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# SMOKING BANS, MATERNAL SMOKING AND BIRTH OUTCOMES

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# Smoking Bans, Maternal Smoking and Birth Outcomes*

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**Abstract:** An important externality of smoking is the harm it might cause to those who do not smoke. This paper examines the impact on birth outcomes of children of female workers who are affected by smoking bans in the workplace. Analyzing a 2004 law change in Norway that extended smoking restrictions to bars and restaurants, we find that children of female workers in restaurants and bars born after the law change saw significantly lower rates of being born below the very low birth weight (VLBW) threshold and were less likely to be born pre-term. Using detailed data on smoking status during pregnancy, we find that relative to the control group, most of the benefits arise from changes in smoking behavior of the mother; the effect of second hand smoke exposure on birth outcomes in this formulation appears to be quite small. However, we find suggestive evidence of substituting behavior, i.e. a greater likelihood of smoking at home among fathers, since children born to male workers in restaurants and bars after this law change appear to have slightly worse outcomes. Using individual tax data, we find that the law change did not result in changes in earnings or employment opportunities for those affected, thus suggesting that the effects seen are likely a direct result of changes in smoke exposure in utero. Using a twins based analysis, we link very low birth weight status to adult labor force participation and suggest that via the improvements in birth weight alone, the smoking restriction law in Norway could result in a 0.2 percentage point increase in full time employment by age 28.

**Keywords:** Maternal smoking, smoking bans, very low birth weight
**JEL codes:** D62, J13, I38

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## 1 Introduction

A vast medical literature has examined the (mainly negative) association between smoking or exposure to smoke and various health outcomes. As a result, governments in many countries all over the world have adopted policies to deter its citizens from smoking. While public information and ad campaigns have been a major force in influencing behavior change in this regard, other methods adopted by governments have included taxes, age restrictions and smoking bans in public areas. While getting people to quit smoking is an important outcome in its own right, one of the key factors in assessing the efficacy of such policies is the extent to which such policies have impacts beyond just smoking behavior change. An important externality in the context of smoking is the health of those not directly affected by smoking, but come in contact with the harmful effects of smoke. We attempt to quantify one such externality - birth outcomes - as a result of laws that ban smoking in certain public areas or work spaces.

Smoking bans in the workplace can affect birth outcomes via two important channels - through behavior change of the mother if she is a smoker, or through the changes in second hand smoke exposure (reduced exposure at work, but perhaps increased exposure at home or other places as a result of substituting behavior). While the impact of maternal smoking on the fetus has received considerable attention in the medical literature (Kramer 1987), with its deleterious effects ranging from low birth weight and other birth defects, to childhood and adult asthma and lower cognitive functioning (Horta, Victora, Menezes, Halpern, and Barros 1997; Sayer and Kleinenman 2002; Weitzman, Gortmaker, Walker, and Sobol 1990; Dolan-Mullen, Ramirez, and Groff 1994), the impacts of exposure to second hand smoke is less well studied. On the policy side, while many papers have examined the immediate impacts of taxes or smoking bans on smoking behavior (Brownson, Hopkins, and Wakefield 2002; Eagan, Hetland, and Aarø 2006; Farkas, Gilpin, Distefan, and Pierce 1999; Bitler, Carpenter, and Zavodny 2011; Anger, Kvasnicka, and Siedler 2011), few papers in the economics literature have examined the consequences of such policies on birth outcomes. Evans and Ringel (2001), Lien and Evans (2005) and Simon (2012) are some of the papers that do examine the impact of such policies on smoking during pregnancy and birth outcomes, however, their policy focus is on changes in cigarette taxes rather than smoking bans. Moreover, their papers are largely silent on the issue of second hand smoke.

Also related to our work is the important work of Adda and Cornaglia (2010) who show evidence consistent with the idea that certain types of smoking bans induce smoking at home, which increases exposure of young children to cigarette smoke.

Adda and Cornaglia (2010) however, do not examine direct health effects of smoke exposure on these young children. Hence, the extent to which smoking bans, a very commonly used policy (according to the American Non Smokers' Rights Foundation (2012) nearly 50% of Americans live under bans that prohibit smoking in public areas, workspace and restaurants and bars), affect birth outcomes, and whether the effects arise as a result of changes in second hand exposure is to the best of our knowledge an open question.[1]

A law change in Norway in 2004 that extended a pre-existing smoking ban to bars and restaurants provides a natural setting for studying some of these questions. Using detailed data on place of work and work histories, we are able to identify mothers who worked in restaurants and bars during this period and find that mothers affected by the law had children with overall better health, especially a 1.8 percentage point lower incidence of very low birth weight (VLBW) and a 2.5 percentage point lower likelihood of being born pre-term These are substantial improvements given the low incidence of VLBW (2.5%) and pre term births (6.4%). On other measures like APGAR scores or birth defects however, the overall effect sizes are small and statistically insignificant. We use a difference in difference design by comparing outcomes before and after the law change for people working in restaurants and bars to the same difference but among people who work in a similar occupation (stores). Importantly, smoking was banned in stores throughout the period we examine. A difference in differences design in this context is important to account for naturally occurring seasonal variation in birth outcomes.

Using data on self reported smoking status at the beginning and at the end of the pregnancy, we are able to identify workers who would have only been exposed to second hand smoke.[2] Comparing their outcomes with those of smokers allows us to examine the consequences of second hand smoke on birth outcomes. We find that second hand smoke exposure as measured in our context has little impact on birth outcomes. The changes in birth outcomes largely come from mothers who quit smoking during pregnancy. For the sample restricted to mothers who smoke at the start of the pregnancy, we find after the ban that mothers in retaurants and bars had healthier children (an improvement in birth weight of 162 grams, for example).

[1]More broadly, associations between smoking bans and birth outcomes have been shown in Kabir, Clarke, Conroy, McNamee, Daly, and Clancy 2009 who examine the consequences of a comprehensive smoking ban in the workplace in Ireland in 2004. While they show significant improvements in the rate of low birth weight and pre term births after the reform, the results only show associations as the reform affected everyone in the workplace, leaving no control group for causal inference.

[2]Unfortunately, we have to rely on self-reported measures of smoking as unlike Adda and Cornaglia (2010) we do not have measures of cotinine levels. We address measurement issues associated with this variable in Section 4.2.

It is perhaps reassuring that our point estimates for changes in birth weight arising from quitting behavior are remarkably similar to the estimates in Lien and Evans (2005). We attempt to address some of the substitution behavior observed in Adda and Cornaglia (2010) by using data on children born to men who worked in bars and restaurants during this period. A smoking ban at restaurants and bars in their framework could lead to increased smoking at home since bars and restaurants could be considered "recreational public places". Such increases, however, are likely to happen for everyone that frequents bars and restaurants, and is likely not specific to workers of these establishments. However, to the extent that employees of bars and restaurants cut back on their smoking less immediately as a result of being used to the ability to smoke at work, the increase in smoking at home could be greater for these workers. Sadly, we do not observe smoking behavior for fathers, but we note that birth outcomes are slightly *worse* for children born to men affected by the law change. We think this is suggestive evidence of the harmful effects due to substitution behavior arising out of smoking bans. The negative effects however, are dwarfed by the positive effects that accrue due to maternal behavior change.

A crucial concern in such an exercise is that the law change might have induced other changes, such as changes in the composition of the workforce working in restaurants and bars, income or even the length of time worked in these occupations. Our entire study is based on mothers who were already pregnant by the time the law came into effect, mitigating any concerns of differential selection into fertility as a result of the law change. Given the availability of individual level income data from tax records we are able to address further concerns. The change in law did not result in changes in the composition of mothers who worked in bars and restaurants, nor did it change their income. An important point to note here is that while we do find evidence that mothers in bars and restaurants were more likely to switch occupations after the reform, the mothers who switch appear identical along observable dimension (including smoking characteristics) to mothers who do not switch. We also show that the usual parallel trends assumption required of difference in differences designs appears valid for the outcomes we examine. A placebo test imposing a change in smoking law two years before it actually happened reveals no effects for the relevant treatment and control groups, further showing support that our results are driven by the change in law. Hence, standard difference in differences robustness checks and other tests point to a causal impact of the law change on birth outcomes.

