Sober mom, healthy baby? Effects of brief alcohol interventions in Swedish maternity care♥

by

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Abstract

A large body of research documents the importance of early life conditions for the health and human capital formation of children. The detrimental effects of alcohol exposure in utero are well documented, and therefore identifying effective methods for preventing harmful maternal alcohol consumption is of great importance. We exploit the stepwise introduction of alcohol screening and brief interventions at Swedish antenatal clinics, to evaluate the causal effect of enhanced alcohol prevention on infant health using a difference-in-differences strategy. We find that the program improves infant health measured by prescription of pharmaceutical drugs and hospitalizations during the child's first year of life. The results suggest that effects are likely driven by changes in maternal behavior after the first trimester and seem to extend beyond the birth of the child.

Keywords: Alcohol; Brief intervention; MI; AUDIT; Maternity care; Child health
JEL-codes: I12; I18

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1 Introduction

A large literature has shown that alcohol exposure in utero has adverse effects on health and human capital (see e.g. Wüst, 2010; Zhang, 2010; von Hinke et al., 2014; Nilsson, 2015).[1] This has led the World Health Organization to recommend women to abstain from alcohol during or when planning pregnancy. But ambiguity regarding the effects of moderate alcohol consumption during pregnancy has lead to a questioning of the strict recommendations to completely abstain from alcohol (see for example Oster; 2013). Moreover, pregnant women may not follow the recommendations. Despite strict recommendations in Sweden Göransson et al (2003) finds in a survey that about 30 percent of the pregnant women used alcohol regularly. Identifying effective methods for preventing *harmful* fetal alcohol exposure, and more generally to find interventions that improve child health, is therefore of great importance.[2] It is also important to understand in what way enhanced preventive interventions against health hazards in utero affect health and early development of children. The contribution of this paper is to do just that.

We exploit regional time variation in the introduction of a screening and brief intervention (BI) program for alcohol in Swedish antenatal clinics from 2004 to analyze the effects of enhanced alcohol prevention on child health and maternal behavior during the first years of life. The program consists of three parts: (i) screening for risky alcohol consumption in gestation week 8-12 using the Alcohol Use Disorder Identification Test (AUDIT) instrument as a pedagogic tool to screen and inform about risks (ii) using Motivational Interviewing (MI) techniques to modify behaviour; and (iii) referral to treatment for those identified as needing more extensive treatment and and specialized

[1] The insight that the fetus is not protected from harm in utero has gained recognition since the 1960s. The documentation of the severe side effects of Thalidomide in the 1960’s (McBride, 1961; Von Lenz and Knapp, 1962) and of adverse effects of alcoholism in the early 1970’s (Jones, Smith, Ulleland and Streissguth, 1973) was important for establishing the vulnerability of the fetus. These and other findings lead Barker (1990) to formulate the Fetal origins hypothesis. There is now a large empirical literature documenting *effects* on health and human capital of fetal exposure to toxic substances (Chay and Greenstone 2003; Almond, Edlund and Palme, 2009; Currie, Niedell and Schmeider, 2009; Currie, Greenstone and Moretti, 2011; Currie and Walker 2011; Black et al. 2013), maternal health shocks (Almond 2006), malnutrition (Lindeboom, Portrait and van der Berg 2010; Almond and Mazumder, 2011; Doblehammer, van der Berg and Lumey 2011;), maternal stress (Currie and Rossin-Slater, 2013; Lindo, 2011), economic conditions (van der Berg, Lindeboom and Portrait, 2006; van der Berg, Doblhammer and Christensen 2011), and alcohol (Wüst, 2010; Zhang, 2010; von Hinke et al., 2014; Nilsson, 2015). The Fetal origins hypothesis is discussed at length by Almond and Currie (2011).

[2] Prenatal exposure to alcohol is identified as an important *preventable* cause of mental retardation with large medical and social costs (Abel and Sokol, 1987; West and Blake, 2005).

care.[3] The roll out involved a major effort to train midwives in motivating behavioral change using MI-techniques; a training likely to have enhanced the midwives' ability to encourage health promoting behaviors also in domains other than alcohol.

By studying heterogeneities—by type of medication and diagnosis, by age and socioeconomic status of mothers, and by sex of the child as well as the impact on the sex-ratio at birth in addition to some measures of maternal smoking and breast feeding—our aim is to provide insights into the mechanisms through which screening and BI for alcohol in antenatal care can affect child health. In particular, we are interested in tracing out effects on health likely to stem from reductions in alcohol consumption, and effects running through more general improved health related behaviors.

Interest in effectiveness of universal alcohol prevention programs as an integral part of antenatal care is motivated by a growing literature of well identified studies establishing a causal link between alcohol exposure in utero and negative birth outcomes (Wüst 2010 and Zhang 2010), school outcomes, educational attainment, labor market outcomes and a lower ratio of boys to girls (Nilsson, 2016) in observational data. While the negative effect of excess alcohol exposure, and binge drinking, has been widely accepted, the recent evidence puts a focus on likely negative effects also of low and moderate consumption (von Hinke et al. 2014). This recent evidence questions a large number of observational correlation studies suggesting that the risks of moderate consumption are ambiguous and depend on the nature of alcohol consumption (see meta studies by Polygenis et al. 1989; Abel and Hannigan, 1995). In particular, this literature typically finds positive associations between wine consumption and low dose drinking throughout pregnancy and negative associations between beer consumptions and binge drinking, and child outcomes. von Hinke et al 2014 and Wüst 2010, however, illustrate how positive insignificant associations between alcohol consumption turn negative as the selection bias in the nature of maternal alcohol consumption is properly accounted for.

Interest in the effectiveness of BI the programs in antenatal care is also motivated by the large body of research on BI using MI. Such interventions are common and claimed to be effective in a number of areas of health: diabetes care, weight loss, smoking

[3] The literature also refers to this type of public health program as SBIRT: Screening, Brief Intervention and Referral to Treatment, see eg Young (2014) et al for a review.

session, drug or alcohol addiction and in promoting reductions in risky behaviours (Rubak et al, 2004). However, in reviewing a large number of reviews, O'Donnell et al (2013) conclude that the evidence regarding interventions during pregnancy is rather weak.[4] Moreover, studies of large scale BI-programs in primary care for general populations are rare and so is the evidence on effects of alcohol prevention on child health. To our knowledge this is the first attempt to evaluate the effects of a population wide nationally implemented screening and BI-program in maternity care on child and maternal behaviour and health outcomes.[5]

Due to training constraints, not all antenatal clinics were able to introduce AUDIT screening and MI simultaneously (Socialstyrelsen, 2008). This resulted in a staggered introduction of the program across antenatal clinics in Sweden so that similar mothers giving birth 2003-2009 faced different screening and alcohol prevention regimes depending on where they lived and when they were pregnant. This allows us to estimate reduced form effects of the program, with a difference-in-differences strategy. We use rich administrative data on prescription drug and hospital care consumption (including detailed information on chemical classification and diagnosis) to construct measures of health of mothers and children, for the universe of children born in Sweden during the period when the screening was implemented. In an additional analysis we use a similar strategy to estimate reduced form effects on self-reported maternal behaviors and child health exploring a survey data set collected by the midwifes at the antenatal clinics covering some 70 percent of births during the years 2003-2008.

We find that the program improves infant health, both as measured by pharmaceutical drugs and by inpatient care utilization during the first year of life. We also find evidence of reduced maternal smoking during pregnancy, and suggestive evidence of increased breastfeeding and improved maternal health. In particular, we find that screening significantly lowered the probability of children being prescribed a pharmaceutical drug during their first year of life by 0.43 percentage points, which implies a reduction of 8.4 percent relative to the population average, and lowered the

[4] A similar conclusion is drawn regarding other types of informational interventions to increase awareness of the risks of alcohol during pregnancy using various forms of media such as commercials, pamphlets etc (Crawford Willims et al 2015).

[5] Nilsen et al 2012 analyze maternal self-reported (but anonymous) drinking habits pre-pregnancy and during pregnancy for mothers registered in antenatal care before and after the program was implemented in the municipality of Linköping. They find no significant differences in reported drinking habits but they do find improved perceptions of and a more positive attitude to the alcohol information received from the midwife.

probability of children being admitted to hospital during their first year of life with 7.5 percent. We find that the health effects are mainly driven by reductions in prescriptions related to infections and by reductions in inpatient care due to injury and 'avoidable' conditions (e.g. asthma, diarrhea and infections) which would not have required hospitalization if the child had access to timely and effective preventive or primary care. We find no effects on conditions that could be connected to congenital malformations or perinatal conditions and complications at birth that would be associated with heavy alcohol exposure in early gestation such as the FAS syndrome. Moreover, we do not find an effect on the sex ratio at birth nor do we find differential health effects by sex of the child. This suggests that the program did not limit hazardous alcohol consumption in early gestation, which is not surprising given that mothers are first exposed to the program towards the end of the first trimester. Instead, the results are consistent with the interpretation that the brief alcohol intervention reduced alcohol exposure later in the pregnancy, leading to improvement in children's immune system. The effects on avoidable conditions and injuries, as well as effects on maternal health and smoking cessation also point to behavioral effects that extend beyond the duration of the pregnancy and improve the early childhood environment. Effects on smoking suggest that the training midwives in MI can improve their ability to support health promoting behaviours in general and not only behaviors related to alcohol.[6]

This paper is a contribution to the literature on the importance of in utero and early life conditions for child health by illustrating the importance of alcohol exposure and maternal behavior for child health. More specifically it is a contribution to our understanding for how policy interventions can impact child development. Our paper thus also contributes to the literature estimating effects of BI in general, and brief alcohol inventions in antenatal care in particular. Showing that the screening and BIprogram improves child health and maternal behaviours when implemented within the context of universally available antenatal care is an important argument for promoting such policy initiatives.

A further contribution of this paper is to the wider literatures on screening and information interventions and alcohol prevention in particular (O'Donnell et al (2013)). Sexton and Hebel (1984), early on showed that informing pregnant women about the

[6] Smoking and possibly also other substance use may be complementary to alcohol consumption, which means that reductions in alcohol consumption may in fact also lead to reduced smoking, as is found in Wüst (2010).

dangers of smoking and how to quit, significantly affects children's birth weight and length. Screening for alcohol use prior or during pregnancy in a context where attitudes towards heavy alcohol consumption during pregnancy are very negative is challenging due to stigma. The design of AUDIT makes it better fit to detect more risk behaviors, in particular among the mothers who have adopted more continental, everyday alcohol consumption patterns compared to previous alcohol screening focusing on consumption while pregnant.[7]

The rest of the paper is organized as follows. The following section reviews the literature on prenatal health and alcohol exposure. Section 3 summarizes antenatal care policies in Sweden and discusses the new screening and brief intervention program. In Section 4, we describe the empirical strategy and Section 5 describes the data. Finally, Section 6 reports the results from the main analysis and Section 7 reports the results using survey data. Section 8 concludes.

