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Auditing Mothers: The Effect of Targeted Alcohol Prevention on Infant Health and Maternal Behavior [a]

by

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Abstract

This study examines the effects of targeted preventive interventions for pregnant women with elevated alcohol risk on infant health and maternal behavior. The detrimental effects of alcohol exposure in utero are well documented and universal alcohol prevention programs are an important part of national strategies to promote maternal and child health. Identifying effective interventions to prevent harmful maternal alcohol consumption is of great importance. We exploit the discrete nature in the decision rule to provide an alcohol preventive intervention to mothers at risk in a regression discontinuity design. The results suggest that the intervention has negligible impact on birth weight and small effects on the gestational age. We are unable to determine if this is due to a low effectiveness of the intervention or due to a low take up of the intervention.

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

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1 Introduction

Public interventions and recommendations concerning pregnant women's alcohol consumption are an important part of national strategies to promote maternal and child health. Universal alcohol prevention programs have been motivated by the overwhelming evidence that heavy prenatal exposure to alcohol has negative consequences for child health and cognitive development (McBride 1961; von Lenz and Knapp 1962; Jones et al. 1973; Barker 1990). Research on the effectiveness of such universal preventive intervention programs is however limited. Yet it is of great importance not only to identify effective methods to prevent harmful fetal alcohol exposure in general, but also to understand which specific features of preventive interventions that are effective in modifying parental behaviors and improving child health.

Beginning in 2004 Swedish maternity clinics introduced the *Risk drinking project*, a screening and brief intervention (BI) program for alcohol aimed at pregnant women with elevated alcohol risk. In Grönqvist et al. (2016) we find that this program improved infant health measured by prescription of pharmaceutical drugs and hospitalizations during the first year of life of infants whose mother were exposed to the program. We also find evidence of reduced maternal smoking during pregnancy, and suggestive evidence of increased breastfeeding. The program has several features: midwives screen pregnant women for risky alcohol consumption in gestation week 8-12 using the Alcohol Use Disorders Identification Test (AUDIT) instrument, use Motivational Interviewing (MI)-techniques to modify behavior, and – if necessary – refer women to other health care professionals or the social services. The midwives also received training in MI-techniques. In Grönqvist et al. (2016) we argue that the impact on child health may be due to either one or a combination of these factors.

The purpose of this paper is to isolate the effects on infant health and maternal behavior, such as breastfeeding and smoking of one of these factors, namely the targeted preventive BI using MI for pregnant women with elevated alcohol risk. BIs using MI-techniques have previously been shown to be effective in many areas of health (e.g. diabetes care, weight loss, smoking cessation, and drug or alcohol addiction, see Rubak et al. 2005).

Evidence regarding the effectiveness of interventions during pregnancy is however still limited (O'Donnell et al. 2013). In addition, studies of large scale BI-programs for general populations, such as the one studied here, are scarce. This paper is thus an important contribution to the literature on the effectiveness of BI-programs in general, and to the evidence on brief alcohol interventions in maternity care in particular. Moreover, this paper contributes to the literature on the importance of in utero and early life conditions for child health by providing insights about how policy interventions can affect infant health.

Interest in the effectiveness of universal alcohol prevention programs in maternity care is motivated by a recent literature in economics suggesting that even moderate alcohol exposure in utero has adverse effects on health and long run human capital development of children (see e.g. Wüst 2010; Zhang 2010; von Hinke et al. 2014; Nilsson 2017). In a context where norms prescribing zero tolerance for alcohol during pregnancy risk being challenged by increased tolerance for every-day, continental alcohol consumption patterns, and by early correlational evidence pointing to unclear or even positive relation between moderate wine consumption and birth outcomes (Polygenis et al. 1998; Abel and Hannigan 1995), it is of particular relevance to identify prevention methods that are effective in modifying behaviors of women who may not realize that their alcohol consumption patterns put their unborn child at risk as well as among mothers with more severe alcohol problems.

The intervention studied in this paper is targeted at women identified as having risky alcohol behavior based on their score on the 10-item AUDIT-questionnaire which the midwife administers during the woman's first registration visit at the maternity care unit. A woman is considered as having risky drinking behavior if she scores 6 or more on the 0-40 AUDIT-scale. For these women, the midwife will during the registration visit initiate a BI using MI with the aim of motivating and encouraging behavioral modification. The woman will also be invited to recurrent supportive motivational talks at the maternity clinic. We exploit the discrete nature of the decision rule used by midwives for when to initiate a targeted preventive intervention to estimate the causal effect of the BI. Using individual level survey data from the Swedish Maternal Health Care Register between the years 2010 and 2014, we estimate a reduced form RDD comparing children of mothers who are just subject to treatment to those whose mothers score just below 6 and hence are not treated, with the AUDIT score as a discrete "running variable". The register contains data collected by the midwife and includes information on the AUDIT score and the main outcome of infant health, measured by birth weight. It also contains data on birth date and expected day of delivery, information about smoking before and during pregnancy and breastfeeding, as well as survey information on socioeconomic characteristics of the mother. The assumption underlying the research design is that while the decision rule induces a discrete increase in the probability of receiving treatment, there is little reason to expect a similar discrete change in characteristics of mothers on either side of the threshold. Instead, absent treatment, we should expect a smooth relationship between birth outcomes, parental characteristics, and maternal behavior and the AUDIT score.

We find that the targeted alcohol preventive MI-intervention has a small to negligible average effect on the birth weight of children whose mothers were treated at the threshold. We find an effect in the order of magnitude of 0–23 grams, and we can rule out effect sizes larger than 42 grams. Estimating the effect across the distribution of birth weight, suggests that the magnitude of the effect is rather stable for normal birth weight children, but that it is larger in the lowest quintile of birth weights and smaller, or even negative in the highest quintile. This pattern indicates that the intervention may have more important health effects for children at greater risk. This result is supported by some suggestive evidence that the intervention may reduce the probability of premature birth. There are however no effects on the likelihood of passing the low birth weight threshold of 2500 grams and only very small effects on the gestational age.

Unlike Grönqvist et al. (2016), we find no evidence that the intervention leads to a higher likelihood of women breastfeeding or induce women to quit smoking during pregnancy.

Since we have no direct information on MI-intervention we cannot determine whether the small to negligible effects are due to a low effectiveness of the targeted MI-intervention or whether the take-up of the intervention is low despite the decision rule.

Grönqvist et al. (2016) suggest that the introduction of the screening and BI program improved infant health by having an impact on maternal behavior. Given that the focus of the current study is on different outcomes, due to different data sources, it is not possible to directly compare the results to those in Grönqvist et al. (2016). It is therefore not possible to draw any definite conclusions about whether it is the BI intervention directed at women at risk – which has a minor effect on birth weight – or if it is the program at large that has been successful.

The rest of the paper is organized as follows. The following section summarizes maternity care policies in Sweden and discusses the AUDIT screening and BI program. In Section 3, we describe the data and Section 4 describes the empirical strategy. Finally, in Section 5 we report the results of the analysis and Section 6 concludes.

2 Maternity care and the intervention

Sweden has a comprehensive maternity care program open to all pregnant women, which is free of charge and easily accessible. The objective of the maternity care is not only to monitor pregnancies but also to provide parental support and to detect and prevent risks and conditions – both medical and psychosocial – that can affect the pregnancy and the development of the fetus, the delivery and the early attachment of child and parents. Health education is an important part of maternity care and focuses on informing pregnant women and their partners about necessary lifestyle changes during pregnancy. Nearly 100 percent of all expecting women are enrolled in maternity care services which are provided primarily through municipality-based public maternity clinics (Socialstyrelsen 2005). Around 560 clinics care for the approximately 100 000 pregnancies annually. During uncomplicated pregnancies, women typically have 6-10 prenatal visits to the maternity clinic. The focus of the first registration visit, which usually occurs around week 8-12 of the pregnancy, is lifestyle habits that may impact the wellbeing of the fetus as well as the woman. By covering nearly all pregnant women in Sweden, the maternity clinics have a strategic position in detecting and preventing prenatal alcohol exposure, and to provide support to women who experience difficulties to stop drinking alcohol during pregnancy.

The detrimental effects of severe alcohol exposure in utero are well documented with the most severe diagnosis being Fetal Alcohol Syndrome (FAS) (see Grönqvist et al. 2016 for an overview of the effects of prenatal alcohol exposure). The causal relationship between alcohol consumption, child health and long run human capital outcomes of children is also well established (see e.g. Wüst 2010; Zhang 2010; von Hinke et al. 2014; Nilsson 2017). While the earlier correlation based studies lacked strong evidence on the negative impact of moderate alcohol consumption and sometimes even suggested that moderate wine consumption was better for child health than total abstention, the recent evidence points to negative effects also at moderate levels of consumption. Swedish maternity care early on imposed strict recommendations to pregnant women to completely abstain from alcohol with the motivation that there is no scientifically proven safe level of alcohol consumption.[1] However, with increased accessibility of alcohol and changed consumption patterns following Sweden's entry to the EU, there were growing concerns for how changes in, and more liberal attitudes towards, alcohol might impact also on the drinking patterns of pregnant women since consumption of alcohol during pregnancy is known to be influenced by established habits (Göransson et al. 2004).

