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The effect of cigarette and alcohol consumption on birth outcomes

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Maternal behaviors and child health:

The effect of cigarette and alcohol consumption on birth outcomes

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Abstract

This paper uses Danish survey and register data to examine the effect of maternal inputs on child health at birth. The paper adds to the literature in several ways: First, while previous studies mainly have focused on maternal smoking, this paper factors in a larger number of maternal health behaviors, most importantly prenatal alcohol consumption. Second, it uses prenatal maternal reports on inputs and objective administrative data on child outcomes. Both features of the data reduce the threat of recall bias and measurement error. Third, the paper identifies the effect of health behaviors by exploiting variation between siblings. The results of the paper confirm and extend earlier findings. Maternal smoking decreases birth weight and fetal growth, with smaller effects in sibling models. The negative alcohol effect on birth outcomes is pronounced and remains intact in sibling models. Both effects suggest a dose-response relationship. Robustness checks suggest that the sibling sample represents the population of multiple mothers well and that smoking results are not driven by misclassification of smoking status.

1. Introduction

With growing empirical evidence of the association between early child health and important outcomes later in life (Black et al. 2007; Case et al. 2005; Currie 2008; Oreopoulos et al. 2008), recent economic research has focused on the *determinants* of child health, especially on the effect of parental pre- and postnatal behaviors on early child health. Prenatal investments are potentially among the most important parental investments in child health, as these investments set the stage for later investments and outcomes: the epidemiological literature has found evidence for programming, i.e. the long-term impact of conditions in the mother's womb on health in adult life (Barker 1997). Moreover, recent economic research suggests the existence of sensitive and critical periods of investment (Cunha & Heckman 2007) and pregnancy is likely to be a critical period.

This paper identifies the effect of maternal prenatal inputs on birth outcomes by exploiting variation between siblings. As one main contribution, the paper explicitly examines the effect of behaviors other than maternal smoking, most importantly maternal alcohol consumption, within a sibling framework. This extension of the economic literature relies on high quality survey and register data from Denmark on a broad set of maternal behaviors that are likely to influence child health jointly or complementarily. This data does not suffer from the matching problems present in many studies from the U.S., the survey reports are not retrospective, and the information on outcomes at birth comes from administrative register data. Thus the data on outcomes is reliably measured.

The introduction of other maternal health behaviors than smoking in a sibling framework is an extension to the recent economic literature on prenatal inputs. Thus, the analysis extends on earlier findings from sibling models which lack this information and solely focus on maternal smoking (Abrevaya 2006), and earlier findings on inputs like alcohol consumption from cross-sectional analysis (Del Bono, Ermisch & Francesconi 2008). Maternal smoking has been labeled the most important modifiable maternal health behavior. A negative correlation of maternal prenatal smoking and birth outcomes is well-established, and the best existing evidence is that maternal smoking has a negative impact on a number of health outcomes at birth (Abrevaya 2006; Abrevaya & Dahl 2008; Bernstein et al. 2005; Lien & Evans 2005; Linnet et al. 2006; Lumley et al. 2004; U.S.Department of Health and Human Services 2004).

Most economic studies on maternal health behaviors have focused on maternal smoking or prenatal care usage with only incorporating few *other* behaviors. Nonetheless, considerable disagreement on health effects of other behaviors remains: heavy maternal alcohol consumption during pregnancy has been shown to be negatively related to child health outcomes (Albertsen et al. 2004; Ebrahim et al. 1999; Stade et al. 2009) and long-term outcomes (Nilsson 2008). However, researchers disagree about both the impact of *lighter* drinking during pregnancy for birth outcomes and the existence of critical periods for alcohol consumption.[2] Some studies and systematic reviews find no convincing evidence for the importance of low levels of exposure, while others argue the opposite (Henderson et al. 2007; Nathanson et al. 2008; O'Brien 2008). Which doses are critical remains unclear, and national guidelines from health agencies for pregnant women vary. The impact of other maternal choices during pregnancy, such as employment and exercise, likewise remains open for discussion. Recent studies suggest a negative correlation between light exercise during pregnancy and the probability of preterm birth, thereby indicating a protective role for such forms of exercise (Juhl et al. 2008).

[2] I do not consider longer-term consequences like health conditions later in childhood (Stade, Bailey, Dzendoletas, Sgro, Dowswell, & Bennett 2009).

In addition to the focus on few health behaviors to the near exclusion of other factors, methodological problems also apply here. Although the negative impact of maternal smoking is well established, the size of its harmful effect on birth outcomes is subject to debate, partly because of identification problems. Parents choose health inputs, i.e., inputs are not randomly distributed across the population of parents, and—most importantly—the researcher does not commonly observe all inputs relevant for the child health production function. Pregnant women who engage in behaviors such as smoking might also have a higher propensity to engage in other unobserved behaviors. Thus researchers run the risk of estimating the effect of maternal smoking with bias. This paper includes a broad set of maternal behaviors into the analysis and its results—if comparable to earlier sibling-based results for maternal smoking—can help to uncover whether sibling models adequately estimate the effect of maternal inputs even with a restricted set of information on maternal behaviors. Adequately estimating the causal effects of health behaviors on child outcomes is crucial for policy makers when they weigh the costs and potential gains of policies such as anti-smoking campaigns targeted at pregnant women. If estimates for the effect of certain behaviors are small for the general population, increasing efficiency calls for targeting policies at subpopulations. Thus questions of heterogeneous effects are important.

My baseline OLS findings reveal that the inclusion of an encompassing set of controls for maternal behaviors reduces the negative smoking effect on fetal growth and birth weight. The results show that the kind of measure employed (e.g., indicator, measure of intensity) matters for smoking and alcohol results. OLS results for maternal alcohol consumption suggest an ambiguous relation to birth outcomes, with no negative effects for light drinking. However, when I account for mother-specific unobserved heterogeneity, the smoking effect decreases while the importance of the negative effect of alcohol consumption (units consumed) increases. Furthermore, my smoking results—which control for the impact of maternal alcohol consumption—are very much in line with the results of earlier studies employing a mother fixed effect but lacking a measure for alcohol exposure of the fetus (Abrevaya 2006; Del Bono et al 2008). Thus, my study tests the validity of those estimates and shows that the potential omitted variable alcohol does not change smoking results.

Finally, I find heterogeneous effects of maternal smoking and alcohol consumption according to maternal characteristics: young mothers and mothers with low education display larger smoking effects, while the oldest mothers drive the alcohol results. Moreover, my findings indicate that both the smoking and the alcohol effect are driven by mothers with above average consumption. This finding indicates a dose-response relationship.

To deal with potential drawbacks in my identification strategy, I conduct a number of robustness checks. First, I examine whether mothers change other behaviors when changing smoking habits, a problem often not testable due to data restrictions. Second, estimating the effect of maternal smoking on different samples drawn from the administrative registries, I test for sample selection based on observables or time-invariant unobservables. Third, I examine the potential impact of error in the survey smoking variable.