2 Prenatal health and alcohol exposure

A large body of research documents the detrimental effects of severe alcohol exposure in utero (Abel, 1984, Streissguth et al., 1994). The most severe diagnosis associated with fetal alcohol exposure is Fetal alcohol syndrome (FAS) and includes a combination of congenital anomalies combined with confirmed maternal alcohol consumption during pregnancy, with the main symptoms being growth deficiency (both pre- and postnatal), FAS-specific facial features, and central nervous system damage causing cognitive and functional disabilities. Fetal alcohol spectrum disorders (FASD) is a non-diagnostic term for permanent birth defects (Sokol, Delaney-Black and Nordstrom, 2003), and includes a broader spectrum of growth deficiency and cognitive and psychosocial impairments and disabilities caused by the mother's consumption of alcohol during pregnancy (Streissguth et al. 1996; Clark and Gibbard, 2003; Riley and McGee, 2005). While effects on the physical development of organs and extremities may be more affected at the early stages of gestation, there are reasons to believe that the development of the central nervous system and the brain as well as fetal growth and birth weight are sensitive to alcohol exposure throughout the pregnancy (eg Guerri, 2002).

[7] See Burns et al (2009) for a comparison of screening tools.

Although the link between heavy alcohol exposure and FAS is widely accepted, there are surprisingly few studies that can convincingly identify a causal relationship between changes in alcohol availability or consumption and child health in a general population of mothers.[8] There are, however, a growing number of well identified studies utilizing sales restrictions to document the detrimental effects of maternal alcohol consumption on child outcomes at the population level (Zhang 2010, Fertig and Watson 2010, and Nilsson 2015).[9] Zhang (2010) examines the relationship between drinking during pregnancy and infant birth outcomes utilizing changes in state-wide alcohol taxation. She finds that higher alcohol taxes reduce binge drinking among pregnant mothers and improves birth outcomes of children. This result is partly due to selection into motherhood, as unplanned pregnancies are more likely for women engaging in binge drinking.[10] Similarly, Fertig and Watson (2010) find that changes in state minimum drinking age laws have effects on infant health mainly by affecting the composition of families: alcohol availability by young adults is associated with more unplanned pregnancies, in particular among low SES parents. Composition effects are also found by Nilsson (2015) who studies a temporary (8.5 month) policy experiment of less restrictive sales rules for strong beer in two Swedish regions in the 1960's. The experiment increased the availability of alcoholic beer for youths below an age of 21 which increased alcohol consumption, most likely in the form of binge drinking. Nilsson also finds detrimental long run effects from alcohol exposure in utero in terms of substantially lower earnings, wages, educational attainments, and cognitive and non-cognitive ability. The negative effects on earnings are found throughout the distribution but are largest below the median. The detrimental effects of increased alcohol availability are found to be strongest for fetuses exposed at early stages of the pregnancy, resulting in a higher than normal ratio of boys to girls and worse outcomes (educational attainment and earnings), in particular for boys. [11]

[8] See discussion in Nilsson (2015) for a discussion of the earlier mainly observational studies.

[9] Barreca and Page (2013) are however unable to find a significant effect.

[10] The health of unplanned children is often worse since these children are more often born to lower SES mothers.

[11] Effects on the sex-ratio, implying a lower ratio of boys to girls, are typically associated with negative shocks or presence of maternal stressors at the time of conception or during the first half of the pregnancy (Valente 2015). This effect is driven by selection at conception but also by spontaneous abortions and can be the result of different mechanisms with different implications for the sex difference in health of the children, conditional on live birth. Almond and Currie, 2011 find evidence of scarring, i.e. that differential survival would be the result of deteriorating maternal health during pregnancy resulting in a low sex-ratio and a sex gap in health at birth to the favour of girls. This is consistent with the findings of Nilsson 2015. Catalano et al 2008, however find evidence of so called culling, i.e. that the survival threshold of boys has shifted to the right such that surviving boys are in fact in better health.

These studies suggest that maternal alcohol consumption, in particular the alcohol consumption of young mothers, is influenced by increased access to alcohol and that this increased consumption is harmful for children. von Hinke et al (2014) instead use so called Mendelinan randomization as a source of exogenous variation to identify effects of fetal alcohol exposure on the educational attainment of UK children. Information on maternal genotypes of a particular gene, shown to influence alcohol metabolism and consumption, is used to instrument for alcohol use during pregnancy. Because carrying this variant of the gene affects alcohol consumption across the distribution and at all ages, they are able to study effects of low or moderate consumption in a representative population of mothers. The interesting feature with this study is that it is first to clearly show that selection is the reason why OLS results indicate positive effects of wine consumption and moderate drinking throughout the pregnancy and negative effects of beer consumption and binge drinking. IV-estimates, instead are consistently negative suggesting that alcohol exposure is negative for educational attainment and that more alcohol, more binge drinking and longer exposure during the pregnancy is worse. Because the gene variant is likely to affect maternal alcohol consumption also after birth, it cannot be ruled out that both in utero and childhood exposure to maternal alcohol consumption matter for child outcomes.

In a study on Danish register data, Wüst (2010) instead uses a sibling fixed effect approach to study the effects of alcohol consumption on child outcomes. She finds that controlling for selection using siblings turns the insignificant association between alcohol consumption and birth outcomes into a significant negative effect. As in the study of UK mothers, this reflects that mothers are positively selected into alcohol consumption during pregnancy. She also finds a dose–response relationship such that more drinking causes more harm, rather than finding that the effects are driven only by excessive consumption.

3 Antenatal Care, Screening and Brief Interventions

Sweden has an extensive system of antenatal clinics, with an objective not only to strengthen parents in their parental role but also to detect and prevent poor health and offer support to mothers. The care received at the antenatal care clinics is free of charge and easy accessible. Health education is an important aspect of antenatal care and focuses mainly on lifestyle changes during pregnancy. Nearly 100 percent of all expecting mothers are enrolled in maternity care services delivered primarily through municipality-based public antenatal clinics (Socialstyrelsen, 2005); around 520 clinics in Sweden care for the about 100 000 pregnant women annually. During uncomplicated pregnancies, women typically have 6-10 prenatal visits to the antenatal clinic. The focus of the first visit, which occurs around week 8-12 of the pregnancy, is primarily to make a physiological assessment and providing information about pregnancy. An important aspect of health care during pregnancy is to identify risks and conditions—both medical and psychosocial—which can affect the pregnancy, the delivery, and the development of the fetus. By covering nearly all pregnant women in Sweden, the antenatal clinics have a strategic position in detecting and preventing prenatal alcohol exposure, and to provide support to women who experience difficulties stop drinking alcohol during pregnancy.

In 2004 the “Risk Drinking project” was initiated in the Swedish maternity care in response of a growing concern for changed alcohol consumption patterns following Sweden’s entry to the EU. In particular, the alcohol consumption among women aged 28-38 increased during the late 1990’s (Bergman and Källmèn, 2003). Since consumption of alcohol most likely reflects established habits, this may have consequences for women's attitudes towards alcohol during pregnancy (Göransson, 2004). The Risk Drinking project was a nationwide effort to implement a brief alcohol intervention as an integral part of routine care. The project was run and financed by the Swedish Public Health Agency and had a large impact on the antenatal clinics’ alcohol preventive work by promoting the use of the AUDIT-instrument to detect hazardous alcohol consumption (Socialstyrelsen, 2009); by introducing and providing training in MI as a tool for motivating reduced alcohol consumption; and by extra councelling and referral to specialists for mothers with risky alcohol consumption.[12]

AUDIT is a 10-item questionnaire, developed by WHO, covering three areas: consumption, addiction, and alcohol related damages (Babor et al., 2001)[13]. The AUDIT-instrument was adapted for use in antenatal clinics by asking, not about present

[12] MI is developed in Miller 1983 and Miller and Rollnick, 1991. See also third edition from 2012 for two decades of evidence in different contexts.
[13] See Appendix B for the AUDIT questionnaire.

but rather, about pre-pregnancy alcohol behavior, and was promoted as a pedagogic tool to be used at the first visit at the antenatal clinic around week 8-12 of the pregnancy. The AUDIT questionnaire is filled out by the midwife or by the mother and is used as a basis for talking about alcohol habits. During the interview the midwife informs about risks with alcohol during pregnancy with the explicit purpose of motivating behavioral change among those who consume alcohol during pregnancy. This involves a motivational discussion exploring habits and the mother's own positive and negative attitudes towards alcohol while maintaining an empathic, non-judgmental atmosphere. Based on the woman's own ambivalence towards alcohol, the role of the midwife is to strengthen the woman's own arguments against drinking by providing facts about the risks for the fetus. It is important that this is done in a compassionate way so as to avoid arguments and negative feelings that might evoke a defensive attitude.[14] One strength with AUDIT is its sensitivity and high specificity—compared to other screening instruments—in detecting risky consumption at different levels of alcohol use and problems (Saunders et al., 1993, Reinert and Allen, 2007). Another strength is that it is focused on women's alcohol consumption prior to pregnancy.[15] Women are more likely to answer truthfully about pre-pregnancy consumption, and pre-pregnancy alcohol intake has been shown to be a good predictor of the alcohol consumption during pregnancy (Göransson et al., 2003).

AUDIT reports alcohol behavior on a 0-40 scale, where a higher score indicates more hazardous alcohol consumption. Originally the cut-point for identifying at-risk drinking behavior in the general population to was set to 8. Studies later showed that the cut-point for women should be set lower and values of 5-6 or even as low as 3 has been suggested for identifying at-risk drinking among females (Reinert and Allen, 2007).[16] Midwives provide all pregnant women with general information on alcohol and pregnancy, but if a woman scores a value of 6 or higher on AUDIT the midwife initiates a BI using MI-techniques to with the goal of motivating the woman to modify behavior,

[14] See eg Handmaker and Wilborne (2001).

[15] It is widely recognized that obtaining reliable self-reports of women's alcohol use during pregnancy is difficult because of stigma and because of uncertainty about what entails risky consumption (Gray and Henderson, 2006).

[16] . Among those diagnosed as having hazardous or harmful alcohol use in a general population, 92% had an AUDIT score of 8 or more, and 94% of those with non-hazardous consumption had a score of less than 8 (Saunders et al., 1993). AUDIT scores in the range of 8-15 is found to represent a medium level of alcohol problems whereas scores of 16 and above represented a high level of alcohol problems. Since the effects of alcohol vary with average body weight and differences in metabolism, lowering the cut off for women with one point—i.e. to an AUDIT of 7—will increase sensitivity for this population groups (Babor et al., 2001).

the woman is also invited back for more frequent visits. If the midwife considers it necessary, or if the woman gets a very high AUDIT score, referral to other professions such as counselors, the social service, and/or an alcohol dependency clinic will also follow (Statens folkhälsoinstitut 2014; Damström Thakker, 2011; Västra Götalandsregionen 2008). Thus, the interventions are aimed at motivating and encouraging behavioral modification rather than coercion or merely providing health information.