Apart from quantifying the degree of externality due to smoke exposure and thereby addressing the effectiveness of policies that ban smoking, this paper also adds to the literature trying to understand the determinants of early childhood health. In recent years, there has been tremendous focus on the role of early childhood health in determining later life outcomes (see Almond and Currie (2011) for an excellent review). As a result, many papers seek to highlight factors that matter for infant health; this paper highlights the role of smoke exposure in in-utero development.

Finally, we link birth weight and VLBW status to adult labor market outcomes. VLBW status severely impedes success in the labor market. Using a twins based analysis to infer the causal relationship between birthweight and income and labor force participation as in Black, Devereux, and Salvanes (2007), suggests that children who are VLBW at birth earn 12% less and are 11 percentage points less likely to be employed by the time they are 28 years old. Using these parameters, and under some assumptions, we infer that this particular smoking ban in Norway will likely result in a gain of full time employment of 0.2 percentage points via the channel of lower rates of VLBW. We consider this benefit as only one small component of the potential multitude of benefits that are not monetized in this study (for example, Sargent, Shepard, and Glantz (2004) show a lower incidence of heart attacks in Helena, Montana after the implementation of a smoking ban in that city).

An important caveat in this work is the idea that along with quitting smoking, mothers could also bring about other changes in their life as a result of the smoking ban. We are not able to distinguish whether the changes in birth outcomes arise specifically out of quitting smoking or through other, broader changes in the mother's life like eating healthier or getting more nutrients. To the extent that the smoking ban induces an overall healthier lifestyle, the policy impact of the smoking ban on birth outcomes is still relevant. The remainder of the paper proceeds as follows. Section 2 discusses the 2004 smoking ban in the hospitality industry in Norway. In Sections 3 and 4, we discuss our data and the empirical strategy. Section 4 presents our main findings. Section 5 quantify the effects by using a twin fixed effect approach and the final section offers some concluding remarks.

## 2 The 2004 Smoking Ban in Norway

Since 1988 Norway has had a Tobacco Act stating that the air is to be smoke-free in premises and vehicles accessible to the general public.[3] This includes work places and all public spaces where more than two individuals are gathered. This was designed in order to protect people against passive smoking; however, there was one exemption

[3] We have examined the consequences for birth outcomes as a result of this law change as well. The main issue with analyzing this law change is that the implementation of the law was not immediate in 1988; certain firms were given a transitionary period to make their spaces smoke free (Wong 2005). Moreover, for this time period, we do not have data on the smoking status of the mother.

from this act: the hospitality industry consisting of bars, pubs, cafes and restaurants. On June 1st 2004, Norway extended this ban on smoking to bars, pubs, cafes and restaurants. The main objective of the smoking ban in 2004 was to protect the employees in the hospitality industry against passive smoking.

The Directorate for Health and Social Affairs in Norway published a review of the first year with the ban on smoking (Directorate for Health and Social Affairs, 2005). Their main findings suggest that the smoking ban was effective immediately after the reform. The compliance rates were high and immediate. A survey of all the municipalities in Norway indicates that very few municipalities had problems with compliance. In addition, shortly after the implementation, a growing number of people (58 %) supported the ban. This number has increased steadily in the years after the reform and was already at 75 % in 2006 (Statistics Norway).

Perhaps most salient for this paper, this report also states that air quality in bars and restaurants improved after the reform. Surveys of guests and employees suggest significantly improved air quality in the workplace. For example, in a survey of guests who frequented bars and pubs, only 14% reported experiencing "good air quality" in 2003 (pre reform). This number jumped to 57% post reform. Considering the positive response in improvements among guests, the effects on workers in bars, restaurants and pubs is expected to be greater. Reports of passive smoking among the employees decreased from 44% to 6% after the reform. Along with overall increased satisfaction with their new work environment (Hetland, Hetland, Mykletun, Aarø, and Matthiesen, 2008), employees were also less likely to report hoarseness, dry throat, heavy headaches, irritated eyes and tiredness (Eagan, Hetland, and Aarø, 2006). Such positive results were also found in the Scottish and Italian experience with smoking bans (see Gorini 2011 for a review). These results are likely due to the fact that the bans in these countries prevented all indoor smoking or allowed smoking but in separate, enclosed areas. Evidence from laws that partially allowed some smoking (as in the Spanish case) do not report similarly positive results (Fernández, Fu, Pascual, López, Pérez-Ríos, Schiaffino, Martínez-Sánchez, Ariza, Saltó, and Nebot, 2009).

While we directly examine changes in smoking behavior among mothers in the affected workplaces, evidence from broader surveys suggest that in the Norwegian case, at least in the short run, it appears that the ban had an effect on the smoking status of people who worked in restaurants and bars and smoked on a daily basis. Their consumption appears to have decreased by 20% (Directorate for Health and Social Affairs, 2005). Similar results have been found in the US (Farrelly, Evans, and Sfekas (1999); Evans, Farrelly, and Montgomery (1999)), as well as in other settings

(Fichtenberg and Glantz, 2002).

Finally, there is little evidence that the reform affected bar and restaurant sales in Norway (although a small reduction in the sale of beers is reported from breweries). Revenue data from the relevant industries indicate that people continued to go to restaurants, bars and pubs even though they could no longer smoke inside (Melberg and Lund, 2012; Directorate for Health and Social Affairs, 2005). In fact, Lund and Lund (2006) show a slight increase in revenues of the hospitality industry after this ban. The economic impact of smoking bans in general is a topic of much debate, with results ranging from no impacts on employment to decreases and in some cases even increases in employment in affected industries (Adams and Cotti, 2007; Adda, Berlinski, and Machin, 2007; Pakko, 2005). The lack of an economic impact in Norway is important to keep in mind when considering some of the mechanisms by which smoking bans might affect birth outcomes.

# 3 Data and Empirical Strategy

## *3.1 Data*

Our analysis employs several data sources that we can link through unique identifiers for each individual. The primary data source is the birth records for all Norwegian births over the period 1967 to 2006 obtained from the Medical Birth Registry of Norway. The birth records contain information on year and month of birth, birth weight, gestational length, age of mother, and a range of variables describing infant health at birth including APGAR scores, malformations at birth, and infant mortality (defined as those who die within the first year). APGAR scores are a composite index of a child's health at birth and take into account Activity (and muscle tone), Pulse (heart rate), Grimace (reflex irritability), Appearance (skin coloration), and Respiration (breathing rate and effort). Each component is worth up to 2 points for a maximum of 10. We are also able to identify twin births from the Medical Registry. Since having data on smoking behavior by mothers is essential for uncovering the mechanisms by which smoking bans affect birth outcomes, we utilize the fact that the Registry contains data on smoking behavior at the start and the end of pregnancy. Self reported smoking status at the start of the pregnancy is reported to medical doctors at a free, recommended consultation around gestational week 8-12.[4] Mothers who choose to respond to the question on smoking are asked not only about smoking status (yes/no) but also how many cigarettes per day they smoke. 95% of mothers

[4] 95 % of mothers attend this consultation. Reasons for absence is related to finding out about pregnancy at a later stage. Then the smoking behavior information will be gathered at the first consultation.

attend this recommended consultation and 82% in the period form 2001-2010 chose to respond to the question on smoking. The birth registry database also collects information on smoking behavior at the end of pregnancy (whether and how much the mother has smoked during pregnancy). This is collected in a survey at the consultation at the hospital around gestational week 36.[5]

We link this data with administrative registers provided by Statistics Norway, a rich, longitudinal database that covers every resident from 1967 to 2006. For each year, it contains individual demographic information (including gender, date of birth, municipality of residence[6] and marital status) and socio-economic data (including years of education, labor force participation and earnings). Essential for this paper, these data contain data on occupations in the form of NACE (abbreviated from French and translates to the Statistical Classification of Economic Activities in the European Community) codes. These are five digit codes with detailed information on type of occupation and includes working in the restaurant and bars industry as separate categories. For example, Section H of the NACE codes deal with "Hotels and Restaurants" and within this category there are further subdivisions like "Bars", "Hotels without Restaurants" and so on. The Statistics Norway data contains the codes at the most disaggregated level. This is important since smoking was always banned in hotels but allowed in restaurants and bars. Hence we are able to obtain rather precise measures of the type of occupation (and hence information on whether smoking was allowed in the individual's work place) from these codes. For individuals with multiple occupations, only the primary occupation code is listed.