In response, Swedish maternity care became part of the nationwide Risk drinking project, with the aim of implementing brief alcohol interventions as an integral part of the routine care in primary care. The project, introduced in 2004, was run and financed by the Swedish Public Health Agency and had a large impact on the maternity clinic's alcohol preventive work by promoting the use of the AUDIT instrument to detect risky alcohol consumption (Socialstyrelsen 2009), organizing MI training programs for midwifes, and by mandating extra counselling and referral to specialist for mothers displaying a risky alcohol consumption pattern.

The AUDIT questionnaire, a 10-item survey instrument developed by the WHO, covering three areas: consumption, addiction, and alcohol related damages (Babor et al. 2001) was adapted to maternity care and promoted as a pedagogical tool to be used to discuss attitudes towards alcohol. An important strength of the AUDIT protocol is its sensitivity and high specificity in detecting risky alcohol consumption also at more moderate levels of alcohol problems (Saunders et al. 1993; Reinert and Allen 2007).

As an adaption to the maternity care setting, women are asked about pre-pregnancy,

[1] Swedish guidelines regarding alcohol were developed in the late 1970's and early 1980's (Socialstyrelsen 1979, 1981).

rather than present, alcohol behavior. This is because women are more likely to answer truthfully about pre-pregnancy consumption. Moreover, pre-pregnancy alcohol intake has been shown to be a good predictor of the alcohol consumption during pregnancy (Göransson et al. 2003). The AUDIT protocol is filled out by the midwife or by the pregnant woman and is graded by the midwife on a 0-40 scale, where higher scores indicate more hazardous alcohol consumption. If a pregnant woman scores a value of 6 or higher she is considered to have an elevated risk for alcohol, and the midwife initiates a BI using MI-technique with the aim of motivating behavioral change. MI implies that the midwife engages the woman in a discussion of health risks with alcohol exploring the woman's alcohol habits while maintaining an empathic and non-judgmental attitude. The aim is to identify and strengthen the woman's own arguments against drinking through a motivational discussion about her attitude towards alcohol. Hence, the intervention provides more than merely health information since it is aimed at mobilizing the woman's own motivation to modify alcohol behavior. The midwife also supports behavioral change throughout the pregnancy through reoccurring supportive motivational talks. In some situations, or if the woman scores a very high (above 9) on the questionnaire, the midwife refers the woman to other professions such as counselors, the social service, and/or an alcohol dependency clinic (Folkhälsoinstitutet 2014; Damström Thakker 2011; Västra Götalandsregionen 2008).

The decision rule to provide the BI to women scoring 6 or higher is rather arbitrary and alternative possible cut points have been suggested. Originally, the threshold for identifying risky alcohol consumption in the general population was set to 8. Studies later showed that the AUDIT test had higher sensitivity and specificity for women than men, suggesting a threshold of 5 or 6 for women, and even as low AUDIT score as 3 has been suggested (Reinert and Allen 2007).

3 Data

This study uses data on pregnant women between the years 2010 and 2014 from the Swedish Maternal Health Care Register. The register was initiated in 1999 in order to improve the quality of care and to enable monitoring and evaluation of the maternity health care, and it is managed by the medical profession. The register is based on a local organization of participating maternity clinics and although participation is not mandatory, compliance is high. The coverage of individual data varies in the studied period from 81 percent to 89 percent and data is registered manually by midwives in the maternity clinic. Since the register was initiated from within the profession and is used by the maternity clinics for benchmarking quality and to compare procedures, the incentives to provide accurate information should be high.[2] For our purposes the data contains information about the mother's AUDIT score and self-reported health status and tobacco use before and at the early stages of the pregnancy. There is also background information about the mothers such as education, country of origin and employment status. Moreover, the data contains post-birth information such as the birth weight of the child, gestational age, and information on behaviors of the mother that could be important for child health such as smoking habits in late pregnancy and whether the mother breastfed the child 4 weeks after birth.

Although the AUDIT test is graded on a 0–40 scale, only the lower range of the scale is in effect relevant; 98 percent of the women in our data have scores of 8 or lower. Figure 1 shows the distribution of AUDIT scores between 0 and 20.[3] Almost 25 percent of the respondents have AUDIT score 0, around 40 percent have scores 1–2 and 15 percent score 3 on the scale. For higher scores the frequency decreases rapidly and monotonically. Since women with AUDIT scores 10 and above are exposed to further interventions which includes referral to other professions and clinics, and because there are too few women with a score above 10 for a meaningful analysis, we will focus on the intervention at the threshold 6.

[2] Petersson et al. (2014) finds that the register has good coverage and internal validity, making it reliable for research.

[3] Figure A1 in the Appendix shows the Kernel Density of birth weight.

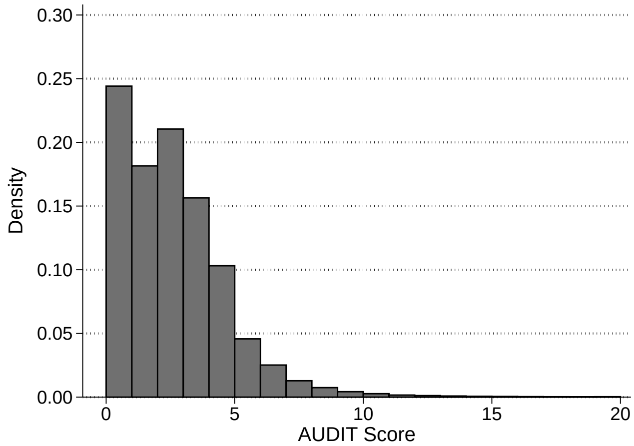


Figure 1 Distribution of AUDIT score 0-20

Figure 2 shows the average birth weight by mothers' AUDIT scores 0–9. Children with mothers with AUDIT scores 1–3 have an average birth weight of about 3550 grams. For higher AUDIT scores the birth weight decreases with the AUDIT score. As illustrated with the vertical bars, the standard error increases with AUDIT score due to the lower number of women with high scores. Women with AUDIT score 0 have children with noticeable lower birth weight. Examining the predetermined characteristics reveals that women with AUDIT score 0 is a selected group.

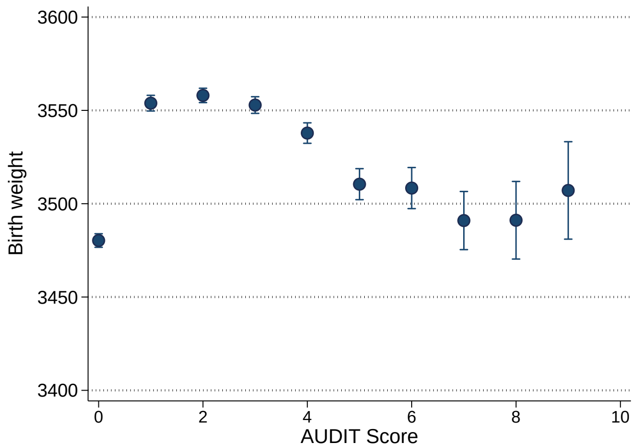


Figure 2: AUDIT score by birth weight

Note: The figure displays the average birth weight by AUDIT score. The vertical bars illustrate the 95 percent confidence interval.

Summary statistics for the variables included in the analysis are displayed in Table 1. Average birth weight among children with mother's with AUDIT score 0–9 is 3547g, 56 percent of the women have a university education, 81 percent are employed and 15 percent smoked before pregnancy. As can be seen by inspecting column 2, the characteristics of the mothers with AUDIT score 0 differ in several aspects from women with positive AUDIT scores. They are less likely to have a university degree, more likely to have an immigrant background and less likely to use tobacco before the pregnancy. Since mothers with AUDIT score 0 differ in many aspects from women with positive scores they will be excluded from the analysis. Columns 3 and 4 separate the group of women scoring between 1 and 9 according to whether they are subject to the intervention or not. As we can be seen, both in Figure 2 and Table 1, women scoring 6 or above have children with lower birth weight. Moreover, they are less likely to be university educated, employed, and more likely to smoke. This suggests that women scoring above the cutoff are negatively selected, both on observables and most likely also on unobservable characteristics.

In order to assess the impact of this targeted preventive intervention at AUDIT 6 this selection needs to be accounted for; simple comparisons between children of treated and non-treated mothers run the risk of being biased.

Table 1: Descriptive statistics.