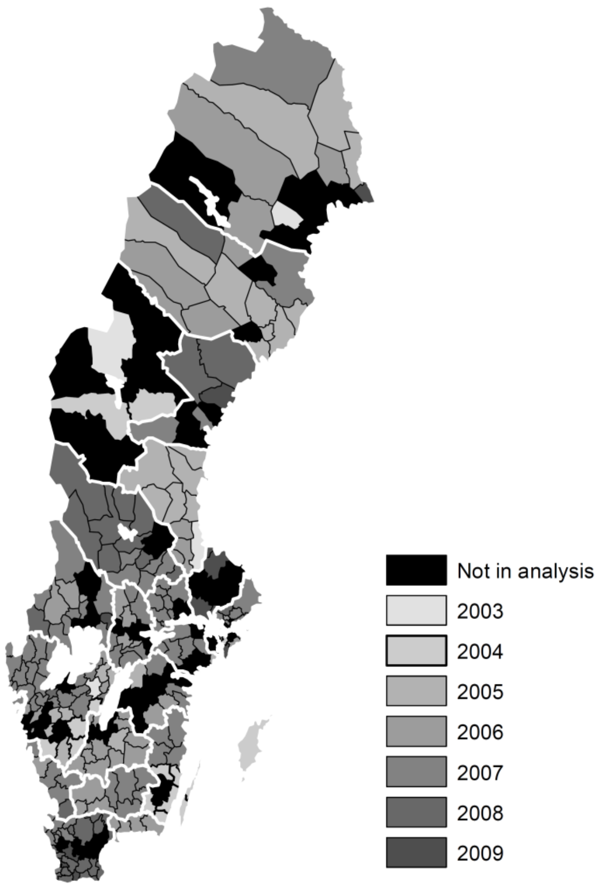
During the roll out of the Risk drinking project midwifes were trained in using AUDIT as well as in motivational interviewing techniques. The training programs were organized by the coordinating midwives at the county level. Training involved a full day training program on the risks of alcohol consumption during pregnancy and how to use the AUDIT questionnaire in antenatal care. A further important part of the program was training in MI techniques. This part of the program involved 3-4 days of training and recurring visits by instructors at the antenatal clinics in order to follow up and support implementation of AUDIT and MI. A limited number of lecturers and instructors were involved in these training programs and hence time constraints implied that it took some time to train midwifes in AUDIT and MI.[17] As a result the program was gradually adopted by antenatal clinics. By 2010, 92 percent of the clinics had introduced AUDIT (Socialstyrelsen, 2008).[18]

In an evaluation of the Risk Drinking project, the National Board of Public Health (Statens Folkhälsoinstitut, 2010) found that the fraction of midwives who thought they had good or very good knowledge about the risks of alcohol during pregnancy rose marginally between 2004 and 2009, from 94 to 99 percent. During the same period, the fraction who judged their ability to identify at risk mothers as good or very good rose from 60 to 92 percent. In a survey of Stockholm midwives, midwives regarded MI-training, in particular, as very important in strengthening their ability to talk to mothers about alcohol (Damström Thakker, 2011)

[17] In Figure A1 in Appendix A we describe the gradual implementation of the AUDIT-MI-program.
[18] For a detailed account of the training program and implementation see eg Nilsen et al 2011.

4 Empirical strategy

To estimate the reduced form effects of a brief alcohol intervention on infant health and maternal behavior, we use a difference-in-differences approach where we utilize the staggered implementation of the program across antenatal clinics within counties. Although antenatal clinics are municipality based, health care in Sweden is organized at the county level: 290 municipalities are divided into 21 counties which are responsible for the provision of health care. For this reason there is some regional variation in the organization and practices across different counties, which may affect health care utilization (Socialstyrelsen, 2011). We will therefore focus on within-county variation between municipalities in the timing of implementation to identify the effects of the screening program. Figure 1 illustrates how the gradual increase in the share of antenatal clinics implementing the program yields a substantial municipal variation within counties (except for the counties of Uppsala, Jönköping, Gotland, Blekinge, and Västmanland) in the years before 2010.[19]



[19] 33 municipalities are excluded from the analysis because the clinics within the municipality introduced the in different years. Sample restrictions are discussed in Section 5.1.

Figure 1. Regional implementation of the program by year

Mothers are regarded as treated by the BI if they—during the first four months of the pregnancy—live in a municipality where the antenatal clinics have implemented the AUDIT-MI-program, and the control group is pregnant women in other parts of the county where the program has not yet been introduced. The empirical model is given by:

$$y_{ickt} = \alpha + \beta Treatment_{kt} + \gamma_k + \eta_{ct} + \theta bm_i + \boldsymbol{X}_i\lambda + \boldsymbol{K}_{kt}\lambda + \varepsilon_{ickt}, \quad (1)$$

where y_{ickt} is the outcome of child/mother i in municipality k in county c year t. With γ_k being a vector of municipal fixed effects, and η_{ct} a vector of county specific time effect, the variations between municipalities within a county identify the effect. $Treatment_{kt}$ is an indicator taking the value 1 if the mother was screened and 0 otherwise. In order to control for seasonal patterns in infant health and drinking patterns we include an indicator for birth month, bm_i. $\boldsymbol{X}_i$ is a vector of controls for predetermined family characteristics. There is a social gradient both in child health (Cutler et al., 2008 and Mörk et al., 2014) as well as in drinking and awareness of the detrimental effects of alcohol consumption during pregnancy habits (Bergman and Källmén, 2003). We therefore include the following characteristics as controls: mothers' and fathers' age; immigrant status and educational level of the mother; whether the parents live together in the year that the child was born; and sex of the child. We also include municipal unemployment level and municipal alcohol sales per capita in the regression to control for time-varying differences in municipal characteristics, $\boldsymbol{K}_{kt}$. The coefficient of interest is β, which is the estimate of the treatment effect. Standard errors are clustered at the municipal level.

The main identifying assumption is that the timing of implementation is unrelated to changes in infant health and maternal alcohol consumption. And since the timing of implementation was determined by when midwives could be scheduled for training in AUDIT and MI, rather than motivated by alcohol consumption patterns we believe that the parallel trends assumption is fulfilled. The assumption is corroborated with a number robustness tests in section 6.6.

A potential threat to the identification comes from Swedish mothers being free to choose antenatal clinic. Mothers could therefore select into clinics based on their alcohol preventive practices: a woman with risky alcohol consumption could for example choose a clinic without screening if she is reluctant to reveal a potential abuse. In order to avoid this selection problem we restrict our attention to municipalities with only one antenatal clinic or municipals where all clinics implemented screening at the same time. The problem of varying screening practices, and the scope for clinic choice, is more pronounced in larger cities and in section 6.5 we present sensitivity analyses with regard to excluding these municipalities.

Another potential threat to the identification strategy is that mothers who was exposed to the program at the antenatal clinic may also have been exposed to new alcohol preventive strategies elsewhere, e.g. at child health clinics after the child was born. Although not as well documented, the implementation of the Risk Drinking project in child health clinics was not coordinated with the implementation effort at antenatal clinics. In fact, child health clinics initiated the Risk Drinking project a few years later and in 2009 the fraction of child health nurses with MI training was substantially lower than the corresponding fraction of midwives (Statens Folkhälsoinstitut, 2010).

4.1 Hypotheses

In order to assess how a brief alcohol intervention for pregnant women affects infant health we analyze heterogeneities by different domains of infant health, by sex of the child and by socioeconomic status. The previous literature suggests that the type and timing of fetal alcohol exposure may give rise to different consequences. Exposure in early stages of gestation and heavy exposure through binging are likely to result in a skewed sex-ratio at birth (selectivity at conception and spontaneous abortion is more likely for boys) and potentially worse outcomes for boys (Valente, 2015)[20]. Long run, but moderate, exposure throughout the pregnancy, on the other hand, is more likely to have detrimental effects on the development of the central nervous system, the brain as well as fetal growth and birth weight (Guerri, 2002).

[20] See Valente, 2015 for a thorough discussion of these mechanisms. Almond and Currie, 2011 find evidence of scarring, i.e. that differential survival would be the result of deteriorating maternal health during pregnancy resulting in a low sex-ratio and a sex gap in health at birth to the favour of girls. This is consistent with the findings of Nilsson 2015. Catalano et al 2008, however find evidence of so called culling, i.e. that the survival threshold of boys has shifted to the right such that surviving boys are in fact in better health.

In order to capture effects of early and heavy alcohol exposure we specifically look at sex ratio at birth and gender heterogeneities in outcomes. Because the screening and BI takes place towards the end on the first trimester, we should not expect it to have any effects on alcohol exposure at the early stages of the pregnancy. Moreover, heavy abuse is likely to have been detected also before the introduction of the studied program. We therefore do not expect effects on congenital malformations. To capture effects of fetal exposure throughout the pregnancy we instead look at *infections* which, as an increased sensitivity or reduced immune function related to birth weight and fetal growth. In addition, we study the most common diagnoses received at the hospital among young children, i.e. *perinatal diagnoses*, and *respiratory* conditions. Although these categories of diagnoses are more difficult to directly link to type of exposure they are more common among children with low birth weight.[21]

In order to capture post natal behavioral changes of the mother we look at *injuries* and a set of conditions which are considered as *avoidable* hospitalizations in the sense that appropriate care and nutrition are likely to reduce their incidence (Page et al. 2007).[22]

The program was designed to better detect at risk mothers and the nature of alcohol consumption varies by maternal characteristics: younger and less educated women are more likely to engage in weekend binge drinking, whereas older and more educated women are more likely to have a consumption pattern with small or moderate quantities of alcohol on a more regular or every day basis (Wüst, 2010 and von Hinke Kessler Scholder, 2014). Differential effects by maternal age and education may thus pick up heterogeneous responses to the BI.

Although he program was focused on alcohol prevention, it is possible that other behaviors are affected. Effects on *smoking* behavior could be a consequence of reduced alcohol consumption, but could also be a spill-over effects of MI training to other areas of health promotion, as could effects on *breastfeeding*.

[21] Since respirator diagnoses include both admissions for asthmatic problems, croup, RS-virus and throat infections, we combine them with diagnoses for eye and ear infections in the admission analysis.