2. Data

This paper uses data from the “Danish National Birth Cohort” (DNBC), which comprises information from 1997 through 2003 on pregnant women and their offspring (Olsen et al. 2001). For the data collection, general practitioners invited pregnant women at their first doctor visit to participate in the study, i.e., the sampling unit is pregnancies. Consequently, an earlier study finds that half of the non-participation in the first pregnancy interview results from a lack of GP cooperation, while the other half is attributable to women's non-response (Nohr et al. 2006). As a result, the data covers about one third of all detected pregnancies in Denmark in the period under consideration.

As to the representativeness of the data, Nohr et al. find that participants on average are more likely to be between 25 and 35 years of age and pregnant with their first or second child.[3] They are also more likely to be normal weighted, non-smoking, and pregnant through in vitro fertilization. Overall, DNBC mothers are "somewhat healthier than mothers in the source population, but differential participation was modest and the estimated effect on the risk estimates was small, even after minimal confounder adjustment" (Nohr et al 2006: 416). These findings illustrate that although DNBC mothers slightly diverge from the general population of mothers, the DNBC is not a convenience sample.

The use of DNBC data contributes to the existing literature in four ways: First, most of the economic studies on the impact of parental inputs on child health come from the U.S. or the UK. Denmark makes an excellent case for testing some of those results, because Danish women's consumption of cigarettes and alcohol has on average been higher than that for other European countries, including for pregnant women (Egebjerg Jensen et al. 2008). Even during pregnancy, Danish women participate in the labor market to a high degree, and all women have universal access to prenatal care. Thus evidence from Denmark makes an informative comparison to results from the U.S.

Second, the DNBC data has been collected explicitly for studying the *determinants* of early child health. The data contains information on an array of health behaviors likely to have an impact on children's health at birth: smoking, alcohol consumption, exercise, nutrition, and employment during pregnancy. Additionally, the DNBC survey data was

[3] Nohr et al look at first time mothers only.

administered to mothers during their pregnancy. Thus the data does not suffer from recall and justification bias, both of which can constitute a considerable problem in retrospective reports after birth (Currie 2000). Third, the linked administrative register data adds reliable and objective outcome measures and controls to the analysis (maternal age, educational attainment, civil status). Given the national personal identifier and the reliability of the administrative register data, this paper does not encounter problems of potential mismatches of parents and children and I expect a very low level of measurement error in left-hand side variables. Fourth, as it contains detailed information on a relative large sample of siblings, the data allows us to account for family-specific time-invariant unobservables.

General practitioners (GPs) recruited 100,418 pregnant women for participation and for 100,309 pregnancies, the DNBC data contains information on a specific outcome (e.g., live birth). From those pregnancies, I identify 94,672 live births in the administrative registers. The pregnancy interviews were scheduled for pregnancy weeks 12 and 30.[4] However, as the survey suffers from attrition and item non-response, my working sample includes 79,483 children with non-missing information from both interviews. Of those, 69,652 children do not have siblings in the DNBC survey data, and 4,845 children are in sibling pairs.[5] Forty-seven children are members of sibling groups of three.

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[4] To date, the DNBC data contains four survey waves, two during pregnancy and two at 12 and 18 months after the child's birth. A seven-year follow-up study is in the making.

[5] Of the children in the sibling sample, only 188 children in sibling pairs do not share the same father. This time-varying factor could be important when interpreting the mother FE partly as capturing genetic endowment. However, controlling for this factor and estimating the model excluding those children does not change the main results of the analysis.

This paper focuses on three global measures of child health at birth: birth weight, fetal growth, and preterm birth. Table 1 gives descriptive statistics for the full sample and the sibling sample. *Birth weight* is a widely used measure of child health in medical, epidemiological, and economic literature. Birth weight is a good predictor for a number of health outcomes later in life. *Fetal growth*, the second measure, is birth weight adjusted for gestational age.[6] I define *preterm birth* as birth before 37 weeks of gestation. All three outcomes are very similar for both the full and the sibling sample.

One unique feature of the data is the possibility of looking at a mother's reports twice during pregnancy. Table 1 illustrates changes in several variables between interviews. For example, twenty-six percent of all mothers report at the first interview that they have smoked during their pregnancy or are still smoking. These mothers include those who have already stopped smoking by interview one, who stopped smoking later in their pregnancy, or who continued smoking throughout their pregnancy. Compared to recent studies, this figure is close to smoking figures reported for the UK and considerably higher than those reported for the U.S. (Del Bono et al. 2008).[7] By the second interview, however, only 16 percent of all mothers report smoking, a figure closer to those reported from the U.S. (Abrevaya 2006). For alcohol consumption, the percentage of mothers who report that they drink at least one unit of alcohol per week rises from 24 to 31 percent between interviews (1 unit equals 1 bottle of beer, 1 glass of wine or 1 glass of liquor). The number of cigarettes

[6] As mean birth weight increases with gestational age (with a downward trend for children born over term), adjusting birth weight for gestational age is relevant.

7 Danish women converge in behavior with women in countries like the UK or U.S. Olsen et al. find in a trial in the 1980s that 38 and 41 percent of pregnant women in two Danish towns smoked during pregnancy (Olsen et al. 1989).

smoked and units of alcohol consumed remain stable for those who engage in those behaviors. However, these numbers suggest that mothers change their behavior not only *between* pregnancies but also *during* a pregnancy.

Table 2 presents a closer look at the mothers' smoking reports during the interviews, on the assumption that mothers who report smoking at one time during pregnancy have been smoking during all the weeks preceding that report. For mothers who report smoking at the first interview but not at the second interview, the greatest percentage stopped smoking extremely early in the pregnancy. Seven percent of all smoking mothers have stopped smoking at some point before interview 1, whereas only very few mothers continue smoking after interview one and then stop before interview 2. These numbers may reflect that interview one took place very early in the pregnancy, so that mothers might be reporting smoking from before they were aware of their pregnancy. The average daily number of cigarettes reported by all smoking women at interviews one and two remains stable, at around eight cigarettes, with no significant difference in the mean number of cigarettes reported at interview one between women who stop smoking early in pregnancy and those who smoke throughout the pregnancy.[8] Women who stop smoking in between the interviews report on average a significantly lower consumption of cigarettes at interview one.

Returning to Table 1, I find a different picture for alcohol consumption. Around 18 percent of mothers change their alcohol consumption between interviews. The majority of these mothers start consuming alcohol between the two interviews. This finding could reflect women's expectations about critical periods with respect to alcohol during

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[8] As shown later, mothers who stop smoking between pregnancies on average smoke considerably less during their "smoking pregnancy."

pregnancy. This possibility is supported by the finding that the women in the sample on average drank even more before their pregnancies.[9]

The DNBC mothers have a mean BMI of around 23.5 before their pregnancy. Eighty-one and 77 percent of the women report that they are working at some kind of paying job at the first and second interviews and they do so equally in both samples. These numbers reflect a high labor market participation for Danish women.[10] Most of the variation in employment comes from changes in working hours and sick leave. This observation is important for evaluating results from this study in the light of the findings from earlier studies from the U.S. or the UK. For Denmark, other mechanisms could be at work; i.e., while financially comfortable women in the UK may chose not to work during pregnancy, the selection out of employment appears different for Danish women. Other controls include mothers' fish intake at interview one (as a proxy for diet) and maternal exercises from the DNBC survey. Furthermore, I include information on mothers' age at birth, highest completed educational level, cohabitation status, and the child's sex and parity drawn from the registers.

