 30 of the pregnancy)

	Preterm birth		*SGA*	
First trimester:				
Employment, first trimester	-0.018**	-0.018*	-0.011	-0.010
	(0.009)	(0.009)	(0.013)	(0.012)
White-collar occupation		0.015		-0.033**
		(0.009)		(0.016)
Missing occupation		0.017		0.027
		(0.012)		(0.018)
Sick-listed, first trimester	-0.009	-0.007	-0.021	-0.020
	(0.013)	(0.013)	(0.016)	(0.016)
Pregnancy complication		0.024***		0.045***
		(0.007)		(0.011)
Mother smoked, first trimester	-0.015	-0.015	0.020	0.021
	(0.011)	(0.011)	(0.016)	(0.016)
Number of glasses/week, first trimester		0.004		-0.000
		(0.004)		(0.006)
N	10330	10330	10330	10330
R^2	0.010	0.014	0.030	0.037
Third trimester:				
Employment, third trimester	-0.028***	-0.028***	0.006	0.007
	(0.008)	(0.008)	(0.012)	(0.012)
White-collar occupation		0.014		-0.032*
		(0.009)		(0.016)
Missing occupation		0.016		0.030*
		(0.012)		(0.018)
Sick-listed, third trimester	0.007	0.007	-0.007	-0.008
	(0.010)	(0.010)	(0.013)	(0.013)
Pregnancy complication		0.025***		0.044***
		(0.007)		(0.011)
Mother smoked, third trimester	0.000	-0.000	0.032	0.034
	(0.015)	(0.015)	(0.028)	(0.028)
Number of glasses/week, third trimester		0.008***		-0.008
		(0.003)		(0.005)
N	10331	10331	10331	10331
R^2	0.011	0.016	0.029	0.037

Notes: For control variables see Notes to Table 2. Clustered std. errors in parentheses; ***significant at the 1 percent level, **significant at the 5 percent level *significant at the 10 percent level

Table 4: Robustness: FE estimates for the effect of maternal inputs on preterm birth, sample of changing mothers who experience unemployment or change educational level between pregnancies

	Unemployed		*Changing education*	
	(1)	(2)	(3)	(4)
Employment, first trimester	-0.039***		-0.015	
	(0.015)		(0.018)	
Employment, third trimester		-0.052***		-0.051**
		(0.014)		(0.023)
N	1533	1431	841	781
R^2	0.037	0.052	0.060	0.083

Notes: For control variables see Notes to Table 2 for the model with extended set of controls. Clustered std. errors in parentheses; ***significant at the 1 percent level, **significant at the 5 percent level *significant at the 10 percent level

Table 5: Robustness: FE estimates for the effect of inputs from first trimester interview and imputed employment at third trimester interview on probability of preterm birth

Employment, third trimester	-0.026***		
	(0.008)		
Employment, imputed 1		-0.015*	
		(0.008)	
Employment, imputed 0			-0.044***
			(0.007)
Sick-listed, first trimester	-0.007	-0.006	-0.004
	(0.013)	(0.012)	(0.012)
Pregnancy complication	0.025***	0.040***	0.039***
	(0.007)	(0.007)	(0.007)
White-collar occupation	0.014	0.014	0.013
	(0.009)	(0.010)	(0.010)
Mother smoked, first trimester	-0.014	-0.013	-0.012
	(0.011)	(0.010)	(0.010)
Number of glasses/week, first trimester	0.004	0.004	0.004
	(0.003)	(0.003)	(0.003)
N	10331	10988	10988

Notes: Additional control variables see Notes to Table 2. Clustered std. errors in parentheses; ***significant at the 1 percent level, **significant at the 5 percent level *significant at the 10 percent level

Table 6: Robustness: Closely spaced siblings and FE for starters/quitters

	Sibling sample, close spacing		*Starters*	*Quitters*	*Starters*	*Quitters*
	Preterm birth	*SGA*	*Preterm birth*		*SGA*	
Employment, third trimester	-0.019*	0.010				
	(0.010)	(0.016)				
Starts			-0.027**		-0.009	
			(0.012)		(0.018)	
Stops				0.027**		-0.020
				(0.012)		(0.017)
White-collar occupation	0.018	-0.019	0.014	0.013	-0.019	-0.029
	(0.013)	(0.024)	(0.010)	(0.010)	(0.018)	(0.018)
Sick-listed, third trimester	0.013	-0.000	0.001	0.010	-0.010	-0.006
	(0.012)	(0.018)	(0.011)	(0.010)	(0.014)	(0.014)
Pregnancy complication	0.025***	0.041***	0.027***	0.027***	0.045***	0.050***
	(0.009)	(0.016)	(0.007)	(0.007)	(0.011)	(0.011)
Mother smoked, third trimester	0.004	-0.007	-0.012	0.006	0.035	0.037
Number of glasses/week, third trimester	0.010**	-0.010	0.008**	0.008**	-0.004	-0.007
	(0.005)	(0.007)	(0.003)	(0.003)	(0.005)	(0.005)
N	5909	5909	9086	9313	9086	9313
R^2	0.020	0.037	0.017	0.014	0.036	0.037

Notes: For control variables see Notes to Table 2; clustered std. errors in parentheses; ***significant at the 1 percent level, **significant at the 5 percent level *significant at the 10 percent level

A Appendix

Table A.1: Estimation sample construction, 1998-2003.