[22] These "avoidable" hospitalizations are admissions for certain acute illnesses and worsening chronic conditions that might not have required hospitalization if they had been managed through timely and effective utilization of primary care and through patient behavior. Note that all such hospitalizations cannot be avoided. Avoidable conditions fall into three categories: vaccine preventable, acute conditions, and chronic conditions; that, if managed well, should not require hospital admission. We use the definition for children suggested by the Public Health Information Development Unit in Australia (Page et al. 2007). Table A1 in Appendix A lists diagnoses groups and the ICD codes included as well as the ATC codes for the categories of drugs.

5 Data

In the main analyses we combine data from administrative registers—e.g. the Population register, Hospital Discharge register and the Prescription Drug register—with clinic level survey data on the implementation of the program from the Maternity Health Care Register, information from different data sources. We describe these data below. In auxiliary analyses we also use individual level survey data from the Maternity Health Care Register. We describe these data in section 7 in connection to the results.

5.1 Study population and screening

Our study population in the main analysis consists of all first-born children in Sweden born 2003-2009 and their parents. The population is identified through the population register held at Statistics Sweden. It covers all Swedish residents with information on year and month of birth, birth order and with a link to the biological parents. The analysis will focus only on first-time mothers since we do not want information given during earlier pregnancies to influence the results. Moreover, given the possibility that the BI affects the probability of having a second child, we avoid biases introduced by selection in second births by focusing on first borns. The sample is also restricted to include only children who are born in Sweden, since we want to make sure that the mothers have been exposed to Swedish maternity care.

For each parent we retrieve information on socioeconomic background characteristics from Statistics Sweden's based on administrative records and population censes; specifically: educational attainment, annual labor income, age, and municipality of residence. The information on educational attainment is based on a 3-digit code, which is a Swedish version of the International Standard Classification of Education 1997. For earlier cohorts covered by this register, and for immigrants, information on educational attainment is obtained from census data, whereas the data for later cohorts come directly from educational registers of high quality. The information on labor income stems from data that employers are mandated to report to the tax authorities for income tax declaration purposes. These data are matched with information on alcohol prevention practice at the municipal level using the municipality of residency of the mother.

Data on the alcohol prevention at each antenatal clinic was collected from the Maternity Health Care Register. It is managed by the medical profession and was initiated in 1999 in order to improve the quality and to enable monitoring and evaluation of the maternal health care, with an aim to create a basis for evidence-based medicine. The register is based on a local organization of participating antenatal clinics. Participation by these facilities is not mandatory; still in 2008 compliance was 89 percent. Since the register was initiated from within the profession and is used to benchmarking quality and to compare procedures, there is an incentive for accurate and high quality of reporting. Every year participating clinics submit information on working practices and services provided. We use this data to determine whether clinics are using a structured tool for alcohol screening for the period 2003-2008, which implies that they have adapted the AUDIT instrument, MI-techniques and standardized procedures for referral to treatment. There is explicit information about the implementation of the program specifically from 2005 an onwards. For 2003 and 2004, clinics instead report whether they used “structured working methods to detect women with risky alcohol consumption”. For 2004 this implies AUDIT since the Risk Drinking project initiated the implementation of program in 2004 and no alternative, structured, methods were is use.[23] Information on working methods at the antenatal clinics is linked to municipalities through the postal code. Most municipalities have only one antenatal clinic: Out of the 274 municipalities represented in Maternity Health Care Register, 72 municipalities have multiple clinics. Among municipalities with multiple units, 29 municipalities have units that introduced the screening simultaneously. Since we lack exact information on which center a woman visits we exclude the 33 municipalities where centers implemented the program in different years. In total, pregnant women from 231 out of Sweden's 290 municipalities are included in the analysis.

A mother is treated if she, when she was pregnant, lived in a municipality that had introduced structured screening. Since we have no information on the exact timing of the screening of women, we create a screening window consisting of the first four months of the pregnancy. Given that we do not have access to information about gestation weeks at birth, nor exact birth dates, we assume that all women are pregnant for 38 weeks, and that the child is born the first of each month. Since the first visit to the

[23] For 2003 it is more ambiguous whether clinics responding that that use “structured working methods to detect women with risky alcohol consumption” in fact are using AUDIT, but it should be note (i) that only 2 percent of the clinics were using such methods in 2003 as can be seen in Figure A1 in Appendix A, and (ii) that these clinics do not change screening status over the period. Details about the implementation are based on an interview with Kerstin Petersson, head administrator of the MHV-register and Coordinating midwife in Stockholm County, October 16, 2015.

midwife usually occurs around week 8-12, screening likely to fall within this four month window.

To determine if a pregnant woman is affected by the program in a specific year, we restrict timing of treatment so that the full screening window has to occur past the turn of the year in order to belong to a "new" screening year. For example, a child born in August a given year is assumed to be conceived in November. Although the screening window overlaps the turn of the year, the treatment status of this child is determined by the screening regime the year prior to birth. In practice, this implies that children born between October and December in a given year are treated according to the screening practice in the birth year, whereas children born between January and September are treated according to screening practice the year prior to the birth year. The reason for the restrictive definition is that it is unlikely that all clinics implement the program in January but rather some time during the year. Therefore, we also exclude the year of introduction in the main specification of the analysis.

5.2 Child health outcomes

Our measures of child health are based on whether the child was admitted to hospital or was prescribed pharmaceutical drugs during the first (second) year of life. We create indicators for child health taking the value 1 if the child was admitted to hospital, respectively prescribed any drug, and 0 otherwise. Register information on all inpatient hospital episodes and on all prescribed pharmaceutical drugs purchased at pharmacies is available from the Swedish National Board for Health and Welfare. The hospital data includes detailed information on admission date and on primary and secondary diagnoses classified according to WHO’s ICD10 classification system. Hospitals are obliged by law to report this data, and the information is typically entered into the hospital administrative system at discharge. Similarly, the drug data includes detailed information date of prescriptions and the chemical classification of the drug according to WHO’s ATC system.[24] Pharmacies have strong incentives to report sales in order to get reimbursed from the public drug benefit. By using information from the ICD10 and ATC classification we define hospitalizations and drug prescriptions for different

[24] The drug data only includes prescription drugs sold at pharmacies. Pharmaceutical drugs administered at hospitals or at primary care facilities are not covered.

conditions and events of ill-health as described in Section 4.1 (see Table A1 in Appendix A for exact ICD10 and ATC codes).

Information from the Hospital Discharge register is available for the whole implementation period 2003-2009, whereas information on drug prescriptions only is available from 2005-2009.

5.3 Descriptive statistics

The first column of Table 1 displays summary statistics for the full population of first-born children during the period 2003-2009. As discussed above we restrict the sample due to i) uncertainty of the exact month the screening was implemented, ii) uncertainty of exposure to screening in municipalities where some centers screened and others did not and iii) access to information on drug prescriptions. The second column includes information on the sample used in the analysis when studying hospitalization and the last column display information on the sample when studying drug prescriptions. As can be seen from the first column, 17.3 percent of all first-borns during the period 2003-2009 are admitted to hospital during their first year of life. In our studied population the incidence is somewhat higher suggesting that hospitalization is more common in the included municipalities. Comparing column 1 to columns 2 and 3 also show that there are some differences in the characteristics of the population. The reason is that municipalities which are excluded due to multiple antenatal clinics with different screening practices are larger cities with a higher share of single mothers, mothers with a higher education and a larger share of immigrant mothers.

As can be seen in the last column hospitalization is much less common than getting a drug prescribed during the first year of life, 18.7 percent of the children are admitted to hospital and 51.2 percent of the children get a drug prescribed. Over time the hospitalization rate of children has decreases somewhat whereas the share of children getting drugs prescribed has been rather constant over the period (see Figure A2 and Figure A3 in Appendix A). It is worth noting that these two health measures may pick up different dimensions of health, in particular hospitalization reflects more severe or urgent health conditions. They may also pick up parental differences in health seeking behavior; if the parents refrain from seeking care in time the child may need hospital care for health problems which could have been resolved with a proper medication.

Table 1. Sample characteristics

	Full population (2003-2009)	Hospital sample (2003-2009)	Drug sample (2005-2009)
Hospitalized children per 1000	173.1	188.9	187.3
	(378.3)	(391.4)	(390.2)
Children w drug prescript(%)			51.19
			(49.99)
Mother's age	29.02	28.29	28.27
	(5.054)	(5.043)	(5.082)
Father's age	31.96	31.41	31.42
	(6.063)	(6.150)	(6.230)
Single mother(%)	12.60	10.34	10.28
	(33.18)	(30.45)	(30.38)
University educ mother(%)	49.99	43.02	44.45
	(50.00)	(49.51)	(49.69)
Income below p20(%)	37.99	41.24	42.64
	(48.54)	(49.23)	(49.46)
Imigrant mother(%)	18.42	16.33	17.43
	(38.77)	(36.96)	(37.93)
Municipal unemployment(%)	3.514	3.545	3.385
	(1.104)	(1.185)	(1.196)
Observations	269819	108562	72690

5.4 Audits scores, maternal characteristics, behaviors and child outcomes

Before proceeding to the analysis we characterize how maternal characteristics, health behaviors and child health relates to audit scores. Table 2 present statistics for first time mothers without AUDIT information; with AUDIT score 0-5; AUDIT score 6-9; with audit score 10 and above. This description is based on individual level data from the Maternity Health Care Register for the period 2010-2014; that is, when screening and BI have been implemented throughout the country. We therefore have AUDIT scores for the vast majority of mothers.

Table 2 reveals that for this later period, 9.6 percent of the mothers have elevated AUDIT scores of 6 or above. Mothers with high AUDIT scores are younger than the average mother, or have just compulsory education. The fraction of non-Nordic immigrants with an elevated AUDIT score is lower than among mothers in general. It is also interesting to note that the sample of mothers from whom AUDIT scores are missing is very similar in terms of observable characteristic to mothers with low AUDIT scores. A possible reason is that midwives may choose not to fill out the AUDIT protocol if the mother at an early stage reveals that she does not drink alcohol at all.[25]

[25] This reason for not having complete AUDIT data was mentioned by Kerstin Petersson in interview in October 16, 2015.

About half of all mothers claim to be in good or excellent health and 25 percent have normal BMI at registration. A remarkable difference between these groups of mothers is that 24 percent of mothers with AUDIT ten or above smoked at registration while the corresponding fraction for low-AUDIT mothers was only 4 per cent. This pattern persists during pregnancy. Moreover, we see that fewer mothers with elevated AUDIT breastfeed fully or partially when the child is a month old.