Earnings are measured as total gross pension-qualifying earnings reported in the tax registry and are available from 1967 to 2010. These are not top-coded and include labor earnings, taxable sick benefits, unemployment benefits, and parental leave payments. We also have a crude measure of work hours, separated in four categories, no work, 1-20 hours per week, 20-29 hours per week, more than 29 hours per week. In order to assess whether the smoking ban resulted in better health for the mothers, we utilize data on absence due to sickness, which is gathered from the social security registers which has information on all absences from work with a duration of at least 14 days.[7] These are the absences where one has to have an approved doctors recommendation for absence. Sickness absence in Norway is fully covered from day 1. The workplace pays the first 14 days of an absence spell while the social security system (financed partly from taxes, partly from oil revenues) pays

[5]99 % of births in Norway takes place at a hospital.

[6]There are 435 municipalities in Norway, organized in 19 counties. We will include dummies for county of residence as control variables in our specifications.

[7]We do not have access to the data on shorter-term sickness absence.

longer term absences.

The coverage and reliability of Norwegian registry data are considered to be exceptional, as illustrated by the fact that they received the highest rating in a data quality assessment conducted by Atkinson, Rainwater, and Smeeding (1995).

### *3.2 Empirical Strategy*

We will employ a difference in difference strategy comparing mothers working in restaurants and bars (our treatment group) to mothers in stores (the control group). The reform of June 1st 2004 will give us variation within treated mothers with those giving birth before the reform being exposed to smoke and those giving birth after the reform not exposed, or at least exposed less, to cigarette smoke at the workplace. Recall that the control group was not exposed to smoke at the workplace since stores had been smoke free prior to the reform. Like most studies that employ difference in differences, regressions take the form:

$$Y_{ijt} = \alpha_1 + \alpha_2 Treat_j + \alpha_3 Post_t + \alpha_4 (Treat_j * Post_t) + \alpha_5 X_{ijt} + \epsilon_{ijt} \quad (1)$$

where i indexes the individual, j the workplace, and t time. Y is the outcomes, $Treat$ is 1 for mothers working in restaurant and bars, 0 for mothers in control occupations, $Post$ is 1 after June 1st 2004, 0 before. We define treatment as working in a particular occupation (restaurant/bars and stores) as of 2003, the year prior to the reform. Occupation codes are not available at the monthly level and hence we resort to assigning occupation categories a full year before the reform. We assign treatment in this manner to avoid capturing people who switch in or out of the treatment and control groups as a result of the law change in 2004. We assess the robustness of this later in the results section where we examine whether mothers who are in these occupations in 2003 remain in the same occupation in 2004. In principle though, our results are robust to restricting the sample to women who worked in these occupations in both 2003 and 2004. $X$ is a set of control variables and $\varepsilon$ is the error term. $\alpha_4$ is the intention to treat effect which is the reduced form effect of the reform on outcomes Y. As described in the section on the reform, evidence points toward close to 100 % compliance to the reform, supporting the fact that our intention to treat effect capture most of the mothers in our sample. In the mechanism part we will explore to what extend the channel is the reduction of passive or active smoking or other changes due to the reform.

In order to account for potential selective changes in fertility as a result of the reform, we focus on mothers who were pregnant by the time the reform is in place. Moreover, we separately analyze mothers who give birth soon after the reform since such mothers were likely exposed very little to the effects of decreased smoke in the work place. Hence, our main specifications use births that occur 5 months before the reform, i.e. from January-May 2004 as the births occurring in the "pre" period and births that occur between November 2004-March 2005 as births that occur in the "post" period. Including the intermediary period of June-October 2004 does not materially change our results,[8] and our results are robust to extending the window of analysis to include more months before and after the reform. We cluster standard errors at the occupation-birth month level.

The birth outcomes we will study in this paper includes birth weight, probability of being born less than 1000, 1500, 2000 and 2500 gram, pre-term birth, APGAR scores at 1 and 5 minutes, gender and being born with a birth defect. Birth weight will capture the average effect of the reform while studying non-linearity in birth weight will let us capture effects at different points on the birth weight distribution. Pre-term birth is defined as being born before 36 weeks in gestational age. APGAR scores capture the immediate responsiveness of the baby just after birth and is shown to be correlated with health and cognitive functioning later in life. There is some new evidence that adverse in utero environments can increase the risk of spontaneous abortion of boys more than girls (Sanders and Stoecker 2011), therefore we look at the effect of the reform on gender at birth. The final birth outcome we examine is whether the child is born with a form of birth defect or not, as some studies have suggested that exposure to smoke in utero can cause birth defects.

We also look at other outcomes as part of our strategy to show the robustness of our design and also for uncovering the mechanisms behind the effects we observe. For mothers we will look at pre-reform characteristics including mother's age at birth, wages and hours worked (full time versus part time) in the years prior to the reform and years of completed schooling measured before the reform in order to validate that the composition of mothers are similar before and after the reform. In addition we will study whether the reform has effects on outcomes like wages, hours of work (full time versus part time), sickness absence and switching jobs. The switching occupation variable is defined as switching to a new NACE code in 2004 or 2005 depending on the NACE code in 2003.

Our final outcomes in this paper will be income and hours of work (full time versus part time) at age 28 for a large sample of twins to study the long term effects of birth weight on income. Our specification here follows the twin fixed effect approach in Black, Devereux, and Salvanes (2007). However, in addition to the effect

[8] Although we do not have enough data to fully estimate the effects by birth month, we will show evidence that effects are increasing in the intermediate period and stabilizing in the post period, in line with the idea that time of exposure during pregnancy could matter.

of birthweight on income and hours worked at age 28, we will also study the effect of VLBW. The specification takes the form where j=1 is the first twin and j=2 is the second twin,

$$Y_{i1k} - Y_{i2k} = \beta\left(VLBW_{i1k} - VLBW_{i2k}\right) + \left(\varepsilon_{i1k} - \varepsilon_{i2k}\right) \quad (2)$$

Under the assumption that $\varepsilon_{ijk}$ is independent of $VLBW_{ijk}$, the twin fixed effect estimator of $\beta$ is consistent.[9]

Appendix Table A1 gives descriptive statistics of all our outcomes, both for children and mothers. The first and second column gives the mean and standard deviation for the treatment group before the reform. The third column shows the mean difference between treatment and control group before the reform and the fourth column after the reform. As we see for birth weight outcomes treated usually have worse outcomes than controls before the reform. For example treated has average birth weight of 3444 gram and probability of VLBW of 2,3 % while control group has on average 63 gram heavier children and only 1.1 % VLBW children. After the reform these differences are eliminated: there is only a gap of 10 gram in birth weight and the probability of having a child with VLBW has reversed with control group having more than treatment group. Looking at mothers' characteristics, mothers in treatment group has around one year less education and are one year younger at time of birth. They also typically earn a little less. However, when we study the difference before and after the reform they are very similar across different outcomes. Finally we see that mothers in restaurant and bars are much more likely to smoke than mothers in stores. When we look at quitting smoking during pregnancy, before the reform the treatment group was much less likely (12 percentage points) to quit smoking than control group while after the reform they are as likely to quit.