	ALL (0-9)	AUDIT 0	AUDIT 1-5	AUDIT 6-9
Child:				
Birth weight	3546.5	3480.4	3549.7	3501.0
	(565.1)	(572.3)	(565.3)	(561.1)
Gestational age	39.37	39.28	39.36	39.40
	(1.914)	(1.922)	(1.909)	(1.983)
Year of birth	2012.2	2012.3	2012.2	2012.1
	(1.381)	(1.365)	(1.381)	(1.365)
Boy %	51.34	51.45	51.32	51.55
	(49.98)	(49.98)	(49.98)	(49.98)
Breastfed in week 4. %	73.41	69.12	73.80	67.97
	(44.18)	(46.20)	(43.97)	(46.66)
Mother:				
University %	56.16	35.91	57.36	38.86
	(49.62)	(47.97)	(49.46)	(48.74)
Employed %	80.96	43.54	81.22	77.36
	(39.26)	(49.58)	(39.06)	(41.85)
Age at partus	30.96	30.15	31.14	28.35
	(5.053)	(5.555)	(4.988)	(5.234)
Immigrant %	9.535	47.31	9.836	5.379
	(29.37)	(49.93)	(29.78)	(22.56)
Height, cm	167.0	164.2	167.0	167.0
	(6.225)	(6.629)	(6.232)	(6.135)
Smoke pre preg. %	14.97	9.553	13.38	37.76
	(35.67)	(29.40)	(34.04)	(48.48)
Snuff pre preg. %	4.357	1.169	3.923	10.44
	(20.41)	(10.75)	(19.41)	(30.58)
Mental illness pre preg. %	6.117	5.885	5.895	9.235
	(23.96)	(23.53)	(23.55)	(28.95)
Poor health pre preg. %	2.189	3.717	2.143	2.837
	(14.63)	(18.92)	(14.48)	(16.60)
Observations	292484	95593	272991	19493

Note: This table presents summary statistics for data on mothers and children born in the years 2010-2014. The means of variables included in the analysis (standard deviation in parenthesis) are presented for different AUDIT-scores.

4 Empirical strategy

The methodological challenge when assessing the effects of being eligible to a motivational BI-intervention is that pregnant women who receive the treatment are different to women who do not, in observable, and most likely also in unobservable dimensions. We address this selection problem by explicitly exploiting the decision rule saying that a pregnant woman who scores 6 or higher on the AUDIT-instrument is subject to the intervention. This rule creates a discontinuous jump in the probability of being treated induced by passing this threshold. Unfortunately there is no available data on the MI-intervention which implies that we do not know which individuals who receive the treatment. Therefore, we apply a reduced form regression discontinuity design (RDD) to identify the causal effect of being eligible to treatment on child health and maternal behavior using the discontinuity resulting from the decision rule at the maternity clinics. The RDD approach implies that we compare health outcomes of children to mothers scoring just below and just above the AUDIT score cutoff. The RDD gives an unbiased estimate of the causal effect as long as confounding factors do not change discontinuously, and no other intervention takes place, at the threshold.

Before turning to the empirical analysis we need to choose over which range of AUDIT scores we should conduct the analysis and the functional form of the running variable. Ideally we would like to compare identical individuals at the threshold for whom assignment to treatment is essentially random. In our setting where the underlying alcohol risk is measured in integer values, this is not possible. The distance between mothers with AUDIT scores of 5 and 6 may be too large for them to be comparable with respect to underlying characteristics. We therefore need to make a projection of the outcome for women and their children on both sides of the threshold. In order to do this we have to expand the range around the threshold to allow us to estimate the relation between AUDIT-scores and the outcome. However, as individuals further away from the threshold are included, the underlying relation between AUDIT-scores and the outcome may change. This has to be accounted for with the functional form, and the risk of having a wide range and using flexible functional forms for the control function is that the projection at the threshold becomes sensitive to the modeling of individuals far away from the threshold (Gelman and Imbens 2017).Hence, there is an argument for estimating the control function locally. But there is also an efficiency argument for expanding the range around the threshold.

In our setting we have a substantial amount of data around the threshold and expanding the range above the threshold will, in fact, increase noise as there are successively fewer individuals with higher AUDIT scores. To avoid this problem we will use the AUDIT score range 3-8 and 4-7. The relationship between birth weight and AUDIT score displayed in Figure 2 suggests a non-linear relationship between the AUDIT score and birth weight. But when restricting the range closer to the AUDIT 6 threshold a linear relationship looks like a better approximation.[4]

On both samples we estimate the following model:

$$Y_i = \alpha + \beta T_i + \gamma_1 (AUDIT_i - 6) + \gamma_2 (AUDIT_i - 6) T_i + \gamma_3 (AUDIT_i - 6)^2 + \varepsilon_i \qquad (1)$$

where

$$T_i = \begin{cases} 0 \text{ if AUDIT} < 6 \\ 1 \text{ if AUDIT} \geq 6 \end{cases} \qquad (2)$$

Y_i denotes the outcome of child/mother i. β is the coefficient of interest and it captures the causal effect of the intervention on the outcome. γ_1, γ_2, and γ_3 reflect the control function and capture the relationship between AUDIT score and the outcome. We vary the flexibility of the control function in three different ways: first we set $\gamma_2 = \gamma_3 = 0$ implying that we use a common linear relation across AUDIT scores ranges 4-7 and 3-8. Second, for the wider sample of mothers between 3 and 8 we set $\gamma_3 = 0$ and estimate equation 1 using separate linear function for AUDIT scores 3-5 and 6-8. In the third model specification we set $\gamma_2 = 0$, and use a common second order polynomial across all AUDIT scores 3-8. Moreover, we include controls for birth year and sex of the child,

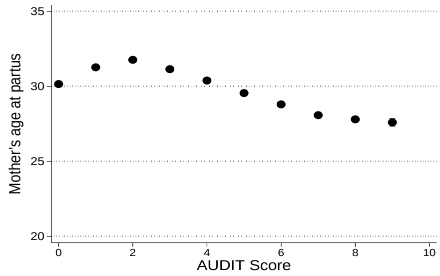
[4] As an alternative, we have also estimated the effect using a local linear regression over the AUDIT range 2-9 weighted using a triangular kernel. The triangular kernel assigns linearly decreasing weights to observations on each side of the treatment cutoff. The results from these estimates are presented in Table A1 in Appendix, and are very similar to the baseline results presented in section 5.1.

and maternity clinic fixed effect to reduce residual variation. As discussed by Lee and Card (2008), a discrete treatment determining covariate may introduce a grouped error component for each value of the discrete covariate. We therefore cluster the standard errors on maternity clinic specific AUDIT score in all specifications. In auxiliary analyses we also study whether there are differences in the effect of the intervention in different parts of the birth weight distribution by estimating unconditional quantile regressions (Firpo et al. 2009).

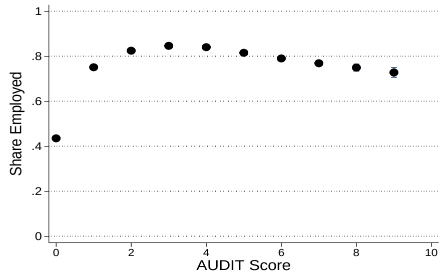
The main outcome in our analysis is child health measured by birth weight, but we also study gestational age, whether the intervention has an effect on the probability of passing the low birth weight threshold of 2500 grams, and the probability of being born prematurely. Moreover, we also test whether the intervention has effects on more general maternal behaviors which should be important for child health such as whether the mother is breastfeeding the child 4 weeks after birth and whether the mother has quit smoking during pregnancy. Smoking may be connected to alcohol consumption because of its cultural association, but use of MI-techniques may also extend beyond motivating reductions in risky alcohol consumption by affecting other behaviors having adverse effects on the child, such as smoking.

The key assumption in a RDD is that subjects do not have control over the forcing variable – in this case the AUDIT score. Although pregnant women are likely to be unaware of the institutional rule that 6 is the cutoff, midwives may induce some women to pass the threshold if they have concerns for the health of the pregnant woman and the child. The distribution of women across AUDIT scores in Figure 1 show no excess mass at either side of the threshold suggesting there is no manipulation of the scores at the threshold. Exogeneity of the intervention can also be examined by analyzing whether predetermined covariates are balanced at the cutoff of the forcing variable. As can be seen in Figure 3 there is no clear jump in any of the pre-determined characteristics of the mothers at the threshold.