Table 2. Mother and child characteristics by maternal AUDIT score 2010-2014

	Audit 0-5	Audit 6-9	Audit >= 10	Audit missing	All
Mother characteristics					
mother's age	29.1	27.7	26.3	29.2	29.0
young (<25)	0.21	0.32	0.47	0.23	0.22
old (>34)	0.16	0.10	0.08	0.18	0.16
university education	0.50	0.37	0.21	0.46	0.48
compulsory education	0.047	0.057	0.161	0.062	0.051
non-nordic immigrant	0.15	0.04	0.04	0.24	0.15
Health and behavior at registration					
In good health at registration	0.49	0.50	0.47	0.45	0.49
BMI normal at registration	24.3	24.5	24.5	24.3	24.3
smoking at registration	0.038	0.104	0.235	0.043	0.047
Health and behavior during pregnancy					
In good health during pregnancy	0.50	0.50	0.48	0.48	0.50
smoking in w 32	0.026	0.071	0.183	0.032	0.033
Child health and maternal behavior post partum					
breastfeeding at 1 month	0.87	0.85	0.79	0.87	0.87
Observations	118496	11863	2256	14037	146652

6 Results

Below we present the estimation results of the effect of screening pregnant mothers for risky alcohol consumption on children's health. First we present results on the probability of being prescribed a drug or being admitted to hospital during the child’s first years of life. Then we present results on specific health problems, heterogeneous effects across groups of mothers and whether screening pregnant women has differential effects on boys and girls or affects the sex ratio. Finally we present some robustness checks.

6.1 The effect of screening and the brief alcohol intervention program on child health

The first two columns in Panel A of Table 3 show the effect of screening on the probability that a child is prescribed a pharmaceutical drug during its first year of life.

The estimate in column 1 shows that the program decreases the probability of being prescribed a drug. To make sure the result is not due to compositional effects we in the second column control for parental and municipal characteristics. The estimate is somewhat lower but still statistically significant at 5 percent level and suggests that children of screened mothers have a 4.3 percentage points, or 8.4 percent, lower probability of being prescribed a drug during their first year of life compared to children of mothers who are not screened during pregnancy. Columns 1 and 2 in Panel B show that the program also reduces the probability of being admitted to hospital during the first year or life. The estimate presented in column 2, which includes family and municipal controls, suggests a reduction in admittance with 1.4 percentage points and is significant at the 10 percent level. Compared to the average incidence of 198 children per 1000 this estimate implies a reduction of 7.5 percent. In the last two columns we analyze effects during the second year of life; the estimates are close to zero. This suggests that effects of the program on drug prescriptions and hospitalization are centered to the first year of life. This suggests that effects are either limited to the first year of life or that our health measures are too coarse to pick-up more long run effects. We will therefore focus the rest of the analysis on hospital admittance during first year of life and choosing the model with control variables as our main specification.

Table 3. Effects of the program on drug prescription and hospital admission

	First year of life		Second year of life	
	(1)	(2)	(3)	(4)
Panel A: Drug prescription (per cent)				
Program	-0.046***	-0.043***	-0.001	0.001
	(0.015)	(0.014)	(0.010)	(0.010)
Controls	No	Yes	No	Yes
Observations	72690	72690	72690	72690
Municipalities	231	231	231	231
Mean of outcome	0.512		0.716	
Panel B: Hospital admissions (per thousand)				
Program	-15.615*	-14.219*	0.821	1.007
	(8.214)	(8.256)	(4.710)	(4.553)
Controls	No	Yes	No	Yes
Observations	108562	108562	108562	108562
Municipalities	231	231	231	231
Mean of outcome	188.91		84.173	

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth month fixed effects. Control variables include age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

6.2 Which health conditions are affected?

To better understand how the program affects alcohol exposure in utero and mothers' behaviors, we study what type of health problems that are reduced as characterized by type of drug or admission diagnosis.

Panel A of Table 4 presents the estimates of the effect of the program on the probability of being prescribed drugs related to respiratory conditions and infections. The effect of screening is negative and statistically significant for drugs against infections, but for respiratory conditions there is no effect. The estimate of the effect on antiinfectives is 4.4 percentage points, or 20 percent, suggesting that children of screened mothers may have a stronger immune system or that they are less exposed to infections. Increased susceptibility to infections through impairment immune system is a potential consequence of poorer nutrition due to impaired placental function by alcohol exposure (Guerri, 2002).

Panel B presents the estimates of the effect of the program on different causes for hospitalization. The conditions included in the first two columns are diagnoses related to the perinatal period, and diagnoses related to infections and respiratory condition. The next two columns are diagnoses are avoidable admissions and hospitalizations which are related to injuries, poisoning or other external causes. The results suggest that it is mainly avoidable causes and injuries that are affected by the program: potentially preventable hospitalizations are reduced with 3.9 percentage points, or 24 percent, while injuries are reduced by 42 percent. The point estimates for perinatal, infections and respiratory conditions are negative and substantial in size but not statistically significant. This suggests that the program affects admissions related to parental behavior after birth rather than alcohol exposure during (especially early) pregnancy.

This is also supported by the results in Table A2 in Appendix A, where we have estimated the baseline results but excluded health events within the first month after birth. While the result for drug prescription is virtually unaffected, the point estimates for hospitalizations are slightly reduced.

Table 4. Effects of the program on drug prescription and hospital admission during the first year of life: Specific conditions

	(1)	(2)	(3)	(4)
	Panel A: Drug prescription (per cent)			
	Respiratory	Infection		
Program	-0.003	-0.044**		

	(0.012)	(0.019)		
Observations	72690	72690		
Municipalities	231	231		
Mean of outcome	0.266	0.217		
Panel B: Hospital admissions (per thousand)				
	Perinatal diagnoses	Eye, Ear, Respiratory diagnoses	Avoidable diagnoses	Injuries
Program	-5.038	-2.434	-3.854**	-3.365**
	(7.612)	(2.924)	(1.949)	(1.511)
Observations	108562	108562	108562	108562
Municipalities	231	231	231	231
Mean of outcome	109.185	29.355	15.855	8.027

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

The differences in results between drugs and admissions in Table 4 may stem from hospitalizations capturing more severe health events than are captured by drugs which are typically prescribed in primary care.

6.3 Heterogeous effects

The characteristics of the parents may be associated with different drinking patterns, as well as with different responsiveness to the screening and treatment. Parental characteristics may thus affect the impact of the program. Table 5 shows the results when the sample is split along socio-economic status. Panel A shows results for drug prescriptions and Panel B for hospital admittance. First we split the sample according to the mother's educational level. The results presented in columns 1 and 2 suggest that the effect of the program do not differ between mothers with a university degree and mothers without a higher education. For drug prescriptions the estimate is slightly larger for mothers with university education but the difference is not statistically different. For hospitalization the estimates for both groups are negative but less precisely estimated and not statistically significant for any of the groups.

In columns 3 and 4, the sample is split according to the mother's income level. For drugs we find no difference in effects between mothers with an income below the 20th percentile of Swedish women and mothers with higher incomes. However, for hospitalizations we find that the program mainly affects low income mothers. The results suggest that children to low income mothers have 2.8 percentage points lower probability of being admitted as a results of the program, while the estimate for children to mothers with higher incomes is close to zero and not statistically significant. We find similar results for fathers' income; for drug prescriptions there is no heterogeneity across fathers, but for hospital admissions the effect is centered to fathers with low income (See Table A3 in Appendix A)

In the last two columns the sample is split by the mother's age, and also here the two health measures show different patterns. The effect on drug prescriptions is more than twice as large for mothers above, compared to mothers below, the age of 30 (p-value of the difference is 0.097). For hospital admissions, on the other hand, the estimated effect of the program is larger for young mothers and significant at the 10-percent level, but not statistically different to older mothers.

An explanation for this pattern may be that children admitted to hospital are in poorer health than children being prescribed a drug. The different results across outcomes could therefore pick-up different health status and health seeking behaviors across socio-economic groups. Low income (and younger) families are more inclined to seek hospital care for their children, whereas older mothers have pharmaceutical drugs prescribed; this is typically done in the primary care.[26]

Table 5. Effects of the program on drug prescription and hospital admission during the first year of life: By socio-economic background

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Drug prescription (per cent)						
Program	-0.038**	-0.055**	-0.038**	-0.047**	-0.032**	-0.070***
	(0.015)	(0.021)	(0.017)	(0.018)	(0.014)	(0.024)
Sample	No University	University	Below inc at P20	Above inc at P20	Below age 30	Above age 30
P-difference	0.479		0.673		0.097	
Observations	40378	32312	40149	32541	49138	23552
Municipalities	231	231	231	231	231	231
Mean of outcome	0.521	0.495	0.507	0.514	0.522	0.485
Panel B: Hospital admissions (per thousand)						
Program	-11.625	-17.621	-28.271***	1.477	-15.366*	-10.232
	(9.598)	(11.711)	(9.373)	(11.755)	(8.994)	(14.318)
Sample	No University	University	Below inc at P20	Above inc at P20	Below age 30	Above age 30
P-difference	0.651		0.010		0.735	
Observations	61858	46704	59764	48798	73596	34966
Municipalities	231	231	231	231	231	231
Mean of outcome	198.14	175.15	195.30	179.60	187.09	190.84

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth month fixed effects, and controls for whether parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. Columns 1, 2, 5 and 6 also control for age of mother and father * Significant at 10%; ** at 5%; *** at 1%

[26] In Table A4 in Appendix A we find no heterogeneity, either for prescriptions or admissions, across municipalities with AUDIT-scores above and below the median. Similarly we find no differences for the effect on admissions between municipalities where alcohol sales are above and below the median. We do fint that effects on drug prescriptions are larger in municipalities where alcohol sales are below average

6.4 Sex differences

Earlier studies have shown that harsh conditions, such as maternal stress, malnutrition and alcohol consumption, in particular in early gestation (up to the 5th months) are likely to be more detrimental for boy fetuses with consequences for the sex-ratio at birth and worse outcomes for boys (e.g. Valente 2015; Almond and Currie, 2011; Nilsson, 2015).

In Table 6 we therefore explore effects of the program on sex-differences in health and on the sex-ratio at birth. In columns 1-4 we report separate effects, on drug prescriptions and admissions during the first year of life, for boys and girls. The results show no sex-differences: for prescriptions the estimates are similar for boys and girls; for hospital admissions the point estimates are larger for boys, but in neither case are the differences statistically significant. In column 5 the baseline model is estimated on an indicator for sex of the child (taking the value 1 if the child is a boy). We find no evidence that the program affects the sex-ratio.

Given that the intervention takes place sometime towards the end of the first trimester, this is to be expected. This result reflects that the health effects of the program are more likely to stem from reductions in alcohol consumption later in the pregnancy or after birth. The results are also consistent with the interpretation that that our effects on health stem from reductions in moderate consumption, as was found in von Hinke Kessler Scholder et al (2014).