The DNBC data reveals similar associations of maternal health behaviors with observable characteristics such as age or educational group as found in earlier studies. For age a u-shaped pattern is present, with the youngest mothers having the highest percentage

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[9] Thirty percent of mothers were consuming more than one unit of beer per week before pregnancy, 60 percent were consuming more than one unit of wine, and 11 percent were consuming more than one unit of spirits. At the first interview the respective figures are 7, 22, and 0.4 percent.

[10] As a point of reference, in the UK and U.S. samples in Del Bono et al (2008) between 32 and 50 percent of women, respectively, are not employed at all during their pregnancy.

of smokers, and women at the mean age for first-time mothers having the lowest. For smoking throughout pregnancy, the percentage of women in the lowest educational groups is highest. In all educational groups, a similar percentage of women report that they stop smoking. The percentage of mothers who report consuming alcohol increases with age. At the same time, a higher percentage of mothers with education above high school report alcohol higher consumption during pregnancy.[11]

Overall, I find a strong persistence of maternal smoking, as we could also expect for addictive behavior. Women in my sample appear to either stop smoking very early in pregnancy or smoke regularly throughout the pregnancy.[12] If we consider a dose-response relationship for smoking and alcohol consumption and birth outcomes, then using mothers' reports at interview two as a proxy for their behavior during the entire pregnancy is reasonable. Thus in my analysis I include both a smoking indicator variable and a variable for the number of cigarettes smoked per day (including cigars and pipes), each based on reports from interview two. For alcohol consumption, an increase in the percentage of mothers consuming alcohol later in their pregnancy could reflect women's beliefs about critical periods of fetal development. I include maternal reports on alcohol consumption from interview two with an indicator for at least 1 unit of alcohol per week and the number of units of different alcoholic beverages consumed per day (beer, wine and liquor). For both variables – number of cigarettes and number of units of alcohol – the standard deviation within-family is half the size of the between-family figure.[13] This observation confirms that

[11] Results are available on request.

[12] Unfortunately, I have no information on pre-pregnancy smoking status.

[13] The within standard deviation (std.dev.) for mothers who smoke or drink at interview two is 2.1/0.09, while the between std. dev. lie at 5.5/0.16.

most of the smoking and alcohol variation lies between mothers, i.e. mothers' behaviors change moderately between births.

3. Background and empirical methods

Economic studies on parental input into children take a household production framework as their point of departure. Parents invest in their children because child quality—and thus child health—enters their utility function. They use various resources and their own health endowment as inputs in producing child health. Within this framework, prices and parental characteristics determine the choice of inputs—among them health behaviors such as maternal smoking and alcohol consumption. Unobserved factors such as tastes, time preferences, and parental health endowment are likely to influence parental choices. Thus when estimating empirical specifications of child health production functions, researchers are concerned with unobserved factors related to both parental inputs and birth outcomes (Reichman et al. 2009).

Important parental investments under scrutiny have been maternal age at birth (Rosenzweig & Wolpin 1995), maternal education (Chou et al. 2007; Currie & Moretti 2003; Lindeboom et al. 2006), maternal postnatal labor market participation (Waldfogel et al. 2002), family income (Case et al. 2002), and for health behaviors prenatal smoking and usage of prenatal care (Abrevaya 2006; Dave et al. 2008; Rosenzweig & Schultz 1983).

Facing omitted variable problems, economic studies have primarily chosen one of two approaches in identifying the effects of prenatal inputs on child health at birth: instrumental variables (IV) and sibling estimators. First, IV studies are found primarily in the context of estimating the effect of maternal schooling on birth outcomes. Few studies have found credible instruments for maternal smoking and assessing the impact of several inputs would require more than one credible instrument at a time. Using variation in taxes, Lien and Evans find that maternal smoking decreases mean birth weight by 182g and increases the probability of having a baby with low birth weight by 7 percent and their IV estimates are very similar to their single equation results (Lien & Evans 2005).

Second, a small number of studies have either exploited variation among mothers who are twins or siblings (Currie & Moretti 2007; Rosenzweig & Wolpin 1995) or who exploited variation in consecutive births to the same mother (Abrevaya 2006; Del Bono, Ermisch, & Francesconi 2008; Rosenzweig & Wolpin 1994). Abrevaya (2006) finds smaller effects of maternal smoking when taking unobserved heterogeneity into account. In his preferred specifications, maternal smoking decreases birth weight by 100-150g. However, his study encounters problems caused by matched panels, constructed from American natality data. Del Bono et al. (2008) find relatively stable effects for maternal smoking when comparing OLS and FE estimates in three data sets from the U.S. and the UK. They find that maternal smoking decreases birth weight to a 140-189g range and fetal growth rates by 3.5-4.7g/week for different samples applied. Their set of control variables is restricted, and they have to rely on retrospective maternal reports for both maternal inputs and child outcomes.

This paper builds on the sibling studies. Consider an empirical specification of a child health production function for two children (i =1, 2) in family s

$$Y_{is} = S_{is}\beta + X_{is}\delta + c_s + u_{is} \qquad 1$$

where Y_{is} as a health outcome of interest (e.g. fetal growth). S is a parental input of interest, i.e. maternal smoking status during pregnancy, X is a vector of other observable variables, c_s is a (time-invariant) unobserved variable common to both children, and u_{is} is an idiosyncratic error term.

Estimating β, the effect of parental investment in child health using OLS (pooling all observations) in a "contemporary specification" depends on a number of important assumptions on the health production process: only current inputs matter for each child, i.e., earlier investment decisions in other children do not apply to current parental investment. Moreover, parents invest equally in all their children, and these investments are unrelated to the part of the child's endowment captured in u_{is}. A further assumption is that all families invest equally. Estimates of the effect of parental inputs will be biased if inputs are correlated with the unobserved characteristics captured in c_s, i.e., that omitted variable bias induced by family-specific factors will occur.

To eliminate the family-specific (and time-invariant) characteristics that might bias estimates, I turn to sibling data, i.e., I estimate the child health equation in a differenced form for siblings as in equation 2. [14]

$$\Delta Y_{is} = \Delta S_{is}\beta + \Delta X_{is}\delta + \Delta u_{is} \qquad 2$$

This strategy allows for family-specific unobservables to be correlated with inputs. Two identifying assumptions have to be met: first, strict exogeneity $(E(u_{is} \mid x, s, c_s = 0), i = 1, 2)$ (i.e., the idiosyncratic errors are not correlated with c, s and all x's not indexed by i), and second, full rank (i.e., only x's that vary between siblings can be included). These assumptions suggest different restrictions for the parameters of the model and for parental behavior. First, the correlation of the explanatory variables with the constant component c is set to be equal for all children of a family (not zero but non-varying between siblings). This restriction also implies that although inputs change, their change has no impact on c. Second, as before, the idiosyncratic errors are uncorrelated with the s and the x's for both

[14] This approach does not account for the endogeneity of the fertility decision.

children. This assumption implies that the idiosyncratic error of the first child (i.e., the first child's endowment) has no impact on parental inputs in the second child $(\text{cov}(x_{2s}, u_{1s}) = 0, \text{cov}(s_{2s}, u_{1s}) = 0)$. This assumption rules out feedback effects from present outcomes to future inputs.