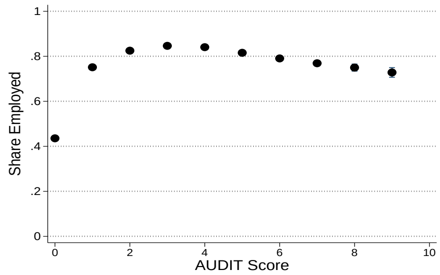
In Table 2, where we more formally test for exogeneity of the models described above by estimating "effects" of passing the AUDIT cutoff on pre-pregnancy characteristics of the mother, we however do find small and statistically significant differences at the thresh-



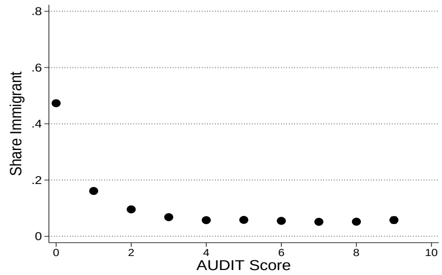
(***a***) Age at partus



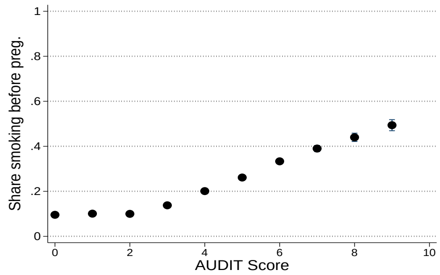
(***b***) University educated



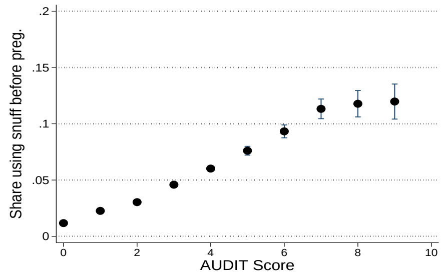
(***c***) Employed



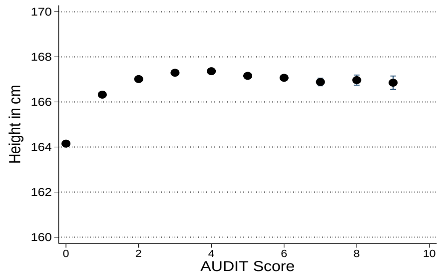
(***d***) Immigrant



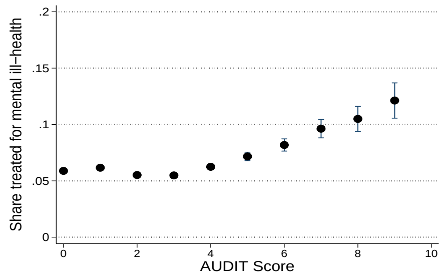
(***e***) Smoke



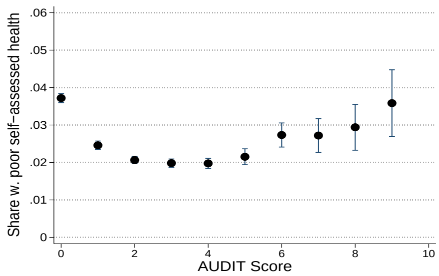
(***f***) Snuff



(***g***) Height



(***h***) Treated for mental illness



(***i***) Poor self-assessed health

Figure 3: AUDIT score by maternal characteristics

Note: The figure displays the mean of different maternal characteristics by AUDIT score. The vertical bars illustrate the standard deviation.

old for some of the characteristics. The first column shows the results for the model with linear control function using the AUDIT score range 4-7. According to the results there is a slightly increased probability of mothers being older, having a university degree and having poor self-assessed health prior to pregnancy at the threshold. Judging by the results in column two, the model with a joint linear control function estimated on the AUDIT range 3-8 does not work well. At the threshold there is an increased probability that mothers are older, university educated, and have an immigrant background. There is also a lower probability that mothers are employed, and they are shorter[5]. In the model with separate linear control function, and in the last model with a second order polynomial on the AUDIT range 3-8, the women are more likely to have poor self-assessed health prior to pregnancy at the threshold. This suggests a risk that effects found on infant health could be due to differences in underlying characteristics.

In order to quantify the impact of these imbalances we regress birth weight on all these background characteristics including a fixed effect for each maternity clinic, and evaluate the joint influence of the obtained significant coefficients from Table 2. This calculation for the model in Column 1 suggests that children to mothers just above the AUDIT 6 threshold weigh 0.08 grams more, relative to mothers just below the threshold, due to these unbalances. Similar calculations for the models in Columns 2, 3, and 4 suggest that the unbalances in maternal characteristics result in a lower birth weight of 5.55 grams, 0.33 grams, and 0.29 grams respectively. Hence the potential bias due to imbalance in background characteristics appears to be fairly limited.

Based on the discussion and results above our preferred specifications are the model with a linear control function and AUDIT score range 4–7 and the model with separate linear control function for AUDIT scores 3–5 and 6–8. The first model is using information close to the threshold and the second allows for the control function to capture shifts in the relation between AUDIT-score and infant health. To investigate whether the pre-determined characteristics are affecting the results we will include them as control variables as robustness test. Although the model with a second order polynomial control function over the AUDIT score range 3–8 performed well in Table 2 our concern is that it

[5] Maternal height has been shown to be an important predictor for birth weight (Cawley et al. 1954).

Table 2: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 on predetermined maternal characteristics

	(1)	(2)	(3)	(4)
Panel A: Maternal age at partus				
AUDIT≥6	0.159*	0.181**	-0.175	-0.044
	(0.091)	(0.077)	(0.108)	(0.092)
R-squared	0.135	0.130	0.130	0.130
Panel B: Probability of university				
AUDIT≥6	0.017**	0.023***	0.001	0.008
	(0.009)	(0.008)	(0.010)	(0.009)
R-squared	0.185	0.167	0.168	0.168
Panel C: Probability of employment				
AUDIT≥6	0.009	-0.013**	-0.002	-0.003
	(0.007)	(0.006)	(0.009)	(0.007)
R-squared	0.044	0.034	0.034	0.034
Panel D: Probability of immigrant background				
AUDIT≥6	-0.006	0.006*	-0.002	-0.001
	(0.004)	(0.003)	(0.005)	(0.004)
R-squared	0.040	0.044	0.044	0.044
Panel E: Probability of smoking prior to pregnancy				
AUDIT≥6	0.010	0.003	0.016	0.012
	(0.009)	(0.007)	(0.010)	(0.009)
R-squared	0.082	0.087	0.087	0.087
Panel F: Probability of using snuff				
AUDIT≥6	-0.002	0.002	0.001	0.001
	(0.004)	(0.003)	(0.005)	(0.004)
R-squared	0.071	0.059	0.059	0.059
Panel G: Height in cm				
AUDIT≥6	0.130	-0.176*	-0.120	-0.100
	(0.119)	(0.093)	(0.130)	(0.111)
R-squared	0.021	0.016	0.016	0.016
Panel H: Probability of being treated for mental ill-health				
AUDIT≥6	0.003	0.005	0.000	0.002
	(0.005)	(0.004)	(0.006)	(0.005)
R-squared	0.028	0.021	0.021	0.021
Panel I: Probability of poor self-assessed health				
AUDIT≥6	0.005*	0.007***	0.008**	0.007***
	(0.003)	(0.002)	(0.003)	(0.003)
R-squared	0.019	0.012	0.012	0.012
Polynomial	1st Joint	1st Joint	1st Separate	2nd Joint
Audit range	4-7	3-8	3-8	3-8
Covariates	Basic	Basic	Basic	Basic
Observations	57,124	107,871	107,871	107,871

Note: The table presents reduced form RD estimates of the effect of Audit Score 6 on different maternal characteristics. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). Basic controls include birth year fixed effects, maternity unit fixed effects, and dummy for child's gender. * Significant at 10%; ** at 5%; *** at 1%.

uses the curvature to approximate a shift in the underlying relation between AUDIT-score and birth weight at the threshold: We prefer the linear model that allows for a shift in the slope at the threshold to this specification. Another concern is that we have too few data points to fit a higher order polynomial.

5 Results

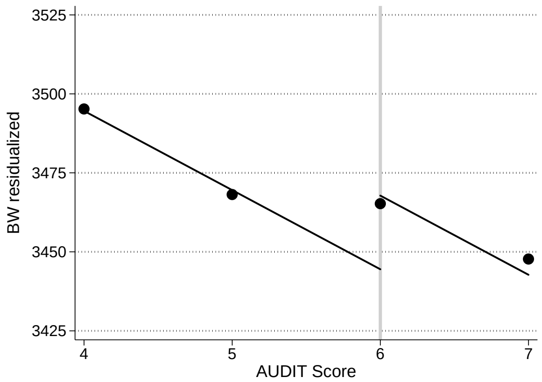
We now turn to the results with respect to the effects of becoming targeted for a preventive BI using MI for pregnant women with elevated alcohol risk. The primary outcome is birth weight, and we provide evidence on both the average effects and the effects over the distribution of birth weight. In robustness analyses we test how sensitive the results are to the inclusion of control variables. We also analyze whether passing the AUDIT 6 threshold affects other measures that could impact infant health: probability of having low birth weight (below 2500 grams), gestational age, and probability of preterm birth (birth before 37 weeks of completed gestation). We additionally present results of the intervention's impact on the likelihood that mothers are breastfeeding their children and if they quit smoking during the pregnancy.