Table 6. Gender differences in effects of the program

	Drug prescription (percent) first year of life		Hospital admissions (per thousand) first year of life		Share boys
	(1)	(2)	(3)	(4)	(5)
Program	-0.042**	-0.049***	-17.469	-9.961	-0.008
	(0.018)	(0.014)	(10.938)	(9.512)	(0.009)
Sample	Boy	Girl	Boy	Girl	All
P-difference	0.663		0.545		
Observations	37512	35178	55994	52568	108562
Municipalities	231	231	231	231	231
Mean of outcome	0.544	0.474	205.080	170.427	0.516

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

6.5 Socio-economic outcomes of the parents

The objective of the Swedish maternity care system is to monitor the health of the mother of fetus during pregnancies; to prepare parents for parenthood; and to discover and help parents in need of special support. Health education is an important aspect of prenatal care and focuses mainly on lifestyle changes during pregnancy. Even if the main focus is on the child, the work can spill over on the parents. As the evidence on avoidable hospital admissions and injuries (in Section 6.2) suggests that the program induces behavioral change beyond the pregnancy, the program may have also have longer run consequences for the health and welfare of parents.

In Table 7 we therefore analyze effects on socio-economic outcomes. The result in column 1 show no effects of the program on family stability; that is, the probability of the mother and father living together the year after the child is born is not affected by the program,. In column 2-5 we assess if the program affects that the likelihood of the parents being on welfare. The result in column 2 suggests that being subjected to the program reduced the probability of mothers being welfare recipients with 0.8 percentage points, which corresponds to 14 percent at the mean. This result is robust to controlling for social welfare the year before the pregnancy in column 3. For fathers, on the other hand, we find no effect on welfare dependency (columns 4-5).

Table 7. Effects of the program on the probability of the parents living together and on being a social welfare recipient the first year after the child is born

	Cohabiting	**Social welfare recipient**			
	(1)	(2)	(3)	(4)	(5)
Program	-0.003	-0.008**	-0.007**	-0.004	-0.003
	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)
Soc. benefit t-1			0.408***		0.466***
			(0.010)		(0.008)
Sample	All	Mother	Mother	Father	Father
Observations	103649	103649	103649	103482	103482
Municipalities	231	231	231	231	231
Mean of outcome	0.089	0.057		0.045	

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

These results suggest that behavioral changes may well extend to other domains, for example, the program may help mothers to break welfare dependence. We also analyze if there is any direct effects on mothers health. The results presented in Table A5 in Appendix A show no effect of the program on prescriptions to mothers’ or on hospitalizations during the first year after giving birth, but show suggestive evidence that hospitalizations in the longer run is reduced. The effect of a hospital admission during the second year after childbirth is reduced 0.8 percentage point, which correspond 13 percent, and is significant at 10 percent. For fathers there is no effect of our health outcomes (See Table A6 in Appendix A).

6.6 Robustness of results

We have done several tests to check the robustness of the results with respect to sampling restrictions and the identifying assumptions.

In Table 8 we analyze the sensitivity of the estimates to the restrictions made on the sample: the exclusion of municipalities with multiple antenatal clinics which implemented the program in different years and the exclusion of the implementation year. Including children for whom there is uncertainty whether their mothers was screened or not dilutes our treatment indicator and increases the measurement error and should weaken the result. Columns 1 and 4 display our main result from Table 3. In columns 2 and 5 we include municipalities with multiple clinics where the year of introduction varies across antenatal clinics within the municipality. Adding these municipalities lowers the estimates but they are still statistically significant. Next we instead include the years when the screening was introduced. The results in columns 3 and 6 shows that including these years also weakens the effect: the point estimate on prescribed drugs is smaller and still statistically significant (10 percent level), but the estimate on admittance to hospital is no longer statistically significant. While weakening the results, the underlying pattern stays the same when relaxing these sample restrictions.

Table 8. Effects of the program on drug prescription and hospital admission during the first year of life: Different sampling restrictions

	Drug prescription (per cent)			**Hospital admissions (per thousand)**		
	(1)	(2)	(3)	(4)	(5)	(6)
Program	-0.043***	-0.023**	-0.020*	-14.219*	-11.697*	-5.258
	(0.014)	(0.010)	(0.011)	(8.256)	(7.046)	(5.460)
Conflict info	No	Yes	No	No	Yes	No
Impl year	No	No	Yes	No	No	Yes
Observations	72690	145645	91653	108562	221259	130594
Municipalities	231	273	231	231	273	231
Mean of outcome	0.510	0.495	0.512	188.300	172.340	188.696

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

An important assumption for the identification strategy in this study is the parallel trends assumption. The concern is that municipalities which implement the program early have a negative trend in hospitalization and drug use among infants giving rise to a spurious negative estimate of the program. A typical way to assess this assumption is to analyze the pattern of pre-effects where treatment is characterized in event—rather than calendar—time. In our setting with a short data window where the implementation centered to few years, the pre-effects becomes relatively noisy when moving away from the implementation year as they are indentified on a limited set of late implementers. Similarly, the precision of the estimated treatment-effects also become noisy if allowing for dynamic effects in the post treatment period. In Table 9 we therefore estimate a model where the impact of the program is captured with our standard post-treatment parameter, but where we let the year before implementation serve as a reference point (i.e. captured by the constant) and allow for a separate parameter to capture pre-treatment outcomes *two years before implementation and earlier*. If the pre-treatment effect is positive our results may be due to a trend, if it is negative it suggests that the year before treatment may be different. For prescription drugs, in column 1, we find the estimated treatment parameter to be of the same size as in our baseline results (in Table 3). We also find pre-treatment outcomes two years before implementation and earlier to be substantially lower than the treatment-effect but still slightly more negative than the year before implementation. For hospital admissions, in column 2, we again see treatment-effect to be of the same size the baseline results (in Table 3), while the parameter for pre-treatment outcomes two years before implementation and earlier is positive but insignificant. These results are consistent with the parallel trends assumption, even if they are not conclusive.

We also assess the parallel trends assumption by re-estimating our baseline model for infant hospitalization (during the first year of life) using children born 6 years earlier in the same municipality as the outcome. The results from this placebo analysis using the population of first-born children born between 1997 and 2002 are presented in column 3 of Table 9. The estimate is not significant, and of opposite sign to those in the main analysis; i.e. consistent with the parallel trends assumption being fulfilled. A drawback with this placebo is that children in this sample are born six years prior to those in the main analysis, which may make them less comparable. Still, the small and not significant point estimate in Table 8 is reassuring.

Table 9. Effects of the program on drug prescription and hospital admission during the first year of life: Pre-effects and placebo

	Drug prescription (per cent)	Hospital admissions (per thousand)	
	(1)	(2)	(3)
Program	-0.0398***	-14.75*	2.143
	(0.0133)	(8.165)	(9.339)
Program t-2 and earlier	-0.0175*	4.493	
	(0.00934)	(6.707)	
Sample			first-born children 1997-2002
Observations	72,724	108562	93052
Municipalities	232	231	231
Mean of outcome	0.510	188.300	191.251

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

Another part of the parallel trends assumption is that the timing of implementation of the screening program must be exogenous. As mentioned, the reason for the staggered implementation across the country was training restrictions for mid-wives. To confirm that the timing of the implementation is not related to the initial alcohol related health situation in the municipality, we have estimated the relation between alcohol related hospitalizations of women in the ages 20-39 in each municipality in 2003 and an indicator for the municipality being an early implementer (=1 if implementing before 2007 and 0 otherwise) as outcome, also including county-fixed effects. As shown in column 1 of Table 10 we find no such relationship, thus suggesting that the implementation among municipalities within a county is not related to the initial alcohol related health among women of childbearing age. Similarly, in columns 2-5 we correlate municipal averages of parental characteristics in 2003 to the timing of implementation. We only find that the age of the father is statistically significant (10 percent level) and weakly related to implementation; more specifically, municipalities with a one standard deviation older fathers are about 4 percent more likely to implement the program 2007 or later.

Table 10. Relation between timing of implementation and municipal characteristics (2003)

	(1)	(2)	(3)	(4)	(5)
Alcohol related hospitalizations	0.027				
	(0.026)				
Average age of mothers		-0.036			
		(0.027)			
Average age of fathers			-0.050*		

			(0.029)		
Share of mothers with uni. degree				-0.304	
				(0.294)	
Share of immigrant mothers					-0.502
					(0.475)
Mean of outcome	1.581	27.908	31.197	0.386	0.135
Standard deviation	0.843	1.083	0.892	0.096	0.064
Observations	188	231	231	231	231

Note: The outcome is an indicator of the timing of implementation (=1 if implementing before 2007 and 0 otherwise). All models include fixed county effects. * Significant at 10%; ** at 5%; *** at 1%

7 Effects of the program on pregnant women's behavior using survey data

The results found so far suggest that the screening and BI at the antenatal clinics affect child health and maternal behaviors, and that the effects extend beyond the birth of the child. To understand these behavioral changes we explore additional information from thesurvey data set described in Section 5.4 covering the years 2003-2008 for women registered at antenatal clinics. The data is collected by midwives and include information on behaviors which should be important for child health such as smoking before and during pregnancy and whether the mother breastfed the child 4 weeks after birth, as well as some information on the child, in particular birth weight and whether the pregnancy ended in a miscarriage. During the period 2003-2008 the data registration was not as developed as after 2009 and it suffers from misreporting and problems with missing data; the coverage is much lower for some questions with missing answers..[27]

As discussed in section 5.4, women with high AUDIT scores are more likely to smoke. Smoking may be connected to alcohol consumption for at least two reasons. First, smoking is culturally associated with alcohol and more socially accepted when drinking. Second, women who are unable to stop smoking when pregnant may also find it difficult to stop drinking alcohol. Thus, studying the effect of the intervention on smoking behavior may be informative of changes in alcohol consumption. It should also be noted that the motivational interviewing technique probably does not only affect how midwifes discuss alcohol consumption, but also other behaviors which have adverse effects on the child, such as smoking.

[27] The original data also include information on birthweight and small for gestation age but according to the register holder theses data are of poor quality and should not be used. Based on an interview with Kerstin Petersson, head administrator of the MHV-register and Coordinating midwife in Stockholm County, October 16, 2015.

Since we know which clinic the women is registered at we can estimate the effect of screening and BI using the staggered implementation of the program across clinics. In other words, we use the same difference-in-difference approach as in previous analyses but at clinic level. To this end we merge the clinic level data on whether the clinic uses the program, with the survey data on pregnant women. As in the previous study we remove the year when the screening was introduced since it is not clear who was screened. Individuals are considered treated if they are registered at a clinic which has implemented the program. We do not capture all women as not all clinics provide the information to the Maternity Health Care Register and because information on all registered women at the clinic is not reported; for example, in 2007 the survey data include 77 percent of all births in Sweden.