4. Results

A. The effect of maternal inputs on child health at birth

Table 3 presents my baseline OLS estimates for birth weight, fetal growth, and preterm birth. For all outcomes I estimate two models, one narrow and one extended. The models with the narrow set of controls are comparable to the studies mentioned earlier, while the extended models include additional controls for maternal behaviors. All models control for maternal educational level, age group, cohabitation status at birth, child sex and parity, and birth year.

For both the narrow and extended versions, maternal smoking has a negative effect on birth weight and fetal growth. Going from the narrow to the extended model, I find a decrease in the effect of the smoking indicator. However, the effect of the number of cigarettes smoked per day adds to the negative effect of maternal smoking in the extended version. In the narrow control models, maternal smoking decreases birth weight by 240g and fetal growth by 5.8gr/week, respectively. In the extended model, these effects decrease to -114g and -2.94g/week. Each cigarette smoked reduces birth weight by 13g and fetal growth by 0.305g/week. In other words, at an average of 8 cigarettes smoked per day, the mean reduction in birth weight amounts to 218g and the reduction in fetal growth amounts to 5.33g/week. Additionally controlling for smoking and alcohol consumption early in the pregnancy (at interview 1) does not change the results for the variables measured at interview 2.

In the linear probability model for preterm birth, the negative effect of maternal smoking is also apparent. The narrow version predicts a 0.7 percent increase in the probability of experiencing a preterm birth, whereas in the extended version the effect is present only for women who smoke at or above the mean of 8 cigarettes per day. In a comparison of these findings to those of other recent studies, my findings resemble those in Abrevaya (2006), who finds similar effects of smoking on birth weight in his sample of births in the U.S., and Del Bono et al. (2008) for the British Millennium Cohort Study.

For other maternal behaviors, for both birth weight and fetal growth, the alcohol indicator displays a very modest positive effect, which is counteracted by the effect of the measure for the number of units of alcohol consumed per day. Units of alcohol consumed have a negative effect on fetal growth, where each daily unit reduces fetal growth by 1.38g/week in the preferred OLS model. At the mean of 0.28 daily units, this effect translates to a weekly reduction of 39g, an effect counteracted by the positive effect of the alcohol indicator. This finding appears to support the negative effect of regular alcohol consumption and the less damaging consequences of very light drinking. Nonetheless, these cross-sectional estimates cannot be interpreted causally.

Other added controls include an indicator for maternal employment, which displays a positive effect for all three outcomes in Table 3. For employed mothers, this effect is counteracted by a negative effect of being sick-listed during pregnancy. Thus the positive employment effect is not present for women who are employed but sick-listed. This finding is in contrast to the negative effects of maternal employment late in pregnancy found by Del Bono et al. (2008) for the U.S. and the UK. For additional controls, the effects show signs of the expected direction in the OLS models. This finding holds for the positive effects of maternal BMI before pregnancy and the negative effects of child's sex and parity. Some of the controls display very modest effects. This statement holds for the positive effects of maternal exercise and maternal fish consumption, both of which have been subject to considerable debate in the scientific and public sphere.

Despite an encompassing number of controls, mother-specific unobservables are of concern in this application. Thus I estimate the models with a mother FE for mothers with multiple births.[15] Table 4 displays results for birth weight, fetal growth, and preterm birth.[16] Again, I compare models with narrow and extended sets of controls for birth weight and fetal growth. For preterm birth I show only results of the extended version, as none of the estimates of the narrow control model was significant.

A look at the sibling estimates reveals that the effects of maternal smoking slightly decrease. For birth weight, the effect of smoking decreases to -97g; for fetal growth, the effect of smoking decreases to -2.66g/week, which compares well to the results in Abrevaya (2006). In the FE models the measure for the number of cigarettes turns out to be insignificant. While the OLS estimates for smoking are sensitive to the inclusion of additional controls, the FE estimates remain stable. This finding indicates that a FE strategy controls adequately for omitted variables as the ones included in my extended model.

For both outcomes, birth weight and fetal growth, the effect of the alcohol indicator is no longer significant. To the contrary, the effect for the number of units of alcohol/day

[15] Of the mothers in my DNBC sibling sample 223 mothers change their smoking behavior and 1,021 mothers change their alcohol consumption between pregnancies.

[16] I have also estimated the OLS regressions on the sibling sample. Results are very similar and available on request.

increases considerably in importance in the FE regressions, although this effect is estimated with less precision. For both outcomes, the estimated effect of number of units of alcohol/day is bigger in absolute size than in the OLS regressions 147g in the birth weight estimation and 3.7g/week in the fetal growth estimation. Again, at the mean alcohol consumption, these figures translate to a reduction of birth weight and fetal growth by 41g and 1.04g/week. For preterm birth, the number of units of alcohol appears to be the most important health behavior, estimated at increasing the probability of preterm birth, with 7.8 percent for each daily unit of alcohol (2.2 percent at mean consumption).

Overall, maternal alcohol consumption has considerable and negative effects on birth outcomes, and the effects remain significant in the FE models. Interestingly, when including a mother FE, the estimates for alcohol consumption increase in absolute size. As opposed to maternal smoking, where mothers mainly quit between births, the effects estimated for alcohol consumption are driven by mothers who start drinking at higher order pregnancies. A positive correlation of maternal alcohol consumption with time-invariant unobserved mother characteristics might lead to a bigger effect in the FE model.

Taking mother FE into account, the employment indicator has significant effects only in the model for preterm birth, where employment reduces the probability of preterm birth with 2.5 percent. The inclusion of a mother FE results in insignificant results for additional maternal behaviors included in the extended version of the model. Only the mother’s BMI before pregnancy remains a significant predictor in the FE model—and only for fetal growth. As expected, the effects of child’s sex (indicator for female child) and parity one remain significantly and negatively related to birth weight and fetal growth. Additionally, maternal age has a positive effect on fetal growth once a mother FE is included.

In sum, my FE estimates compare well to earlier estimates for maternal smoking. Furthermore, my results add to our understanding of the effects of additional inputs, mainly maternal alcohol consumption. They are based on a reasonable sample size and do not rely on retrospective reports. Both factors avoid problems faced in earlier studies.

To check whether the effect of maternal inputs varies by maternal characteristics, I split my sibling sample in groups defined by the mother’s age and educational level at first pregnancy and I look at the outcomes birth weight and fetal growth. For convenience, and as the results are very similar for both outcomes, I report only the birth weight results. Tables 5 and 6 indicate heterogeneous effects according to mothers’ observable characteristics, although the estimates based on smaller subgroups of the sibling sample are less precise. The smoking effect is most pronounced for the youngest mothers (below 27 years at first birth) and mothers with short- and medium-term education. Maternal alcohol consumption has the most negative effects for mothers above the age of 27.