5.1 Birth weight

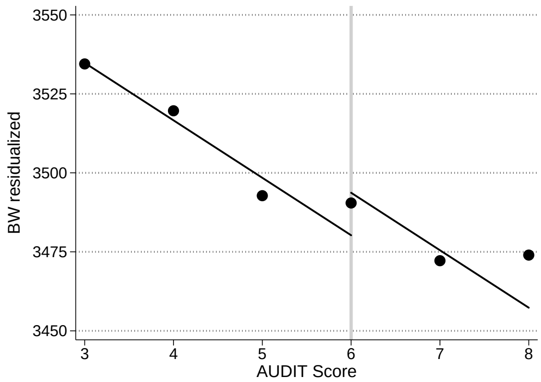
5.1.1 Baseline results

Figure 4 shows graphical evidence of the effect of becoming targeted for the intervention on the child's birth weight (residualized for maternity clinic, child's sex, and birth year).[6] The four plots correspond to the different model specifications discussed in section 4, which use different AUDIT-score range and varying flexibility of the control function. All four plots indicate a small increase in birth weight at the threshold, suggesting a positive impact of the intervention.

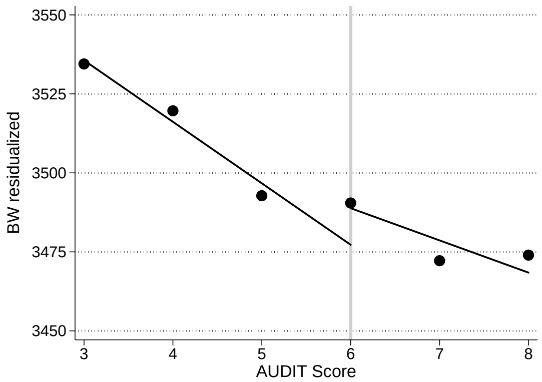
[6] Plots without residualized effects are found in Figure A2 in Appendix.



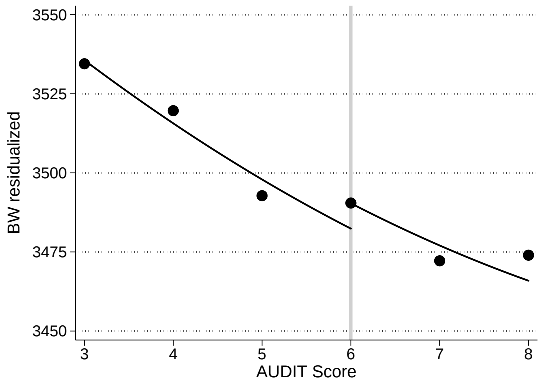
(***a***) 1st order joint polynomial, AUDIT 4-7



(***b***) 1st order joint polynomial, AUDIT 3-8



(***c***) 1st order separate polynomial, AUDIT 3-8



(***d***) 2nd order joint polynomial, AUDIT 3-8

Figure 4: Effect of passing the threshold to AUDIT 6 on birth weight residualized for maternity clinic, child's sex, and birth year

Note:The figure shows the average birth weight by AUDIT score in the ranges 4-7 and 3-8 using different control functions, residualized for maternity clinic, child's sex, and birth year. The vertical line indicates the threshold for being eligible to treatment.

Table 3 shows the corresponding regression estimates of the effect of becoming targeted for the intervention on birth weight using equation 1. All estimations include maternity clinic fixed effects, birth year fixed effects and control for the gender of the child.[7]

The upper left plot in Figure 4 uses the AUDIT-score range 4-7 and a linear specification with a joint slope on both sides of the threshold to capture the effect. This is our most local specification, only using information close to the threshold. The figure displays a discrete jump in birth weight at the threshold. Table 3 Column 1 reports the effect to be 23.6 grams and statistically significant at the 5 percent level, which corresponds to an

[7] Results from estimating the models without control variables show similar results and are reported in Tables A2 to A4 in Appendix.

Table 3: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 on birth weight.

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Birth Weight
AUDIT$\geq$6	23.628**	13.584*	2.172	7.974
	(9.509)	(7.087)	(10.197)	(8.650)
Observations	73,185	137,348	137,348	137,348
R-squared	0.023	0.020	0.020	0.020
Polynomial	1st Joint	1st Joint	1st Separate	2nd Joint
Audit range	4-7	3-8	3-8	3-8
Covariates	Basic	Basic	Basic	Basic

Note: The table presents reduced form RD estimates of the effect of Audit Score 6 on birth weight. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). Basic controls include birth year fixed effects, maternity unit fixed effects, and dummy for child's gender. * Significant at 10%; ** at 5%; *** at 1%.

increase of 0.67 percent (or 4.2 percent of a standard deviation of the birth weight). In the upper right plot we see that the linear specification with a joint slope is a worse fit to data when expanding the AUDIT range to 3-8. The jump in birth weight is slightly smaller for this model; the regression estimate in Column 2 suggests the effect to be 13.6 grams, and significant at the 10 percent level. When instead allowing for separate linear slopes in the range 3-8, in the lower left plot, the model allows for the intervention to shift the underlying relation between AUDIT-score and birth weight. We see that this is a better fit to data. In this specification the discrete jump in birth weight at the threshold is even smaller. The point estimate in Column 3 is 2.2 grams and the effect is not statistically significant. Finally, in the lower right plot we allow for a joint second order polynomial over the range 3-8. The curvature allows this specification to approximate a shift in the underlying relation between AUDIT-score and birth weight at the threshold. The point estimate in Column 4 is slightly larger, 8.0 grams, but the effect is not statistically significant.

On the basis of our preferred specifications (joint linear slope over the range 4-7 and the linear model with separate slope over the range 3-8) the effect of becoming targeted for a preventive BI using MI on the birth weight of children to pregnant mothers with elevated alcohol risk is 0-23 grams, and we can rule out average effects larger than 42 grams. That is, the intervention has a small to negligible average effect on the birth weight of children.

5.1.2 Robustness

In section 4 we saw that our model specifications do not pass all the exogeneity tests, even if background characteristics graphically appear to be smoothly distributed over the distribution of AUDIT-scores. Although calculations of the impact on birth weight from these imbalances in covariates suggest that the bias is small in our preferred specifications (less than 1 gram in absolute value), the estimates found in Table 3 could be biased due to selection at the threshold.

In Table 4 we therefore assess whether our estimates are biased by inspecting how sensitive our baseline estimates are to including different controls for maternal characteristics. In the first panel of Table 4, we extend the control variables to include dummies for mother's education, employment, country of birth, and age. The estimated effects are slightly altered by the inclusion of these controls. The estimate in Column 1 is still statistically significant at the 95 percent level. In panel B, we also include controls related to mother's health and behavior in the form of dummy variables for self-assessed health prior to pregnancy, whether or not the mother had been treated for mental ill-health, height at first visit, and whether or not she used tobacco (cigarettes and snuff) prior to pregnancy. The estimated effect in the column 1 model is slightly reduced whereas the effect in column 2 increases somewhat further when adding these additional controls.[8]

We are reassured by the fact that our preferred specifications remain relatively stable as we include the different sets of controls. This robustness analysis does not lead us to revise that the intervention has a small to negligible average effect on the birth weight of children.

5.1.3 Effects across the distribution of birth weight

Even if the estimated average effect suggests that the intervention has a small to negligible impact on birth weight, this can mask larger impacts in different parts of the weight distribution. The effects may well be larger for children with elevated risk; for example children whose health is more susceptible to alcohol exposure or children who are at

[8] Since the number of observations is reduced due to missing data on some of the control variables in the extended controls, we estimate the model with basic controls on the same amount of observations as in Table 4 and, reassuringly, the results are not altered (see Table A.5 in Appendix).

Table 4: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 in birth weight using different controls.

	(1) Birth Weight	(2) Birth Weight
Panel A: Extended controls 1		
AUDIT$\geq$6	26.281**	3.450
	(10.605)	(11.961)
R-squared	0.028	0.024
Panel B: Extended controls 2		
AUDIT$\geq$6	25.709**	5.681
	(10.521)	(11.880)
R-squared	0.061	0.055
Polynomial	1st Joint	1st Separate
Audit range	4-7	3-8
Observations	57,124	107,871

Note: The table presents reduced form RD estimates of the effect of Audit Score 6 on birth weight. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). Extended controls 1 include birth year fixed effects, maternity unit fixed effects, controls for child's gender, as well as controls for mother's educational level, employment, age, and country of birth. Extended controls 2 include, apart from those just mentioned, controls for tobacco usage and maternal well-being prior to pregnancy. * Significant at 10 %; ** at 5 %; *** at 1 %.

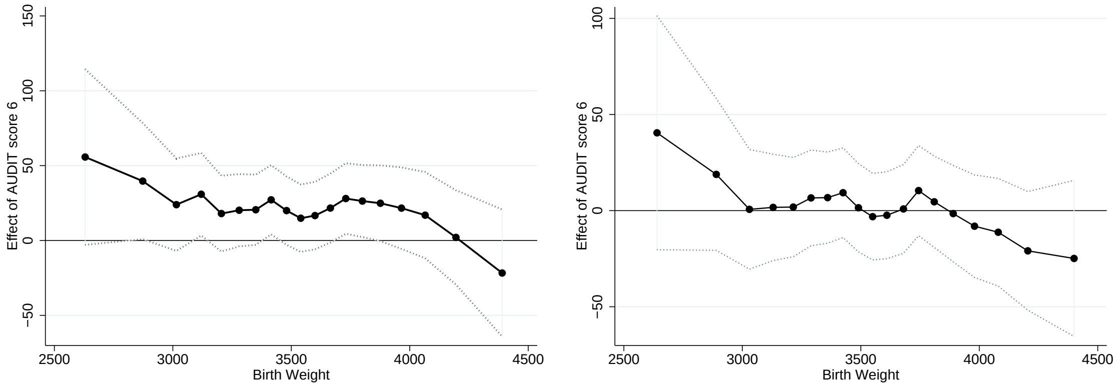
higher risk for other reasons.