For this clinic level analysis the empirical model is given by:

$$y_{ic(a)t} = \alpha + \beta Treatment_{at} + \gamma_a + \eta_{ct} + \boldsymbol{K}_{kt}\lambda + \varepsilon_{ic(a)t}, \qquad (2)$$

where $y_{ic(a)t}$ is the outcome of child/mother i at antenatal clinic a in year t. Similar to the previous analysis, we control for η_{ct} a vector of county specific time effect and γ_a being a vector of antenatal clinic fixed effects section. The variations between clinics within a county identify the effect. We also include municipal unemployment level and municipal alcohol sales per capita in the regression to control for time-varying differences in municipal characteristics, $\boldsymbol{K}_{kt}$. However, as we are not able to link the individual level survey data to population registers it is neither possible to control for background characteristics of the parent nor the birth month. According to the instruction to the midwives, the data should however be registered on the year the child is born. As in the previous analyses we exclude the year of introduction of the treatment since we do not know when during the year the program was implemented. Again, the coefficient of interest is β, which is the estimate of the treatment effect. Standard errors are clustered at the clinics. We focus on women pregnant with their first child and singleton births only.

Using the survey data we construct an indicator of whether the pregnant women smoked at registration in week 8-12 but not in week 32 (*quit smoking*) and a variable indicating whether she began smoking in the same time period (*start smoking*). We also study whether the child was breastfed fully or partially 4 weeks post birth and whether the birth ended in a *miscarriage*. The number of observations differs across variables since, not all of the questions are reported for all women. If screening combined with BI affected behavior in a positive direction we expect smoking to decrease and higher likelihood of breastfeeding. , However, miscarriages may not be affected as the program is unlikely to affect outcomes related to early alcohol exposure.

The identification strategy hinges on the assumption that implementation of structured screening and BI was not determined by antenatal clinics characteristics, or that pregnant women systematically choose clinic based on screening practices. This last point could potentially be a greater problem when studying clinics rather than municipalities, since it easier to select a specific type of clinic if there are several to choose from. To test whether the registered pregnant women at the clinics implementing structured screening were different we study whether women were more likely to smoke at the first visit at the antenatal clinics or more likely to have quit smoking before the first visit, ie. outcomes that are predetermined.

The first column in Table 11 shows that when introducing the program at a clinic pregnant women are induced to cease smoking. The probability of quit smoking between registration and week 32 is 0.6 percentage points; 25 percent at the mean. Since 7.5 percent of the women smoked at registration at the end of the first trimester, this implies an 8 percent decrease in smoking. Very few pregnant women take up smoking during pregnancy; in column 2 we see that also this probability is reduced with 0.02 percentage points, which is implies a reduction of 45 percent. The results are also suggestive of a positive effect on the likelihood of breastfeeding, even if the point estimate does not reach statistical significance (P-value=0,123). There are no statistically significant effects on miscarriages in column 4. And in the last two columns we see that women registered at clinics which implemented the program do not differ in the sense that they were as likely to smoke or have stopped smoking before the initial visit at the clinic.[28]

[28] The population used in this section differs somewhat to the population used in the analysis in Section 6. To compare the results we restrict the population to the same clinics as in the previous analysis and weight the regression with the number of firstborn births in the municipality that year, see Table A7 in Appendix A. The results show a qualitatively similar pattern from smoking, albeit somewhat stronger. In this sample there is also a positive effect of screening on breastfeeding.

Table 11. Effects of the program on maternal behavior and child health indicators using survey data

	(1)	(2)	(3)	(4)	(5)	(6)
	Quit smoking between registration and week 32	*Start smoking between registration and week 32*	*Breast-feed at 1 month*	*Mis-carriage*	*Smoke at registration*	*Quit smoking between 3 months before pregnancy*
Program	0.006*	-0.002**	0.010	-0.001	0.005	-0.010
	(0.003)	(0.001)	(0.006)	(0.001)	(0.004)	(0.006)
Observations	132,135	132,135	116,372	133,860	134,077	133,938
Mean of outcome	.023900	.004458	.88880	.005409	.074837	.113127

Note: Standard errors in parenthesis, clustered at clinic level. All models include clinic and county-year fixed effects. * Significant at 10%; ** at 5%; *** at 1%

These results give further support to the notion that the program affects maternal behavior after pregnancy. However, we cannot determine if the effects on smoking cessation (or not starting to smoke) and breastfeeding are spillovers from screening and BI related to alcohol, or to what extent midwives have utilized their MI training also in other domains.

8 Conclusion

Most expecting mothers are aware that alcohol consumption during pregnancy can be harmful for the child. But changing consumption patterns with a shift towards more continental drinking habits (Göransson, 2003, 2004) and an increased questioning of the recommendations to completely abstain from alcohol during pregnancy (Oster, 2013), raises concerns for increased alcohol exposure in utero.

Hence, identifying effective methods for preventing harmful alcohol consumption is of importance for policies aimed at improving health and development of children. In this paper we study the introduction of a brief alcohol intervention at Swedish antenatal clinics. Within the program midwives screen pregnant women for alcohol in gestation week 8-12 with the AUDIT instrument; use MI-techniques to induce behavioral change; remit women—if necessary—to other health care professionals or to the social services. By exploiting the staggered implementation of the program across municipalities we are able to identify causal effect of the program on infant health.

We find that the screening and BI combined with targeted preventive interventions improves infant health. Screening lowers the probability that a child is prescribed a pharmaceutical drug during the first year of life by 4.3 percentage points, which implies a reduction of 8.4 percent relative to the population average, and lowered the probability that children are admitted to hospital during their first year of life by 7.5 percent. We find no evidence that effects on hospitalizations extend after the first year of life. While the program reduces the likelihood that infants of low income (and young) mothers are hospitalized, the program reduces the likelihood that infants of older mothers are prescribed drugs. This may reflect age differences in maternal alcohol consumption behavior, with more binging among younger low income mothers and therefore that screening impact on more severe conditions that lead to hospitalizations. At the same time this result could reflect differences in health seeking behavior, where older women may be more likely to consult primary care at an earlier stage.

For hospitalization we find that the health effects are mainly driven by reductions in inpatient care due to injuries and avoidable conditions. This suggests that behavioral changes caused by the program extend beyond the birth of the child through improved care. The reductions in drug prescriptions are mainly related to respiratory conditions and infections, which would suggests that the impact of screening may also run through an improved fetal conditions throughout the pregnancy. Still it is difficult to rule out that the effects on these conditions also stem from improved care and attention after birth. We also find that the program reduced social welfare dependency. And when we directly study maternal behavior during and after pregnancy using individual level survey data we find the program reduced smoking.

The results suggest, overall, that the program led to behavioral changes in the treated mothers and that these effects persist after birth.

Are the results a consequence of reduced alcohol intake during and after pregnancy? This can unfortunately not be answered with certainty. It is possible that the effects shown in the various indicators of children's health are a result of reduced drinking both during and after pregnancy. But it is also possible that midwives' training in MI gives them tools to promote a healthy lifestyle more broadly. Smoking and alcohol consumption are often related, and we could therefore infer that if smoking has decreased then it is likely that also alcohol consumption is reduced.

Our results are important from a policy perspective. Whatever the exact mechanisms underlying the improvements in children's health, the effects of the program have been beneficial. Poor health due to fetal and early childhood alcohol exposure is preventable, and screening and BI is shown to be an effective instrument to modify maternal behavior.

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Appendix A

Table A1. ICD and ATC codes

Hospital admission	**International Statistical Classification of Diseases and Related Health Problem, ICD 10**
Certain conditions originating in the perinatal period	=1 if admitted to hospital with code P00-P96
Eye and Ear conditions, and Diseases of the respiratory system	=1 if admitted to hospital with code J00-J99, H00-H95
Avoidable Conditions	=1 if admitted to hospital with code D50, E10-E11, E13-E14, E86 G40-G41, H66-H67, H66-H67, I11, I20, I29, I50, J02-J03, J06,J43-J47, K24, K26-K28, K52, N10-N12, N70, N73-N74, O15, R56
Injury, poisoning and certain other consequences of external causes	=1 if admitted to hospital with code S00-T98
Drug prescription	**Anatomical Therapeutic Chemical Classification, ATC**
Respiratory system	=1 if prescribed a pharmaceuticals in chapter R
Antiinfectives	=1 if prescribed a pharmaceuticals in chapter J

Table A2. Effects of the program on drug prescription and hospital admission during the first year of life excluding events with one month after birth

	Drug prescription (per cent)		**Hospital admissions (per thousand)**	
	(1)	(2)	(3)	(4)
Program	-0.045***	-0.041***	-10.099**	-9.198*
	(0.015)	(0.014)	(4.928)	(4.980)
Controls	No	Yes	No	Yes
Observations	72690	72690	108562	108562
Municipalities	231	231	231	231
Mean of outcome	0.495		86.752	

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth month fixed effects. Control variables include age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

Table A3. Effects of the program on drug prescription and hospital admission during the first year of life: By fathers' level of income

	Drug prescription (per cent)		**Hospital admissions (per thousand)**	
	(1)	(2)	(3)	(4)
Program	-0.040**	-0.041**	-32.084***	5.624
	(0.017)	(0.020)	(10.962)	(10.933)
Sample	Below inc at P20	Above inc at P20	Below inc at P20	Above inc at P20
P-difference	0.955		0.005	
Observations	38845	33845	57853	50709
Municipalities	231	230	231	231
Mean of outcome	0.511	0.508	193.923	182.106

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

Table A4. Effects of the program on drug prescription and hospital admission during the first year of life: by fathers' level of income: By ADUIT-score and alcohol consumption in the municipality.