# 4 Results

## *4.1 Birth Outcomes*

Table 1 shows the effect of the reform on a whole range of birth outcomes for children, including birthweight, being less than 1000 gram (Extremely Low Birth Weight) at birth, less than 1500 (Very Low Birth Weight), less than 2000 and less than 2500 grams (Low Birth Weight) at birth, pre-term birth (defined as being born before gestational week 36), APGAR scores at 1 and 5 minutes, being born a boy and having some form of birth defect, estimated in the form of Equation 1. As discussed

[9]See Black, Devereux, and Salvanes (2007) for strength and weaknesses of using twin fixed effects for causal interpretations of the effect of birth weight on later adult outcomes.

earlier, we exclude the intermediate months where mothers are only partly exposed to the smoking ban in this analysis. In the figures presented later we will also show the intermediate months. The *Post* dummy captures the effect of being born in the period after the reform, compared to the period before, the treated dummy captures the effect of being born to mothers in bars/restaurants (treated occupation) to mothers working in stores (control occupation). The *Post*Treated* dummy captures the differential effect of the reform on children born to mothers in restaurant/bars. Focusing on the interaction term in the regression, i.e. the effect of the reform, we see that there is a positive, although not significant effect on birth weight of 58 grams. While there is no effect on being born less than 1000 gram we see a substantial effect of 1.9 and 2.5 percentage points of being born less than 1500 and less than 2000 gram, respectively. This effect on the lower tail of the birth weight distribution is also captured in a significant effect on pre-term birth. Children born to mothers who happened to benefit from the reform were 2.5 percentage points less likely to be born pre-term. We see no effect on being born less than 2500 gram, nor on APGAR scores, being born a boy or birth defects.[10]

An important assumption for the difference in differences analysis is common trends for the outcome variable prior to the reform. Figure 1 sheds light on this for the VLBW outcomes and Figure 2 includes panels with all the other outcomes of Table 1. We see that before the reform the treatment group has a higher likelihood of giving birth to VLBW children. The average is about 2.5 % compared to only 1 % in the control group. However, the trends are fairly similar and show that the common trend assumption is close to satisfied. In the intermediate period were mothers are partly exposed to the reform during pregnancy we see a sharp decline in children with VLBW for mothers in the treatment group, while there is no change for the control group. After the reform the treatment group is consistently below the control group and average VLBW is now as low as 1 %. The trends continue to be similar in the after period. Figure 2 shows similar pictures for the other outcomes of Table 1. With some exceptions the general picture is the same as for the VLBW outcome, and the trends are similar pre-reform. The pre-term birth is also consistent with an effect of the reform. The other figures show no effect of the reform on the different outcomes, confirming the regression estimates from Table 1.

[10]We also estimate these results only for first born births given that mothers could react differently if she is pregnant with the first child or not, and the effects are stronger although we cannot reject that they are the same for first born as for later born children. We also run a specification where we include a linear trend in birth month interacted with the reform which is allowed to vary on each side of the discontinuity and by treatment status. This does not alter the main results and are available upon request.

### *4.2 Maternal Smoking Behavior and Birth Outcomes*

We next examine whether the reform changed maternal smoking behavior. Table 2 presents results on mother's smoking status before the reform, and, for the subset of mothers that smoke; whether the reform induced workers in bars and restaurants differentially to quit by the end of the pregnancy. Since about 16% of the relevant sample does not report smoking status, we also test whether the reform differentially affected whether mothers respond to the question on smoking behaviors. In our sample of mothers working in restaurant/bars and stores about 20% smoke at start of pregnancy, 64% do not smoke at start of pregnancy and the remaining 16% have missing information on smoking. Since mothers are allowed to not report their smoking status, it is possible that mothers do not misreport information once they choose to respond to the questions on smoking; however, it is certainly possible that they choose to respond and respond incorrectly. While not directly assessing the accuracy of smoking status in this dataset, studies in Norway using smaller samples have suggested that self reported smoking status correlates well with measured nicotine levels in the mother (Kvalvik, Nilsen, Skjærven, Vollset, Midttun, Ueland, and Haug (2012)).

The first two columns of Table 2 measure whether the reform differentially affected pre-reform smoking behavior. We should not expect to find any effects since these variables are measured prior to the reform[11] and we show that this is indeed the case. The last column of Table 2, however, shows an important result. For the subset of mothers in both the treatment and control group who report smoking at the start of the pregnancy, we find a differentially larger proportion of women quitting by the end of the pregnancy in the treatment group. We take this as evidence suggesting that the reform changed maternal smoking behavior for the group working in restaurants and bars. After the reform, mothers in the treated group are 15 percentage points more likely to quit smoking during pregnancy. Given that nearly 40% of mothers who smoke at the start of their pregnancy tend to quit by the end, the effect due to the reform is quite large.

We can use smoking status at the start of the pregnancy to assess whether the impact on birth outcomes is due to changes in maternal smoking behavior or due to changes in exposure to second hand smoke (for the mothers that did not smoke at the start of the pregnancy). Table 3 shows the effects of the smoking ban on birth outcomes by the smoking status of the mother at the beginning of the pregnancy. As

[11]Note that motthers giving birth in the last birth month of our window, March 2005, could have reported this after the reform. None of our results are driven by this birth month. In general this is the main reason for being careful on increasing the window too far away from the reform.

we see from Table 3 ,panel A, the effects are strongest for the group where mothers report smoking at start of pregnancy. From Table 2, we know that this group has a reform effect on quit rate and hence, the large positive effects are likely driven by changes in maternal smoking. The effect of the smoking ban on birth weight for this subsample is significant and amounts to a 160 gram increase on average. As expected, most of the effects are concentrated at the lower tails of the birth weight distribution, with significantly fewer VLBW births for this group. In panel B of Table 3 we report the effect for the group of non-smokers. We see no effect on birthweight for these children and this effect is significantly lower than the effect for the children with smoking mothers. We interpret these results as suggesting that passive smoking has a small and negative but imprecisely estimated effect on birth outcomes. The last panel (C) of Table 3 shows effects more closely aligned with panel A. While it is difficult to draw conclusions from a group that refuses to report smoking status, if we think it is likely that those not reporting smoking status are likely to be smokers, then that helps resolve some of the effects seen in panel C. In panel C, we note that the effects of the reform appear to increase birth weight and also reduce the incidence of pre-term births.

### *4.3 Other mechanisms*

To assess whether the channel of maternal smoking behavior change is the main factor inducing the changes in birth outcomes seen in Table 1, we examine a host of other maternal outcomes as a result of the reform. In Table 4, we examine income, health and job switching status of the mother as a result of the reform. The income measures are from tax records rather than self reported measures, so we expect very little role for systematic measurement error here.[12] We find that the reform did not differentially affect income in 2004 or 2005. Hence the improvements in birth weight are likely not coming from changes in income. We then examine whether the reform led to better health for the mother in the form of reduced absence from work. We measure absence from work as being absent for at least 1 month in the last 3 months of the pregnancy or being absent for the entirety of the last 3 months of the pregnancy.[13] While reports suggest that workers in restaurants and bars after the reform reported better working conditions and fewer incidences of itchy eyes and

[12]If a large component of the income comes from tips, then this will be unobserved and could have changed as a consequence of the reform. This is less of an issue in Norway than the US as wages are mostly based on fixed salaries. Moreover, tipping like in the US of 15-20% is not the norm in Norway. Especially in bars tips are usually only left over change from a bill paid in cash. In nicer restaurants tips are usually about 5-10 % of the bill.

[13]As we saw from Table A1 43% of mothers are absent at least one of the final three months before giving birth and 6 % are absent for the entire 3 months.

other smoke related symptoms, we do not find any meaningful improvements in the health of the mother after the reform as measured by absence rates.

The only dimension along which we do find changes for mothers is in their occupational status. Mothers in the treated group appear to disproportionately switch *out* of working in restaurants and bars after the reform (see Figure 3 for further evidence). This is somewhat surprising as working conditions improved after the reform. However, this is an important consideration as some of the changes in birth outcomes might be driven by the fact that some mothers stopped working in bars and restaurants altogether after the reform. This would still be an important effect to capture as part of the overall "policy" impact of the smoking ban, as long as mothers move to jobs that are also smoke free. However, this does affect our interpretation of pinning down the main mechanism of birth outcome change as arising via changes in maternal smoking behavior.