Another factor is the question of which mothers drive the results for smoking and alcohol consumption. Both distributions—for number of daily cigarettes and alcoholic drinks—have a long tail, with few mothers at the high end of the distribution. When comparing the effects of smoking for smoking mothers above and below the median of eight daily cigarettes in their first observed pregnancy, both OLS and FE results indicate that results are driven by mothers who smoke above this cutoff line. The results are not precisely estimated because of the smaller sample size. For alcohol consumption the effects are also driven by consuming mothers reporting above average levels of drinking. The effect for mothers above the 75th percentile (0.28 units/day or 2 units/week) resembles the effects found in the full sample, although the small sample size again results in imprecisely and insignificantly estimated effects.[17]

B. Robustness checks

As identification of the effect of maternal inputs is based on the comparison of siblings, one might challenge the results for four reasons: First, mothers' behavioral changes might be correlated, e.g., estimates based on mothers who change their smoking status could be biased by other unobserved changes of behavior. Second, mothers who participate twice in the survey could be part of a selected group of multiple mothers. If this process is related to behaviors and children's outcomes, my estimates will not be valid for all multiple mothers (i.e., the selectivity problem). Third, measurement error in the independent variables is a concern for self-reported input measures. Fourth, if mothers change their behavior in higher-order pregnancies because of outcomes from their previous births, the exogeneity assumption is not valid (i.e. the updating problem).

To confront the problem of correlated changes, I compare changing and non-changing mothers on an array of behaviors. Although this strategy does not eliminate the potential threat of correlated and unobserved changes, it illustrates where we could expect bias to arise in studies that lack a broad range of controls. I confront the selectivity problem by comparing my results to those based on other samples of mothers drawn from the population of multiple mothers in Denmark. To assess the potential impact of measurement error, I use maternal smoking report from the administrative registers. Both the selectivity and measurement error analysis focus only on smoking, as I lack information on other health behaviors in the administrative registers. Tackling the updating problem, demands

[17] Results are available on request.

researcher’s having information on at least three consecutive births to the same mother and using this information to instrument for changes in maternal behavior between births. Recent research has demonstrated rather stable results when comparing FE models and FE models instrumenting for changes in maternal behavior (Del Bono et al 2008). However, we still lack convincing empirical evidence for the potential impact of parental dynamic investment (for a discussion, see Almond and Currie 2010). I leave the updating problem for future research.

Changing mothers identify the effect of smoking, for example, in my identification strategy. If, however, changing mothers also change other unobserved behaviors, the results are potentially biased. Most studies cannot examine this question properly as they lack data on other maternal behaviors. Table 7 gives an overview of two groups of mothers in my sibling sample: those who permanently smoke and those who stop smoking before their second pregnancy in the sample. As for other behaviors, mothers who change smoking consume slightly fewer units of alcohol/day on average. However, I find a non-significant difference in means. “Changing” mothers are to a higher degree employed. As for maternal exercise, “changing” mothers exercise more in their first (smoking) pregnancy and a smaller share continues exercising in the second pregnancy when compared to permanently smoking mothers.

As one significant difference, stopping mothers smoke less in their first DNBC pregnancy, i.e., an average of 4.6 cigarettes a day, while continuously smoking mothers smoke 8.7 cigarettes a day in their first pregnancy. As for birth outcomes, changing mothers in my sample have a slightly heavier first child than permanently smoking mothers. However, the difference is not statistically significant. Thus there is some indication that mothers change other behaviors when they stop smoking. However, which directions those changes take is not fully clear. As opposed to earlier studies, I include a broader number of controls capturing maternal behaviors in my analysis. While this strategy does not fully rule out bias introduced by unobserved changing behaviors, my encompassing set of controls captures the most important changes.

The selectivity problem arises if mothers who participate in the DNBC with more than one birth are different from multiple mothers who do not. Selection into the sibling subsample could be caused by at least three factors: First, women who have participated once might not be as encouraged by their GP to do so again. Second, personal characteristics of the woman herself, such as choices on birth spacing, might contribute to differences in participation. For example, women who have participated once might be less likely to participate again because of a workload related to their earlier children. The importance of this factor might thus depend on the spacing between births. Moreover, the "usual suspects" for non-response—observables such as age and education—might be important for the composition of the sibling sample. Third, factors related to one birth might make it more or less probable that mothers participate again. For example, problems relating to the first child's health status could contribute to differential participation, making the sibling sample "healthier" than the overall sample.

To assess selection into the DNBC sibling sample, I compare DNBC mothers to those in other samples drawn from the population of multiple mothers.[18] I draw samples of mothers from the Danish administrative registers according to the following criteria: I look

[18] This strategy is inspired by Abrevaya (2006). He checks the representativeness of his sample because of his matching procedure, whereas I am concerned with the issue of survey non-response. As I do not have access to GP information, assessing this factor is impossible. However, mothers are allocated to their GPs according to their municipality of residence.

at the universe of mothers who experience more than one birth in the period from 1994 through 2003 and who at least have one child in the sampling period of the DNBC.[19] Some of these mothers participate in the DNBC, while others do not. I term pregnancies "eligible" if they occur in the DNBC sampling period. From the registers I draw information on maternal background, birth outcomes, and maternal smoking during pregnancy. Each mother reports this smoking measure to her midwife.

Table 8 displays descriptive statistics for four different groups of mothers: Column 1 contains all multiple mothers (1994-2003), who participate at least once in the DNBC. Column 2 contains this information for all multiple mothers with at least one birth in the DNBC period, irrespective of their DNBC participation (source population). Columns 3 and 4 display the descriptive statistics for mothers who have at least two eligible pregnancies. While column 3 contains only mothers who participate at least twice in the survey, column 4 contains all mothers with more than one birth in the sampling period irrespective of their participation status.

The table shows differences between participating mothers and the source populations. For their first child in the time period, multiple mothers who participate in the DNBC have heavier children—both all participating women and women in the sibling sample. Mothers in the DNBC samples and the source populations are similar for age at birth of their first child in the period. A smaller percentage of mothers in the participating samples experience prenatal care characterized as a "complicated check-up" in the register data. Mothers who

[19] The Danish Patient Register changed its codes from ICD8 to ICD10 in 1994. Additionally, some information such as mother's smoking status is available only from the 1990s. Given these data restrictions, I use information only on the subsample of children born to multiple mothers from 1994 through 2003.

participate with more than one pregnancy in the DNBC (sibling sample) report the lowest percentage of smokers compared to all the other samples.

Taking these observations as a point of departure, I estimate a pooled OLS model for birth weight on the different samples. Table 9 reveals that, despite the observed differences, results based on the different samples greatly resemble one another. The discrepancy is biggest for estimates based on the DNBC sibling sample.