	(1)	(2)	(3)	(4)
	Panel A: Drug prescription (per cent)			
Program	-0.036**	-0.047**	-0.026*	-0.077***
	(0.018)	(0.020)	(0.014)	(0.025)
Sample	Above median ADUIT score	Below median ADUIT score	Above median alcohol cons.	Below median alcohol cons.
P-difference	0.676		0.074	
Observations	25727	46963	34764	37926
Municipalities	87	144	130	101
Mean of outcome	0.504	0.513	0.502	0.517
	Panel B: Hospital admissions (per thousand)			
Program	-15.017	-16.179	-14.122	-14.921
	(11.038)	(12.384)	(11.774)	(10.771)
Sample	Above median ADUIT score	Below median ADUIT score	Above median alcohol cons.	Below median alcohol cons.
P-difference	0.944		0.960	
Observations	39669	68893	52731	55831
Municipalities	87	144	130	101
Mean of outcome	165.234	201.606	183.532	192.804

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

Table A5. Effects of the program on drug prescription and hospital admission for mothers

	Drug prescription (per cent)				**Hospital admissions (per thousand)**			
	First year after childbirth		Second year after childbirth		First year after childbirth		Second year after childbirth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program	-0.010	-0.007	-0.003	-0.000	0.640	0.417	-8.089*	-7.818*
	(0.010)	(0.010)	(0.010)	(0.010)	(5.467)	(5.579)	(4.719)	(4.706)
Observations	71744	71744	71744	71744	108877	107094	108877	107094
Municipalities	231	231	231	231	231	231	231	231
Mean of outcome	0.679		0.699		97.354		60.131	

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

Table A6. Effects of the program on drug prescription and hospital admission for fathers

	Drug prescription (per cent)				**Hospital admissions (per thousand)**			
	First year after childbirth		Second year after childbirth		First year after childbirth		Second year after childbirth	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Program	-0.002	-0.001	-0.009	-0.007	-0.291	-0.557	-3.168	-3.397
	(0.011)	(0.011)	(0.012)	(0.012)	(2.628)	(2.604)	(3.275)	(3.231)
Observations	71532	71532	71532	71532	106432	106432	106432	106432
Municipalities	231	231	231	231	231	231	231	231
Mean of outcome	0.419		0.463		30.645		34.838	

Note: Standard errors in parenthesis, clustered at municipality level. All models include municipality, county-year and birth-month fixed effects, and controls for age of mother and father, if parents live together at time of birth of the child, immigrant status of mother, maternal educational level, municipal unemployment level, municipal level of alcohol sales, and sex of the child. * Significant at 10%; ** at 5%; *** at 1%

Table A7. Effects of the program on maternal behavior and child health indicators using survey data and municipal level variation

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	Quit smoking between registratio n and week 32	*Start smoking between registratio n and week 32*	*Breast-feed at 1 month*	*Mis-carriage*	*Smoke at registratio n*	*Quit smoking between 3 months before pregnancy*
Program	0.015***	-0.004***	0.037*	-0.000	-0.018	0.001
	(0.005)	(0.001)	(0.021)	(0.003)	(0.018)	(0.013)
Observations	83,717	83,717	83,717	83,717	83,717	83,717
Mean of outcome	.0273145	.0050756	.8665916	.005468	.0875983	.1190173

Note: Standard errors in parenthesis, clustered at clinic level. All models include clinic and county-year fixed effects. * Significant at 10%; ** at 5%; *** at 1%

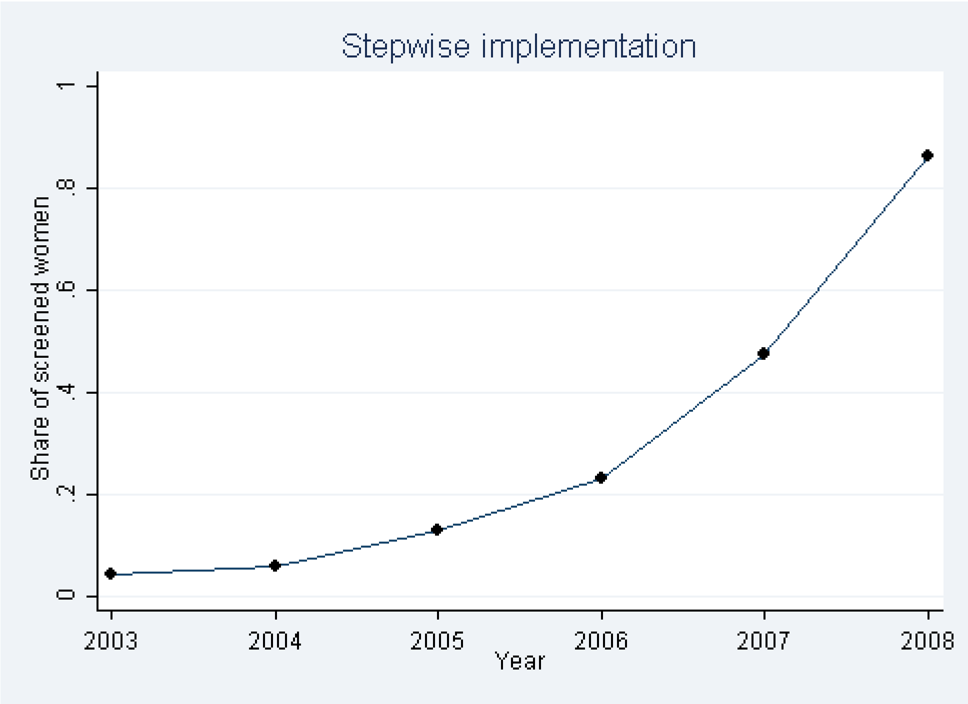

Figure A1. Share of clinics with a structured working methods to detect women with risky alcohol consumption 2003-2008

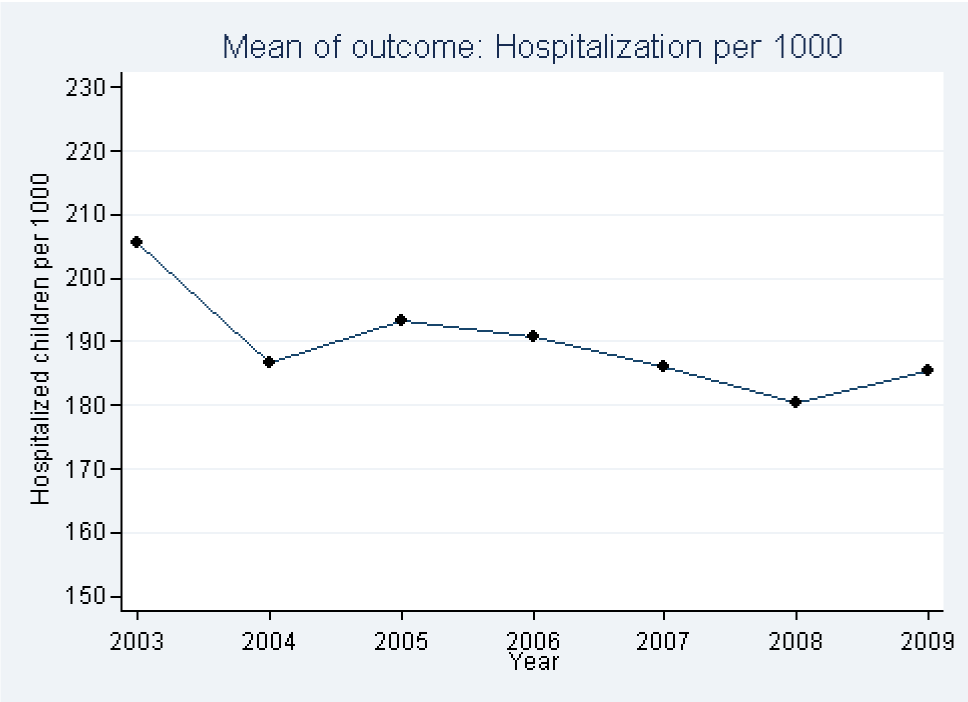

Figure A2. Share of children hospitalized during first year of life 2003-2009

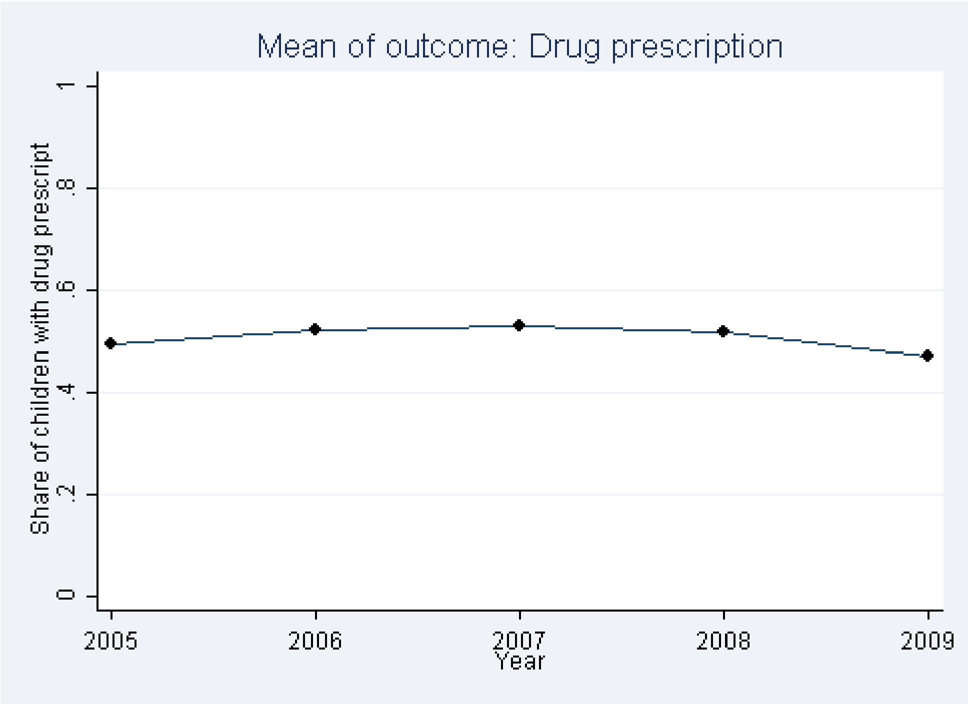

Figure A3. Share of children with drug prescription during first year of life 2005-2009

Appendix B. AUDIT-questionnaire

Box 10

The Alcohol Use Disorders Identification Test: Self-Report Version

PATIENT: Because alcohol use can affect your health and can interfere with certain medications and treatments, it is important that we ask some questions about your use of alcohol. Your answers will remain confidential so please be honest.
Place an X in one box that best describes your answer to each question.

Questions	0	1	2	3	4	
1. How often do you have a drink containing alcohol?	Never	Monthly or less	2-4 times a month	2-3 times a week	4 or more times a week	
2. How many drinks containing alcohol do you have on a typical day when you are drinking?	1 or 2	3 or 4	5 or 6	7 to 9	10 or more	
3. How often do you have six or more drinks on one occasion?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
4. How often during the last year have you found that you were not able to stop drinking once you had started?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
5. How often during the last year have you failed to do what was normally expected of you because of drinking?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
6. How often during the last year have you needed a first drink in the morning to get yourself going after a heavy drinking session?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
7. How often during the last year have you had a feeling of guilt or remorse after drinking?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
8. How often during the last year have you been unable to remember what happened the night before because of your drinking?	Never	Less than monthly	Monthly	Weekly	Daily or almost daily	
9. Have you or someone else been injured because of your drinking?	No		Yes, but not in the last year		Yes, during the last year	
10. Has a relative, friend, doctor, or other health care worker been concerned about your drinking or suggested you cut down?	No		Yes, but not in the last year		Yes, during the last year	
					Total	

Source: Babor et a. (2001)