To further examine this, we obtain a smaller sample of mothers who did not switch jobs between 2003 and 2004 and our results are robust to examining just the subsample of stayers (Table 5). Importantly, in Table 6, we show that mothers who switch jobs after the reform are not observably different to mothers who did not switch after the reform. In particular, along smoking behavior, mothers who switch from the treatment group appear to be very similar to mothers who do not switch from the treatment group. The regressions estimated in Table 6 include all double interactions and also the main effects so as to accurately capture the differential effects.

Finally, we examine the birth outcomes of children born to fathers working in restaurant/bars and stores and perform the same analysis as for mothers. Fathers have no direct contact with the unborn child at work during pregnancy and hence, there should be no effect coming from changing active or passive smoking behavior at work. The only way the father could affect the child is through either reducing active smoking at work and thereby also at home, exposing the mother less to passive smoking or continue smoking but smoke more at home and less at work and thereby exposing the mother *more* to passive smoking.[14] We can of course also not rule out peer effects, for example that the father's changing behavior would also effect the mother's smoking behavior.[15] All these channels are more indirect than the channels affecting the mothers working in restaurant/bars and may work in different directions so theoretically it is not clear what to expect for the children born to men working

[14]Unfortunately we do not have smoking status of fathers as the smoking status is linked to the medical birth records only asking mothers about their smoking behavior during pregnancy.

[15]We partly address this by looking at smoking status of the wife of the husband working in bars/restaurants and we do not find evidence of a peer effect in smoking behavior.

in the affected industries. From Table 7 we see that birth weight appears to *decrease* for children born to fathers in the treated group after the reform; however, this is only significant at 10 % level. Along other birth outcomes there does not appear to be much that is significant although they tend to be in the different direction from the effect for mothers.[16] We take this as suggestive evidence that after the reform, fathers are perhaps more likely to smoke at home and affecting birth outcomes by exposing the mother to more second hand smoke, or perhaps by not encouraging the mother to quit smoking while she is pregnant.

### *4.4 Robustness Tests*

In this section we provide evidence that the results discussed in the previous sections are indeed driven by the change in law rather than any other event or seasonality across month of birth. An intuitive and simple way to test this is to move the reform two years earlier[17], to June 2002 and see if there is any effect after this "fake" reform. As we see from Table 8 there is no evidence of an effect, the point estimate is close to zero for all birth outcomes.

In the appendix we perform a number of robustness checks to support our findings that the smoking ban had an effect on mothers' smoking habits and children's birth outcomes. In Table A2 we change the control group to all mothers in our sample not working in restaurant/bars.[18] This makes the control group very large compared to treatment group and is therefore not our preferred specification as the parallel trends assumption is more likely to be violated in a larger, more heterogeneous control group. However it is reassuring to see that the results are in line with results in Table 1. Yet another alternative is to use mothers who worked in restaurants and bars two years before the reform as the control group. If the main reason for adopting a difference in differences approach is to control for seasonality, then this alternative specification should also be valid, under the assumption that seasonal effects are the same in 2002 as in 2004. The advantage of this control group is that baseline characteristics of treatment and control group are very smilar (since both groups work in same industry). In Table A3 we use mothers working in restaurants and bars in 2001 as the control group and *Post* is defined as giving birth after July.[19]

[16]Note that because fewer fathers than mothers work in bars/restaurant we have increased the window to 9 months around the discontinuity. Using 5 months as with mothers give an effect on birth weight that is even larger, although quite imprecisely measured.

[17]We move the reform two years to ensure no overlap of window with the actual reform.

[18]We have also examined other control groups such as workers in hotels, and matching a random group of workers with similar characteristics as those in restaurant/bars with similar results.

[19]We report effects for a 9 month window to increase precision as there are fewer workers in restaurants and bars than in stores, however effects for 5 months window is similar though less

Table A3 shows that with this alternative specification, our results are very similar to Table 1 where we use mothers working in shops over the same time period (2004) as the control.[20] This again shows that our results are not driven by a particular control group.

In Table A4 we study the composition of mothers before and after the reform. One worry for our empirical strategy is that mothers working in restaurant/bars could be different as a result of the reform. Since we condition on working status in 2003 this is less of a concern. However, it is still reassuring that mothers' characteristics like years of education, age at birth and income prior to reform are the same for mothers in restaurants/bars before and after the reform, relative to mothers in stores. Combined with our results showing that mothers who switch occupations are not observably different, we are quite convinced that the reform did not have any selection or composition effects in the very short run.

Table A5 looks at the robustness of our results with respect to changing the window around the reform. Our main results uses a window of 5 months. This is chosen to not move too far from the reform, because the further away we move, the more likely mothers are to be different and no longer working in restaurant/bars (as defined in 2003). In addition, conception[21] and measures like smoking status in the beginning of pregnancy will be before the reform for all mothers in a 5 month window. It is also not clear that the common trend assumption holds further away from the law change since again different mothers are selected into different occupations over time. However, our results are not driven by the choice of a 5 month window around the reform. The results are similar even with a window of 12 months on each side of the reform. We have to move to as far away as a window of 24 months on each side before the effect is reduced significantly. Even for this window the effect is in the same direction and just below significance at the 10% level.[22]

Table A6 looks at how our results vary by sequentially adding control variables. As we see from Table A6 the results are robust to excluding and including control variables. This is reassuring as it shows that underlying observable characteristics are not driving the birth weight effects we see. We also present a different clustering

precisely estimated.

[20]We simply do not have the sample size to further cut this alternative control group by smoking and non-smoking status. This is perhaps driven by the fact that in this treatment-control formulation, after the reform, mothers are more likely to not respond to the question on smoking status (results not shown, available upon request). Compared to our original control group, this is not the case.

[21]Number of births around the reform are balanced both in treatment and control group. An estimated effect on number of births show a coefficient of -.005 with a t-stat of 1.04.

[22]Studying a figure using 24 months on each side of the reform, we clearly see that the common trend assumption is violated. There could be potential explanations for this, like selection of mothers into different industries changing over time.

than the treatment-birth month by including clusters by region of birth. While this slightly increases the standard errors, the results are still significant.

Finally, in Table A7 we include the intermediary period and find our results to be largely unchanged. Indeed, as the Figure 1 might suggest, while we do not expect to see impacts on births in the first few months after the reform, including that period does not meaningfully change our results. In fact, as might be expected, the effect sizes and t-statistics for most of the outcomes are smaller when we include the intermediary period.

# 5 Quantifying the effects

Given the large literature on how important birth outcomes are for later life success, we assess the gains in later life earnings and employment opportunities due to the positive impact of smoking bans on birth outcomes. Following Black, Devereux, and Salvanes (2007), we link birth weight and very low birth weight status to adult labor market earnings at age 28 (we choose age 28 to maximize sample at the same time as coming close to minimizing life cycle bias Bhuller, Mogstad, and Salvanes (2011)). We obtain arguably causal estimates by using a twins fixed effect. This gives us predictions on how much birth weight and VLBW matters for labor market outcomes. Table 9 shows that a 100 gram increase in birth weight increases adult income by age 28 by around 1.7%, and income conditional on full time employment by around 0.7%. If we use the effect estimate on birth weight for the sample of children born to mothers reporting smoking at start of pregnancy we see that birth weight for these children increases by nearly 160 grams. This translates into an increase in wages of about 2.7%. However, we recognize that this is at the high end of the effect size estimates that we obtain.

The twins estimates from Table 9 suggests that very low birth weight status, while not significantly affecting adult income, does significantly affect the probability of full-time work. Being very low birth weight, reduces the possibility of being engaged in full time work by age 28 by 11 percentage points, which is a really large effect. Our aggregate estimates on the impact of the smoking ban from Table 1 suggests that the ban reduced the probability of a VLBW birth by 1.89 percentage points. This is a large effect given the rate of VLBW births in the population is around 3%. Since the average rate of full time employment for 28 year olds is 80%, we compute that this reform might result in an increase in overall employment rate of 0.2 percentage points.[23] Recall that these increases in wages and employment are

[23]We compute this as follows: If population were 100, given a 3% rate of VLBW, we can think of 97 people being employed at a rate of 80%, while 3 people employed at a rate of 70%. Post smoking

just through improvements in birth weight and incidence of very low birth weight. There could certainly be other benefits to this reform that we are not capturing here.