In order to assess if effects are heterogeneous by birth weight, we examine how the intervention impacts the quantiles of the distribution of birth weight (Firpo et al. 2009). Figure 5 shows the estimates from an unconditional birth weight quantile regression for our preferred specifications: it tells us how the birth weight quantiles are affected by passing the AUDIT threshold and becoming targeted for the MI-intervention. The large dots represent the point estimates at each quantile.[9] In the left plot we see that for the joint linear specification over the AUDIT-range 4-7 the effect is positive at around 15-25 grams but mostly statistically insignificant (95 % level) across the distribution of normal birth weight children. However, at the lowest quantiles (p=0.05 and p=0.10) the effect increases to 56 grams and with an upper bound of 114 grams.[10] At the highest quantiles (p=95), on the other hand, the estimate becomes negative but is not statistically significant. The right plot shows the corresponding estimates for the separate linear specification over the AUDIT-range 3-8. Also here the estimates are stable across the distribution of normal birth weight children, but are close to zero. Again we find the largest point estimates for the lowest quantiles, but these results do not reach statistical significance. In this model we can rule out effects larger than 100 grams in the lowest quantiles.

The results suggest that the MI-intervention contribute to differential impact across the birth weight distribution: health benefits are larger for infants at higher risk. Still, this analysis does not lead us to revise the view that the intervention has a small to negligible average effect on the birth weight of children.

[9] The estimates from the unconditional quantile regressions for different quantiles are found in Table A6 in Appendix.

[10] Table A.7 in Appendix shows the birth weight of different quantiles in our samples.



(***a***) 1st order joint polynomial, AUDIT 4-7 (***b***) 1st order separate polynomial, AUDIT 3-8

Figure 5: Unconditional quantile effects of passing the threshold to AUDIT 6 on birth weight.
Note:The figure displays the estimates of unconditional quantile regressions with basic controls including birth year fixed effects, maternity unit fixed effects, and dummy for child's gender. The solid line shows how passing the threshold to AUDIT score 6 affects the birth weight quantile (where each dot represents a separate quantile). The dotted lines represent the 95% confidence interval.

5.2 Effects on other measures of infant health

The results from estimating the effects of having an AUDIT score of 6 or higher over the distribution of birth weight suggest that the effect of the treatment is larger for children of low birth weight. If the treatment impacts children at risk rather than children of average birth weight this may be important from a policy perspective. To better understand how the targeted MI-intervention affects child health, we study the likelihood of passing the threshold for low birth weight (2500 grams), gestational age, and the probability of being born prematurely (born before 37 completed weeks of gestation). Furthermore, we test whether there are differences in the size of the effect depending on gestational age.

Table 5 Columns 1 and 2 reports the effects of having an AUDIT score of 6 or higher on the probability of passing the threshold for low birth weight using our preferred specifications. The point estimates in Panel A, when only controlling for the basic covariates, suggest that the probability of being born above 2500 grams is increased by around 0.5-0.6 percentage point, but the effect is only marginally, or not statistically significant. When adding controls for predetermined socioeconomic characteristics the point estimates are stable in size and not statistically significant, as seen in Panels B and C. This suggests that although the effect of the treatment was higher in the lower parts of the birth weight distribution, the treatment has no effect on the probability of passing the low birth weight threshold.

Columns 3 and 4 show the effect of becoming targeted for the MI-intervention on the gestational age in our preferred specifications. The point estimate in Column 3 Panel A suggests that gestational age increases by 0.07 weeks. Although statistically significant, the estimate is small and corresponds to an increase of less than 0.2 percent relative to the average gestational age of 39.3 weeks. The point estimate in Column 4 for the separate linear specification over the AUDIT-range 3-8 is of similar size, and the small effects are stable when adding the different set of controls for predetermined maternal characteristics in Panels B and C.

Columns 5 and 6 show the effect of having an AUDIT score of 6 or higher on the probability of being born prematurely (born before 37 completed weeks of gestation). The point estimate in Column 5 Panel A suggests that the probability of being born preterm is reduced by 0.8 percentage point which corresponds to a reduction of 14 percent relative to the average. As seen in Panels B and C, the size of this effect is stable to the inclusion of extended controls but the statistical significance drops to the 90 percent level when including controls related to maternal health before pregnancy. When including a the full set of controls for predetermined maternal characteristics in Panel C, the point estimate from the model in Column 6 also suggest a reduction in probability of being born prematurely.

In an additional analysis (see Table A8 in the Appendix), we separate the sample of women depending on gestational age in order to test whether the effect of the targeted preventive intervention on birth weight is larger for preterm born infants (born before 37 completed weeks of gestation).[11] While these results suggest that the effect is larger for preterm infants than for children born at term, the estimates become noisy and are not statistically significant in most of the specifications.

Taken together, the results in Table 5 and those from splitting the sample of women according to gestational age do not lead us the change the conclusion that, although there is suggestive evidence of a reduction in risk of being born preterm, the intervention has small to negligible effects on factors related to infant health.

[11] Note that since gestational age is also an outcome, these results myst be interpreted with care.

Table 5: Reduced form RD estimates of the effects of passing the threshold to AUDIT 6 on the likelihood of passing the low birth weight threshold, gestational age, and probability of preterm birth.

	(1)	(2)	(3)	(4)	(5)	(6)
	Above 2500 grams		Gestational Age		Born Premature	
	Panel A: Basic controls					
AUDIT≥6	0.006*	0.005	0.069**	0.063*	-0.008**	-0.006
	(0.003)	(0.003)	(0.033)	(0.037)	(0.004)	(0.004)
R-squared	0.008	0.005	0.014	0.009	0.010	0.006
Observations	73,185	137,348	71,637	134,481	73,185	137,348
	Panel B: Extended controls 1					
AUDIT≥6	0.005	0.004	0.076**	0.068*	-0.009**	-0.008
	(0.003)	(0.004)	(0.036)	(0.041)	(0.004)	(0.005)
R-squared	0.011	0.007	0.018	0.012	0.012	0.008
Observations	57,124	107,871	57,124	107,871	57,124	107,871
	Panel C: Extended controls 2					
AUDIT≥6	0.005	0.004	0.076**	0.071*	-0.009*	-0.008*
	(0.003)	(0.004)	(0.036)	(0.041)	(0.004)	(0.005)
R-squared	0.015	0.011	0.024	0.018	0.015	0.010
Observations	57,124	107,871	57,124	107,871	57,124	107,871
Polynomial	1st Joint	1st Separate	1st Joint	1st Separate	1st Joint	1st Separate
Audit range	4-7	3-8	4-7	3-8	4-7	3-8

Note: The table presents reduced form RD estimates of the effect of Audit Score 6 on the probability of birth weight above 2500 grams, on the gestational age in weeks, and on the probability of being born premature. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). Basic controls include birth year fixed effects, maternity unit fixed effects, and dummy for child's gender. Extended controls 1include birth year fixed effects, maternity unit fixed effects, controls for child's gender, as well as controls for mother's educational level, employment, age, and country of birth. Extended controls 2 include, apart from those just mentioned, controls for tobacco usage and maternal well-being prior to pregnancy. * Significant at 10%; ** at 5%; *** at 1%.

5.3 Effects on pregnant women's behavior

In addition to the direct effects on birth weight, we also examine whether the targeted MI-intervention has effects on a wider range of maternal behavior. There are several arguments for why the intervention could affect also other dimensions of mothers' behavior. Activities such as smoking can for example be complementary to alcohol consumption, and it can also be that midwifes at the targeted MI-interventions are able to promote behavioral changes in other dimensions that are beneficial to the child.

In Grönqvist et al (2016) we find that the introduction of the Risk drinking project within Swedish maternity care had effects on maternal behavior extending beyond the birth of the child and on a wider range of health behaviors. In fact we find evidence of reduced smoking during pregnancy and suggestive evidence of increased breastfeeding, but it is not clear whether it was the targeted intervention or if it was other parts of the program that generated the effects. We therefore analyze if passing the AUDIT threshold and becoming targeted for the MI-intervention affects the likelihood that the child is exclusively breastfed one month after delivery and whether the mother quit smoking during pregnancy. We restrict attention to our preferred specifications.