Although the regressions include only a very narrow set of controls (available in the register data), they are informative for a comparison of the samples at hand. Most importantly, I am interested in comparing the coefficient for maternal smoking. The coefficient for the maternal smoking indicator is very stable and significant across the different samples (about -229g). This stability implies that the results for maternal smoking conditional on observables are not different for the DNBC samples when compared to those drawn from the registry data. However, the estimates for the DNBC sibling sample are less precise.

As expected, the smoking coefficients in Table 9 fall somewhere between the coefficients obtained with my DNBC sample for the estimations with a narrow and extended set of controls. Table 10 shows that when adding a mother FE to the models, I find reasonably close coefficients. Again, the estimates for the small DNBC sample are less precise. The coefficients for maternal smoking are smaller than in the main analysis, where I use the survey measure. I will return to this issue when discussing the potential bias induced by measurement error. In sum, using different samples and information only drawn from the administrative registers, I find relatively stable results for the effect of maternal smoking for both OLS and models including a mother FE. This finding indicates that the estimates for the effect of maternal smoking based on the DNBC sibling sample are not driven by selection into this sample according to observables or time-invariant unobservables.

Finally, I examine the bias that potentially results from measurement error. Estimates suffer from attenuation bias in the presence of measurement error and this bias will be bigger in FE models than in OLS (Griliches 1979). There are two reasons for concern about errors in variables in estimating the effect of maternal smoking on birth outcomes: timing of report and justification bias. First, many studies on health behaviors are based on mothers' report of prenatal behaviors from after birth. Recall problems can induce measurement error. However, I do not encounter the problem of retrospective reports, so recall problems should not occur. Second, measurement error can occur when mothers misreport socially deviant behaviors. By the late 1990s, first-time mothers had been exposed to a considerable amount of information and campaigning about negative smoking effects. Although the DNBC promises anonymity, some degree of misreporting among smoking mothers is a possibility.[20]

Comparing the percentage of smoking mothers in the DNBC to the best available evidence for Denmark shows a similar pattern for the mothers' reports and available information from the administrative registers (Egebjerg Jensen, Jensen, Nøhr, & Krüger 2008): the youngest and oldest mothers smoke the most, and overall smoking rates are

[20] A number of studies have considered the question of misclassified smoking status through different forms of validation (urine, breath or blood tests), see e.g. Campbell et al. (2001), Hughes et al. (1982), Patrick et al. (1994). The studies find varying reliability of reports for different subpopulations, different study purposes, and different testing methods. Overall, they find reasonable precision of self-reports for observational studies, as opposed to intervention studies.

declining over time.[21] Thus, the comparison of DNBC data to recent studies based on register data with similar trends confirms the overall validity of the DNBC smoking reports.

However, if measurement error (i.e., misclassification) in the smoking indicator is present, that error is very unlikely to be classical, i.e., uncorrelated to the true value. Measurement error in limited variables always induces a correlation of the error and the true value (Black et al. 2000). This finding makes an IV strategy unfeasible for obtaining estimates of the true smoking effect. However, to assess the impact of misclassification, I follow Black et al. (2000) and exploit two imperfect measures to bound my estimates of the smoking effect. I use information on smoking from the DNBC survey and the Danish Patient Registry. Thus I have two imperfect measures for maternal smoking. Although the measures are both reported by the mother herself, she gives that information to different persons at different times and under different circumstances.

Table 11 shows the results for birth weight regressions using different measures for maternal smoking and instrumenting for the survey report using the smoking measure from the administrative register. The analysis is based on the sample of first-time mothers in the DNBC data. Furthermore, the lower part of the table shows estimations for the sibling sample in the DNBC. Here I estimate mother FE models and instrument for the difference in maternal smoking report by using the difference in the register smoking information. For both panels I have estimated the models using two different instruments: one, that includes and one that excludes women with missing information on the smoking variable in the register data. I use these two instruments because one might expect that a reasonable percentage of mothers who do not report smoking to midwifes actually smoke.

[21] Descriptive results are available on request.

The upper part of the table shows that the estimates for maternal smoking remain unchanged at -0.20 when I use the survey or registry measure for the sample of first-time mothers. Using IV—which should be biased "away" from zero in case of a negative correlation of the measurement error and the true value—the smoking coefficient is bigger (-0.25).[22] When using the extended smoking instrument including mothers with missing information, the coefficient is further away from zero (-0.28), suggesting more measurement error in the register measure (as expected when including mothers with missing information). For mothers with two agreeing smoking measures, the smoking effect is -0.24.

For the FE estimates, the smoking effect drops to -0.107 when I use the survey measure. Using the register measure (panel 2 in the lower part of the table), I again find indication for more misclassification in the register measure (greater attenuation), i.e., mothers are more likely to hold back information when reporting to midwifes than in the DNBC interview. The IV results for the differences lie at -0.26 and -0.36, both not statistically significant. Looking at the mothers with two agreeing smoking reports, I find a smoking effect at -0.14.

Overall, looking at the data generation process, other existing studies for Denmark and the results from the comparison of different measures for maternal smoking, I find that my study is less affected by measurement error in the smoking variable than other studies potentially are. Thus my FE estimates, which are smaller in absolute size than the OLS estimates, are credible and not exclusively due to measurement error leading to attenuation.

[22] The (bivariate) first stage for the instrument on the smoking report from the survey (reliability ratio) is 0.83. For the regressions on differences, the respective coefficient drops to 0.21.

5. Conclusion

This paper uses high-quality data from Denmark to estimate the effects of maternal prenatal health behaviors on birth outcomes. My results not only are in line with earlier sibling-based studies on maternal prenatal smoking but also add to them by factoring in information on important maternal behaviors such as prenatal alcohol consumption.

I find that the negative smoking effect on birth weight and fetal growth is smaller than that suggested by cross-sectional analyses but remains rather stable in FE models in spite of inclusion of additional covariates. Exploiting sibling variation, I find that smoking reduces birth weight by 97g and fetal growth by 2.66g/week. When taking a mother FE and a number of maternal life styles into consideration, I find no significant smoking effects for the probability of experiencing a preterm birth. Maternal alcohol consumption in particular plays an important role, as it increaser the risk of preterm birth significantly by 7.8 percent for the number of units consumed per day (i.e., at the average of 0.28 units, an increase of 2.1 percent). I find that maternal alcohol consumption reduces birth weight by 147g for each daily unit, i.e., 26g at the mean alcohol consumption, and fetal growth by 3.7g/week, i.e., 1g/week at the mean alcohol consumption.

The findings from an analysis of light and heavy smokers/drinkers show that the results are driven by mothers who smoke/drink above average. There is additional indication of heterogeneous effects, as earlier studies also argue: smoking effects are more pronounced for younger mothers and those with lower education. Yet the effect of maternal alcohol consumption is driven by older mothers and mothers with a higher educational level. Maternal employment during pregnancy—as also considered by some earlier studies—displays a modest negative effect for the probability of preterm birth. As this finding for my

Danish sample stands in contrast to evidence from the UK and the U.S., the differences might be due to institutional and cultural factors leading to different selection processes.