## 6 Conclusion

In this paper, we analyzed the implications for birth outcomes due to a smoking ban in Norway that extended smoke free work place status to bars and restaurants in 2004. Soon after the law change, we find that birth outcomes, particularly the probability of being born very low birth weight, decreased for mothers working in the affected industries. The main mechanism for these changes that we can shed some light on is the channel of maternal smoking. After the law change, mothers in restaurants and bars are 15% more likely to quite smoking, compared to mothers working in stores. For the mothers that quit smoking, we find large effects on birth weight and lower incidences of pre-term births.

While we find little support for the role of passive smoking, we think that our data is quite limited in being able to study this effectively. Perhaps data with fewer non-responses to questions related to smoking could get at this channel more clearly. While we, to some extent, monetize the gains of such smoking bans that arise due to improved birth weight and therefore labor market outcomes, future research should focus on other important outcomes like adult smoking behavior and other cognitive and health outcomes as a result of in utero exposure to cigarette smoke. Along with other important studies like Evans and Ringel (2001); Lien and Evans (2005); Adda and Cornaglia (2010), our paper underscores the importance of public policy regarding smoking in bringing about maternal smoking change, and thereby improving birth outcomes.

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ban change, we shift these numbers as 98.5 people, 80% of whom are employed, while among the remaining 1.5 people, 70% are employed. Taking these differences leads us to the 0.2 pp estimate.

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**Figure 1.** Very Low Birth Weight

*Notes:* the figure shows the average probability of very low birth weight (<1500 gram) for each birth month ranging from december 2003 to april 2005. The solid line is the treatment group and the dotted line is the control group. In the estimates in Table 1 we only use the five months before (January 2004- May 2004) and five months after (November 2004 - March 2005). In robustness tests we include both longer windows and the intermediate period.

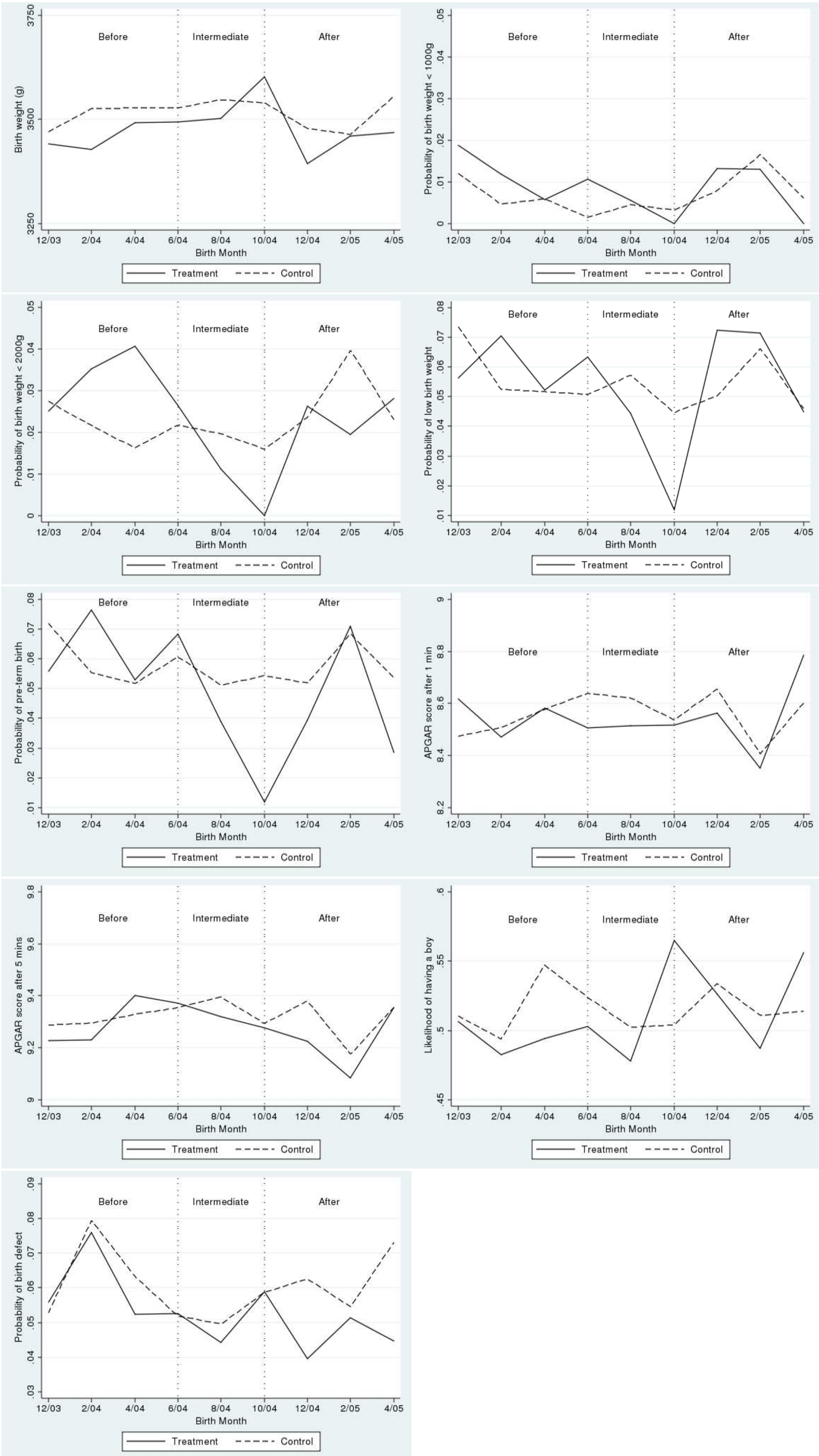


**Figure 2.** Other birth outcomes

*Notes:* the figures show the average probability of different birth outcomes for each birth month ranging from december 2003 to april 2005. The solid line is the treatment group and the dotted line is the control group

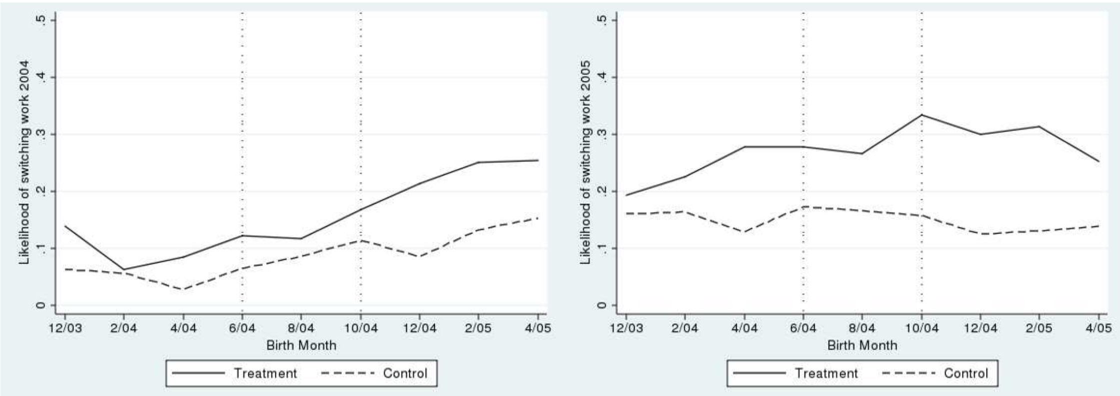


**Figure 3.** Switching occupations in 2004 and 2005 based on occupation in 2003

*Notes:* the figures show the average probability of switching occupations in 2004 and 2005 depending on occupation in 2003 for each birth month ranging from december 2003 to april 2005. The solid line is the treatment group and the dotted line is the control group.