Table 6 Column 1 reports the effects of having an AUDIT score of 6 or higher on the probability of breastfeeding for the joint linear specification over the AUDIT-range 4-7. The point estimate in Panel A, where we only control for the basic covariates, suggests that the likelihood of breastfeeding is increased by 1 percentage point, but the effect is not statistically significant. In Panel C where we also control for predetermined socioeconomic characteristics (dummies for mother's education, employment, country of birth, and age) and controls related to mother's health and behavior (dummies for self-assessed health prior to pregnancy, whether or not the mother have been treated for mental ill-health, height at first visit, and whether or not she used tobacco (cigarettes or snuff) prior to pregnancy) we find that the estimated effect is reduced to 0.8 percentage points and still not statistically significant. In column 2 we see a similar pattern when using the separate linear specification over the AUDIT-range 3-8: The estimated effect is relatively unaffected as we add additional control variables in Panels B and C. These results suggest that the targeted MI-intervention has no impact on the likelihood of breastfeeding, unlike the results found in Grönqvist et al (2016).

Columns 3 and 4 show the effect of becoming targeted for the MI-intervention on the probability of smoke cessation. The outcome variable is an indicator for whether the pregnant woman smoked at registration in week 8-12 but not in week 32. In section 4 we saw that for our preferred specifications, the likelihood of smoking prior to the pregnancy is higher for mothers passing the AUDIT threshold. In this analysis it is therefore important to control for tobacco use (cigarettes or snuff) prior to pregnancy. In column 3, which reports the effects for the joint linear specification over the AUDIT-range 4-7, we in Panel A find a positive and statistically significant effect of being eligible to treatment on probability of ceasing to smoke: The estimate suggests that the probability to quit smoking between registration and week 32 is increased by 0.6 percentage points, corresponding to 23 percent at the mean. When adding controls for maternal characteristics in Panel B, the estimate is unchanged. The effect is however reduced in size and becomes statistically insignificant in Panel C when controlling for tobacco use before pregnancy. In column 4, where we use the separate linear specification over the AUDIT-range 3-8, the estimates are closer to zero (and become slightly smaller when controlling for previous tobacco use). Hence, we find no support that the reduced likelihood of smoking following the introduction of the Risk drinking project (reported in Grönqvist et al. 2016) follows from the targeted MI-intervention.

Table 6: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 on breastfeeding and smoking.

	(1)	(2)	(3)	(4)
	Probability of breastfeeding		Probability of smoke cessation	
	Panel A: Basic controls			
AUDIT≥6	0.010	0.014	0.006**	0.005
	(0.008)	(0.009)	(0.003)	(0.004)
R-squared	0.029	0.023	0.023	0.019
Observations	60,475	113,426	72,098	135,506
	Panel B: Extended controls 1			
AUDIT≥6	0.006	0.012	0.006*	0.001
	(0.009)	(0.010)	(0.003)	(0.004)
R-squared	0.049	0.043	0.041	0.034
Observations	47,658	89,925	56,698	107,119
	Panel C: Extended controls 2			
AUDIT≥6	0.008	0.014	0.004	-0.000
	(0.010)	(0.010)	(0.003)	(0.004)
R-squared	0.061	0.054	0.111	0.109
Observations	47,658	89,925	56,698	107,119
Polynomial	1st Joint	1st Separate	1st Joint	1st Separate
Audit range	4-7	3-8	4-7	3-8

Note: The table presents reduced form RD estimates of the effect of Audit Score 6 on the probability of breastfeeding 4 weeks after pregnancy and on the probability of smoke cessation. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8).Basic controls include birth year fixed effects, maternity unit fixed effects, and dummy for child's gender. Extended controls 1include birth year fixed effects, maternity unit fixed effects, controls for child's gender, as well as controls for mother's educational level, employment , age, and country of birth. Extended controls 2 include, apart from those just mentioned, controls for tobacco usage and maternal well-being prior to pregnancy. * Significant at 10%; ** at 5%; *** at 1%.

6 Conclusions

In this paper, we have evaluated whether targeted preventive BI impacts infant health and maternal behavior such as breastfeeding and smoking. Based on the decision rule at Swedish maternity clinics to initiate a BI using MI techniques to women who score 6 or higher on the AUDIT instrument, we applied a reduced form RDD to identify the causal effect of being eligible to treatment.

We find that the targeted alcohol preventive MI-intervention has small to negligible average effect on infant health measured by birth weight. Estimating the effect of the BI across the distribution of birth weight suggests that the impact is larger in the lowest quintile of birth weight, which indicates that health benefits are larger for infants at risk. Overall however, the magnitude of the effects across the distribution of birth weight is stable and small. Since we have no direct information on MI-intervention we cannot determine whether the small to negligible effects on birth weight is due to a low effectiveness of the targeted MI-intervention or whether the take-up of the intervention is low despite the decision rule.

Results from the analysis where we study gestational age and the probability of being born above the low birth weight threshold of 2500 grams further support the conclusion that the targeted alcohol preventive MI-intervention has minor effects on infant health. We do however document suggestive evidence that being eligible for treatment reduces the probability of being born prematurely.

We find no evidence of the BI leading to more women breastfeeding or ceasing to smoke during the pregnancy. Grönqvist et al. (2016) finds that the introduction of the screening and BI program improved infant health by having an impact on maternal behavior. Given that the focus in this study is on different outcomes, it is difficult to directly compare the results to those in Grönqvist et al. (2016). It is therefore not possible to definitely conclude whether it is the BI targeted towards women at risk or if it is the program at large that affected maternal behavior after birth.

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Appendix

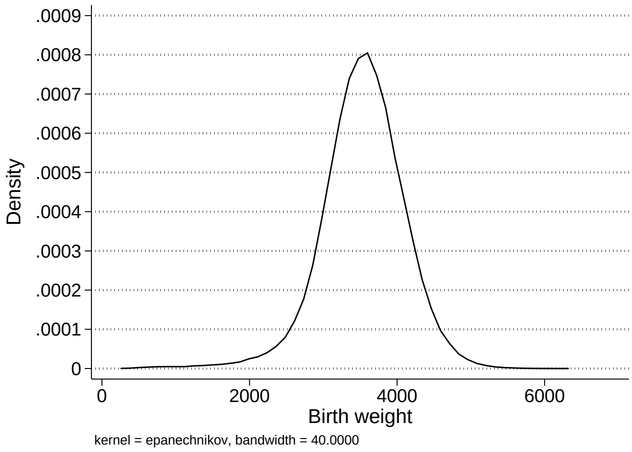
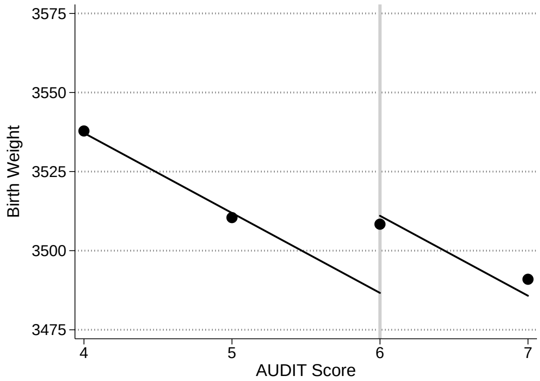
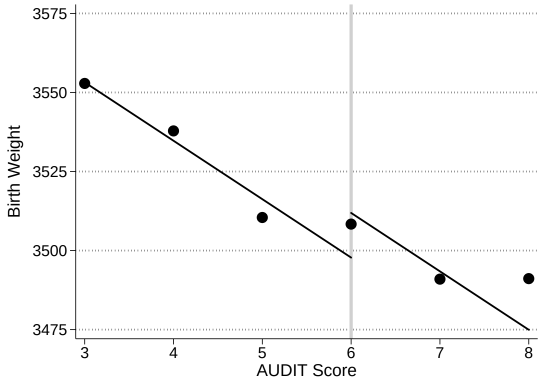


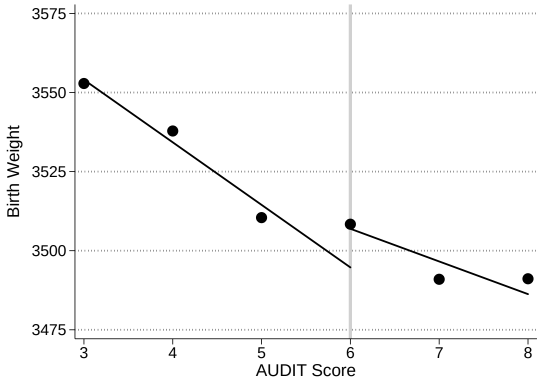
Figure A1: Kernel Density of Birth Weight



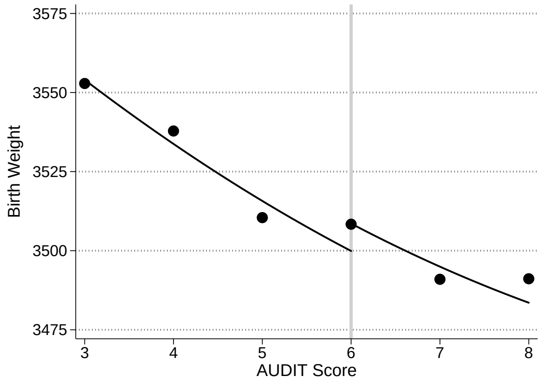
(*a*) 1st order joint polynomial, AUDIT 4-7



(*b*) 1st order joint polynomial, AUDIT 3-8



(*c*) 1st order separate polynomial, AUDIT 3-8



(*d*) 2nd order joint polynomial, AUDIT 3-8

Figure A2: Effect of passing the threshold to AUDIT 6 on birth weight, not residualized

Note:The figure shows the average birth weight by AUDIT score in the ranges 4-7 and 3-8 using different control functions. The vertical line indicates the threshold for being eligible to treatment.