As for the potential pitfalls of my study, I find that sample selection into my sibling sample based on observables and time-invariant unobservables is not an important problem. Drawing different samples of multiple mothers from the administrative registry reveals differences in observables between participating and non-participating mothers. However, estimation results do not differ for maternal smoking. Furthermore, my analysis has shown that measurement error is a minor concern in my application. The comparison of the maternal survey report and the maternal report to the administrative registry suggests that the survey report is more trustworthy. This finding emphasizes the importance of timing and the way of measurement of inputs for the validity of estimation results. Finally, composite measures of behavior during pregnancy potentially cover over important changes in maternal behavior during pregnancy. This factor is worth exploring in future research in terms of indication of critical periods during pregnancy.

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Tables:

Table 1. Descriptive statistics. DNBC pooled sample and sibling sample.

	Pooled sample		Sibling sample	
	Mean	Std.	Mean	Std.
Child outcome variables				
Birth weight, kg	3.59	0.55	3.64	0.53
Gestational age, weeks	39.6	1.65	39.68	1.54
Preterm birth, percentage	0.04		0.03	
Child sex (female)	0.49		0.49	
First born child	0.46		0.39	
Maternal age at birth, years	29.96	4.27	29.59	3.83
Mother smoked during pregnancy, i1	0.26		0.19	
Mother smoked, i2	0.16		0.11	
Cigarettes/day for smokers, i1	8.25	5.63	8.17	5.34
Cigarettes/day for smokers, i2	8.68	5.38	8.49	5.9
Alcohol consumption, i1				
>1 unit per week	0.24		0.26	
Alcohol consumption, i2				
>1 unit per week	0.31		0.33	
Units per day for drinkers, i1	0.26	0.18	0.24	0.16
Units per day for drinkers, i2	0.28	0.19	0.27	0.19
BMI before pregnancy	23.59	4.25	23.5	4.19
Father smoked, i1 (N. of obs.: 79,444 in full sample; 9823 in sibling sample)	0.3		0.25	
Maternal lm participation, i1	0.81		0.81	
Maternal lm participation, i2	0.77		0.77	
Number of observations with both survey waves:	79,482		9,829	
- number of single children	69,652			
- number of sibling-pairs	4,844			
- number of sibling-triplets	47			
- number of mothers	74,544			

Notes: i1: interview one, i2: interview two.

Table 2: Maternal smoking behavior during pregnancy.

Mother's smoking type	Share of pregnancies
Only smoking early in pregnancy (<i1)	0.074
Smoking until i1	0.008
Smoking longer than i1 (>i1)	0.016
Smoking until i2	0.16

Notes: i1: interview one, i2: interview two. The table assumes smoking to be constant for periods before the reporting date, e.g., mothers who report smoking until i1 are assumed to have been smoking without interruption prior to interview one.

Table 3: OLS estimates for birth weight, fetal growth and preterm birth.

	(1) Birth weight, kg	(2) Birth weight, kg	(3) Fetal growth, g/week	(4) Fetal growth, g/week	(5) Preterm birth	(6) Preterm birth
Smoking indicator	-0.240 (0.005)**	-0.114 (0.009)**	-5.8 (0.123)**	-2.92 (0.209)**	0.007 (0.002)**	-0.003 (0.004)
Number of cig./day		-0.013 (0.001)**		-0.305 (0.020)**		0.001 (0.000)*
Alcohol indicator: 1 or more per week		0.020 (0.006)**		0.373 (0.144)**		-0.005 (0.002)*
Number of glasses/day		-0.045 (0.018)*		-1.381 (0.404)**		-0.002 (0.007)
BMI before pregnancy		0.017 (0.000)**		0.402 (0.011)**		0.000 (0.000)
Indicator for exercise during pregnancy		0.018 (0.004)**		0.269 (0.095)**		-0.009 (0.002)**
Indicator for fish consumption		0.003 (0.001)**		0.073 (0.023)**		0.000 (0.000)
Employment indicator	0.008 (0.005)	0.027 (0.005)**	0.07 (0.106)	0.171 (0.106)	-0.007 (0.002)**	-0.012 (0.002)**
Sick-listed		-0.056 (0.006)**		-0.745 (0.140)**		0.035 (0.003)**
Observations	79482	79482	79482	79482	79482	79482

Notes: Robust standard errors in parentheses;* significant at 5% level; ** significant at 1% level; Models additionally control for maternal education, maternal age group, birth year, cohabitation status, and child sex and parity.

Table 4. FE estimates for birth weight, fetal growth and preterm birth.

	(1) Birth weight, kg	(2) Birth weight, kg	(3) Fetal growth g/week	(4) Fetal growth g/week	(5) Preterm birth
Smoking indicator	-0.108	-0.097	-2.687	-2.66	0.006
	(0.036)**	(0.042)*	(0.808)**	(0.951)**	(0.018)
Number of cig./day		-0.001		0.016	-0.002
		(0.004)		(0.101)	(0.002)
Alcohol indicator		0.015		0.625	-0.009
		(0.022)		(0.493)	(0.009)
Number of glasses/day		-0.147		-3.7	0.078
		(0.069)*		(1.566)*	(0.030)**
Indicator for exercise		-0.005		-0.130	-0.003
		(0.014)		(0.308)	(0.006)
Indicator for fish consumption		-0.001		-0.038	0.001
		(0.004)		(0.089)	(0.002)
BMI before pregnancy		0.005		0.200	0.002
		(0.004)		(0.099)*	(0.002)
Employment indicator	0.012	0.018	-0.03	-0.02	-0.025
	(0.016)	(0.018)	(0.370)	(0.397)	(0.007)**
Sick-listed		-0.003		0.099	0.016*
		(0.019)		(0.425)	(0.008)

Standard errors in parentheses; * significant at 5% level; ** significant at 1% level; Models additionally control for maternal education, maternal age group, birth year, cohabitation status, and child sex and parity. Number of obs.: 9829. Number of groups: 4891

Table 5. Heterogeneous effects for mothers in different age groups. FE estimation

	(1) <=26 Birth weight	(2)27-33 Birth weight	(3)>33 Birth weight
Smoking indicator	-0.185	-0.092	0.241
	(0.071)**	(0.063)	(0.166)
Number of cig./day	-0.001	0.001	-0.012
	(0.007)	(0.006)	(0.022)
Alcohol indicator	0.010	0.030	-0.163
	(0.044)	(0.032)	(0.069)*
Number of glasses/day	-0.105	-0.224	0.075
	(0.160)	(0.106)*	(0.176)
Indicator for exercise	0.012	-0.014	0.018
	(0.024)	(0.020)	(0.047)
Indicator for fish consumption	0.002	-0.002	0.007
	(0.007)	(0.006)	(0.013)
BMI before pregnancy	0.006	0.003	-0.006
	(0.007)	(0.007)	(0.016)
Employment indicator	0.004	0.006	-0.029
	(0.026)	(0.027)	(0.061)
Sick-listed	0.011	-0.011	-0.076
	(0.030)	(0.029)	(0.065)
Observations	3070	5228	1531
Number of groups	1527	2915	1075

Notes: Standard errors in parentheses. Age groups according to mother's age at first birth in the sibling sample. * significant at 5% level; ** significant at 1% level. Models additionally control for maternal education, birth year, cohabitation status, and child sex and parity.