**Table 1.** The effect of smoking ban on birth outcomes

| | Birth weight | <1000g | <1500g | <2000g | <2500 |
|---|---|---|---|---|---|
| Post *Treated | 57.69 | -.0033 | -.0189** | -.0254** | -.0006 |
| | (1.28) | (-.46) | (-2.51) | (-2.75) | (-.05) |
| Post | -13.66 | .0010 | .007 | .0058 | -.0060 |
| | (-.66) | (.25) | (1.04) | (1.14) | (-.83) |
| Treated | -54.05** | .0042 | .012*** | .012** | -.0003 |
| | (-2.21) | (1.13) | (2.95) | (2.52) | (-.03) |
| N | 4007 | 4007 | 4007 | 4007 | 4007 |
| | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
| Post *Treated | -.0255* | -.0283 | -.0494 | .0388 | -.0003 |
| | (-1.79) | (-.32) | (-.58) | (.40) | (-.02) |
| Post | .0016 | .0093 | -.0263 | .0044 | -.0135 |
| | (.24) | (.16) | (-.51) | (1.22) | (-1.32) |
| Treated | .0115 | -.0161 | -.0244 | .00246 | -.008 |
| | (1.44) | (-.30) | (-.48) | (.57) | (-.48) |
| N | 4004 | 4000 | 4000 | 4007 | 4007 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table 2.** The effect of smoking ban on mother's smoking status

| | Mother smoke start | Mother no info | Quit smoking |
|---|---|---|---|
| Post *Treated | -.0309 | .0221 | .154** |
| | (-1.05) | (.66) | (2.15) |
| Post | .0228 | .0214 | -.0758** |
| | (1.45) | (1.46) | (-2.27) |
| Treated | .0660*** | -.0019 | -.132** |
| | (3.57) | (-.15) | (-2.42) |
| N | 3004 | 3532 | 767 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table 3.** The effect of smoking ban on birth outcomes: by smoking status

| a) Mother smokes at start of pregnancy | Birth weight | <1000g | <1500g | <2000g | <2500 | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
|---|---|---|---|---|---|---|---|---|---|---|
| Post *Treated | 162.5** | -.0107 | -.0290* | -.0290 | .0065 | -.0298 | .0580 | .0069 | -.0060 | -.0261 |
| | (2.17) | (-1.18) | (-1.87) | (-1.16) | (.20) | (-.90) | (.21) | (.03) | (-.09) | (-.98) |
| N | 793 | 793 | 793 | 793 | 793 | 793 | 793 | 793 | 793 | 793 |
| b) Mother does not smoke at start of pregnancy | Birth weight | <1000g | <1500g | <2000g | <2500 | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
| Post *Treated | 14.70 | .00222 | -.0058 | -.0212 | -.0025 | -.0154 | -.0103 | -.0178 | .0131 | .0062 |
| | (.24) | (.20) | (-.53) | (-1.68) | (-.16) | (-.55) | (-.09) | (-.16) | (.32) | (.27) |
| N | 2548 | 2548 | 2548 | 2548 | 2548 | 2548 | 2548 | 2548 | 2548 | 2548 |
| c) Missing information on mother's smoking status | Birth weight | <1000g | <1500g | <2000g | <2500 | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
| Post *Treated | 105.5 | -.0144 | -.0477** | -.0323 | .0099 | -.0642 | -.0818 | -.114 | .0660 | .016 |
| | (1.27) | (-.90) | (-2.73) | (-1.16) | (.28) | (-1.64) | (-.33) | (-.45) | (.67) | (.45) |
| N | 642 | 642 | 642 | 642 | 642 | 642 | 642 | 642 | 642 | 642 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *$p<0.10$, **$p<0.05$, ***$p<0.01$.

**Table 4.** The effect of smoking ban on mother's outcomes

| | Log Income 2004 | Log Income 2005 | Sick at least 1 out of last 3 months of pregnancy | Sick all of last 3 months of pregnancy |
|---|---|---|---|---|
| Post *Treated | .0644 | -.0283 | -.0120 | .0008 |
| | (1.02) | (-.34) | (-.07) | (.03) |
| Post | .130*** | .309*** | -.121 | -.0128 |
| | (7.85) | (13.91) | (-1.01) | (-.96) |
| Treated | -.0824** | -.0104 | .0298 | .0131 |
| | (-2.36) | (-.14) | (.33) | (.91) |
| N | 3972 | 3908 | 4007 | 4007 |
| | Switch job 2004 | Switch job 2005 | | |
| Post *Treated | .043* | .0844* | | |
| | (2.02) | (1.87) | | |
| Post | .0613*** | -.0174 | | |
| | (3.89) | (-1.07) | | |
| Treated | .0347* | .0861** | | |
| | (1.99) | (2.72) | | |
| N | 2917 | 2668 | | |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table 5.** The effect of smoking ban on birth outcomes for non-switchers

| | Birth weight | <1000g | <1500g | <2000g | <2500 |
|---|---|---|---|---|---|
| Post *Treated | 71.78 | -.0082 | -.0208* | -.0275** | .0102 |
| | (1.02) | (-.84) | (-1.88) | (-2.45) | (.51) |
| N | 2625 | 2625 | 2625 | 2625 | 2625 |
| | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
| Post *Treated | -.005 | -.0110 | -.0178 | .0719* | .0094 |
| | (-.20) | (-.08) | (-.14) | (1.94) | (.38) |
| N | 2625 | 2621 | 2622 | 2625 | 2625 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table 6.** Compositional changes of switchers

| Mother | Age at birth | Wage in 2003 | Years of schooling | Smokes | Does not smoke | No info on smoke |
|---|---|---|---|---|---|---|
| Post *Treated*Switching | 1.854 | 25392 | .0776 | -.0317 | .0488 | .001 |
| | (1.38) | (.93) | (.08) | (-.24) | (.52) | (.01) |
| N | 2917 | 2917 | 2917 | 2917 | 2917 | 2917 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table 7.** Fathers: The effect of smoking ban on birth outcomes

| | Birth weight | <1000g | <1500g | <2000g | <2500 |
|---|---|---|---|---|---|
| Post *Treated | -77.09* | -.0067 | -.0036 | .0051 | .0028 |
| | (-1.90) | (-1.42) | (-.51) | (.52) | (.23) |
| Post | 24.84 | .0022 | .00322 | -.034 | -.0084 |
| | (.88) | (1.00) | (1.02) | (-.68) | (-.94) |
| Treated | -25.63 | .0041 | .0056 | -.0022 | -.0041 |
| | (-1.00) | (1.12) | (1.04) | (-.31) | (-.43) |
| N | 3941 | 3941 | 3941 | 3941 | 3941 |

| | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
|---|---|---|---|---|---|
| Post *Treated | .0114 | -.101 | .0492 | -.0159 | .0093 |
| | (.97) | (-1.33) | (1.05) | (-.58) | (.70) |
| Post | -.0064 | .123*** | .0401* | -.0102 | -.0055 |
| | (-.63) | (4.01) | (1.86) | (-.64) | (-.68) |
| Treated | -.0159* | .0206 | -.0244 | .0264 | -.0272*** |
| | (-1.96) | (.34) | (-.60) | (1.29) | (-2.75) |
| N | 3915 | 3933 | 3933 | 3941 | 3941 |

*Notes:* We use a 9 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table 8.** Placebo effects of smoking ban on birth outcomes (pretend reform is in June 2002)

| | Birth weight | <1000g | <1500g | <2000g | <2500 |
|---|---|---|---|---|---|
| Post *Treated | 25.66 | -.0011 | .0041 | .0055 | -.0000 |
| | (.81) | (-.21) | (.67) | (.70) | (-.00) |
| Post | 33.60* | -.0022 | -.0042 | -.0098** | -.0114* |
| | (1.74) | (-.61) | (-.96) | (-2.21) | (-1.78) |
| Treated | -20.37 | -.0009 | -.0038 | .0008 | .0037 |
| | (-.76) | (-.27) | (-1.23) | (.13) | (.37) |
| N | 3710 | 3710 | 3710 | 3710 | 3710 |

| | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
|---|---|---|---|---|---|
| Post *Treated | -.0097 | -.0333 | .0616 | .0116 | -.0162 |
| | (-.85) | (-.56) | (1.07) | (.27) | (-1.16) |
| Post | -.0110 | .0035 | -.0109 | .0026 | .0179** |
| | (-1.71) | (.13) | (-.51) | (.19) | (2.20) |
| Treated | -.0004 | .0598* | -.0048 | -.0114 | .0066 |
| | (-.06) | (1.77) | (-.09) | (-.76) | (.67) |
| N | 3689 | 3689 | 3710 | 3710 | 3710 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2001; age of mother at birth; mother's years of education; mother's work hours in 2001; dummies for birth order, single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table 9.** Twins FE: The effect of birth weight on outcomes at age 28

| | Log income age 28 | Log income age 28 given full time | Work full time |
|---|---|---|---|
| Birthweight | .000179*** | .0000748** | .000076*** |
| | (5.01) | (2.56) | (4.70) |
| VLBW | -.018 | -.120 | -.114*** |
| | (-.14) | (-1.43) | (-2.82) |
| <2000g | -.104* | -.0329 | -.0780** |
| | (-1.85) | (-.91) | (-3.40) |
| N | 11627 | 7235 | 15676 |

*Notes:* *p<0.10, **p<0.05, ***p<0.01.