Table A1: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 on birth weight using triangular weights.

	(1) Birth Weight	(2) Birth Weight
AUDIT≥6	15.630**	5.982
	(7.039)	(10.136)
Observations	137,348	137,348
R-squared	0.020	0.020
Polynomial	1st Joint	1st Separate
Audit range	2-9	2-9
Covariates	Basic	Basic

Note: Standard errors in parenthesis are clustered at unit*bin level. The Table shows the effect of being eligible to treatment using weighted local linear regression. We use a triangular kernel, as suggested by Lee and Lemieux (2010), which assigns linearly decreasing weights to each observation which decrease with the distance to the AUDIT cutoff of 6. This implies that observations farther away from the cutoff are given less importance in the estimations. The weights are constructed manually and put weight 0 on observations with AUDIT score 2 and 9 (implying that these observations are not included in the estimations), small weights on observations scoring 3 and 8, slightly higher on observations scoring 4 and 7, and the highest weight on observations scoring 5 and 6. Column 1 shows the results for the specification with a joint linear slope and Column 2 show the results from the model with a separate linear slope. * Significant at 10%; ** at 5%; *** at 1%.

Table A2: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 on birth weight, estimated without controls.

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Birth Weight
AUDIT≥6	23.493**	13.661	2.612	8.326
	(11.250)	(8.536)	(10.848)	(9.375)
Observations	73,185	137,348	137,348	137,348
R-squared	0.001	0.001	0.001	0.001
Polynomial	1st Joint	1st Joint	1st Separate	2nd Joint
Audit range	4-7	3-8	3-8	3-8
Covariates	No	No	No	No

*Note:*The table presents reduced form RD estimates of the effect of Audit Score 6 on birth weight. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). * Significant at 10%; ** at 5%; *** at 1%.

Table A3: Reduced form RD estimates of the effects of passing the threshold to AUDIT 6 on breastfeeding and smoking, estimated without controls.

	(1)	(2)	(3)	(4)
	Probability of breastfeeding		Smoke Cessation	
AUDIT≥6	0.008	0.013	0.006	0.004
	(0.013)	(0.012)	(0.005)	(0.005)
Observations	60,475	113,426	72,098	135,506
R-squared	0.001	0.003	0.002	0.004
Polynomial	1st Joint	1st Joint	1st Separate	2nd Joint
Audit range	4-7	3-8	3-8	3-8
Covariates	No	No	No	No

*Note:*The table presents reduced form RD estimates of the effect of Audit Score 6 on the probability of breastfeeding 4 weeks after pregnancy and on the probability of smoke cessations. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). * Significant at 10%; ** at 5%; *** at 1%.

Table A4: Reduced form RD estimates of the effects of passing the threshold to AUDIT 6 on likelihood of passing the low birth weight threshold, on gestational age, and on probability of being born premterm, estimated without controls.

	(1)	(2)	(3)	(4)	(5)	(6)
	Above 2500 grams		Gestational Age		Born Premature	
AUDIT≥6	0.006*	0.005	0.068	0.063	-0.008*	-0.006
	(0.003)	(0.003)	(0.042)	(0.042)	(0.005)	(0.005)
Observations	73,185	137,348	71,637	134,481	73,185	137,348
R-squared	0.000	0.000	0.000	0.000	0.000	0.000
Polynomial	1st Joint	1st Separate	1st Joint	1st Separate	1st Joint	1st Separate
Audit range	4-7	3-8	4-7	3-8	4-7	3-8
Covariates	No	No	No	No	No	No

*Note:*The table presents reduced form RD estimates of the effect of Audit Score 6 on the probability of birth weight above 2500 grams, on the gestational age in weeks, and on probability of being born premature. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). * Significant at 10%; ** at 5%; *** at 1%.

Table A5: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 on birth weight, estimated with basic controls using the reduced sample for which we have information on all covariates.

	(1) Birth Weight	(2) Birth Weight	(3) Birth Weight	(4) Birth Weight
AUDIT≥6	27.970***	14.419*	3.003	9.282
	(10.609)	(8.053)	(11.942)	(10.041)
Observations	57,124	107,871	107,871	107,871
R-squared	0.025	0.021	0.021	0.021
Polynomial	1st Joint	1st Joint	1st Separate	2nd Joint
Audit range	4-7	3-8	3-8	3-8
Covariates	Basic	Basic	Basic	Basic

Note: The table presents reduced form RD estimates of the effect of Audit Score 6 on birth weight. Standard errors in parenthesis are clustered at unit*bin level (2190 clusters in AUDIT range 4-7, and 3223 clusters in AUDIT range 3-8). Basic controls include birth year fixed effects, maternity unit fixed effects, and dummy for child's gender. * Significant at 10%; ** at 5%; *** at 1%.

Table A6: Unconditional quantile regression estimates of the reduced form effect of passing the threshold to AUDIT 6 on birth weight

	5th (1)	10th (2)	25th (3)	50th (4)	75th (5)	90th (6)	95th (7)
				Panel A: Range 4-7, 1st joint			
AUDIT≥6	55.745*	39.681**	17.995	14.880	24.882*	2.068	-21.709
	(29.928)	(19.797)	(12.913)	(11.473)	(12.905)	(16.082)	(21.646)
Observations	73,185	73,185	73,185	73,185	73,185	73,185	73,185
R-squared	0.009	0.011	0.016	0.021	0.022	0.017	0.015
Polynomial	1st Joint	1st Joint	1st Joint	1st Joint	1st Joint	1st Joint	1st Joint
Audit range	4-7	4-7	4-7	4-7	4-7	4-7	4-7
				Panel B: Range 3-8, 1st separate			
AUDIT≥6	40.469	18.840	1.852	-3.141	-1.540	-20.928	-24.877
	(31.026)	(20.145)	(13.175)	(11.495)	(12.790)	(15.732)	(20.720)
Observations	137,348	137,348	137,348	137,348	137,348	137,348	137,348
R-squared	0.005	0.007	0.013	0.018	0.018	0.014	0.011
Polynomial	1st Sep.	1st Sep.	1st Sep.	1st Sep.	1st Sep.	1st Sep.	1st Sep.
Audit range	3-8	3-8	3-8	3-8	3-8	3-8	3-8
Covariates	Basic	Basic	Basic	Basic	Basic	Basic	Basic

*Note:*The table presents the estimates from the unconditional quantile regressions. Each column shows how passing the threshold to AUDIT score 6 affects the birth weight at a specific quantile. Basic controls include birth year fixed effects, maternity unit fixed effects, and dummy for child's gender* Significant at 10%; ** at 5%; *** at 1%.

Table A7: Average birth weight at each quantile for two different AUDIT ranges.

	AUDIT 4-7	AUDIT 3-8
Quantile	Birth Weight (g)	
5th	2630	2640
10th	2873	2890
15th	3015	3030
20th	3120	3130
25th	3205	3215
30th	3280	3290
35th	3350	3360
40th	3415	3425
45th	3480	3490
50th	3540	3550
55th	3600	3610
60th	3665	3680
65th	3730	3744
70th	3800	3810
75th	3875	3890
80th	3965	3980
85th	4065	4080
90th	4195	4205
95th	4390	4400

Table A8: Reduced form RD estimates of the effect of passing the threshold to AUDIT 6 on birth weight where sample is split according to gestational age.

	(1)	(2)	(3)	(4)
	Preterm Birth		Term Birth	
	Birth Weight	Birth Weight	Birth Weight	Birth Weight
AUDIT$\geq$6	31.688	27.000	13.532	-7.040
	(56.294)	(58.498)	(8.630)	(8.951)
Observations	4,122	7,538	69,063	129,810
R-squared	0.136	0.088	0.031	0.028
Polynomial	1st Joint	1st Joint	1st Separate	2nd Joint
Audit range	4-7	3-8	3-8	3-8
Covariates	Basic	Basic	Basic	Basic

*Note:*The table presents reduced form RD estimates of the effect of Audit Score 6 on birth weight, separated according to gestational age. Standard errors in parenthesis are clustered at unit*bin level. Basic controls include birth year fixed effects, maternity unit fixed effects, and dummy for child's gender. * Significant at 10%; ** at 5%; *** at 1%.