Table 6. Heterogeneous effects for mothers with different educational level. FE estimation. Birth weight.

	(1) Short education Birth weight	(2) Medium and long education Birth weight
Smoking indicator	-0.142	-0.018
	(0.050)**	(0.080)
Number of cig./day	-0.002	0.008
	(0.005)	(0.012)
Alcohol indicator	0.001	0.035
	(0.029)	(0.034)
Number of glasses/day	-0.133	-0.164
	(0.091)	(0.109)
Indicator for exercise	0.015	-0.029
	(0.018)	(0.021)
Indicator for fish consumption	-0.006	0.006
	(0.005)	(0.006)
BMI before pregnancy	0.003	0.009
	(0.005)	(0.008)
Employment indicator	0.008	0.035
	(0.021)	(0.031)
Sick-listed	0.005	-0.022
	(0.023)	(0.033)
Observations	6004	3825
Number of groups	2986	1906

Standard errors in parentheses. Educational level according to mother's age at first birth in the sibling sample. * significant at 5% level; ** significant at 1% level. Models additionally control for maternal age, birth year, cohabitation status, and child sex and parity.

Table 7: Differences between changing and non-changing mothers in the sibling sample.

	Mothers who stop smoking between pregnancies		Mothers who smoke during both pregnancies	
	1st	2nd	1st	2nd
Mean number of units of alcohol/day, mean and std. dev.	0.62 (1.1)	0.59 (1.1)	0.68 (1.5)	0.68 (1.6)
Number of cigarettes during first observed pregnancy, mean and std. dev. **	4.6 (4.4)		8.7 (5.4)	
Mother exercises, percent	31	11	20	16
Mother is employed during pregnancy, percent	78	71	71	67
Birth weight of first child, mean and std. dev.	3.45 (0.53)		3.38 (0.53)	
# of mothers	141		396	

Notes: Sibling sample of mothers who participate twice in the DNBC, excluded: 3rd children of mothers in the sibling sample. Differences in means between mothers who stop smoking and continuously smoking mothers: * significant at 5% level; ** significant at 1% level.

Table 8: Descriptive statistics for different samples; means and std. dev.

	(1) Mothers who participate at least once in DNBC, information on their births 1994-2003	(2) All mothers who have at least one eligible pregnancy in the period 1998-2003, information on their births 1994-2003	(3) Mothers who participate at least two times in DNBC, only births in the sampling period 1998-2003	(4) All mothers who have at least two eligible pregnancies in the period 1998-2003, only births in the sampling period 1998-2003
Birth weight	3.52 (0.56) [60 862]	3.47 (0.6) [237 934]	3.58 (0.53) [4 367]	3.42 (0.64) [85 967]
Mother's age	28.54 (4.25) [61 483]	28.58 (4.81) [241 971]	28.3 (3.62) [4 410]	27.89 (4.33) [87 582]
Complicated prenatal check up	0.08 [61 272]	0.11 [240 089]	0.07 [4 400]	0.11 [87 068]
Smoking dummy	0.15 [48 929]	0.21 [182 198]	0.11 [4 196]	0.17 [78 902]

Note: All columns only contain mothers with multiple children. Means for first observed child after 1994 for each mother in the respective period. Number of observations (first child per mother) in brackets.

Table 9: Pooled OLS on different samples, only information from administrative registry data.

	(1) Mothers who participate at least once in DNBC, all their births 1994-2003	(2) All mothers who have at least one eligible pregnancy in the period 1998-2003, all their births 1994-2003	(3) Mothers who participate at least two times in DNBC, only births in the sampling period 1998-2003	(4) All mothers who have at least two eligible pregnancies in the period 1998-2003, only births in the sampling period 1998-2003
	Birth weight	Birth weight	Birth weight	Birth weight
Smoking dummy	-0.229 (0.006)**	-0.229 (0.003)**	-0.228 (0.022)**	-0.228 (0.005)**
Cohabitation status	0.027 (0.011)*	0.049 (0.005)**	0.063 (0.046)	0.056 (0.008)**
Dummy for complicated check up during pregnancy	-0.074 (0.005)**	-0.115 (0.003)**	0.013 (0.017)	-0.118 (0.004)**
Child's sex	-0.127 (0.003)**	-0.124 (0.002)**	-0.120 (0.011)**	-0.122 (0.003)**
# of observations	99 811	346 401	8 419	159 745
# of mothers	59 649	225 826	4 368	83 775
R-squared	0.04	0.05	0.04	0.05

Notes: All columns only contain mothers with multiple children. Only children born between 1994 and 2003 are included, due to data restrictions. Models additionally control for mothers' educational group, age and age squared. Robust standard errors in parentheses. The data on the different samples is only from the administrative registers. * significant at 5% level; ** significant at 1% level.

Table 10: FE estimation on different samples and using smoking information from administrative registry data only.

	(1) Mothers who participate at least two times in DNBC, only births in the sampling period 1998-2003 – registry information only Birth weight, kg	(2) All mothers who have at least two eligible pregnancies in the period 1998-2003, only births in the sampling period 1998-2003 – registry information only Birth weight, kg
Smoking indicator (registry data)	-0.055 (0.035)	-0.080 (0.008)**
Cohabitation status	0.083 (0.051)	0.034 (0.009)**
Dummy for complicated check up during pregnancy	0.018 (0.019)	0.050 (0.005)**
Child's sex	-0.121 (0.011)**	-0.132 (0.003)**
# of observations	8419	159745
# of mothers	4368	83836

Standard errors in parentheses. Models additionally control for mothers' education, age and age squared. * significant at 5% level; ** significant at 1% level

Table 11: IV estimation for maternal smoking, outcome: Birth weight.

	OLS	OLS	OLS	IV (for survey report)	IV (for survey report, including missing values in registry as smoking)
Maternal smoking, survey report	-.20 (0.008)**			-.25 (0.01)**	-.28 (0.01)**
Maternal smoking, registry		-.20 (0.008)**			
Maternal smoking, Survey=1, registry=1			-.24 (0.009)**		
Maternal smoking, survey=1, registry=0			-.08 (0.016)**		
Maternal smoking, survey=0, registry=1			-.01 (0.021)		
# of children (sample of first born children)	35 270	35 270	35 270	35 270	36 843

	Mother FE	Mother FE	Mother FE	FE-IV (for survey report)	IV (for survey report, including missing values in registry as smoking)
Maternal smoking, survey report	-.107 (0.037)**			-.26 (0.17)	-.36 (0.23)
Maternal smoking, registry		-.05 (0.036)			
Maternal smoking, Survey=1, registry=1			-.14 (0.047)*		
Maternal smoking, survey=1, registry=0			-.081 (0.045)		
Maternal smoking, survey=0, registry=1			.003 (0.053)		
# of children	9 417	9 417	9 417	9 417	9 794

Notes: * significant at 5% level; ** significant at 1% level. OLS regressions contain controls as in Table 3 except for number of cigarettes smoked. FE estimates additionally control for indicator for first-born child.

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