# Appendix Figures and Tables

**Table A1.** Descriptive statistics for the treatment group and the control group before and after the reform.

| | Treated before the reform | | Treated-comparison | |
|---|---|---|---|---|
| | Mean | SD | Before | After |
| *Child* | | | | |
| Birthweight | 3444 | 717 | -63.3 | -10.4 |
| P(<1000) | .011 | .106 | .004 | .001 |
| P(<1500) | .023 | .150 | .012 | -.006 |
| P(<2000) | .034 | .182 | .013 | -.011 |
| P(<2500) | .060 | .237 | .001 | .003 |
| Pre-term birth | .064 | .246 | .007 | -.015 |
| APGAR-score after 1 min | 8.54 | 1.49 | -.01 | -.05 |
| APGAR-score after 5 min | 9.29 | 1.21 | -.01 | -.08 |
| Boy | .495 | .095 | -.024 | -.009 |
| Birth defect | .059 | .237 | -.009 | -.008 |
| *Mother* | | | | |
| Age of mother | 26.99 | 5.01 | -1.09 | -1.50 |
| Mother's years of education | 10.28 | 4.00 | -1.09 | -1.14 |
| Income 2003 (in 1,000 NOK) | 186 | 94 | -20 | -18 |
| Mother's full-time 2003* | .57 | .50 | .01 | -.03 |
| # of previous live-born children | .72 | .95 | -.03 | -.10 |
| # of children born this birth | 1.03 | .16 | -.00 | .00 |
| Income 2004 (in 1,000 NOK) | 182 | 95 | -13 | -21 |
| Income 2005 (in 1,000 NOK) | 149 | 98 | -17 | -21 |
| Mother's full-time 2004* | .59 | .49 | .03 | -.07 |
| Mother's full-time 2005* | .56 | .50 | -.00 | -.04 |
| Sick 1/3 final months | .43 | .50 | .022 | .005 |
| Sick 3/3 final months | .057 | .232 | .011 | .013 |
| Mother smoked start+end | .197 | .399 | .079 | .052 |
| Quit smoking during pregnancy** | .393 | .491 | -.121 | .028 |

*Notes:* * Work hours are measured as: (1=less than 20 h/pw, 2= 20-29 h/pw , 3=30+ h/pw), ** Reported smoking at start of pregnancy

**Table A2.** The effect of smoking ban on birth outcomes: all mothers not in restaurant/bars as controls

| | Birth weight | <1000g | <1500g | <2000g | <2500 |
|---|---|---|---|---|---|
| Post *Treated | 53.27 | -.0026 | -.0154** | -.0207** | -.0048 |
| | (1.28) | (-.39) | (-2.36) | (-2.55) | (-.43) |
| N | 47174 | 47174 | 47174 | 47174 | 47174 |
| | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
| Post *Treated | -.0240* | -.0646 | -.097 | .0196 | -.0126 |
| | (-1.88) | (-.94) | (-1.40) | (.89) | (-.73) |
| N | 47174 | 47174 | 47174 | 47174 | 47174 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table A3.** The effect of smoking ban on birth outcomes: restaurant/bar workers in 2001 as control

| | Birth weight | <1000g | <1500g | <2000g | <2500 |
|---|---|---|---|---|---|
| Post *Treated | -9.7 | .0021 | -.0146** | -0.0159 | -0.0340* |
| | (-.21) | (-.41) | (-2.56) | (-1.66) | (-2.03) |
| N | 3098 | 3098 | 3098 | 3098 | 3098 |
| | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
| Post *Treated | -0.0240* | -0.0303 | -0.114 | .030 | .0152 |
| | (-2.00) | (-0.24) | (-1.25) | (1.00) | (.96) |
| N | 3089 | 3088 | 3089 | 3098 | 3098 |

*Notes:* We use a 5 month window, we cluster on treated*birth month, controls included in regressions are mother's income in 2003 (income in 2001 for control group); age of mother at birth; mother's years of education; mother's work hours in 2003 (work hours in 2001 for control group); dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table A4.** The effect of smoking ban on mothers' characteristic

| | Mothers' years of education | Mother's age at birth | Mother's ln income 2003 |
|---|---|---|---|
| Post *Treated | -.0448 | -.406 | .00293 |
| | (-.12) | (-1.19) | (.04) |
| Post | -.0475 | .0384 | -.0956*** |
| | (-.44) | (.19) | (-3.78) |
| Treated | -1.092*** | -1.090*** | -.156*** |
| | (-3.40) | (-5.02) | (-3.40) |
| N | 4029 | 4029 | 4029 |

*Notes:* We use a 5 month window. No controls. *p<0.10, **p<0.05, ***p<0.01.

**Table A5.** Robustness to window

| | 4 months | 5 months | 6 months | 12 months | 24 months |
|---|---|---|---|---|---|
| Birthweight | 54.44 | 57.69 | 17.32 | 21.70 | 23.92 |
| | (1.09) | (1.28) | (.39) | (.67) | (1.08) |
| N | 3172 | 4007 | 4733 | 9251 | 17604 |
| | 4 months | 5 months | 6 months | 12 months | 24 months |
| VLBW | -.0240** | -.0189** | -.01999*** | -.0124*** | -.0062 |
| | (-2.87) | (-2.51) | (-3.08) | (-2.73) | (-1.65) |
| N | 3172 | 4007 | 4733 | 9251 | 17604 |

*Notes:* We cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table A6.** Robustness to adding control variables

| | No controls | Add mother's characteristics | Cluster birth month* treated*county |
|---|---|---|---|
| VLBW | -.0181** | -.0186** | -.0189* |
| | (-2.19) | (-2.48) | (-2.00) |
| N | 4007 | 4007 | 4007 |

*Notes:* We cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

**Table A7.** The effect of smoking ban on birth outcomes: including intermediary period of June-October 2004

| | Birth weight | <1000g | <1500g | <2000g | <2500 |
|---|---|---|---|---|---|
| Post *Treated | 44.23 | -0.00185 | -0.0153*** | -0.0209*** | -0.00178 |
| | (1.36) | (-0.38) | (-2.77) | (-2.96) | (-0.19) |
| N | 6160 | 6160 | 6160 | 6160 | 6160 |
| | Pre-term birth | APGAR 1 min | APGAR 5 min | Boy | Birth defect |
| Post *Treated | -0.0191 | -0.0613 | -0.0345 | 0.0224 | 0.00242 |
| | (-1.65) | (-0.95) | (-0.51) | (0.89) | (0.13) |
| N | 6161 | 6145 | 6143 | 6160 | 6193 |

*Notes:* We use a 9 month window (since we include the first 3 months after the reform change), we cluster on treated*birth month, controls included in regressions are mother's income in 2003; age of mother at birth; mother's years of education; mother's work hours in 2003; dummies for birth order; single birth; and dummies for each county of birth (19 counties). *p<0.10, **p<0.05, ***p<0.01.

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