	Full sample	Sibling sample
All singleton live births	366,367	
All singleton live births/all siblings in survey sample	84,970	10,992
No missing items, first trimester	84,889	10,986
No missing items, third trimester 2	78,851	10,331
Mothers who change employment status (changers in main analysis)		
Children with mother who starts		902
Children with mother who quits		1,148

Notes: All number of observations are at the child level. Starting mothers are mothers to siblings who do not work during trimester 3 in their first pregnancy, but work in the third trimester of their second pregnancy. Quitting mothers are working in third trimester the first pregnancy but not in their second pregnancy.

Table A.2: Effect of first trimester employment on third trimester maternal behaviors.

	Mother smokes	Mother drinks alc.	Glasses/week	Mother smokes	Mother drinks alc.	Glasses/week
Employment, first trimester	0.008	0.018	0.008	0.005	0.000	0.001
	(0.010)	(0.013)	(0.036)	(0.010)	(0.016)	(0.034)
White-collar occupation	-0.053***	0.030**	0.050	0.001	0.010	0.004
	(0.011)	(0.014)	(0.036)	(0.011)	(0.022)	(0.046)
Missing occupation	-0.032**	0.057***	0.083*	-0.005	0.047**	0.022
	(0.015)	(0.018)	(0.044)	(0.013)	(0.022)	(0.046)
Sick-listed, first trimester	0.007	-0.081***	-0.133***	-0.007	-0.013	-0.000
	(0.015)	(0.018)	(0.048)	(0.012)	(0.019)	(0.043)
Pregnancy complication	0.009	-0.026**	-0.011	-0.002	-0.028**	-0.052*
	(0.008)	(0.012)	(0.032)	(0.006)	(0.012)	(0.028)
N	10331	10331	10331	10331	10331	10331
R^2	0.062	0.042	0.042	0.010	0.015	0.018
Mother FE	No	No	No	Yes	Yes	Yes

Notes: Additional controls: Sick listing at first trimester interview, pregnancy complications, occupational status indicators and family's disposable income in year before birth, indicator for child sex and parity, maternal age and age squared, cohabitation status, year indicators. Clustered standard errors (mother level). ***significant at the 1 percent level, **significant at the 5 percent level *significant at the 10 percent level

Table A.3: Heterogeneity of employment effects across subgroups of mothers.

	Preterm birth		SGA	
	Mother's educational level			
	High	*Low*	*High*	*Low*
Employment, first trimester	-0.024	-0.018	-0.005	-0.018
	(0.017)	(0.012)	(0.022)	(0.016)
N	4579	5751	4579	5751
R^2	0.024	0.013	0.026	0.048
Mean of dep.var.	0.029	0.030	0.071	0.095
	Mother has white/blue collar occupation, year before birth			
	White	*Blue*	*White*	*Blue*
Employment, first trimester	-0.032**	-0.033**	-0.011	-0.010
	(0.012)	(0.017)	(0.017)	(0.022)
N	6680	2623	6680	2623
R^2	0.020	0.018	0.029	0.058
Mean of dep.var.	0.027	0.038	0.081	0.089
	Family above median income in year before birth, cohabiting parents			
	High	*Low*	*High*	*Low*
Employment, first trimester	-0.026	-0.015	0.014	-0.022
	(0.023)	(0.010)	(0.025)	(0.015)
N	3985	5734	3985	5734
R^2	0.021	0.016	0.034	0.057
Mean of dep.var.	0.027	0.030	0.074	0.086

Notes: Sample divided by educational level during first pregancy (above high school), occupation in year before first birth (white collar vs. blue collar, excluding mothers with missing occupational status in year before birth), and family income for cohabiting mothers in year before first observed pregnancy (above median). Additional controls: Sick listing at first trimester interview, pregnancy complications, indicator for child sex and parity, maternal age and age squared, cohabitation status, maternal smoking and alcohol consumption at first trimester interview, year indicators. Clustered standard errors (mother level). ***significant at the 1 percent level, **significant at the 5 percent level *significant at the 10 percent level